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| **Physics\_1999-** | |
| **ID** | 0069 |
| **Biographical** | A man who knows everything”. This, reportedly, was my reply to a school teacher asking me what I’d like to become when I grow up. I was eight years old, or thereabouts, and what I wanted to say was “professor”, but, still not knowing everything, I had forgotten that word. And what I really meant was “scientist”, someone who unravels the secrets of the fundamental Laws of Nature.  This perhaps was not such a strange wish. Science, after all, was in my family. Just about at that time, 1953, my grand-uncle, [Frits Zernike](https://www.nobelprize.org/nobel_prizes/physics/laureates/1953/index.html) had earned his Nobel Prize for work that had led him to the invention of the phase contrast microscope. He had worked out the theory and singlehandedly constructed his microscope, with which he had stunned biologists by showing them moving images of a living cell. My grandmother, Zernike’s sister, used to tell us anecdotes about her brother when they were young. One day, for instance, he had purchased a telescope at a local market. That night, the police came at their door to warn her parents that there were “zinc thieves on their roof”; it was Frits however, trying out his new telescope and studying the heavens. She herself had married her professor, a well known zoologist, Pieter Nicolaas van Kampen at the University of Leyden. I never knew him, he passed away, after a long illness, when my mother was eighteen years old.  My uncle, Nicolaas Godfried van Kampen was appointed Professor of Theoretical Physics at the State University of Utrecht. My mother did not opt for a scientific career. “It never came up”, she now says, adding that actually math and science were not particularly difficult for her at school, but being a girl, you wouldn’t admit that you actually liked such subjects. She went to art school but later achieved a degree in French, and now she teaches that language in a private class.  Was it the environment or was it in my genes to choose to become a physicist? My grandmother adored scientists and by that she may have further determined my choice, but I think that my mind was made up long before I could talk. A picture was taken of me, at the age of two, studying a wheel. I do not remember the event, of course, but I do remember being fascinated by wheels when other kids were just running around, playing. My very earliest recollections are about being obsessed with phenomena I observed. I watched the ants crawling in the sand, and wondered what life would be like if you were an ant. You would be able to go into the tiniest spaces between the pebbles, and those would be as big as houses for you. But, I realized, an ant’s life must be totally different from ours. Still being a toddler I saw one day how the wheels of two children’s bikes, which were upside down, touched each other. If you turn one wheel, the other one would start rotating as well. You can make one wheel turn by rotating the other. The principle of transmission. How fascinating Nature is. I was well over two years old before I started to speak. Was it because there were so much more interesting things I wanted to understand than to communicate with people? I was also late in reading and writing. This, I remember, was because I thought reading meant being able to decipher my mother’s handwriting.  Though born in Den Helder, I spent my childhood in The Hague, with my parents, my older sister who had changed her official name Elise into Ita as soon as she could talk, and my younger sister Agnes. My father had obtained a degree at Delft in naval engineering. He made his career at the dockyards of the big ocean cruisers of the Holland-America Line. He used to talk of the giants “Maasdam” and “Rijndam” as his ships. Then for a long time he worked at an oil company until he had enough of that. Like his father, he loved ships and all high-tech industry having to do with the sea. Noticing my interest in natural phenomena, he thought that it would be easy to get me interested in engineering as well. He bought me books about ships and car engines, which I never touched. “Those things have already been invented by someone else”, I objected. “I want to investigate Nature and discover new things.”  When I was eight, my family moved for a ten month’s period to London, England, where for the first time I was forced to master a foreign language, English. Too late, my parents discovered that sending their children to a private school would have required registration three years or more ahead. We went to a public school. School uniforms were not required, but there were strict regulations on clothing. One cold day I entered the school in long trousers. I was allowed in because I was a foreigner, and they always were very kind to me, but shorts, reaching until the knee, were the norm for the school. In summer time, during the week-ends, we would make long trips in the beautiful country-side. It seemed that all rain in England fell during the week-ends. I saw my first mountains, that is, hills taller than 100 meters, which hardly exist in the Netherlands. I was thrilled to notice that the tree trunks grow along the lines of gravity and ignore the direction of the slope. I also noticed some fundamental differences in English and Dutch architecture, so that, if you show me some houses, old or new, I can immediately tell the Dutch and English ones apart.  My father made more money than usual, and this afforded him to buy me some expensive boxes of Meccano. It was one of the great things he did for me. However, I had to make a deal with my father. Alternatingly, I would construct a model described in the book, and then construct something out of my own imagination. He thought the models in the book were more instructive, but I preferred my own imagination. The most fantastic things I constructed were robots, that I could persuade to pick up something, although infinite diligence was needed for that.  After primary school I went to the Dalton Lyceum, also in The Hague. It is a school system where students are given extra hours for studying homework material in the presence of teachers, and it worked well for me. After one year the choice was to be made between a non-classical and a classical continuation, the classical one including ancient Greek and Latin, which would take one year more, and it would be more demanding. My uncle said the choice would be immaterial. “You don’t need Latin and Greek for physics”, he said, “but it doesn’t do any harm either.” I chose to take the challenge. Why? I think I couldn’t stand the idea that some kids would learn things I didn’t know. I never regretted the choice.  My father bought me a book about radios, and that did interest me. “You know, Gerard”, a schoolmate had once said to me, “nobody in the world understands how a radio works”. This I found difficult to believe. “Look at all those things inside”, I said, “the guy who designed that must have had some idea.” But if there were any not understood secrets, I was going to find out about them, that I promised myself. The radio in the book had lamps in it, diodes, triodes, pentodes. Later I learned that transistors work the same way, and you could buy sets with complete instructions how to assemble a radio. I would never build a radio before I understood why it had to be assembled precisely this way. Why, for instance, would the designer always suppress the amplification power of a transistor by back coupling? I tried to make an amplifier with fewer transistors and no suppression. Can you make a radio with just one transistor for both the high frequency and the low frequency signal? I learned the answers to all these questions.  Of the modern languages, English, French and German, besides Dutch, were obligatory. I had difficulties with the logic of linguistic arguments and besides, the texts we had to translate were such that even in my own language I could hardly understand what they were about. But I managed, and now I am happy that I can communicate with the inhabitants of a major fraction of Europe.  So much easier were math (of which there was surprisingly much: algebra, analysis, trigonometry, stereometry), physics and chemistry. My physics teacher was a friendly, middle aged man with a small beard and a soft voice. He taught physics using a book that he and another teacher at our school had written, and which was being used throughout the country. It was sound and pedagogical, but not always equally accurate. Where fluids were discussed, it explained that the cross section of an airplane wing has “a droplet form” because “droplets take a shape of least resistance”. Elsewhere, the rainbow is derived, and there droplets were spherical.  Being pedagogical was high on my teacher’s priority list. But he also inspired us and made us think. “If there were any real geniuses in this class”, he would say, “then they could have argued as follows, … “. But then, he assured us, there were of course no real geniuses in this class. Then, there was an interesting page in his book about photons. “A light bulb emits about 109 photons per second,” it said. The argument was simple. “A single photon has a wave packet of about 10-9 seconds long. If there were much more than 109 photons, then for each photon vibrating in this way, you could find another photon vibrating in the opposite direction. You’d have destructive interference, and so there would not be any light.” I had long arguments with him about this. Finally, with the help of my uncle, we could sort things out. This page does not appear in the later editions of the book.  Biology was taught by an elderly lady, too kind for this world. She would never give anyone failing marks, unless someone really asked for it, but high marks were also rare. My marks quickly dropped when the lessons became boring, such as the discussion of symmetry patterns in flowers (I thought the symmetry was never perfect anyway), or incomprehensible, when the human body was discussed (some parts were hardly mentioned, except outside hours among the pupils, and there were things that no-one could explain to me, and I didn’t dare to ask).  Then, one of the teacher consultation days, my father noticed that none of the parents wished to talk to our biology teacher, since she never made anyone fail. He stepped towards her and said: “Did you know Professor P.N. van Kampen?” Of course she did, surely she did, she had attended all his lectures. He was such a scholar, he was so bright! Is Gerard really his grandson? If only had she known! The next day she started with zoology. I was given special attention. Van Kampen’s grandson! My grades skyrocketed. She gave me the assignment to write a thesis. I chose to write about bacteria. Our local library had nothing about bacteria. One pre-war book was there, written in German in Gothic letters. I still don’t know how I managed to produce a thesis using that. But it did not matter. My grade for it was superb.  I had the good fortune of having an enthusiastic art teacher. I suspect it was only because of my good geometric insight that I could make quite realistic drawings. But my mother spotted the weak points in my art. If you want to draw a human face or body, you have to know exactly how the bones and the muscles go, she said, otherwise you do it all wrong, and it doesn’t look good. I was too shy to make a careful study of human bodies, and so I specialized in animals and landscapes. This will never make me a very good artist, I decided.  When I was ten I encountered my first piano. We were on a vacation in the hills in the south-east of Belgium. It was continuously raining during the entire two weeks. The cottage we had rented had an old piano in it. There were a few books with some songs in them. My father explained how the notes relate to the keys on the instrument. “The rest you can figure out by counting”. Both my parents had suffered from compulsory piano lessons when they were young, and had intended not to subject any of their children to such a torment. But now that I wanted them, I could get my piano lessons. I had a private teacher. She was tough. She herself had had lessons from the wellknown Dutch pianist Cor de Groot, and she wanted me to reach similar heights. I had to practice scales. It amazed her that the first time I tried to play a scale simultaneously with left and right hand, I nevertheless had the right idea to switch fingers left and right at different moments. “Most people first do this wrong”, she said. She taught me Beethoven, Chopin, Debussy, Mendelssohn and many others. Much of it was too difficult for me, but I still play many of the pieces, and piano has become part of my life.  At age 16, the opportunity was offered to participate at the Dutch National Math Olympiad. It was the second time the olympiad was held. I passed easily the first round; only by being nervous I had misread the first exercise, which had been done correctly by most other participants. But I had done well with the others, and so I went with some 100 schoolkids to Utrecht for the next round. It was a tough one, and I had missed several questions. On hindsight, the questions had been very good ones, and I had only missed them because of lack of rigorous mathematical training. Today, math questions are phrased in such awkward ways, supersaturated with pedagogical nonsense, that I’d probably have missed them all.  Anyway, it came as a surprise when during a school break my younger sister came rushing towards me. “We searched for you everywhere,” she said, “you’re among the first ten!” The exact order was still kept secret. We came to Utrecht to learn that I had obtained the second prize. It consisted of two volumes of a book by Georg Pólya, “Mathematik und Plausibles Schliessen”, and I devoured them. This was math of a kind that I liked very much. They must have seen by the way I had answered the questions that this was math to my liking. It contained, among other things, Euler’s theorems for polygons in three-dimensional space, and this knowledge would turn out to be quite handy later in my career. I could have been number one in this Olympiad, if I hadn’t flunked the first exercise in the first round, but then, probably, the others too had made avoidable mistakes.  The final examinations at high school, 1964, were tough. My only real problem was the languages, but what about biology? The high grades given by my teacher were ridiculous. Biology would be examined orally, and this time there would be a biology university professor who would independently judge the answers. When I entered the room, the first thing my teacher said to the university professor was: “Now this is Professor van Kampen’s grandson! ” His face brightened, ‘Really?”, he said, he had followed all of Van Kampen’s lectures. Such a brilliant zoologist. And here is his grandson. He must be very bright. They asked something about some obscure sponge. I vaguely remembered the text in the book, and tried to reproduce it. “Yes, yes!”, they cried, “and sometimes it is said that…” and then came the real text, which I had practically forgotten. They gave me a 10 out of 10. I gladly dedicate this result to the memory of my grandfather.  I passed the examination and went to the State University of Utrecht. Leyden was closer to The Hague, but my uncle was teaching at Utrecht, and his lectures I desired to follow. My father insisted that I become a member of the most elite student organization, the Utrecht Studenten Corps. Freshmen were shaven bold. This was actually one of the lesser humiliating things they did; the elderly students had developed a special skill at humiliating their freshmen. Some of the new students had already been in military service; for them, it was all only too familiar, and they had no problem. But I was easy to crack, and they could ridicule my lack of interest in anything but science. “So you wrote a thesis about bacteria? What kinds of bacteria are there?” It was an elderly medicine student who asked the question. When I mentioned the spirochetes, he asked: “and which disease is caused by them?” I knew what he wanted to hear. “Syphilis”, I said. His opinion was that I should go into medicine, not physics.  But now I was near the Theoretical Physics Institute. I had rented a room just around the corner. Theoretical Physics occupied three adjacent houses opposite to a canal. One of the houses was owned by a lady who had introduced herself as a countess. There was some dispute as to whether she really was one. In summertime, when you opened the windows, chicken would hop in from the garden, and walk over the desks. Staff members would have coffee, lunch and discussions in a cellar. Through a narrow window you saw the legs of the pedestrians passing by. In earlier days the cellar had probably been in use by prostitutes. Of course, I was only a first year student, and I was not supposed to come in here. But more often than not, my uncle invited me in, and I adored the discussions, and the laughter.  The student organization forced me to spend time also on other things besides physics, which was exactly why my father had wanted me to become a member. I was coxswain in their celebrated Rowing Club, Triton, where I was appreciated because I could keep their boats coasting in straight lines. There was a student science discussion club, “Christiaan Huygens” where I have many fond memories, and together with some other students I organized a national congress for science students. But it was also at the student clubs where I learned to hate interminable meetings and pointless discussions. Especially the student revolts of the 60’s I found silly and I kept at the greatest possible distance.  I wanted to go into what I saw as the heart of physics, the elementary particles. Unfortunately, my uncle had developed a dislike of the subject. People in that field are very aggressive, he warned. He had also investigated elementary particles, deriving what the mathematical consequences are of the fact that no information can go faster than light. You find equations, he explained, called dispersion relations, but they don’t tell you everything about the particles. He had written a few articles, meticulously deriving these consequences. “And what happened? Others wrote dozens of papers, full of unwarranted assumptions, sloppy arguments and incredible results. But there were so many of those papers, that only they got all the citations. ” He thought that statistical physics was more to his liking.  There was a newly appointed Professor of Theoretical Physics who did specialize in subatomic particles, [Martinus Veltman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1999/index.html), or Tini, as he was normally called. When time came that I had to write an undergraduate thesis, somewhere in 1968, he was the person to advise me and judge me for it. Veltman naturally thought that those high grades of mine were just because of my family background, and if I were any good, he would first need some convincing. This never even bothered me, all I wanted was learn about elementary particles, and if he didn’t think much of me, so be it. First things first, he said. Here is a paper by [C.N. Yang](https://www.nobelprize.org/nobel_prizes/physics/laureates/1957/index.html) and R.L. Mills. This stuff you must know.  Now this was a brilliant paper. It was beautiful, elegant and unique. But it was also considered to be useless. “It describes particles which do not exist in Nature”, Veltman explained, “but in some modified form, they might”. What modified form? To a fellow student, Veltman gave the assignment to study spontaneous symmetry breaking. There was a lot of confusion concerning the so-called Goldstone theorem. Jeffrey Goldstone had derived that spontaneous symmetry breaking implies the existence of massless particles. Spontaneous symmetry breaking could not be the resolution of the Yang-Mills problem because such massless particles do not exist. Later, this would be recognized as just one more example of too much adoration for abstract mathematical theorems; people did not bother to read the small-print, where Goldstone clearly said when his theorem does not apply. I am glad I ignored the problem; I did not understand why people thought there were massless particles if I could not see any in the equations.  My assignment was to study the so-called Adler-Bell-Jackiw anomaly. This was a subject in which Veltman was involved. He had a formal theorem saying that neutral pions cannot decay into photons. But when you actually calculate the decay, you find that it should occur. And the experimental data agree with that: neutral pions decay predominantly into photons. Something is wrong with the formal theorem. It was based upon flawed mathematics. The flaw was something highly interesting, and it would continue to play an interesting role later in particle physics. There were related problems with the eta particle. It decays into three pions while it shouldn’t. The resolution to this problem was still entirely unknown.  They say that organizing a student congress causes one year delay in your studies. But I had never stopped thinking about physics, and I could begin my PhD studies in 1969. In Holland, the PhD is a very serious matter. I remembered my physics teacher being so proud of his thesis. My history teacher obtained his PhD late in his life, and he too had been telling us all about his defense of his lifetime work. Veltman was to be my advisor. He gave me the choice between various topics, but none could catch my imagination more than the subject he himself was working on: the renormalization of Yang-Mills fields. He explained to me that vector fields must be playing an elementary role in the weak interactions, but also in the strong interactions there were vector fields. All these fields were associated to spinning particles with mass. The mass was where the problem started. “These mass terms in the equations look quite innocent”, he explained, “but in the end they impede all my attempts to obtain a finite, meaningful theory.”  But he knew something else. He had studied the experimental data concerning the weak interactions. There, he had found very strong indications that the weak interactions have something to do with the theory of Yang and Mills. “But the matter becomes so complicated that you cannot do it by hand anymore”, he said, and he had started designing a computer program to handle the complicated algebraic expressions. Computers were still in their infancy those days. Today’s simplest hand-held calculators contain more electronic switches and are much faster than the bulky constructions that were called computers then. The monsters had to be fed with paper cards in which you had to punch your programs. His effort was an heroic one.  What I began thinking about was my own version of the Goldstone theorem, but I could not read those pompous mathematical theories. What I reconstructed in my own way was something that actually did exist already: it is now known as the [Higgs](https://www.nobelprize.org/nobel_prizes/physics/laureates/2013/) mechanism, but important elements of it had also been derived by [François Englert](https://www.nobelprize.org/nobel_prizes/physics/laureates/2013/) and Robert Brout. Unfortunately, these ideas were not along Veltman’s line of thought. He wanted to derive everything just by looking at the experimental data, and by performing field transformations for which he could use his computer program. In his opinion, I clearly lacked insight in experimental subjects. Something had to be done about that. We sent my application to various summer schools in theoretical physics. My first choice was a school at Les-Houches, a ski resort high in the French Alps, near Chamonix. Famous French physicists would be teaching there. Presumably because my application was late, I was not admitted.  The next choice was Cargèse, and here I was admitted. Near this small town on the French island Corsica, right at the sea, the French physicist Maurice Lévy had established an Advanced Science Institute, ten years earlier. The story goes that Lévy had looked up in the atlas which French town has the maximal amount of sunshine during summer, and then he found this location. Now, Lévy had developed a model for the strongly interacting particles together with [Murray Gell-Mann](https://www.nobelprize.org/nobel_prizes/physics/laureates/1969/index.html). Formally, the model could be renormalized, but in practice there were numerous problems, and they were going to be discussed. It was summer 1970. Lecturers were, besides Lévy and many others: the Korean Benjamin W. Lee, the German of Polish descent Kurt Symanzik, and many Frenchmen such as Jean-Loup Gervais.  The Gell-Mann-Lévy model is a model with spontaneous symmetry breaking. The pions are here interpreted as Goldstone particles. These lecturers were talking about renormalization in the presence of spontaneous symmetry breaking, and they were telling us that the mass terms that are generated (the mass of the proton) cause no problems whatsoever. As far as I remember, I only asked one question, both to Benjamin Lee and to Kurt Symanzik: “why can we not do the same for Yang-Mills theories?”. They both gave the same answer: “if you are a student of Veltman’s, ask him, we are no experts on Yang-Mills.”  A general picture of how to deal with massive vector particles was forming in my mind, but I could not understand the negative attitude of all the experts towards such theories. Later, I would find out that they all had different reasons for rejecting such approaches: some people thought that there would be Goldstone bosons with physically unacceptable properties. Some thought that introducing fundamental scalar particles would not serve any fundamental physical principle such as local gauge invariance. To many people, a renormalization programme would seem to be so complicated that mathematical clashes would be unavoidable. Finally, there was the scaling problem. It was thought that scaling towards asymptotic freedom in the ultraviolet region would never happen in a field theory; this would imply that any relativistic quantum system with strongly interacting particles would explode nonperturbatively in the near ultraviolet, and therefore no perturbative quantum field theory would ever apply to such systems. Because of this universal agreement among the experts, no-one realized that all these arguments were wrong. Why had this faulty counter evidence not deterred me? Probably, Veltman’s determination that there had to be something right about quantum field theory influenced me. But as a student I had also learned only to believe those arguments that I could truly understand.  What I did understand from the Cargèse lectures is that renormalization is complicated and delicate. At least at this point I could agree with my advisor, Veltman. When I returned to Utrecht, his assignment to me was that I should first study the pure Yang-Mills system, without anything resembling a Higgs mechanism for generating masses. There was not much literature on the subject, except some very elegant papers by [Richard Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html), Bryce DeWitt and by Ludwig D. Faddeev and his coworker Victor N. Popov at Leningrad. But some of the papers seemed to contradict one another, and so I began to collect the pieces of information that I could understand.  I learned how to formulate the Feynman rules for these Yang-Mills particles, and I learned that the discrepancy between the different papers was only an apparent one: you could perform gauge transformations to relate one to the other. I thought I was making tremendous progress towards formulating the exact renormalization procedure for this case, but Veltman had various objections. After long discussions, which again gave me many more insights, my first publication appeared. I had derived identities among amplitudes which were subsequently used by A.A. Slavnov and J.C. Taylor to derive more general identities, and their first references to my work made me feel very proud. The generally accepted name for these identities would be the “Slavnov-Taylor identities”.  After having learned so much about renormalizing massless Yang-Mills fields, doing the same thing for theories with Higgs mechanism was relatively easy. But it was this second paper with which I caught world-wide attention. Veltman realized that now the problem that he had been working on for years had been solved, and he was enthusiastic. As he was one of the organizers of an international conference on particle physics at Amsterdam in 1971, he decided to use his new pawn (me) in his battle for the recognition of Yang-Mills theories, and gave me 10 minutes (but no place in the Proceedings) to explain our new results. A period of intensive cooperation followed. Together, we worked out the so-called dimensional renormalization technique. Certainly, the work I had done was considered to be good enough for a PhD degree, and I graduated in 1972.  This, by the way, was also the year of my marriage. While I was making my great discoveries in physics I had also discovered whom I wanted to marry: Mrs Albertha A. Schik (Betteke). She had grown up in the town of Wageningen, and had studied medicine at Utrecht University.  We went to CERN, Geneva, where I had a fellowship, and Betteke could begin her work to obtain her certificate as a specialist in anesthesia, at the Hôpital Cantonal of the town of Geneva. The day before she was to meet her new superiors and colleagues there, we had made a trip to the Mont Blanc; on the way back we were in a minor car accident, and she fractured a bone in her foot. Her entry at the hospital will be remembered.  Veltman also came to CERN, and together we refined our methods for Yang-Mills theories. We were delighted with the great impact that our theories had. From 1971 onwards, all theories for the weak interactions that were proposed were Yang-Mills theories. Experiments were set up aimed at selecting out which of these Yang-Mills theories were correct. One of the simplest models continued to be successful; every now and then some particles were added to it, but its basic structure remained the same.  At CERN, I became interested in the quark confinement problem. I could not understand why none of the expert theoreticians would embrace quantum field theories for quarks. When I asked them, why not just a pure Yang-Mills theory?, they said that field theories were inconsistent with what J.D. Bjorken had found out about scaling in the strong interactions. This puzzled me, because when I computed the scaling properties of Yang-Mills fields, they seemed to be just what one needs. I simply could not believe that no-one besides me knew how Yang-Mills theories scale. I mentioned my result verbally at a small conference at Marseille, in 1972. The only person who listened to what I said was Kurt Symanzik. He urged me to publish my result about scaling. 1f you don’t, someone else will”, he warned. I ignored his sensible advice. I had also made a remark about scaling in my 1971 paper on massive Yang-Mills fields. No-one had taken notice.  Veltman told me that my theory would be worthless if I could not explain why quarks cannot be isolated. He attached more importance to another project we had embarked upon: we had started a lengthy calculation concerning the renormalizability of quantum gravity models. Although complete renormalization would never be possible, it was still worth-while to study these theories at the one-loop level, and there were some important things to be learned. Our work would be continued by Stanley Deser and a fellow PhD student of Veltman’s, Peter van Nieuwenhuizen, who discovered patterns in the renormalization counter terms that would lead to the discovery of supergravity theories.  But I also continued to think of gauge theories for the strong interaction. Quark confinement was indeed a problem, and I started to work on it. It was this question that led me to discover the magnetic monopole solutions in Higgs theories, the large N behaviour for theories with N colours (instead of 3, the physical number), and later the very important effects due to instantons. In the mean time, the scaling properties were rediscovered by [H. David Politzer and by David Gross and Frank Wilczek](https://www.nobelprize.org/nobel_prizes/physics/laureates/2004/index.html) in 1973, who now realized that this invalidated the age-old objections against simple, pure Yang-Mills theories for the strong interactions. The pure Yang-Mills theory with gauge group SU (3) was finally being accepted as the most likely explanation for the strong interactions, and it received the beautiful name “Quantum Chromodynamics” (QCD).  In 1974 we returned to Utrecht. I had been given an assistant professorship there. I was making progress understanding confinement as an effect due to Bose condensation of colour-magnetic monopoles. An important observation by Kenneth Wilson was that permanent quark confinement appears naturally if one performs the 1/g expansion instead of the g expansion in gauge theories, provided that a lattice cut-off is used. We were just beginning to see the extremely rich topological structure of gauge theories, and its consequences for the quantized system.  In 1976, 1 was invited for guest positions at Harvard (Morris Loeb lecturer) and Stanford. I worked on the question whether the delicate effects due to instantons – topologically twisted field configurations that should play a role in quantum chromodynamics – would survive when a renormalized perturbation expansion was applied. This led to one of the most complicated calculations I ever did: the one-loop corrections to instantons. It turned out that instantons in QCD give finite and well-defined contributions to the amplitudes. They give the symmetry structure a twist in such a way that many riddles in the experimental data concerning chiral symmetry were finally resolved, the most notable one being the problems with the eta particle, mentioned earlier. Several of my friends and colleagues at Harvard, MIT and Princeton such as Roman Jackiw, Sidney Coleman and David Gross but also physicists elsewhere (Moscow), students and postdocs joined the game of unraveling the secrets of instantons and monopoles. In the mean time my first daughter, Saskia Anne, was born, at Boston. When I returned to Utrecht I was appointed Full professor there. My second daughter, Ellen Marga, was born at Utrecht in 1978.  The years that followed I spent much energy and inventiveness to shed more light on the quark confinement problem. The neat and clean treatment of QCD that I hoped to find did not exactly materialize, but by the beginning of the 1980s the elementary mechanism for this phenomenon had become clear. QCD can be treated numerically when lattice cut-offs are used, and nowadays increasing accuracies are being reached by investigators using ever improving hardware and software. The problems remaining seem to be rather mathematical ones and not physical ones. QCD had become an integral ingredient of the Standard Model. I decided to turn towards the many open questions concerning the physics of this model.  I felt pain and sadness when for personal reasons Veltman left Utrecht in 1981. What about the deep, open problems in the Standard Model? Many of my colleagues agree that supersymmetry, a symmetry relation between particles with different spins, should play an essential role. I had seen how supersymmetry was born, back at CERN during the early 1970s. Bruno Zumino and Julius Wess were producing highly intriguing papers, while Van Nieuwenhuizen and Sergio Ferrara, and many others were making progress in supergravity. But what should a supersymmetric “parent theory” be like? How and why should supersymmetry be broken to explain the world as we observe it today? Do we really have to believe that there are dozens of particle types called “super partners”, none of which have ever been seen? Such questions make me feel uncomfortable with supersymmetric theories.  The true answers must undoubtedly come from the incorporation of the gravitational force. At first sight it may seem difficult to believe that such an extremely weak force could cause so much havoc in a theoretical construction such as the Standard Model. The point is, however, that if gravity really corresponds to the curvature of space and time, as we must conclude from the successes of Einstein’s theory of General Relativity, then Quantum Mechanics predicts quantum fluctuations in this curvature that, at the tiniest distance scales, grow out of control. This means that either gravity theory, or Quantum Mechanics, or both, must be replaced by some superior paradigm when we wish to describe physics at distance scales smaller than 10-33 CM. Whatever paradigm this would be, it is likely to entirely reform our understanding of the fundamental interactions, answering all our present questions at one stroke.  In 1984, the superstring revolution took place. Many of my colleagucs were enchanted by the coherence of the mathematical structures they saw in this theory. Would this not be exactly what we are looking for, a new paradigm that naturally generates the gravitational force and an apparent complete unification of all interactions?  But to me, superstring theories presented as many new problems as they may solve; I still cannot quite fathom the fundamental logical coherence of these ideas. The short distance structure is as mysterious as it was before and the predictive power of these theories was disappointing, to put it mildly. I decided to try a different route. When Stephen Hawking discovered that black holes will radiate due to quantum field theoretical effects, this to me appeared to be a more solid starting point. Are black holes elementary particles? Are elementary particles black holes? I was stunned to learn that Hawking’s result would put black holes in a category fundamentally different from any ordinary form of matter. If that were so, then what exactly are the laws of physics for black holes? The answer is that present theories are inconclusive. They clash. They lead to a paradox that may be as elementary as the paradox that, one century ago, led [Max Planck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1918/index.html) to revise the black body radiation law, and which ultimately gave us Quantum Mechanics. By studying this paradox, I hoped to stumble upon something equally great. Needless to say, I was asking for more luck than in the average lottery system. The problem is a sturdy one, and it still has not been solved. To illustrate the paradoxical nature of our problem I formulated a feature of the quantum gravitational degrees of freedom which, in discussions with Leonard Susskind, was called the “Holographic Principle”.  For a long time, I was among a small selected group of extravagants who studied quantum black holes. But superstring theory was catching on. As I had expected, superstring theory was not within a stone’s throw of “the final theory”, which had been what its addicts had prophesied, but it underwent fundamental changes. Membranes of various dimensionalities (“p-branes”) were added, and now a door was opened for studying black holes in string theory. Suddenly, I found myself to be nearly back in the “mainstream” of physics: string theoreticians are now seeing the “holographic principle” everywhere. But the solution to our problems, bringing the gravitational force fully in agreement with Quantum Mechanics, has not yet been achieved. As long as this is the case, we will not be able to produce verifiable predictions concerning the enigmatic details of the Standard Model. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q4 | **Professor ‘t Hooft, a key word in physics today, or one of the key words, is unification. There is a lot to talk about unification of forces, according to your mind is unification necessary?** |
|  | Gerardus ‘t Hooft: Usually there is a danger that one overemphasises the need for unification; it’s rather the other way around. The need in physics is understanding how things work and every now and then we have different regions of physics or different kind of forces or whatever, which give a problem when you try to combine them. We’re meeting problems and we understand that the situation as it exists is then not satisfactory because their forces cannot be combined in a proper way. We need in the overlap region a better theory that takes account of both sets of features of physics, say quantum mechanics and productivity or the weak force and the electromagnetic force or any such sort of different regions of physics where we have different forces or different phenomena which have to be combined. Then we won’t understand how it works and finally in practice the situation is very often this way that once we understood how to combine these different features in physics that you discover much to your surprise, that the utter theory contains more unification, it unifies.  But there’s a danger. People sometimes want to unify just for sake of unifying and then it doesn’t work. I’ve seen various examples of attempts to unify things which don’t need to be unified. There are examples of forces in nature which work perfectly well together without any further unification so then it is not necessary to unify. Then the attempt to unify may be misleading, we see this fairly often, say in the old days, when the elements were being discussed. The big question was how many elements do we have? Three elements, four elements? Water, fire, earth – how many? The correct answer turned to be 92 and more elements. There’s no unification, these were the elements and that was the correct answer. The attempt to unify was given more emphasis than it turned out to be correct at that stage of physics. Later we would discover how all the elements actually consist of the same kinds of matter but that had to be postponed until a different region of physics was being opened up. |
| Q14 | **The December issue of *Scientific American* has a theme what science will be known in 2050 and**[**Steven Weinberg**](https://www.nobelprize.org/prizes/physics/1979/weinberg/facts/)**there says that to unify the forces we need revolutionary new ideas in theoretical physics. Do you agree with that?** |
|  | Gerardus ‘t Hooft: Not necessarily, I think in this case he is correct but again, I will not put the emphasis on unification, I would put the emphasis on combined understanding. Right now we have a very deep problem in physics which is that on the one hand we have a theory of general relativity which describes the gravitational force in a remarkably accurate and beautiful way, so everybody believes general relativity is basically correct, a correct description of the gravitational force. On the other hand we also have quantum mechanics, quantum mechanics is an equally beautiful theory. The strange thing is that general relativity and quantum mechanics don’t seem to go together. There’s something very strange in physics. We have different sets of laws of physics, both are right, but they don’t go together. The point being that the domain of physics for quantum mechanics is relevant and the domain of physics where general relatively is valid in the ordinary experiences are very, very far separated. In ordinary experiments we never see the domain of physics where these two come together. But we can always fantasise, we can do Gedanken experiments and we can try to think what happens if both theories have to be applied together, then what? Very likely the resolution of this very deep problem will imply a kind of new unification but I think it’s dangerous to first try to unify and then try to understand. Its better first try to understand what’s going on and then we will see that the new theory will be more unified than before. |
| Q8 | **Do you have any hobbies? Something you like to do in your free time?** |
|  | Gerardus ‘t Hooft: Oh yes, that’s quite a different question. I play some piano, I used to paint a lot to make pictures, drawings, for some time I haven’t done that anymore. Then I also collect shells which is a hobby of mine because every now and then I come to strange places for conferences, very often there is a beach so I like to go across that beach and pick up shells. It’s a very nice, you are alone with the sea and the sand and nothing else and I let my mind wander away. In the meantime I try to pick up beautiful shells. So that’s a hobby. |
| Q9 | **I know that you practice music, often even together with your wife. What does music mean to your physics research?** |
|  | Gerardus ‘t Hooft: For me it’s just a way to relax. I never went into music in the same deep way as I went into physics. I realised that those composers whose music I like to play, like Beethoven, Mozart, Chopin, Debussy, that they are so far ahead in their expertise than I am that I can never improve on what they wrote down so I just play what they wrote. Because of this I realise that I’ll never be a very good musician because very good musicians they understand, they won’t play what somebody else has composed, they want to play their own pieces. I realised that I am playing music as an amateur but I like to do it just to relax. |
| Q9 | **In your work you have a lot of rather complicated mathematical equations and I wonder when you think about them can you think about them or do you have to write them down or can you think about them while you walk in the wood or something?** |
|  | Gerardus ‘t Hooft: Both. What happens is that very often I think very deeply, I write down equations, try to work them out and run into problems because very often the equations are too complicated to solve or some other obstacle is found on my way and I don’t understand how to get beyond that obstacle. Then I sometimes have to relax and I’ll just sleep or something else and while just sleeping I try to simplify the problem as much as possible. I realise there here is this obstacle, what was the cause of this obstacle? Why couldn’t I solve it? There’s some very deep problem here, the equations were too complicated so now I do everything in my mind and then I’m forced to simplify. I simplify and I simplify and simplify and then the obstacle stands out very, very clearly and when you simplify things so much you often find a way around the obstacle. Next morning I go to my work and I work on the equations and I find the answer. That’s the way I very often work. |
| Q3 | **How did you develop your interest in physics and research?** |
|  | Gerardus ‘t Hooft: From a very early age on I was interested in the laws of matter, of the abstract things in contrast to human beings who I thought they are far too complicated ever to understand. I didn’t understand, as a kid you, the human relations very well or what happened there just too complicated to me but on the contrary the laws of physics seemed to be honest, there’s no way to cheat on the laws of physics. Understanding those I thought would be much easier and much more interesting. From a very early age on I wanted to understand the laws of physics and I set myself as a goal to make new discoveries. I was thinking of making inventions but then later that became discoveries, I wanted to understand the laws, and that is as far as I remember. In the aeroplane to Stockholm here my mother gave me a picture taken of me when I was two years old and in the picture you see that I am studying a wheel and I’m really studying it, I want to understand what is a wheel, what makes it so special? Later, I don’t remember of course when I was two years old and I was studying a wheel, but I do remember that later I happened to see two wheels touching each other, of children’ bicycles or something like that, and you rotate one wheel and the other wheel starts to rotate as well, the transmission, and I was really intrigued by that. Those were laws of physics and I remember that I wanted to understand laws of physics. I remember that I always was very intrigued by what this world was made of and why. Then besides there were stories in my family and of course the heroes like Einstein and Planck and Schrödinger and so on who made all their beautiful discoveries and I wanted to be like them. |
| Q9 | **In many countries the number of students interested in science seems to be decreasing. What is the situation in your country and what do you think can be done about it?** |
|  | Gerardus ‘t Hooft: The situation in my country is the same. The interest in science is decreasing at a rather alarming rate and we try to understand why that is so. I’m afraid I do understand why it is so, our field of physics and physics in general, it doesn’t have the glamour it use to have. Shortly after World War there was nuclear physics, there were the semi-conductors, the super-conductors, all these beautiful, marvellous, revolutionary inventions which were portrayed enormous big in the media and caught everybody’s imagination. The more recent achievements of physics do not compare with those. We still find very many interesting things in physics, but they don’t catch the same imagination and interest by the people because it gets to abstract, people don’t understand any more what science is. When the glamour is taken away they now see all the negative sides of science, they see the negative sides of nuclear physics, they see the negative side of too much television. Science is put in a much more negative light than it used to be and I think as a consequence people are turning away from science. On top of that what irritates me rather a bit is that on very many popular movies and scenes on television, television is a very important medium, so very often on television you see a movie and the big hero is never a scientist, the big hero is a lawyer. The kids are inspired by that, they want to be a lawyer just like the hero they see on television. I can’t blame them, I think it’s understandable what’s happening but of course we don’t like it. |
| Q11 | **A somewhat related question is a question of women in science. If we look back to the Nobel Prizes during the 20th century, they’re almost all or a majority are men. Do you think the same will be true for the 21st century or will there be a change?** |
|  | Gerardus ‘t Hooft: I’ve no idea. I don’t see why women should be any worse scientists than men so that’s for certain. But indeed, as you say, in practice it is men who make the big discoveries, at least percentage wise much more of them than women. I’m sure it doesn’t have to be like this. What the cause of this is I do not understand, I don’t know. I can guess but my guess won’t be any better than your guess what the cause of that is. |
| Q11 | **Are there any special moves in that direction in Holland, to interest girls in science in the early ages?** |
|  | Gerardus ‘t Hooft: Of course you always try to get girls interested in science. Unfortunately in my field it seems as if theoretical physics is an absolute minimum. There’s practically no branch of science with fewer girls or women in that branch of science than theoretical physics, even in mathematics or experimental physics or other branches of science you see more women but not in theoretical physics. I think it’s very unfortunate. I wish it were different. |
| Q10 | **What do you think is different between being a student today and when you were a student?** |
|  | Gerardus ‘t Hooft: Our field is evolving very rapidly. In my field, but now I’m only talking about my field, these 30 years since we made our discoveries enormously much has happened. It’s very unfortunate but there are developments in our field that nobody could foresee and nobody can change but it seems to be that the most ardent questions in our field are those which we mentioned earlier in this interview. We mentioned about Weinberg, unification of gravitation and practical physics but it is very unlikely that one will be able to do direct experiments in such a field. That is a big change, when we did our work experimental physics and theoretical physics were going hand in hand and they are together solving the problems of our world. Now the experimentalists have a very hard time in getting deeper into the structure of matter because they have to be extremely clever, they make very, very large machines and they get effect of 10 improvement in resolving the structure of matter and its always very important, then perhaps another factor of 10 very important. But in our field, in theoretical physics, you’re not talking about factors of 10 but about 10 factors of 10 so factors of 10 billion or even 100’s of millions of billions. Such factors where the unification of gravity with matter becomes important. Unfortunately we cannot make a machine that is 10 billion times as powerful as the machines we have at present, those would have to be 10 billion times as big and that we cannot make. Experiments cannot be done at that stage. That means that these are very esoteric regions of physics, very abstract and very vague and mysterious and it worries me a lot that this may perhaps not be physics at all. The striking thing is that nevertheless in spite of the absence of direct experiments progress is being made very, very slowly and I don’t know how things will continue in the future but that is very a big difference with what our field was thirty years ago. |
| Q4 | **There is a lot of talk about the elusive Higgs particle and how important it would be. What would be the consequences according to you if it is not found?** |
|  | Gerardus ‘t Hooft: The present theory does predict the Higgs. If it is not found then the theory would not really work properly. In fact the situation would become very confusing at those energy scales where we would have expected the Higgs. If the Higgs is not found there then something else must happen. In practice some kind of particle must exist in place of all of the Higgs. One thing is that the Higgs doesn’t have to be an elementary particle, it could be a composite object. That is not so strange, that has been thought of many times before and it could be a possibility. But if that is so then we have to understand all those new forces at work which create composite particles at that energy range and it would definitely mean that there is very, very much new physics taking place. The way theory is now, a theory with no Higgs is not very different to a theory where the Higgs is very, very heavy. Having no Higgs is in some formal sense the limit where the Higgs mass goes to infinity. That limit is ill-defined, the theory is ill-defined you have to have something else going on then.  If they find no Higgs at all that will make our world much more interesting than if they find a Higgs exactly in the region where most people expect it, because then we don’t need new physics just around the corner. If there’s no Higgs we need no physics and that means that the future machines which will increase their power by a factor of 10 or a factor of 100, those new machines will produce many, many new objects which we have no idea about at present. Quite generally speaking it would make life very interesting if they don’t find the Higgs where it is expected to be. But I think they will find the Higgs and I think it will be found where it is expected to be and then the problem is of course what then, because then the standard model again shows that it survives another factor of 10 and no /- – -/ need for further particles is there although the standard model as it is it cannot be the ultimate truth. Now it’s not obvious that new machines will find anything new even if they get a factor 100 or so beyond the present energy. We simply will be in a more difficult situation if they do find a Higgs. In any case, whatever they will find, no Higgs or Higgs it will always be interesting because we have no good clue as to why the Higgs has the mass it has. No matter what, the Higgs could be 100 gv it could be a 1,000 gv or anything in between or when its 2,000 that’s practically synonymous to having no Higgs and that would make the world very, very interesting. In all these cases you have to ask the next question: Why this particular Higgs mass? |
| Q16 | **I wonder is there some subject except physics that you’re interested in?** |
|  | Gerardus ‘t Hooft: I like to read the science pages of the newspapers, in particular astronomy, so I very much like to see the features defined in other planets. I like the recent discoveries in the recent field of biology, I’ve read this book by Dawkins, *The selfish gene* and this was a revelation to me that finally biology is becoming scientific as far as I am concerned. I didn’t understand biology before because my teachers used to say that birds have wings otherwise they can’t fly but that wasn’t a satisfactory explanation to me, other creatures have no wings and they don’t need to fly. Why are there birds with wings and why are there birds without wings and so on. There may need to be other answers and those new developments answer that question in a much more satisfactory way than I’ve seen before. I like those branches of science. |
| Q4 | **One of the questions coming in to the Nobel site from students is the following, it’s about the electron and the question is the following: Now the electron is known to be or assumed to be point like particle, still it has a mask. It is therefore considered to be a tiny black hole?** |
|  | Gerardus ‘t Hooft: You might if you want call the electron a tiny black hole but it doesn’t make sense. The reason is that the electron is too light. If you were to compute the size of that black hole you would get a size much, much smaller than was believed to be the smallest existing size in nature. When you try to describe it as black hole you are pushing things too far. However, there’s another thing we might consider which is the point like particles such as electrons, neurons and heavier particles, they form whole families and it is not obvious whether they are the most heaviest point particles, it’s not clear where it will stop, so there could be heavier and heavier and heavier particles. Eventually those particles might blend with the black holes but only if they are billions and billions times as heavy as the electron. Then it becomes sensible to describe such particles as black holes but not particles as light as the electron. It could be that there’s a gradual change from black holes to particles and that the exact boundary, this is a particle, this is a black hole, the exact boundary may be not impossible to indicate or be irrelevant. But to call the electron a black hole wouldn’t make much sense. |
| Q2 | **The last few decades has seen a very rapid development of electronic communication and electronic publishing. What does it mean for collaboration between scientists, do you think?** |
|  | Gerardus ‘t Hooft: It changes our world. Nowadays scientists are using internet all the time and we send all our papers to the net and it seems as if the net is replacing already the scientific journals much to the regret of the publishers. In practice we find that those journals are hardly needed any more because we have all those papers which are on the internet so whenever someone wants a paper one just clicks into the internet site and finds the paper and we don’t have to look up the journal anymore, its quicker. In practice the electronic revolution – and I think it is a revolution – is changing our field dramatically. I don’t know what will be coming next, I’m looking forward to the time in the distant future when it will be unnecessary to use paper anymore, that we only work from screens. Right now the situation is still such that although we use the screen to write our papers and to devise our texts in practice whenever we want to study the paper we make a printout and we make all this waste of paper. Since the electronic revolution more paper is being used than before because we all make these printouts. I’m wondering when that will end, that we will no longer feel the need to make a printout because the screen is so versatile and so flexible that it’s easier to work with the screen and it’s no longer necessary or sensible to make a printout. But that will be the distant future. |
| Q10 | **The electronic age, would that change in any way the competition between individuals or groups working?** |
|  | Gerardus ‘t Hooft: I think it makes the competition a bit more honest at least for those people who have access to the internet. Everybody with access to the internet and nowadays it is nearly everybody even developing countries have access to internet at least many of them and they can now participate in the process. They don’t have to subscribe to expensive journals because they can link onto any of the papers that any other groups could also reach. You can find everything on the net. As soon as you have access to the net you can participate fully with the whole process of science, at least in my field because the theoretical physics as you know basically requires just pure thought and you are processing the experimental data which can also be found on the net and in theory that’s all you really need, at least in my field. In former days it was more difficult to get hold of information particularly if you were living in either a developing country or in the old days in the Cold War when you were behind the iron curtain it was very hard to get information in time. People were often half a year or so behind developments because they couldn’t get the information. That has not changed. On the other hand of course if there are countries which are so poor that they don’t have the access to the computers, access to the net and then you have a problem, those really can’t participate. |

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| **Physics\_1999-** | |
| **ID** | 0070 |
| **Biographical** | I was born on Saturday June 27, 1931, in a town called Waalwijk in the south of the Netherlands. My father was the head of the local primary school. One brother and two sisters of my father were primary school teachers as well, and in my family learning was held in high regard. My mother came from more practically oriented people: her father was a contractor and also ran a café. I have a bit from both sides. I was the fourth child in what became a six child family.  The town of Waalwijk had approximately 20 000 inhabitants. It was dominated by the shoe industry. For many life was not easy in the depression years but a head teacher was relatively well off. Consequently the life of my family was quiet and relatively uneventful.  In 1940 the Netherlands were overrun by the German army. I saw them marching in, and I heard people speak of the difficult times coming. In this part of the Netherlands things were not as bad as in the big cities, and the main thing I remember is that my father’s school was requisitioned by the Germans and troops were lodged therein. In the fall of 1944 we were liberated, contrary to the part of the Netherlands “above the rivers” that would have to endure till the capitulation in May 1945. This was due to the failure of the operation Market Garden; the last bridge near Arnhem was not taken. Thus we escaped what is called the hunger winter of 1944-1945, in which many Dutch people died of hunger.  The period of the war in which we were liberated, while the north was still occupied, was characterized by lots of artillery fire from our side, as Waalwijk was very near the front line. The allied troops were not very careful with their ammunition, and as a young boy of 14, I was very interested in playing with that. I remember how we would extract the powder from a failed tank grenade found in a ditch near where tanks had been firing. We would take the grenade by the point and then beat the lower end on the ground till the point came loose and we could then shake out the powder. I do not know how I survived this, but I did. We also survived the VI flying bombs that came over on their way to Antwerp. Two of them actually fell on houses in Waalwijk, one of them at about 100 m of our house. With me is still the memory of dead bodies being extracted from the ruins.  In 1943, I went to high school (the Dutch HBS). The war period was marked by irregularities and at one point our class was in a horse stable. While I had been a very good student at the elementary school, I was quite mediocre at the HBS. Much of that was due to my bad aptitude for languages, which is a real handicap when they require you to learn three foreign languages. So it happened that I narrowly passed the final exam in 1948, at the age of 17.  In those years I had (and still have) electronics as a hobby. This is a somewhat exaggerated qualification, because there was practically no electronics material around. The Germans had confiscated all radios, and there was a great scarcity of anything that could catch a radio signal. I remember spending a whole day walking around in the nearby larger town of ‘s Hertogenbosch, trying to get a radio tube. Finally some kind person, having pity on me, gave me one.  I acquired my knowledge of electronics from the local plumber. I used to spend many evenings in his house, and during the holidays I would work for him. I thus also learned plumbing. When I started to understand radio’s a bit better I became the local radio repairman. My only measuring instrument was my right index finger. If I touched a sensitive connection the radio would produce a hum. If a connection had the correct high voltage (about 200 V) I would get a shock. Commercially I was a failure, as I would usually not dare to ask money for my services.  When I passed the final high school exam the big question was: what now? Traditionally somebody like me would go to a medium level technical school in ‘s Hertogenbosch called MTS. However, given my low grades this did not go smoothly. At this point my physics high school teacher came to my home and suggested to my parents to send me to the University. This was a big thing, practically nobody did that in Waalwijk in those days. Universities were still very exclusive, and the south of the Netherlands was quite backwards in this respect. As the money situation was very tight the main point was to find a University where I could go to by train. This was possible with the University of Utrecht. For three years I commuted back and forth from Waalwijk to Utrecht, a 90 minute trip each way. I am still grateful to this high school teacher, Mr Beunes, as he did the extra thing, going to my parents house. Since then I have found out that many physicists owe their career to a good high school teacher.  Worse however was the state of the education at the University. The war had left the Netherlands ravaged, many good physicists had left the country or were killed. In retrospect, it is really a pity that Abraham Pais left Utrecht to go to the US. The teaching in Utrecht was uninspiring, and failed to awake much physics interest in me. After three quite mediocre years I left home and started to live in Utrecht. At that time I had no income as my father could not support that, and I was forced to work on the side. My main activity was typing lecture notes. Sometimes it was even difficult to get decent meals. But by and large I lived a happy life, mainly bumming around.  I also got involved in another job, trying to sell some rather silly tools to the unsuspecting citizens. In this I was a complete failure. I cannot sell anything to anybody. In some sense that has remained true in my scientific life.  After five years (2 years longer than normal) I passed what was called the candidaats exam. Then I happened to stumble on a popular book on the theory of relativity. Mind you, up to then no teacher had ever mentioned this. This booklet really excited me, and I went to the Institute of Theoretical Physics to get a real book on the subject. After some nagging they gave me [Einstein’s](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/index.html) book “The Meaning of Relativity”. Since then I was hooked. Also my financial situation improved slightly: given the very big shortage of high school teachers in the Netherlands it was not difficult to get a job as part-time teacher. Actually, I started as a teacher at a lower technical school, teaching plumbers about physics. None of that was helpful in speeding up my studies.  After my candidaats exam I started initially as an experimentalist. I worked for some time studying medical physics, in particular the physical aspects of percussion sound (the sound that is produced when a doctor pounds your chest). Later I worked on a mass-spectrometer, mainly doing the electronics. I found out that this was not my real destiny, and switched to theoretical physics. However, I still have a considerable fondness for experimental physics.  In 1955, I landed a job as assistant to Prof. Michels, of the Van Der Waals laboratory in Amsterdam. Michels was an experimental physicist, involved in high pressure physics. My task was the upkeep of the library, remarkably well stocked, and occasionally preparing a talk for Michels. I remember that as a good period, and it brought me also into contact with the members of the theory institute in Amsterdam. Mostly they were interested in statistical mechanics, a subject that has never evoked the slightest enthusiasm in me. Sneeringly I used to say: you guys average out anything of interest.  Science wise my life improved greatly with the coming to Utrecht of Leon Van Hove, I believe in 1955. He was an excellent lecturer, and I volunteered to make official notes of his lectures. I finished my graduate studies in 1956, after which I had to go into military service for two years, coming out of that in February 1959. Van Hove was so kind to take me as a PhD student despite my relatively advanced age (27 years). Thus I started doing real theory.  As Van Hove did statistical mechanics like all other theorists in Utrecht there was some problem, because I wanted to do particle physics. At that time many European Universities did not have anybody doing research in that field, and the way to learn that was via physics schools. Typically, such schools would run over a period of two weeks, with internationally known speakers. In the spring of 1959, I went to such a school in Naples, where among others Kurt Symanzik and Bruno Zumino lectured. In August 1960, I went to yet another school, in Edinburgh, and that school has been of quite some importance to me. I met [Shelly Glashow](https://www.nobelprize.org/nobel_prizes/physics/laureates/1979/index.html), at that time a student there, who was working on the subject for which he was to get the Nobel Prize. If someone had told him that, he would have been quite surprised. At these schools I became friends with several other students, among them Nicola Cabibbo and Derek Robinson (who went astray into statistical and mathematical directions). They have remained friends to this day. From the Edinburgh school I do remember fondly the lectures of Dave Jackson, now in Berkeley.  In 1960, Van Hove became director of the theory division at CERN, Geneva, Switzerland, the European High Energy laboratory. I followed him in 1961. Meanwhile, in 1960, I was married to my present wife Anneke, and before joining me in Geneva she delivered our daughter Hélène in the Netherlands, living in the house of my parents. Hélène followed in my footsteps and in due time completed her particle physics thesis with Mary Gaillard at Berkeley. She now works in the banking world in London. She is the one member of our family that understands what I have been doing.  At first I felt a bit lost at CERN, since my elementary particle physics knowledge was quite sketchy. For some time I had been working on a rather field theoretical problem, namely unstable particles. When that was finished I wanted to go into something closer to the experiments. This happened thanks to Sam Berman, an erstwhile student of [Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html). He was aware of the situation at the CERN theory division at the time, and he did put up an advertisement: If you have nothing else to do and wish to be kept off the street please knock my door”. So I did, and I am still very grateful to him. He suggested a calculation: Coulomb corrections to the production of vector bosons in the CERN neutrino experiment. This, after consultation with Van Hove, was then to be the second part of my thesis.  The problem consisted of two parts. One, a part very analogous to a previous calculation of [Bethe](https://www.nobelprize.org/nobel_prizes/physics/laureates/1967/index.html) and collaborators (Coulomb corrections to pair production), and secondly a part that was not solved by Bethe. I remember sitting in my office for several months, staring at a single differential equation, trying to solve it using confluent hypergeometric functions. These are very disgusting functions, and after a while I felt that perhaps I should consult the world expert on that matter, Eyvind Wichmann. He happened to be in Copenhagen at the time, and I made a pilgrimage. Seldom have I made such a useless trip. Wichman tried to understand what I wanted, but he did not get it. He looked at me as if I was some strange animal.  Well, at some point I solved that problem, and that then completed my thesis. In due time (April 22, 1963) Van Hove and I went back to Utrecht for the ceremonies, in tails and white tie. I now understand that this is a preparation for the Nobel Prize. The thesis contained my work on unstable particles as well as the treatment of Coulomb corrections for vector boson production by neutrinos. Title of the thesis: Intermediate particles in S-matrix theory and calculation of higher order effects in the production of intermediate vector bosons.  At CERN, meanwhile, the experimentalists were gearing up for the CERN neutrino experiment. I was asked to speak at one of their meetings on vector boson production. I became almost instantly very good friends with Bernardini, the leader of the group. They then wanted me to do extensive calculations for them concerning vector boson production, as that would be needed for analysis of the data. Computer calculations of that type had been done already by [Lee](https://www.nobelprize.org/nobel_prizes/physics/laureates/1957/index.html), Markstein and [Yang](https://www.nobelprize.org/nobel_prizes/physics/laureates/1957/index.html), and when Lee came to CERN I took the occasion to ask for those programs, which he curtly refused. I then asked him if he could give me some advice, to which he answered: “don’t make mistakes”. I thought this funny, and started to laugh, but that was not appreciated by Lee, who took some moments to teach me the seriousness of this enterprise. Well, even if in the making of computer programs the not making of errors is usually the main problem, I still feel that I did not really need that advice!  The CERN neutrino experiment was a very big happening in my life. When they started I was more or less permanently around, looking at the pictures as they came out. When no spectacular events came out the enthusiasm of the experimentalists waned, and after a while the only ones to look at the pictures were Bernardini and myself. And so it came to pass that I became the spokesman for the group at the Brookhaven Conference in 1963. Somewhere in that period I acquired two lifelong friends, the experimentalists [Mel Schwartz](https://www.nobelprize.org/nobel_prizes/physics/laureates/1988/index.html) (Nobel Laureate 1988) and Val Telegdi.  The 1963 CERN neutrino experiment left me with an interest for experiments that never went away. I am a deep believer in the importance of experiments for the progress of physics. Also, the experiment left me with a feeling for these things, to recognize what is important and who are the good guys. All theorists ought to go through some such experience. These days, however, that is not really practical any more. The experiments have become gigantic enterprises, involving hundreds of physicists and a large number of engineers. The modern experimentalist is often more manager than physicist.  In 1963, I went to SLAC at Stanford, where Pief Panofsky was building a Iinear electron accelerator. Also Sam Berman was there, in fact he was much of the reason for going there. Meanwhile, at CERN, I had become good friends with John Bell who was one of the very few theorists that had any interest in the neutrino experiment. He also came to SLAC, and in fact we wrote a paper together that we however never published. He became quite involved with what is now known as the Bell inequalities, while I started constructing my symbolic computer program Schoonschip. That also had its origin in the neutrino experiment: in doing the necessary algebra for vector boson production I was often exasperated by the effort that it took to get an error free result, even if the work was quite mechanical. In a discussion on the CERN terrace, including among others Mike Levine, we concluded that somebody ought to write a program to do that type of work. I started doing that at SLAC, in the autumn of 1963. Many good things have been invented at the CERN terrace. Mike Levine later successfully completed the first QED sixth order calculation.  In the spring of 1964, I went back to CERN and worked there till my departure for Brookhaven in 1966. There I had the pleasure of getting to know Maurice Goldhaber, then the very successful director of Brookhaven National Laboratory. A man that impresses me to this day. He liked me as well, and in fact tried to get me to that laboratory, which I did not. I do remember getting a phone call from Brookhaven while sitting with my parents in law in the Dutch town of Leeuwarden. How they ever found me there is still a mystery to me.  In the meantime, just prior to going to the US, our son Hugo was born. He now runs a restaurant called Solstice in Los Angeles. If I want a really good dinner that is where I go. I hope the reader gets the hint.  September 1966, I went back to Utrecht, as successor of Van Hove, i.e. professor of theoretical physics. There was still nobody doing particle physics there, so I started to build that up. That took some time; it was really a big change after the rather hectic CERN life. I made a mistake: I thought that being relatively isolated in Utrecht it would be a good idea to become editor of Physics Letters. Indeed, it is a way to keep in touch with the action in the field; however, I received on the average 1 article a day, and I rejected about 90% straight away. In other words, the big majority was junk, just cluttering up my mind. As far as I can see it has become worse, not better. Many a physicist has come to hate my “high handedness”, as one of the victims called it. I was happy to get out of that job by the summer of 1968.  A turning point in my scientific life occurred during a one month visit (April 1968) to Rockefeller University. In the quietness of that institution I embarked on the scientific venture that has now been honoured with the Nobel Prize. I am still indebted to Bram Pais who got me there and counseled me in that period. Too bad that he left the Netherlands in 1945; I am sure that he would have kept Dutch particle physics on a high level. One man can make a big difference.  In the summer of 1968, I went to Orsay, near Paris, on the invitation of the French physicists Claude Bouchiat and Philippe Meyer. The stay lasted till September 1969, it was a sabbatical year (after two years in Utrecht…). As Utrecht partly paid me during that period, I told the French people that I did not need much in the way of a salary, and subsequently they did put me in some low job. This had an unpleasant consequence; Christmas 1968, I was fired, as de Gaulle had decided on some cost saving operation. Luckily they succeeded in patching it up, in some mysterious French way. Some well known French physicist told me: luckily that it happened to you and not to T.D. Lee. That made me aware of my place on the totem pole.  Back in Utrecht I continued my work, and had several students under my supervision. Among them Peter van Nieuwenhuizen, now director of the C.N. Yang Institute at Stony Brook, Bernard de Wit , now holding my former position in Utrecht and [Gerard ‘t Hooft](https://www.nobelprize.org/nobel_prizes/physics/laureates/1999/index.html), my co-laureate. Our group became known, especially so after the work of ‘t Hooft and myself that is cited in connection with this years prize. Besides my own research I was very busy in that period: reforming the physics educational system in Utrecht (see my complaints above), and trying to get a good computer system. The latter required endless meetings, mainly caused by some mathematician who insisted that the machine could run Algol, a by now largely forgotten computer language. I had literally to learn some 6 or 7 computers inside out to get to the final result: a CDC 6800 computer. During one of these meetings (January 1971) I received a phone call telling me that my wife was about to deliver another child; I went out of the meeting to the hospital and came back after about one hour. I gave everybody a cigar, celebrating the birth of my son Martijn and continued with the meeting. Martijn is now working in Hollywood, in the movie industry.  ‘t Hooft and I worked together for a few more years, after which we drifted apart. I went my way doing calculations of radiative corrections, something that he was not interested in. The fame of Utrecht had spread, and two young Italian physicists came to work with me: Giam-Piero Passarino and Maurizio Consoli. Some Dutch students at that time were Jochum van der Bij, now professor at Freiburg, Germany and Michel Lemoine, now a free lance Senior Petroleum Engineer. The latter has convinced me that theoretical physics is a good science to be educated in, it prepares for no job in particular but the scientific methods learned are of use in many positions in modern society. So never worry too much what kind of job you will get after finishing a theoretical physics education. For example, the first Prize in Economic Sciences in Memory of Alfred Nobel went to [Tinbergen](https://www.nobelprize.org/nobel_prizes/economics/laureates/1969/index.html), a former theoretical physicist. And nowadays the banking world is full of particle theorists.  In the summer of 1979, I received an invitation from Ed Yao from the University of Michigan to spend a sabbatical year there. I knew Yao from his scientific work, and I immediately called my wife, asking if she was interested in a year in Michigan. For reasons that I have now forgotten we left in March 1980, to stay till December of that year. In Michigan we were asked to stay, but initially we answered rather firmly that I was not interested. In November we started wavering, and in fact Fermi lab (under the directorship of [Leon Lederman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1988/index.html), another experimental friend of mine) started to express interest as well. In December a nice house was auctioned near were we lived in Ann Arbor, and my wife told me: if you buy that house I will stay. I did not get the house, some richer medical person got it, but this somehow made us decide to stay in Ann Arbor. Part of it was a certain unhappiness with the situation in the Netherlands, and another part was the happiness of our sons with the American school system. My Utrecht colleagues were quite upset when I told them and they did sent me a telegram asking me to remain in Utrecht. But we decided to accept the offer of Ann Arbor, and when we came back in Utrecht I started preparations to leave, which we did in September of 1981.  I did my part in the scientific life in the United States, serving among others on the various committees that decide on experiments at the big laboratories, Fermilab near Chicago, SLAC at Stanford and Brookhaven National Laboratory, Long Island.  Shortly after we arrived I was offered a named chair, the John D. and Catherine T. MacArthur chair. Europeans think that this means an extra income, but that is not true. There are other things however. Apart from the prestige of this chair it had a really nice feature going with it: a yearly amount of $ 35000 that I could spend for scientific purposes. While it may seem a small amount, it nonetheless made quite a difference. I could pay certain deserving students during the summer, buy computer stuff, help the group with little things, visit conferences, invite colleagues etc. It is wonderful that you can buy a new computer almost immediately if there is a need for it. In Europe that took often a long time, you had to put it on a budget and wait for approval etc. This is one aspect of the greater flexibility of the American system. If you want something done you either use discretionary funds at your disposal or you go around and try to find money (discretionary funds) from people or groups that feel it is also to their advantage to support the purpose. For example I remember supporting a Russian scientist partly from my MacArthur fund, partly from a fund from the astro-physics group.  In hindsight we do not regret this move to the USA, but it would take me too far to explain that. Too many strictly personal considerations are involved here. The University of Michigan has been good to me, and I feel loyal to that institution. Also the life in Ann Arbor was quite nice, and Anneke felt very much at home there, enjoying membership of a great many clubs. Among others she learned to make beautiful stained glass windows. Nonetheless, it was quite a step for a 50 year old man and his family to emigrate. Dutch people abroad have a saying: rather nostalgia than Holland. I would not go that far, we had certainly many friends in the Netherlands, and also most of our families (from my wife and me) live there.  My main tie with Europe during the US period (1981-1996) was with the University of Madrid (the Autonoma), Spain. There was a particle theory group headed by F.J. Yndurain. I would go there up to two months during the summer time, and conversely he would often come to Ann Arbor. This type of collaboration is usually very fruitful, not only for doing science, but also because it fosters the exchange of graduate students.  It was just in this period that Spain decided seriously to catch up with the rest of Europe, and that was an interesting and exciting thing to watch. While not everything went perfect, I would say that Spain made enormous strides forward in a relatively short time. Up to this day I have very good relations with Spanish physicists, both in Spain and at CERN. I should perhaps add that to them CERN is of crucial importance, as it has been to me.  On retirement we decided to return to the town of Bilthoven in the Netherlands that we had left in 1981, to find still many of our old friends there. That is where we now live happily. Our children however did not go back, they would really not fit anymore in the Dutch society. It rains too much. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q10 | **Professor Veltman, you have long experience of research, teaching and students in the United States, and in Europe, what would you say are the main differences between the United States and Europe in this respect?** |
|  | Martinus Veltman: The main difference, at least in Holland, I think on the average I got students of a higher level. This is something that you never know for sure and sometimes you get much better students in America as well of course, but on the average, and that’s because of the difference between the American and European system. In America, students get different shares of and the best go to Harvard and the next one goes to Princeton and the next one goes to Yale, and we got sort of number ten. So that is a pre-selection in America, and in Holland there is no pre-selection, and generally in Europe there is not accepted /- – -/ England. So the consequence is that we got student of which the best we had selected out in America. |
| Q6 | **What is the difference between being a student today and when you were a student? Being a student today and when you were a student.** |
|  | Martinus Veltman: The difference now as then? I don’t know, it was less disciplined. In my student time you could do nothing for a few years and then start working again and I liked that a lot, I did do nothing for a number of years.  But today in Europe that’s no more possible because they stop supporting you and so on, and they make the system such that you cannot do it. I’m very strongly in favour of a system where someone can momentarily go out and later on come back again. For whatever reason. Maybe he’s not mature enough, or maybe he wants to have other experiences. But the present day systems compare as, and that was the way the system was in my time. Today I think that is no more possible, very difficult. Is it possible here in Sweden that you can study for a very long time without you being kicked out of the University? |
| Q10 | **I don’t really know; can you get kicked out?** |
|  | Martinus Veltman: Of course it happens very seldom and it is the one ray of hope in the life of many miserable people! |
| Q4 | **What is really a Higgs particle?** |
|  | Martinus Veltman: A Higgs particle is just one of the many particles that are part of the game of the standard model, but the special thing about a Higgs particle and the reason that we are talking so much about it, is because we know which experiments to do to get at it. There are many things in the standard model that we don’t understand, we don’t know why there are three generations, we don’t know why the top quark is so heavy, or the bottom quark, of all these quarks, we don’t know why the towers are so heavy. We know nothing, there are so many of these questions, but we do not know what experiment to do to find an answer to these questions.  Now in the case of the Higgs particle we know what experiment to do to get an answer. We must go to /- – -/ of the order of 500 GeV and then you will have access, you should either make the extr,a see something, so what is special about Higgs I would say is mainly because it’s accessible to experiments. So we’re talking a lot about it, and we know how to make a machine that looks at it, but there are many other questions I would just as well want to know the answer to, except I don’t know how to go about it. |
| Q4 | **What would be the consequences if this elusive Higgs particle isn’t found?** |
|  | Martinus Veltman: Well, by all the rules of logic, which I believe do apply in particle physics, we will have to find something, because it has to appear as some sort of a cut off in intervals that we have observed here or there, so we see a /- – -/ correction to which either the Higgs or whatever goes for its contributes, so we know there is something there, unless of course the rules of logic don’t hold any more, then everything stops. So don’t ever say what would happen if we don’t see it, I think that would mean that there’s something wrong with the rules of logic and that we cannot have. So far that’s not the way it works. |
| Q3 | **I wonder how did you develop your interest in research and physics?** |
|  | Martinus Veltman: I don’t know, like most of us, we go into the domain of particle physics and you start with an experiment and you see results coming, and you find it very exciting discovering new particles and interactions and stuff, and my whole life which started out by looking at a neutrino experiment, and that experiment was a total failure otherwise, but standing there and seeing these events coming and seeing these reaction coming, and try to guess what’s going on, it’s a very exciting something. It’s like entering a domain that no other person has been before. That’s the very wonderful stuff about this. And in physics, you are in another domain, unknown, you are truly an explorer. I liked that very much. It’s a pity that much of what you see today is no more so simple, but at that time that was like that. So that has always fascinated me from the first day that I saw it onwards. |
| Q14 | **One of the primary goals of physics is to understand the wonderful variety of nature in a unified way, the December issue of the *Scientific American* has the theme What science will be known in 2050. 50 years from now. Do you share**[**Steven Weinberg**](https://www.nobelprize.org/nobel_prizes/physics/laureates/1979/weinberg-facts.html)**‘s … when he says that a unified theory of all forces probably requires radical new ideas?** |
|  | Martinus Veltman: Well, it’s usually what I find these things very annoying, if not stupid, because there is nothing in nature that says that we should have unifying field series, we could equally well have non-unified field series, that’s up to nature to do it. We don’t know about it. So I don’t know why these people are always talking about united field series. I really don’t know, I honestly don’t know, and the unfortunate thing is that on top of it in the past 20 years or so they have been making propaganda of us having unified the weak and the electromagnetic interactions, but if you look at it, there’s been no unification. None whatsoever. The electric coupling constant is independent of the weak coupling constant so what’s the unification? I honestly don’t know, and the main unification is that you write the laws on the same page.  Other than that I honestly don’t know what is this unification they talk about. It’s SU(2) cross U(1) remember? SU(2) is weaker but actually U(1) is electro magnetism, there’s a cross in between, they are not connected. So I don’t know why they talk about unification. They only do it to sell it, and then they speak about unified field series, because people have been told this so often because Mr. [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/einstein-facts.html) has been saying these things for a while, but it is, in my opinion, not a correct thing to say.  Nature is whatever nature is. If nature is unified – fine. If nature is not unified – fine too. There is no intrinsic law in physics that says that we should have a unified gauge series, and so far we don’t have any and moreover have no indication of that, and even unification of weak and electromagnetic interactions is a pseudo unification, it’s not a real one, despite what they say. |
| Q16 | **I wonder is there some other subject that interests you except physics?** |
|  | Martinus Veltman: Oh yes. We don’t want to talk about that. I used to always to be very interested in electronics and things like that, so it’s sorts of pseudo physics and making computer programs and stuff like that. It’s not always in the same domain. I am not good in things like music or the arts, so there I have, well, I have some interest but I’m not very good at it. So beyond physics, well I guess I’m sort of a professional idiot. Physics, well, and surrounding things in all honesty. |
| Q18 | **Some people say that the 20th century was a century for physics and that the 25th century would be that on biology, what do you think about that?** |
|  | Martinus Veltman: I don’t think anything about it because you know perfectly well if you had put such a question to somebody in 1899 he would for sure have given you the wrong answer. He would not have known [Max Planck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1918/planck-facts.html) who did his invention the first year in 1900 which changed everything, and this you cannot know. Research brings you into situations you didn’t know. We don’t know what’s going on and maybe physics comes to some sort of a dead end, if we sort of get bogged down in the Higgs sector one way or the other, and if you don’t know how to follow that up then that might be a very hard time for particle physics and it might be another century before we get over that. But you never know. You know it, you have, think of the difference in life that we had when you and I started to know, gigantic difference, and you couldn’t have guessed it. Could we have guessed at ’65 what we have today? |
| Q10 | **But does it change something in terms of competition between people and groups?** |
|  | Martinus Veltman: It probably does but I don’t know to what extent. Everything goes faster and quicker and so on, so I have no experience with it, how it is today. I notice that still the same people are accusing the other people of not quoting properly, many things remain as they always were. |
| Q8 | **Doesn’t it exist for you – spare time.** |
|  | Martinus Veltman: It exists and it doesn’t exist because I was very free all of my life. Most of my life I never had to set an alarm clock, and I would have like two or three lectures a week, and the rest of the time you do research, talk to students, what have you. Now going on retirement, well of course the students have disappeared out of my life, but for the rest it doesn’t change so much. You just, I’m used to keeping myself busy one way or the other, what I do a little bit more today is taking long walks, because you need some movement to keep your body in shape, so I do that for that purpose.  Other than that my life is very much as it was over the past 40 years. It doesn’t change that much. That’s the nice thing about this profession, you know. It would be different if I were an experimentalist because there you need an experimental environment and all kinds of things and I don’t need that, all I need is my hat, and as long as there is no hole in there it’s okay. |
| Q11 | **You received the last prize of this century. Most Nobel Prize winners of this century are men, do you think that we will see a change in that in the next century and what would be the causes on this dominance of men?** |
|  | Martinus Veltman: This is a very difficult question because you know that I have a daughter who did her thesis also in this high energy physics, she did her thesis with Mary Gaillard if you know that lady, from Berkeley. So I’ve seen it with my daughter, extra difficult if you are a woman, and they are serious, they’re really serious, that puts an extra degree of complication on your relationship. When you sit together with a man there’s always some tension in the room, in the collaboration, only if you work together with other women that’s not there but then that happens very rarely because there are not enough women. So what happens is that these women are there and they have a problem of finding a good collaboration and well usually there it goes usually wrong one way or the other. It’s very difficult, it’s very difficult. So in a sense they are more isolated than everybody else, because there’s always sex playing a role. You can’t help it. |
| Q9 | **What does this Nobel Prize mean to you?** |
|  | Martinus Veltman: Well, that’s often asked of me, but I have no ready-made answer. I think if anything it means of course appreciation of the things you did in the past which I always thought people had more or less forgotten. So I’m happy that they didn’t forget it, what can I say? Every man of course likes to have appreciation for what he did, so in this I’m no different from anybody else. Money wise it doesn’t mean that much to me because I had a comfortable existence anyway. So well it’s nice to be recognised. But after that I think I will go back to my existence and live quietly, so as far as that goes it will probably not make so much of a difference. So to a large extent it’s in your own head. I think that’s right, yes |
| Q6 | **Do people ask you questions about physics when you are at parties with non-physicists?** |
|  | Martinus Veltman: Oh no. No. If they do I may start, but after about two minutes they walk away! No problem! Sometimes I do and I go at it full /- – -/ but it is certainly at a party it’s not possible, just not possible, so you give that up. The only way to do it would be as some organised contacts, give lectures or so, and I plan to do that a little bit more than I did in the past. But in ordinary life we never get to deal with this. Not that I, no, never. |
| Q4 | **How do you consider the discussion about the muon collider versus the linear e+ e-?** |
|  | Martinus Veltman: I’ve looked at them but I’m not too clear about what kind of knowledge they would add and so I can’t really tell you. I have listened to them but I’m not clear about it. Especially the muon collider, I don’t know what it will do. |
| Q4 | **One of the questions coming in to the website was the following: The electron is supposed to be a point like particle without any dimensions, but still it has a mass, can you therefore consider it as that a little mini black hole?** |
|  | Martinus Veltman: This question you must not direct to me, you must direct it to ‘t Hooft. You will not get an answer, but it will sound very impressive what he will tell you. |

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| **Physics\_1999-** | |
| **ID** | 0071 |
| **Biographical** | I was born on 1 November, 1950 in Visalia, California, a medium-sized town just south of Fresno in the San Joaquin Valley. It was at that time an agricultural community more like the Middle West or West Texas than Hollywood or Beverly Hills. The main highway into town was lined with magnificent walnut orchards and stands of valley oaks. My childhood home backed onto wheat and cotton fields. And when the navel orange crop was threatened by a freeze there was smudge in the air by day and talk of little else. A 10-minute drive in any direction brought one out of the town and into rows of tidy farms with peach orchards, olive orchards, avocado orchards, nuts of all sorts, row crops, and dairies. And above us stood the mighty Sierra Nevada, John Muir’s Range of Light, the rivers of which irrigated the land and turned what would otherwise have been oak savannah into the richest farmland in the world. The mountains were obscured most of the time by the haze caused by irrigation and too many automobiles or the dense radiation fog that hides the sun most of the winter in that part of the world. My great Aunt recalled how they were visible most of the summer when she first came there after the great San Francisco earthquake of 1906. But on brilliant winter mornings just after a Pacific storm had blown through there they would be, a blazing wall of white stretching north and south as far as the eye could see, topped by the silhouettes of Sawtooth, Mineral King, and the Great Western Divide.  Both sides of my family landed in Visalia by accident. My mother was the daughter of a local doctor, Irvin Betts, who had come down from San Francisco after medical school “temporarily” and induced my grandmother to accompany him by promising her a return in a couple of years. She always laughed when she told this story. My father had grown up a widow’s son in Chico, served as a naval officer in the war, followed his brother into the law, and had come to Visalia fresh out of law school to work in the Tulare County District Attorney’s office. There he met and married my mother, and together they raised four children, of which I was the first. Like so many other American families mine had roots that were deep but temporary. We attended church, joined the Boy Scouts, contributed casseroles to PTA pot luck suppers, and celebrated many a Thanksgiving with family and friends, but in the end moved away. My father died in Visalia 18 years ago, and all of us, including my mother, now live elsewhere.  Early on in his career my father left the District Attorney’s office and set up a private law practice in town. He worked very hard but was, as one of my uncles later put it, an “artist lawyer”, meaning that he was more concerned with correctness than profits and often did work for needy clients for free. As a consequence while we had a roof over our heads, food on the table, and clothes to wear to school we were constantly conscious of being of modest means. Whether caused by this or our home environment generally it came to pass that all of us became quite self-reliant at an early age. I, for example, used to take appliances apart when they broke in an attempt to fix them, which I rarely did successfully, being a kid. I am better at this now. My sister Margaret, who is an attorney, still enjoys doing needlework from scratch. My brother John, a software engineer, prides himself in being able to fix any broken thing. It was through such creative play that I first learned about pump impellers, refrigerant cycles, material strength, corrosion, and the rudiments of electricity, and more importantly the idea that real understanding of a thing comes from taking it apart oneself, not reading about it in a book or hearing about it in a classroom. To this day I always insist on working out a problem from the beginning without reading up on it first, a habit that sometimes gets me into trouble but just as often helps me see things my predecessors have missed.  Another important aspect of our home was respect for ideas. At dinnertime one of my parents, usually my father, would lead a discussion about some controversial matter, such as racial integration of schools, whether John Lennon should have compared himself with Jesus Christ, support of Israel, or the morality of the Vietnam war, and all of us were expected to air and defend our views on these things, even if we did not want to. Over the course of time this gave us a deep respect for ideas, both our own and those of others, and an understanding that conflict through debate is a powerful means of revealing truth. This was, of course, before any of us understood rhetoric and how easily it can be misused. But the need for conflict to expose prejudice and unclear reasoning, which is deeply embedded in my philosophy of science, has its origin in these debates.  My mother, who was professional schoolteacher, was particularly concerned about our formal education and even went so far as to start a private school together with some other parents so that our intellectual needs would be met. They acquired an old two-room schoolhouse out in the country among the walnut groves at the foot of Venice Hill, added some indoor plumbing, and hired a small faculty to teach us a broad curriculum that included such things as Latin and French. I am afraid the money was largely wasted on me because I was not ready to learn French, or much of anything else, at that time, although I did rather enjoy watching the machines shaking walnuts off the trees in the fall. But it was impressed upon me that there was such a thing as good study habits and that I would have to acquire them if I wanted to be a scholar. My mother also had us take piano lessons, and this had a similar effect. I hated those lessons, but I now play regularly for pleasure and have even tried my hand at composing. So mothers everywhere take heart. The indoctrination you administer now may have unanticipated positive effects years later.  I was an extremely reclusive and introverted boy. It was to my parents’ credit that they weathered the storm and encouraged my self-motivated study, even though it scared them to death, especially my mother. While still at Venice Hill, for example, I got very interested in how televisions worked, and electronics generally, so my parents bought me a Heathkit color TV, which I soldered together and eventually made work. It was a magnificent thing filled with vacuum tubes. One could probably have heated the living room with it. I found building this kit rather unsatisfactory, actually, because the manual did not explain how the circuits worked but only how to assemble them. So I went back to old discarded black-and-white models, which my father dutifully acquired for me, and began reading about what the various parts did and then testing the theory by removing them one at a time. It was in this way that I learned why it is bad to allow the 10 kilovolts stored on a cathode-ray tube to discharge through one’s body. Thank God my mother never knew. I also taught myself how to blow glass using a propane torch from the hardware store and managed to make some elementary chemistry plumbing such as tees and small glass bulbs. The latter I filled with isopropyl alcohol and attached with a piece of surgical tubing to the intake of a cooling compressor I had scavenged from a broken refrigerator. This lowered the pressure sufficiently to boil the alcohol and lower the temperature well below the freezing point of water. I had ambitions of making liquid nitrogen, and could probably have done it with more compressors and some dewars. I also tried to make sodium metal by electrolysis of molten salts. I discovered that common wood lye had the lowest melting temperature of all the available materials, so I melted some in a orange juice can and electrolyzed it using an auto battery charger and an ice pick as the cathode. It worked, except that the sodium lived only a second or two before being oxidized by the surrounding air. It was at this time that I picked up the can to check for corrosion on the bottom and accidentally poured its contents all over my right hand, burning it severely. My father rushed me to the hospital, had it dressed, and then invented a story to tell my poor mother so that she would not have a heart attack. By good fortune the molten sodium hydroxide was so hot that it had vaporized the water in my skin and sloughed off without burning me chemically. My hand recovered fully. My parents would probably never have encouraged these things had they known how foolish and dangerous they were, but it is nonetheless a testament to their belief in the value of self-motivated exploration that they allowed me to cultivate such interests even though I got no credit from them toward college or employment.  In parallel with the development of my interests in technical gadgetry I began to acquire a profound love of and respect for the natural world which motivates my scientific thinking to this day. My maternal grandmother had a mountain cabin deep in the Tule river canyon just south of Sequoia Park, to which we were often invited as a family or as individuals. My grandfather had built it as a kind of hunting lodge before I was born, so it had a very masculine feeling despite being my grandmother’s home. It had a big stone fireplace, knotty pine walls, a big cast iron chandelier for light, and a marvelous old Aeolean player piano with plenty of rolls. My grandmother was a complex person, but she loved the mountains and welcomed anyone else who did, including reclusive grandsons. So I spent time there whenever I was able, which was not very much because I had responsibilities at home, and over the course of time came to understand what a treasure it was – the house-sized boulders left in the riverbed by retreating glaciers, the massive ponderosa pines six feet in diameter at the base, delicate mosses and lichens of every imaginable color, the complex geometries of pine cones and oak boughs, the hundreds of fragrant herbs along the riverbank, the quiet rush of the river at night on a cool summer evening, and the vast tracts of wilderness beyond known to no one. I realized that nature is filled with a limitless number of wonderful things which have causes and reasons like anything else but nonetheless cannot be forseen but must be discovered, for their subtlety and complexity transcends the present state of science. The questions worth asking, in other words, come not from other people but from nature, and are for the most part delicate things easily drowned out by the noise of everyday life.  I owe my interest in mathematics to my father, or more precisely the sense that mathematics was something important and mysterious. He knew very little mathematics himself but was always reading about it and encouraging everybody else to do the same. He even mounted blackboards in the hall so that a person could write down a brilliant idea if he happened to be passing by. I remember particularly one day hearing a shout from my father’s bedroom and rushing in to discover that he had just discovered Euler’s theorem. He did not understand the proof completely, so it appeared to him more astonishing than it does to those of us with technical training, but he had correctly understood its significance and elegance. I did well in my mathematics courses in school but was not that challenged and, truthfully, not that interested either. But through my interests in electron motion in vacuum tubes I discovered a need to describe trajectories of moving particles with equations. So I taught myself calculus. I was terribly proud of this at the time, but I realize now that people at this age are simply developmentally ready to learn such things, which is why calculus is now taught in high school. But I was certainly the only person in my town to have done this, and my father’s own interest in mathematics was the underlying cause.  **Berkeley** The experience that firmly placed me on a course toward a professional career in science was the four years I spent as an undergraduate at Berkeley. I entered in the fall of 1968 as an electrical engineer, my parents having prevailed upon me to take the economic facts of life seriously. I had applied to more elite schools but had not gotten in, presumably because my grades were not high enough, and also because I was what we now call an “angular” student, i.e. not well-rounded. My parents were not that disappointed, for they had themselves attended Berkeley, as eventually did my brother and two sisters. Berkeley was as different from the quiet country town of my youth as one could possibly imagine. It was full of coffee shops, politics, book stores, theaters, ethnic restaurants, stray dogs, junkies, street musicians, and fascinating people from every conceivable walk of life. As time passed I became more and more intoxicated with all this freedom and more and more convinced that the university was where my future lay. Here was the place ideas mattered, where everybody was eccentric, where originality was not only accepted but had actual market value. It was easy to get lost in the crowd at Berkeley, particularly in the great lecture courses, but this did not bother me because I had no intention of getting lost in the crowd, and anyway considered it a small price to pay for the freedom to think as I saw fit.  At Berkeley I had my first encounter with real professional scientists. I remember the Berkeley faculty as being particularly visionary and inspirational. In the physics department in particular there was a palpable sense of history going back to [Heisenberg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1932/index.html), [Pauli](https://www.nobelprize.org/nobel_prizes/physics/laureates/1945/index.html), and [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/index.html). I later came to understand that Berkeley has always been a special place in American physics and that many of the greatest physicists in the world, perhaps even most of them, can trace their roots back to Berkeley in some way. It was this faculty that defined for me what physics was and should be, and thereby helped me make up my mind to pursue physics as a career. I came home in the middle of my sophomore year and announced, much to the horror of my parents, that I was switching to physics from engineering. After some discussion they gave in, as well-meaning parents tend to do in this situation, and I remember my father musing afterward that it would probably come out all right because these things usually did. Meanwhile at school I was experiencing such wonderful things as the surprise appearance of [Charles Townes](https://www.nobelprize.org/nobel_prizes/physics/laureates/1964/index.html), winner of the Nobel Prize for invention of the laser, in one of my large lecture courses to explain simply and accurately how lasers work and how they came to be invented. I took quantum mechanics from [Owen Chamberlain](https://www.nobelprize.org/nobel_prizes/physics/laureates/1959/index.html), who had won the Nobel Prize several years before for the discovery of the antiproton, and who was happy to discuss all sorts of unrelated things such as whether fusion would ever work and whether one should go East to graduate school. I learned electrodynamics from J. D. Jackson’s wonderful book and had many occasions to ask him questions about the subject. I took introductory solid state physics from Charles Kittel, the acknowledged father of the field in which I was eventually to work. I took Goeffrey Chew’s advanced quantum mechanics course and learned more about the S matrix than he probably intended. I also had many useful exchanges with Ray Sachs, who helped me learn differential geometry and general relativity on my own and guided me to a thesis. My work with Ray began with the question of whether a charged particle dropped in a gravitational field should radiate light, since the relativity principle said it was actually not accelerating. The correct answer is yes because electromagnetic field knows about the curvature tensor. This line of thought led us to a calculation of the cross-section for scattering gravitational radiation off of a charged particle, the roles of the gravitational and electromagnetic fields in this case being exactly reversed. It was a wonderful time in my life. On commencement day we were addressed by [Emilio Segré](https://www.nobelprize.org/nobel_prizes/physics/laureates/1959/index.html), sharer of the Nobel Prize for the antiproton discovery and author of a book on nuclear physics that is a delight to read to this day. He took the long view, told us all not to worry too much, and recounted how he and his fellow students in Rome had regularly scanned the obituaries in hopes that a job would become available soon. Many years later when I returned to Berkeley to talk about fractional quantization it was Professor Segré who rushed up after the lecture to ask if the particles we had identified in the fractional quantum hall effect might have something to do with quarks. It was his life’s work to ask questions like that, and this was the reason I had found him and his colleagues so inspiring.  My years at Berkeley coincided almost exactly with the worst of the Vietnam war. It is not necessary to recount here the many terrible events of that time, but the political unrest at Berkeley caused ultimately by the war was a major constraint on student life, both intellectually and physically. It was also a real lesson in how people’s perceptions of exactly the same facts can be profoundly different. I had no sympathy at all for the disrespect for property and formal education implicit in these demonstrations, but I did think long and hard about the issues raised and, more importantly, about what these events said about politics. Western society has many flaws, and it is good for an educated person to have thought some of these through, even at the expense of losing a lecture or two to tear gas. As to the war, I had no idea what to think about it, except that there were already scattered reports of people in my high school class having come back in body bags. So it came as quite a shock when President Nixon canceled student deferments arguing, correctly in my view, that they were unfair, held a lottery, and picked for me a draft number equal to my age – nineteen.  I remember vividly the day it was announced and the coldness I felt as the full implications slowly became clear. It was common knowledge that theoretical physicists do their best work before age 27, sometimes even earlier. I could not possibly meet this deadline now. There was also the moral question of whether to serve at all. Many people at that time were fleeing the country to avoid the draft, others were faking health problems, and still others were enlisting for long periods in exchange for safety. After stewing over this a long time I decided that I did not think defending one’s country was wrong – although the Vietnam war had very little to do with defending one’s country – that I could not lie about so important a matter, that I did not want to flee the country, and that I should obey its laws if I stayed. So that was that. I often question now whether this was the right decision, but in any event it is the one I made. But the weight of it bore down on me more and more heavily as my senior year progressed, and at the very end I lost focus, failed a laboratory, and graduated with only a degree in mathematics rather than with the double degrees in mathematics and physics I had actually earned. So I left Berkeley with everything I had come to value in ruins. The only thing I had left was the faith in myself instilled by my parents and the certainty that I had understood what theoretical physics was and was extremely good at it.  **Military** At the time I felt that my induction into the military was a giant step backward. It was certainly unfair taxation of my time, but then life is unfair, and getting reminded of this from time to time is perhaps not such a bad thing. I had decided not to become an officer because to do so would have required me to stay in a year longer, and time was critical. So I became an enlisted man and let the system do with me as it saw fit. Skill as a theoretical physicist matters very little in the lower ranks of the army – or perhaps has negative value. It is an interesting fact that during my tour I was never allowed access to computers, radios, or anything else that I might damage through curiosity, or perhaps something more sinister. What matters most is that one blends in. In basic training, which I had at Fort Ord near Monterey, one’s identity and past are excised and a new one substituted. All one’s clothes and possessions are removed and shipped home. All one’s hair is shaved off so that one looks like a concentration camp victim. All people get the same hemorrhoid examination. All people get the same equipment. All people run with this equipment to the firing range. All people get the same cold. All people do the same chin-ups before meals. It was about as different from Berkeley as one could imagine, the suppression of individuality and freedom for the purpose of preventing mischief. In retrospect I consider my induction to have been not so much a step backward as an important lesson in civics, for it eventually became clear that these things I found so abhorrent were the very things required to make a large organization run well under stress. So I learned the hard way that freedom and efficiency conflict, that more of one means less of the other, and that this is fundamental. To this day I break out in a cold sweat every time I hear the term “programmic science”, for I know it really means tight bunks, shiny boots, and digging holes that will be filled back up by someone else the next day.  Some time near the end of basic training a computer somewhere decided that I was suited for missile school, so I was ordered to Fort Sill, Oklahoma to learn how to fire Pershing missiles. This was a good deal less stressful than basic training, as the pace was slower, and this part of Oklahoma is laid back and rather beautiful, with rolling brown hills not unlike the ones in California. The Pershing missiles, on the other hand, were not beautiful. They were horrible weapons of war – solid-fuel rockets five feet in diameter at the base, long as a moving van, and capable of throwing a tactical nuclear warhead 500 miles. They were launched from trucks and required a team of 10 men to service and fire. The most interesting thing I learned during this time was how small a nuclear warhead was. The nose cone of a Pershing is only about 18 inches in diameter at the base. I had not been interested at all in nuclear weaponry as a student, and so I had never thought through carefully about their “efficiency”. It is sobering thought that these missiles were actually deployed in continental Europe in those days and that on at least one occasion, namely the 1973 Arab-Israel war, there was an alert serious enough to leave the commanding officers trembling.  While at Fort Sill I met, or more precisely was grouped with, the people who were to be my companions for the rest of my tour in the military. They were a very personable bunch mostly from the upper Middle West, Pennsylvania, and Nebraska, and rather like a selection of the smarter students from my high school, except that the contingent from Detroit was rabidly racist, something that I had never encountered before and still have trouble understanding. Getting to know these people was my first of many reminders that the world is full of intelligent, well-meaning people who, for one reason or another, did not attend university but are nonetheless well-read and educated. Out there on the prairie lost opportunities of youth were the rule rather than the exception, and I slowly became disabused of the myth of the Bright Young Thing and have not believed in it since.  After missile school I was ordered to southern Germany, where I spent the remainder of my tour. This assignment was a welcome turn of events, but it was not a vacation, and it was in some respects extremely unpleasant. Most of the locals in my parents’ generation were very accepting and helpful, for they were afraid of the Russians and remembered the many kindnesses done to them after the war. They were also prospering economically, which I know from personal experience helps one overlook indignities. But the people my age and younger hated the whole idea of a foreign army on their soil, especially one with nuclear weapons, felt little personal guilt for Germany’s past, and felt that the Vietnam war had thoroughly discredited the alleged ethical superiority of English-speaking countries. So we were tolerated but not liked all that much. Also there were terrible morale problems in the unit to which I was assigned in Schwaebisch Gmuend, a small town near Stuttgart, which caused particularly heavy and widespread drug usage. These were largely corrected by a change in command about halfway through my tour, but they were nonetheless extremely scary.  During this time I tried to think about physics, and about university life generally, and I made a point of visiting the nearby technical universities and the great medieval university at Tübingen, but it was hopeless. I had a job to do, my time was too fragmented, and my unit discouraged much contact with university types, this being politically dangerous. Tübingen, in particular, was frowned upon because of the safe house for AWOL soldiers alleged to be there. So I decided to make the best of a bad situation and invest the time studying language. This turned out to be a better expenditure of time than reading physics books, for like most of my countrymen I had an incomplete understanding of how language is a vehicle for ideas rather than the other way around. While my language ability is still poor, I can still remember the day that radio stations began to sound clear, when newspapers began to inform more than frustrate, when I began to get jokes, and when I told my first joke. So in the light hindsight, I judge this time to have been well spent.  At the end of my tour I was released from duty in Europe, as I had elected to travel around a bit as a free man before going home. On the day of my emancipation I celebrated in traditional fashion by burning my boots – although in an especially thoughtful and creative way. I went downtown and bought 3 kilos of saltpetre, mixed it with sugar, and filled both boots up to the brim, fully laced, and lit one off. There was a tremendous pink flame, fierce heat, and dense smoke that began shooting straight up 30 feet as from a volcano as the fire ate down into the boot. Several of the battery officers came running up just as the experiment was ending to see what was left of the sole curling up like a shriveled bug. They had been playing baseball nearby and had thought that a radio unit was on fire. I assured them that the radio unit was not on fire and then proved it by lighting off the other boot.  **MIT** I entered graduate school at MIT in the fall of 1974 with a sense of urgency sharpened by my two-year absence from academic life. I was behind all my friends and I was very impatient with any activity not leading directly to fundamental discovery, i.e. taking classes. However I soon found that things were not that simple. Physics graduate schools in America are for the most part set up as a first priority to service federal contracts, not to make fundamental discoveries, and a graduate student career makes no sense outside the context of one of these contracts. Indeed it was, and is, the practice at MIT to admit graduate students directly into research groups on an as-needed basis as a kind of labor pool. It took me a while to fully understand this depressing fact of life, but I eventually did and then proceeded to look for a home in a research group as a means of supporting myself while learning the things essential to achieving my larger ambitions. I had by this time become quite cynical about and suspicious of institutions of all kinds, and I felt that government-sponsored science was no more likely to be immune from economic pressures than business. So I directed my attention toward the branch of physics with the largest number of experiments, namely solid state physics, figuring that this was the best way to cut out the intellectual middleman and go directly to nature. I have since discovered that most good theorists think this way.  It was my good fortune at this time to fall in with John Joannopoulos, a young faculty member who had just come from Marvin Cohen’s group at Berkeley. I had heard John talking at a research fair and had noticed that he was the only theorist who seemed genuinely interested in his own work, so I contacted him and asked for a job. Neither of us knew it at the time, but John was to become one a truly great trainer of graduate students, for the list of alumni from his group includes Prof. E.J. Mele at the University of Pennsylvania, Prof. A.D. Stone at Yale, Prof. Karen Rabe at Yale, Prof. D. Vanderbilt of Rutgers, Prof. T. Arias at MIT, Prof. E. Kaxiras at Harvard, Prof. D.H. Lee at Berkeley, and me. His main expertise was in using local exchange methods (c.f. [Walter Kohn](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1998/index.html)‘s 1998 Nobel lecture) to model electronic materials, which in those days meant defective silicon, silicate glasses, and amorphous selenium. I figured at the time that this was a good way to learn the basics, and I knew that [William Shockley](https://www.nobelprize.org/nobel_prizes/physics/laureates/1956/index.html) had started out doing similar things for John Slater at MIT. So I worked for John for a long time and published several papers with him that were not that memorable but kept dinner on the table while I was coming up the learning curve on the vast subject of solid state physics. John’s strategy was to give students simple problems they could market right away and then invest enormous amounts of personal time making sure the research was on track. The physics training I got from John emphasized bread-and-butter things such as the basics of semiconductors, tight binding modeling methods, and pseudopotentials. The truly invaluable things I learned from him, however, were not technical at all but organizational: how to mount a research campaign and execute successfully, how to render a big body of work down to its essence, how to package work so that it is interesting and comprehensible to an audience, how to look for new physical content in old results, and how to think experimentally. John took as his highest priority that all his students have a professional niche to live in after graduation, something I now understand to be of paramount importance, for the science will come later if the person has what it takes, but it will never come if the student has no job in the critical years right after graduate school.  One of the terrific aspects of MIT in those days was the enormous variety of experimental work that either took place there or was talked about in seminars by outside speakers aggressively recruited by the faculty. It was motivated by questions that did not interest me that much, such as whether the Kosterlitz-Thouless transition could actually be observed in the laboratory or what renormalization group principles told one about scattering lineshapes. The important thing for me was the experiment itself, how it worked, and whether it might be saying something that the experimentalist himself had overlooked. So I learned about X-ray diffraction, neutron scattering, raman scattering, infrared absorption spectroscopy, heat capacity, transport, time-dependent transport, magnetic resonance, electron diffraction, electron energy loss spectroscopy – all the experimental techniques that constitute the eyes and ears of modern solid state physics. As this occurred I slowly became disillusioned with the reductionist ideal of physics, for it was completely clear that the oucome of these experiments was almost always impossible to predict from first principles, yet was right and meaningful and certainly regulated by the same microscopic laws that work in atoms. Only many years later did I finally understand that this truth, which seems so natural to solid state physicists because they confront experiments so frequently, is actually quite alien to other branches of physics and is vigorously repudiated by many scientists on the grounds that things not amenable to reductionist thinking are not physics.  It was at MIT that I met and married my wife Anita. We used to swim at the same time after work at the MIT swimming pool and were annoyed by the same guy in a leopard suit who obviously thought he was beautiful and talented. So one day I said, “That guy may look tough, but he keeps his suit on in the shower.” It was absolutely true, of course, for I could not have made up such a good story. This broke the ice. Anita corrected many of my worst habits, in particular the one of returning to my office after swimming and working until midnight. We would instead go up to Harvard Square for a late-night snack or attend a movie or a poetry reading, the usual staples of student life. Also, Anita’s family lived nearby and was quite close-knit and warm, so we used to escape from Cambridge regularly to visit them. They had a wonderful old saltbox house out in Concord with a huge fireplace heating an equally huge kitchen with low wooden beams and an old plank floor. In winter the hearth was always lit and there was always something interesting simmering on the stove. Her father, who was then Dean of Graduate Studies at Lesley College, is a yankee with a wicked sense of humor who had grown up on a dairy farm in Massachusetts and then gone on to a life of scholarship at Yale and Harvard. Her mother had grown up as a doctor’s daughter in Palo Alto and graduated from Stanford. Thanks in part to this latter fact, Anita and I were married by candlelight at Memorial Church at Stanford, an interesting turn of events considering what was to happen later. Anita’s mother got a bit carried away with this wedding and went so far as to get us onto the New York Times society page. But her father kept things in perspective. One afternoon, totally unprovoked, he held out a wedding gift that looked suspiciously like a dentist’s bowl and said “spit please”.  **Bell Labs** I must have been doing something right at MIT, for at the end of my graduate career there the faculty got together and recommended me for a position in the Theory Group at Bell Labs, the best placement a young theoretical physicist could possibly have gotten. I had wanted to go to Xerox Palo Alto, but a job did not materialize, and in light of what happened later it was probably just as well. Bell Labs had been a kind of holy place of solid state physics since the 1950’s when it was built up by Shockley after the invention of the transistor. I had no idea at the time of the significance of this placement, but I did notice during my job talk that everybody understood what I was saying immediately – this had never happened before – and that the audience had an irresistible urge to interrupt, heckle, and argue about the subject matter loudly among themselves during the talk so as to lob hand grenades into it, just like back-benchers do in the House of Commons. Being a combative person I rather liked this and lobbed a few grenades of my own to maintain control of my seminar. I later came to understand that this heckling was a sign of respect from these people, that the ability to handle it was a test of a person’s worth, and that polite silence from them was an extremely bad sign, amounting to Pauli’s famous criticism that the speaker was “not even wrong.”  It was at Bell Labs that I first made direct contact with real semiconductor experts and thus began to fully understand what amazing materials they were and what they could do. I knew a little about semiconductors already, having worked on the theory of silicon-oxide interfaces at MIT and also having intimate familiarity with Marc Kastner’s experimental amorphous silicon work there. But a thorough grasp of this great subject was not possible to acquire at MIT or any other university, because no faculty could ever be big enough. I learned about cyclotron resonance measurements of electron masses and the associated disorder broadening from Jim Allen, defect-pair recombination luminescence from Michael Sturge, deep levels from John Poate and Dave Lang, silicide Schottky barriers from Marty Lepselter through Jim Phillips, infrared spectroscopy of shallow donors and acceptors from Gordon Thomas, and transport in the 2-dimensional electron gas from [Dan Tsui](https://www.nobelprize.org/nobel_prizes/physics/laureates/1998/index.html). The theorists at Bell had all done work in semiconductors at some time or another and were very helpful in the learning process, particularly through their constant give-and-take with the experimentalists. While I was there, for example, Gordon Thomas and Tom Rosenbaum verified the continuous nature of the metal-insulator transition in phosphorus-doped silicon predicted by [Phil Anderson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/index.html) and his “gang of four”. Don Hamann and Michael Schlüter were doing ab-initio density functional computations for semiconductor surfaces, interfaces, and defects. Patrick Lee was working hard on the field theory of weak localization. I was also familiar with the cutting-edge work in gallium arsenide heterostructures being done at the time through seminars and informal conversations with Mike Schlüter, who was good friends with [Horst Störmer](https://www.nobelprize.org/nobel_prizes/physics/laureates/1998/index.html). The fact that the two German expatriates at Bell were in the thick of this subject was no accident, for semiconductor physics had been particularly emphasized at that time in the German research establishment, and most of the careful, scholarly work on the subject, particularly the 2-dimensional electron gas, was being done in Germany.  It is a great irony that the work leading to the Nobel Prize this year began in a time of terrible defeat for me personally, as I had just learned that I would not get a permanent job at Bell. John Joannopoulos had recommended that I work closely with Mark Cardillo, who was diffracting neutral helium atoms from semiconductor surfaces as a means of diagnosing their structures, presumably on the theory that my proper niche at Bell would be as a modeler. Unfortunately, Mark was such a good experimentalist and so good at understanding the meaning of his results before I had even seen them that there was little left for me to do but confirm his insight after the fact. Also, there was no profound conceptual issue at stake. By about one year into my appointment I could see the inevitable but was unable to do anything about it. I had actually made a breakthrough in my helium diffraction work – I had discovered empirically by studying atomic beam experiments that the potential felt by the incoming helium atom was a universal constant times the electron density of the target – and was writing it up when Jim Phillips pointed out that the same idea had just been published by somebody else in Physical Review Letters. So it didn’t count. The fateful vote on my promotion to permanent status came shortly thereafter, and rumor had it that I had only one supporter. While I had been expecting the axe to fall for some weeks my blood froze when it actually did. Once again my ambitions had been thwarted due to circumstances beyond my control, only this time the damage was much greater and almost certainly unrecoverable. I went home and told Anita, and together we began making plans for what to do. She was not all that unhappy, actually, for she did not like the New York metropolitan area all that much and had had ambitions to live in New England or out West.  It was at this moment that I wrote my first important paper in theoretical physics. I was 32 years old, 5 years beyond the alleged age of senility for theorists. Dan Tsui had come into the tearoom one afternoon with a copy of [Klaus von Klitzing](https://www.nobelprize.org/nobel_prizes/physics/laureates/1985/index.html)‘s famous paper on the integral quantum Hall effect to see what the theorists thought about it. Everybody was interested, for localization in the 2-dimensional electron gas was a timely topic. The version of it unique to two dimensions called weak localization had been discovered at Bell by [Doug Osheroff](https://www.nobelprize.org/nobel_prizes/physics/laureates/1996/index.html) and Gerry Dolan shortly before my arrival, and there was a raging controversy over the sign of magnetoresistance of these systems in weak fields. Von Klitzing’s experiment was in the strong-field limit, for which there was no theory. I remember Phil Anderson’s making a mumble about how there was probably a “gauge argument”, by which he meant something like the physics of the Josephson effect, and this stuck in my mind. I knew that the enormous accuracy of Klitzing’s effect precluded any complicated explanation, I knew from my work with John Joannopoulos what the Hamiltonian appropriate to the problem was, and I knew localization had to be occurring. I also knew how the experiment worked, in particular that the gate voltage on a field-effect transistor fixes the density and not the chemical potential, so that a gap in the density of states as proposed by Ando could not be the right answer. Within a few days I had hit on the idea of replacing a calculation of the current with a derivative of the energy with respect to vector potential, and shortly thereafter I made this physical by imagining an experiment in which the sample was wrapped into a ring. Thus was born what later became known as the “gauge argument” for accurate quantization of the Hall conductance. The upshot of this theory was that localization caused the effect and that the Hall conductance was accurately quantized because it was a measurement of the charge of the object being localized, in this case the electron.  The response at Bell to these events is a fascinating case study in how even well-informed people find a truly new idea difficult to understand and accept. Anderson complains regularly about this problem, and he often cites [Planck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1918/index.html)‘s complaints about it, so I am in good company. A week later I gave a journal club presentation about von Klitzing’s discovery, and finished off with my explanation, which could be given to that audience in two minutes. I got some questions about the experiment, but none about my ideas that were on the mark at all. I remember being challenged over, that well-known fact that all states were localized in two dimensions, something that made no sense at all in light of the experiments I had just shown. I remember giving the right answers, namely that the experiments showed the current theory of localization to be wrong in strong magnetic fields, and that there had to be a band of extended states below the fermi level carrying the current. But they were not convinced, and it was not until Bert Halperin wrote a paper repeating these and elaborating upon them that they were accepted at Bell, by which time I was long gone.  **Livermore** By the time I began looking for jobs my fame for this work had begun to spread and I eventually was offered a job at Purdue which I accepted. A few weeks later I un-accepted and took a job at the Livermore lab as a post-doc, an act for which I feel guilty to this day, as Al Overhauser and Sergio Rodriguez had gone out on a limb on my behalf. I had received a call from Andy McMahan months earlier asking if I might want to come out, and on a whim I went for an interview and gave a talk on my quantum Hall theory. By good fortune one person in that audience, Dick More, understood its significance and caused an offer to be generated, even though my interests and training did not match the Laboratory’s needs at all. Like most of the physicists at Livermore, Dick was an expert in atomic and plasma physics, but he had been trained as a solid state physicist and had even held Ted Holstein’s old position at the University of Pittsburgh. I first turned the Livermore offer down, knowing full well that it was not an academic job. But after Anita and I had discussed it at length, I decided that I felt completely betrayed by the academic establishment and saw no reason to trust it a second time, especially for so little money. She felt the same way, noted that everybody switches careers nowadays, and suggested that we move to California where the economy was strong and just go out on the open market if things went bad at Livermore. So it was decided. We flew back to Newark, and that very evening I put Anita on a plane to San Francisco to look for a place to live. I remember watching her plane take off through a cyclone fence at North Terminal and standing there for a long time afterward with tears in my eyes wondering if I had done the right thing.  Livermore was and is a real industrial laboratory, by which I mean that it considered its job to be maintenance and development of technology for inertial-confinement fusion and nuclear weapons and not the generation of public-domain scientific knowledge. I was hired into what was then known as H-Division, the group responsible for generating equation-of-state and opacity tables for use in design codes. My job description said I was to work on modeling matter at a temperature of about 10 eV and a density of about 1/10 that of ordinary solids, a particularly difficult regime relevant to the X-ray laser program. However, it was the practice in those days to induce people to work on such applied problems by providing them with resources to work on real science as well. This is how Hugh DeWitt’s famous Monte Carlo work on the 3-dimensional one-component plasma came to be done, for example, or Ceperley and Alder’s excellent numerical work on the phase diagram of metals at zero temperature. So I was encouraged to continue thinking about the quantum Hall effect on the side and even given permission to use my computer account for any calculations that I might need to do. Also, for the first six months I was with Livermore I worked in a trailer known as the “cooler” outside the fence waiting for my clearance to come through. So at least in the short term my decision had been a a good one.  It was while I was in the cooler that I received the preprint from Horst and Dan about their discovery of fractional quantum Hall effect. I remember flipping through to the figure at the back, staring at it for 10 seconds, and realizing that they must have found a many-body condensate with excitations carrying charge e/3, for there was no plausible explanation for the existence of a plateau other than localization of such a carrier. The temperature in the experiment was not that low by modern standards, so the plateau was not that flat and the parallel conductance not that small, but I knew Dan was very careful about localization physics and would have said something if this conductance had not been converging rapidly to zero with decreasing temperature. Also the bare eye could see that the quantization was at least 1% accurate, and there was no reason it should have been even that good unless it was von Klitzing’s effect. I quickly telephoned Horst to make sure I had understood correctly and to find out all the little experimental details that never get published in formal papers, including in particular any evidence that the localization was incomplete. There was none. I told Horst what I thought it meant, and he told me they had had a similar idea. Dan had apparently seen the chart recorder plot, measured the field strength of the integral plateau with his fingers, displaced this 3 times to the right to land under the new plateau, and said “quarks”.  So the task remaining was to find a prototype for this condensate simple enough to be convincing. I was familiar at the time with the theory of fractionally charged domain walls in 1-dimensional chains, an idea attributed by solid state physicists to Su and Schrieffer but actually going back further to a particle physics paper by Jackiw and Rebbi. I had learned about them from my fellow graduate student Gene Mele, who had worked on them at Xerox Webster. Gene had thought about these “solitons” deeply from every possible angle, knew as a result that they had to be real, and convinced me of it over the course of several meetings. It was actually a hot idea in those days, and there had been much experimental activity attempting to detect these solitons in polyacetylene, usually with light scattering. However, despite claims of success, the matter was never resolved because sample imperfection always corrupted the data and allowed the experiments to be interpreted more than one way. So my first attempt to write down a prototype for the fractional quantum Hall ground state borrowed heavily from the literature of solitons, in particular the idea of discrete broken symmetry on which it was based. I wrote up a theory and sent it in to Physical Review Letters. It was rejected, thank God. The referee, who I later discovered to be Steve Kivelson, observed that the discrete broken symmetry I had written down was actually a continuous broken symmetry and that its pinning on impurities would cause the sample insulate. This was correct, and furthermore I had known about the problem before I sent in the paper and had deceived myself into believing that it did not matter.  It was at this time that I wrote the paper for which I have been awarded the Nobel Prize. Realizing that most people would require more than experimental phenomenology to be convinced I went back to the beginning and began computing the properties of the interacting 2-dimensional electron gas problems by the exact-diagonalization method. For most many-body problems this would have been a foolish thing to do, but I knew from the experiments that the system had an energy gap and that this would protect the calculation and give it meaning even when the number of particles was small. So I solved the problem for one and two particles, then powered up the computers to do three, four, five, and six. Each time the system locked in at particular densities as the pressure on it was increased, and thus exhibited the behavior seen in experiment. There was no sign of any tendency to crystallize, which would have shown up as a near degeneracy in the eigenvalue spectrum. So I knew that the right answer was indeed a uniform fluid with an energy gap. Having seen the behavior with small numbers of particles I began trying to guess the functional form of the wavefunction in hopes of then extrapolating to the thermodynamic limit. One particular functional form, a product of pair factors, caught my attention because it had occurred naturally as a basis element in the numerical calculations and had a particularly large weight in the correct ground state at filling factor 1/3, sometimes as much as 99.9 %. But it was not exact. Also there did not exist any standard mathematical machinery for computing the properties of such a state in the thermodynamic limit. Feeling rather discouraged I went to the library to read up on many-body physics, hoping to find some reason that the state I had proposed would be exact. I was looking through Eugene Feenberg’s book on helium and chanced to open up the chapter on Jastrow ground states and there, in front of my eyes, was the functional form I had guessed! It was not exact at all, but rather a well-known variational technique for approximating the ground state of strongly-interacting many-body systems. I eagerly read about the analogy between such wavefunctions and the statistical mechanics of classical fluids and then realized that the fluid analogous to my proposed ground state was the very one-component plasma I had been learning about from Forrest Rogers and Huge DeWitt in the “cooler”, albeit in one lower dimension. So I went to them to get guidance on how to compute the properties of this plasma. Once I had mastered the hypernetted chain and semiclassical Monte Carlo techniques and understood their error bars the rest was straightforward. The ground state energy was computed and found to be variationally superior to all known crystals. The charge-1/3 excitations were constructed from this ground state with an adiabatic thought experiment very similar to that used in the integral quantum hall effect. Wavefunctions for these excitations were proposed and their computed variational energies found to match the experimental activation energy for the parallel conductance in the plateau regions. It all fit. So I wrote the new theory up and sent it to Physical Review Letters. It was published there a few months later.  These rather heady events coincided almost exactly with the purchase of our first house and the birth of my first son, Nathaniel. Anita had been near the end of her pregnancy when we closed the deal, and I remember her scooting along the floor painting the baseboards white, the only chore she could do comfortably. Nat arrived a few weeks later in the dark of the morning on one of the rainiest days I can ever remember. We somehow made it to the hospital, where he was delivered by Caesarean with me in attendance. It is quite something witnessing surgery on one’s spouse while she is awake. My mother drove up from Visalia when she got the news and was almost annihilated by weather-induced traffic accidents. It was a wild and beautiful day. Thus I became a father and a homeowner at the same time and experienced all the changes in perceptions and priorities that happen to a person at this time in life. Our house was quite small, and in particular had one bathroom that needed its floor replaced twice due to dry rot caused by the children’s shenanigans in the bathtub. But in the back was a small creek, a little grove of redwoods, and three wonderful apple trees which kept Anita busy canning apple sauce in the fall. Nat and my second child, Todd, who was born two years later, used to play endlessly back there. The larger environment was also quite rural. On occasion I would take one of the kids in a backpack across the street and up into Briones park to explore the wild oak woods and the herds of horses that roamed freely on the ridges.  We lived in this house many years, but the day finally came when we had to sell out and move to Stanford. I remember the last day very clearly. The moving van had left, Anita was just driving away with the last load of stuff, and Todd and I were left to vacuum up the last bits of dust. The house was echoing, as houses do when they have no people in them. I pointed out to him that this was the sound of ghosts. Here was where my children were born. Here was where we had run madly around the apple trees and smashed the violets. Here was where I had planted the Monterey pine which was now shading the patio. Todd stared at me for a moment and then said with obvious annoyance, “Let’s go, Daddy.”  **Stanford** My career at Livermore was effectively derailed by the fractional quantum hall theory, for I became so famous on account of it that I had to travel constantly and could not begin learning the classified parts of the Laboratory’s business. I am to this day rather poorly educated about nuclear weapons, and I know virtually nothing about laser fusion capsules. The Lab had plenty of money in those days, and the head of H-Division, Hal Graboske, always supported my requests for travel, including once a trip to Denmark, Finland, and the Soviet Union. But the handwriting was on the wall. In 1984 offers began pouring in from universities, and there was a particularly good one from Stanford. Stanford at that time had a terrific solid state physics faculty and was quite a bit smaller, and therefore more malleable, than my old alma mater Berkeley. Also Anita and I were worried about the anti-intellectual attitudes towards the University of California expressed by the state legislature, worries that proved well-founded when a few years later salaries were capped, teaching loads were increased, and the state budget was not passed on time, causing faculty to be paid in IOU’s. I had originally turned down all these university offer on the theory that the research environment would be better inside at Livermore where somebody else took care of salaries and research support. But Stan Wojcicki, Sandy Fetter, and Mac Beasley were particularly persistent, and I got sound advice from both Berni Alder and Anita not to let this train go by, so in the end I relented and accepted their offer. Little did I know that in a few years the Cold War would end, the Department of Energy’s budget would be squeezed, and the “expendable” public-domain science activities paid for by fat budgets would be the first thing to go. This is a well-known effect in industrial laboratories, as the recent histories of Bell Labs, Xerox, and IBM Research have sadly reminded us. But at this time I accepted mainly because of Berni’s advice.  When I moved to Stanford I began to pursue the line of research I have been following ever since, namely trying to understand the larger implications of fractional quantum hall discovery. The historical significance of the effect is a matter of some debate, but my own view is that it sets a precedent for completely trivial equations of motion to generate particles carrying fractional quantum numbers and a concomitant set of gauge forces between them, both of these also being postulates of the Standard Model. Thus I think the trail leads ultimately to big questions about the universe and cosmology. Progress in this direction has been painfully slow because the experiments have not cooperated. Most of the leads for finding other effects in nature analogous to the fractional quantum Hall effect turned out to be false, including particularly high-temperature superconductors, which have been my main materials physics research interest here for the last several years. However, Phil Anderson’s idea that the phenomenology of the cuprates might be related to the known behavior of 1-dimensional antiferromagnets, which has a beautiful formal relationship to the fractional quantum hall ground state discovered by Duncan Haldane and Sriram Shastry, is still very intriguing, especially since the particles carrying fractional quantum numbers in the latter system, which we call spinons, are relativistic.  My experience with high-temperature superconductors has been very different from the fractional quantum Hall effect. The problem has been difficult to formulate clearly, and progress has been slow. I do not know whether this is due to an historical paradigm shift or just intellectual incompetence on our part, but I certainly find myself yearning for the good old days when the entirety of a problem could be understood in 10 seconds. It has been my good fortune to have an excellent group of experimentalists at Stanford with whom to work, notably Aharon Kapitulnik, Ted Geballe, and Mac Beasley, known collectively as the KGB, and Z.-X. Shen. I predicted optical rotary activity in bulk cuprate superconductors, which was unfortunately disproved by Aharon, although the symmetry breaking I predicted was eventually found by Laura Greene at the University of Illinois. I also predicted the so-called large pseudogap in the cuprates that was eventually discovered by Prof. Shen in photoemission from samples in extreme underdoping. I have also done some mathematical physics work for which I am very proud, most notably the invention of the Kalmeyer-Laughlin spin liquid vacuum and “anyon” superconductivity, although these things broke with my tradition of going directly to nature for inspiration and are in this sense flawed.  My job at Stanford is rather different from the ones I had held previously in that my own ambitions must take a back seat to the well-being of the students with whom I work. But this actually is not very difficult. My own sons are almost college age now, and my rule is simply to do for my students exactly what I hope someone else will do for my sons when the time comes: I teach them to have faith in themselves and in their own compass, to listen to nature to find truth, to love knowledge for the sake of itself, and to strive for greatness. A few days after the Nobel Prize announcement I got the following wonderful e-mail from Andrew Tikofsky, one of my best graduate students, who is now on Wall Street:  Hi Bob, Ian McDonald, Steve Strong, and I are getting together for a beer near Grand Central Station this coming Tuesday in honor of your prize. You are cordially invited to attend. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Biographical** | I was born on April 6, 1949 in a regional hospital in Frankfurt am Main in Germany. Having the umbilical cord wrapped twice tightly around my neck, my parents’ fear for the mental health of their first-born son subsided only gradually.  My forefathers had been farmers, inn-keepers, blacksmiths, carpenters and shop keepers in the region. My mother, an elementary school teacher, and my father, having finished an apprenticeship, had been married during the previous year, shortly after a devastating war. Opening a store for interior decoration in my father’s home town of Sprendlingen, they were trying to build an existence and start a family at the same time. Eighteen months later a brother, Heinz, was born without the umbilical complications.  Sprendlingen, today a part of Dreieich, just south of Frankfurt, was a town of some 15,000 inhabitants. I was raised in the circle of an extended family of four uncles and aunts, who, together with my parents, lived in two houses with barns and sheds and the store surrounding a large yard. It was an ideal playground for two boys growing up with their cousins – this group always extended by a horde of friends. Constructing huge sand castles with moats and bridges, cardboard tents from the shop’s packing material, building elaborate knight’s armour from scrap floor-covering and intricate race tracks for marbles from curtain rails remain fond memories of childhood.  I began kindergarten at age three and was soon after joined by my brother. The kindergarten’s seemingly unlimited amount of toy building blocks must have fascinated me and I soon became somewhat of the establishment’s chief architect. School, at six, was a happy time, complemented in the afternoons by playing soccer in our yard, roaming about the fields surrounding my home town, and building dozens of detailed cardboard model ships and airplanes from “Ausschneidebögen”.  There was never a doubt in my parents’ mind that their sons would receive the best possible education. Although none of my forefathers graduated from high school, my parents regarded highly the merits of a good education as a tool for social advancement. In their value system knowledge always ranked above wealth – although not rejecting a possible fortuitous marriage of both. To enter “Gymnasium”, at ten, required the passing of a test. I was accepted and from then on commuted for eight years, five km each way, to the “Goethe Gymnasium” in the neighboring town of Neu Isenburg.  Gymnasium was hard. I was not a particularly good student. I loved mathematics and the sciences, but I barely scraped by in German and English and French. Receiving an “F” in either of these subjects always loomed over my head and kept me many a year at the brink of having to repeat a level. Luckily there was “Ausgleich”, balancing a bad grade in one subject with a good grade in another. Mathematics and later physics got me through school without repeat performance. I also excelled in sports, particularly in track and field, where I won a school championship in the 50 m dash. But sports could not be used for “Ausgleich”.  One of my teachers stood out, Mr. Nick. He taught math and physics. A new teacher, basically straight out of college, young, open, articulate, fun, he represented what teachers could be like. His love and curiosity for the subjects he was teaching was contagious. As 15 or 16 year-olds, we read sections of [Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html)‘s Lecture Notes in Physics in a voluntary afternoon course he offered.  Having mastered wooden building blocks and cardboard models, passed erector sets and toy trains, I had reached the level of “Elektro-Mann” and “Radio-Mann”. Dozens of telephones and light boxes to communicate between the sheds at home were designed, constructed, improved, and mercilessly wrecked, possibly foreshadowing my later employment by a communications company. And then, of course, there was chemistry, a subject I did not appreciate in school, but it held the secrets for making explosives. I built a rocket that propelled a modified car of a toy train into the air. After several exhilarating launches, the rocket exploded in my hand and ripped off half my right thumb. I learned an important lesson: a rocket and a bomb differ only in the exhaust. Affecting me somewhat during adolescence, the missing thumb also relieved me from army duty. Today, it is only an unimportant, physical curiosity.  I always wanted to become a physicist. Supposedly, at age six, I had told just that to a technician, who was repairing a TV set in our home. Obviously, I had little clue as to what a physicist did. Nevertheless, the goal persisted all through high school, but suddenly got overthrown during the last year of “Gymnasium” when an art teacher discovered my talent for design. I passed my baccalaureate with average grades – quite good in the sciences but quite poor in the humanities – and started to study architecture at the Technical High School in Darmstadt, about 20 km south of my home town. Being too late at application time, I had to register for “Lehrfach für Bauwesen”, a related subject, that consisted of similar freshmen courses as architecture. I turned out to be very good in making any technical drawing of a bird cage from any requested angle, but very poor in freehand drawing and decided that architecture was not for me. Instead I went on to pursue my true love – physics.  As with architecture in Darmstadt, I was too late for registration in physics at the Goethe University in Frankfurt and took up mathematics instead, transferring to physics the following year. The year was 1968. Student revolts swept the campuses from Berkeley to Berlin. Frankfurt was a major site for riots in the streets and in the lecture halls. For a young student, hardly familiar with university life, largely ignorant of the aim of the different protests, these were uncertain times. Legitimate educational reform requests became confused with larger political issues leading to absurd happenings around campus. Damage was done to the institution of the university and its teaching staff but, at the same time, 1968 marked the beginning of a gradual and rational reform.  Studying physics and mathematics was wonderful. It was a far cry from Gymnasium. I loved the rigor of mathematics. In physics we had fascinating beginners lectures by two descendants of the famous “Pohl School” of Göttingen, Prof. Martienssen and Prof. Queisser. I had joined a group of likeminded students that studied together and hung out in “Café Bauer” for relaxation. Life was good, until I took the “Vordiplom”, the major exam in all courses at the end of the fourth semester.  All physics and math exams – some six to eight written or verbal tests – went very well. They went so well, that I thought I needn’t study at all for the dreaded verbal chemistry test. With straight “A”s in physics and mathematics, what was the chemistry professor to do but let me pass? I was mistaken and flunked badly, requiring *all* tests to be taken again, six months, later. Thankfully, physics and math professors – some having had experiences of their own with chemistry tests – conspired and promised to maintain my grades in those subjects. It gave me six months, to study nothing but chemistry. I never felt more confident walking into an exam and succeeded getting an “A” in *chemistry.* I had been wary of the field of chemistry throughout high school and during much of my studies. Counting valences and bonds, memorizing dozens of exceptions to the rules and hundreds of arcane compounds never made much sense to me. I came to revise my attitude towards chemistry once I had grasped quantum mechanics and the origin of the chemical bond.  The thesis work for my Diploma – in Germany a required step towards the Ph.D. – was performed in Professor Werner Martienssen’s Physical Institute under the supervision of a young assistant professor Eckhardt Hoenig. Professor Hoenig had just returned from the United States, where he had worked on highly-sensitive superconducting detectors, so-called SQUIDS. The aim was to use these new devices to study the magnetic properties of hemoglobin to derive the geometry of its bond with oxygen. It was a time of immense joy paired with intense learning of intricate low-temperature techniques. Hoenig was a wizard in inventing and building sophisticated instrumentation to attack physics questions. [Gerd Binnig](https://www.nobelprize.org/nobel_prizes/physics/laureates/1986/index.html), who later shared the Nobel Prize for the invention of the Scanning Tunneling Microscope, was another student of a total of four working with Hoenig at this time in the same lab. It is probably coincidental, nevertheless, I believe our education in experimental physics down in this basement of the “Neubau” was second to none and strongly affected our experimental approaches throughout our careers. Hemoglobin did not bow to our instruments, at least over the course of a year, and I quickly performed some measurements on iron impurities in magnesium. I wrote an unimpressive diploma thesis on the magnetic anisotrophy of their susceptibility and received the necessary license to start with a Ph. D. thesis.  At this time, my horizon unexpectedly widened. It had never occurred to me, nor to many of my town’s youngsters, to go to university anywhere else but Frankfurt or Darmstadt. We went to the closest one and lived at home, where our families had been based for generations. However, in the fall of 1974, a former student from Frankfurt, Wolfgang Kottler, visited. He had since moved to Grenoble, France, where the Max-Planck-Institute for Solid State Research in Stuttgart was operating a high-magnetic field facility together with the French National Center for Scientific Research, CNRS. He was just finishing his Ph.D. thesis under Professor Hans-Joachim Queisser and was beating the bushes for his own replacement in Grenoble. Initially hesitant to make such a big step, moreover to a foreign country, the mastery of whose language I largely failed in school, I visited Grenoble and asked myself: Why not?  Going to Grenoble was the single most important step in my life. Leaving the familiar surroundings of home, diving into another culture, another language, meeting new people, making new friends was initially frightening, but eventually immensely educational and gratifying. Meeting my wife, Dominique Parchet, in Grenoble certainly added to the city’s attractions. Grenoble, at the edge of the Alps, not far from Switzerland was the French Science City. The magnet lab had been established only a few years back. Professor Klaus Dransfeld was the local director. There existed a frontier atmosphere with an exhilarating “can do” sentiment. It was an international place. Many famous scientists passed through and, due to the informality surrounding the lab, even the students were able to meet them on a very personal basis. This was quite different from other, more hierarchically structured research institutes. In a certain sense, students were kings at the magnet lab. They knew all the ins and outs of the magnets and the visiting collaborators were willing to share their scientific knowledge with them in return. It also was there, I first met [Daniel Tsui](https://www.nobelprize.org/nobel_prizes/physics/laureates/1998/index.html) from Bell Labs.  My thesis project was to work on the properties of electron hole droplets in high magnetic fields, a subject that had been proposed by Dieter Bimberg of the magnet lab. I was joined by Rolf Martin, who had just received his Ph.D. from the University of Stuttgart. Together we spent hundreds of immensely enjoyable and very productive research hours – daytime or nighttime – around the colossal magnets. Sharing a French “villa” with Ronald Ranvaud, where many distinguished visitors from abroad were often guests, life revolved totally around science. I finished my thesis in just over two years and received my Ph.D. from the University of Stuttgart, where my thesis advisor, Prof. Queisser, now a director at the Max-Planck-Institute in Stuttgart, held the position of an honorary professor. Instead of the usual dedication, my thesis had started with a cartoon. I learned only recently, that this had been a major cause of irritation and that removal of the cartoon as well as cutting my shoulder-length hair could barely be warded off.  All through my Ph.D. years, Prof. Queisser had urged me to finish my thesis swiftly and move on to the United States. He himself had been in the US, working at Bell Labs and later with Shockley, one of the inventors of the transistor. Bell Labs, the research arm of American Telephone and Telegraph (AT&T), was the “Mecca” of solid state research. Strongly encouraged and supported by my thesis advisor, I had visited Bell Labs and worked with John Hensel on electron hole droplets for several weeks during the spring of 1976. The visit was also intended to make contact with Raymond Dingle of Bell Labs. At the time, he was working on semiconductor quantum wells, an exciting new area of research made possible by the invention of molecular beam epitaxy (MBE) in the late ’60s by Alfred Cho, also of Bell Labs. I had heard Dingle speaking on the topic at the 1975 March meeting of the German Physical Society and had decided that *this* was the subject I wanted to pursue. As it turned out Queisser knew Dingle personally and with partial financial support from the Max-Planck-Institute in Stuttgart I was accepted into a consultant position in Venky Narayanamurti’s Department, working effectively as a postdoc with Ray Dingle. I moved to Bell Labs in June 1977.  Modulation-doping, the technique to generate ultra-high mobility two-dimensional electron systems, instrumental for practically all of my later research, was conceived about two weeks after my arrival at Bell Labs in a conversation with Ray Dingle. In his office, he had outlined their recent efforts to introduce free carriers into semiconductor superlattices and had sketched the positions of band edges, impurities and electrons on his white-board. It occurred to me that by placing impurities exclusively into the potential barriers, while keeping them out of the potential wells, the scattering of electrons by impurities should be reduced, thus increasing mobilities. It was a casual, almost trivial observation, which, however, turned out to have big impact.  Modifications to the MBE crystal growth instrumentation of Arthur Gossard and his assistant William Wiegmann to allow for such a selective doping were made over the course of a few months, and they demonstrated the anticipated gains in mobilities. Initially, mobilities improved by a mere factor two or three over conventionally doped superlattices, but they have since grown by another factor of ~1000. Loren Pfeiffer and Ken West, both from Bell Labs, have led this effort and have consistently provided the most exquisite samples for research. Much of our experimental success rests on our direct access to their “candy store”.  Modulation-doping gained me a permanent position at Bell Labs in the fall 1978, and I was soon joined by my long-time assistant, Kirk Baldwin. With such high-quality material available, many physics experiments – previously conducted on two-dimensional electron systems in silicon – became feasible in gallium arsenide. It also opened the door to many optical experiments on two-dimensional electron systems, largely performed by Aron Pinczuk and his colleagues at Bell Labs in Holmdel.  At the time, Dan Tsui of Bell Labs was already recognized as one of the world’s leading experts on two-dimensional electron systems in silicon. He quickly recognized the potential of the new material for research and invited him on his frequent trips to the MIT Francis Bitter High Magnetic Field Lab in Cambridge, Massachusetts. It was the beginning of a scientific collaboration and personal friendship, which has lasted now for almost 20 years.  The quantum Hall effect, having just been discovered in 1980 by Klaus von Klitzing, was a major topic of our research. Another topic was the electron crystal, which was theoretically predicted to form in very low electron density samples in very high magnetic field. An exceptionally high quality, low electron density specimen had just been fabricated by Art Gossard and Willy Wiegmann. Dan Tsui had succeeded in contacting it electrically, and in October 1981 we took it to the Magnet Lab to look for signs of an electron crystal. What we discovered instead, during the evening of October 6, was the fractional quantum Hall effect.  Since this discovery, many outstanding graduate students (Gregory Boebinger, Robert Willett, Andrew Yeh, Wei Pan), postdocs (Albert Chang, Hong-Wen Jiang, Rui Du, Woowon Kang) and colleagues (James Eisenstein, Peter Berglund) joined us and made discoveries of their own in this fascinating research area. Other postdocs working with me (Edwin Batke, Rick Hall, Joe Spector, Ray Ashoori, and Amir Yacoby) have performed research in neighboring areas, but affected our thinking in lower-dimensional physics in general.  In 1983, I was promoted to head the department for Electronic and Optical Properties of Solids. Administration was a minor chore during those days, and I could continue to pursue my own research, practically full time. They were very exciting and intense research days during which the fractional quantum Hall effect and its implications were established in many laboratories around the world. Theoretical progress was rapid and exhilarating.  In 1991, I was promoted to director of the Physical Research Laboratory, heading some 100 researchers in eight departments in William Brinkman’s Physics Research Division at Bell Labs. The time available for my own research dwindled, but I was compensated by becoming exposed to a wide range of exciting research topics. The initial satisfaction faded when the physical sciences at Bell Labs came under strong pressure from management to contract. These were difficult years, not just for me, but much more so for many of my friends and colleagues at Bell Labs. I was reminded of Gymnasium and the power of teachers. With the split-up of AT&T in 1996, the creation of Lucent Technologies, which subsumed Bell Labs, and a change of leadership, the physical sciences at Bell Labs are blossoming again today.  I always had thought of becoming a teacher one day. Being totally immersed in exciting research at Bell Labs, the idea had faded. It was resurfacing. I stepped down from my position in the Summer of 1997 and joined Columbia University in January of 1998 as a Professor of Physics and Applied Physics, while remaining Adjunct Physics Director at Bell Labs, part-time. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0073 |
| **Biographical** | I tend to partition my life into three compartments: childhood years in a remote village in the province of Henan in central China, schooling years in Hong Kong, and the years since I came to attend college in the United States. The only thread connecting them is the kindness, generosity and friendship from the people around me that I have experienced all my life.  My childhood memories are filled with the years of drought, flood and war which were constantly on the consciousness of the inhabitants of my over-populated village, but also with my parents’ self-sacrificing love and the happy moments they created for me. Like most other villagers, my parents never had the opportunity to learn how to read and write. They suffered from their illiteracy and their suffering made them determined not to have their children follow the same path at any and whatever cost to them. In early 1951, my parents seized the first and perhaps the only opportunity to have me leave them and their village to pursue education in so far away a place that neither they nor I knew how far it truly was.  In Hong Kong, I began my formal schooling at the sixth grade level with fear and trembling, mixed with some pride and elation. I remember the difficulties that I encountered in not knowing the Cantonese dialect in the beginning, but, even more vividly, the overwhelming kindness of schoolmates who went out of their way to help by offering me their friendship, bringing me into their circle, and taking me to their out-of-class activities. In the middle of my second year in Hong Kong, I entered Pui Ching Middle School, which was known for being outstanding, especially in natural science subjects. Many of the teachers there were overqualified. They were the brightest graduates of the best universities in China and under normal circumstances would have been highly accomplished scholars and scientists. The upheaval of war in China, however, forced them to hibernate in Hong Kong teaching high school kids. They might not have been the best teachers pedagogically, but their intellects and their visions inspired us. Even their casual remarks and the stories from their romantic reminiscences of the glorious days at Peking University could leave indelible marks on us. It was they, I think, who in their unconscious ways dared us students, living in a most commercialized city, to look beyond the dollar sign and see the exploration of new frontiers in human knowledge as an intellectually rewarding and challenging pursuit.  I graduated from Pui Ching in 1957 and was admitted to the medical school of National Taiwan University in Taiwan. However, since it was unclear at the time how my parents were and whether I could return to them in China, I stayed in Hong Kong and entered a two-year special program run by the government to prepare Chinese high school graduates for the University of Hong Kong. In late spring the next year, I received the surprising good news from the United States that I was admitted with a full scholarship to my church pastor’s Lutheran alma mater, Augustana College in Rock Island, Illinois. I arrived on campus right after Labor Day 1958, and there spent the best three years of my life. It was there that I had for the first time the leisure to wrestle with my Lutheran faith and to think through and make some sense out of my life experience. In Hong Kong, I was always extremely busy as a scholarship student, heavily involved with church activities and responsibilities, and worn-out from long distance daily commuting. Here, I was free to read, to learn and to think through things at my own pace. I knew from the start that I would go to graduate school, and the choice of subject and school was never a problem. [C.N. Yang](https://www.nobelprize.org/nobel_prizes/physics/laureates/1957/index.html) and [T.D. Lee](https://www.nobelprize.org/nobel_prizes/physics/laureates/1957/index.html) were awarded the Nobel Prize for Physics in 1957 and they both went to the University of Chicago. Yang and Lee were the role models for Chinese students of my generation and going to the University of Chicago for a graduate education was the ideal pilgrimage.  The University of Chicago was intense and intellectual. I liked its being in a major city, its cosmopolitan atmosphere, and even its grimy buildings and the austerity they appeared to convey. There, I luckily met and fell in love with Linda Varland, an undergraduate in the college, and we were married after her graduation. I was also fortunate that Royal Stark, who had just joined the physics faculty as a solid state experimentalist, took me on as a research assistant in the building-up of his laboratory. I realized quite early that I wanted to do experimental physics and that I lacked the aptitude for colossal experimental setups and also the taste for grandeur. I wanted to do tabletop experiments and be allowed to tinker. Royal Stark trusted me and let me try my hands on everything in his laboratory. I was given the best opportunity to learn from the bottom up: from engineer drawing, soldering, machining, and design, to construction and building of our laboratory apparatus. By the time I received my Ph.D., I was confident that I could make a living using the technical skills I had learned there. Since I could always fall back on a job using my technical skills, I reasoned, why not then take a risk and try a research position doing something entirely novel and at the same time intellectually challenging.  I left Chicago in early spring 1968 and took a position in Bell Laboratories in Murray Hill, New Jersey to do research in solid state physics. I found myself a niche in semiconductor research, though I never got into the main stream either in semiconductor physics, which was mostly optics and high energy band-structures, or its use in device applications. I wandered into a new frontier, which was dubbed the physics of two-dimensional electrons. In February 1982, shortly after the discovery of the fractional quantum Hall effect, I moved to Princeton and started teaching.  Many of my friends and esteemed colleagues had asked me: “Why did you choose to leave Bell Laboratories and go to Princeton University?”. Even today, I do not know the answer. Was it to do with the schooling I missed in my childhood? Maybe. Perhaps it was the Confucius in me, the faint voice I often heard when I was alone, that the only meaningful life is a life of learning. What better way is there to learn than through teaching! |
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| **Physics\_1999-** | |
| **ID** | 0074 |
| **Biographical** | My father, Ju Chin Chu, came to the United States in 1943 to continue his education at the Massachusetts Institute of Technology in chemical engineering, and two years later, my mother, Ching Chen Li, joined him to study economics. A generation earlier, my mother’s father earned his advanced degrees in civil engineering at Cornell while his brother studied physics under Perrin at the Sorbonne before they returned to China. However, when my parents married in 1945, China was in turmoil and the possibility of returning grew increasingly remote, and they decided to begin their family in the United States. My brothers and I were born as part of a typical nomadic academic career: my older brother was born in 1946 while my father was finishing at MIT, I was born in St. Louis in 1948 while my father taught at Washington University, and my younger brother completed the family in Queens shortly after my father took a position as a professor at the Brooklyn Polytechnic Institute.  In 1950, we settled in Garden City, New York, a bedroom community within commuting distance of Brooklyn Polytechnic. There were only two other Chinese families in this town of 25,000, but to our parents, the determining factor was the quality of the public school system. Education in my family was not merely emphasized, it was our raison d’être. Virtually all of our aunts and uncles had Ph.D.’s in science or engineering, and it was taken for granted that the next generation of Chu’s were to follow the family tradition. When the dust had settled, my two brothers and four cousins collected three MDs, four Ph.D.s and a law degree. I could manage only a single advanced degree.  In this family of accomplished scholars, I was to become the academic black sheep. I performed adequately at school, but in comparison to my older brother, who set the record for the highest cumulative average for our high school, my performance was decidedly mediocre. I studied, but not in a particularly efficient manner. Occasionally, I would focus on a particular school project and become obsessed with, what seemed to my mother, to be trivial details instead of apportioning the time I spent on school work in a more efficient way.  I approached the bulk of my schoolwork as a chore rather than an intellectual adventure. The tedium was relieved by a few courses that seem to be qualitatively different. Geometry was the first exciting course I remember. Instead of memorizing facts, we were asked to think in clear, logical steps. Beginning from a few intuitive postulates, far reaching consequences could be derived, and I took immediately to the sport of proving theorems. I also fondly remember several of my English courses where the assigned reading often led to binges where I read many books by the same author.  Despite the importance of education in our family, my life was not completely centered around school work or recreational reading. In the summer after kindergarten, a friend introduced me to the joys of building plastic model airplanes and warships. By the fourth grade, I graduated to an erector set and spent many happy hours constructing devices of unknown purpose where the main design criterion was to maximize the number of moving parts and overall size. The living room rug was frequently littered with hundreds of metal “girders” and tiny nuts and bolts surrounding half-finished structures. An understanding mother allowed me to keep the projects going for days on end. As I grew older, my interests expanded to playing with chemistry: a friend and I experimented with homemade rockets, in part funded by money my parents gave me for lunch at school. One summer, we turned our hobby into a business as we tested our neighbors’ soil for acidity and missing nutrients.  I also developed an interest in sports, and played in informal games at a nearby school yard where the neighborhood children met to play touch football, baseball, basketball and occasionally, ice hockey. In the eighth grade, I taught myself tennis by reading a book, and in the following year, I joined the school team as a “second string” substitute, a position I held for the next three years. I also taught myself how to pole vault using bamboo poles obtained from the local carpet store. I was soon able to clear 8 feet, but was not good enough to make the track team.  In my senior year, I took advanced placement physics and calculus. These two courses were taught with the same spirit as my earlier geometry course. Instead of a long list of formulas to memorize, we were presented with a few basic ideas or a set of very natural assumptions. I was also blessed by two talented and dedicated teachers.  My physics teacher, Thomas Miner was particularly gifted. To this day, I remember how he introduced the subject of physics. He told us we were going to learn how to deal with very simple questions such as how a body falls due to the acceleration of gravity. Through a combination of conjecture and observations, ideas could be cast into a theory that can be tested by experiments. The small set of questions that physics could address might seem trivial compared to humanistic concerns. Despite the modest goals of physics, knowledge gained in this way would become collected wisdom through the ultimate arbitrator – experiment.  In addition to an incredibly clear and precise introduction to the subject, Mr. Miner also encouraged ambitious laboratory projects. For the better part of my last semester at Garden City High, I constructed a physical pendulum and used it to make a “precision” measurement of gravity. The years of experience building things taught me skills that were directly applicable to the construction of the pendulum. Ironically, twenty five years later, I was to develop a refined version of this measurement using laser cooled atoms in an atomic fountain interferometer.  I applied to a number of colleges in the fall of my senior year, but because of my relatively lackluster A-average in high school, I was rejected by the Ivy League schools, but was accepted at Rochester. By comparison, my older brother was attending Princeton, two cousins were in Harvard and a third was at Bryn Mawr. My younger brother seemed to have escaped the family pressure to excel in school by going to college without earning a high school diploma and by avoiding a career in science. (He nevertheless got a Ph.D. at the age of 21 followed by a law degree from Harvard and is now a managing partner of a major law firm.) As I prepared to go to college, I consoled myself that I would be an anonymous student, out of the shadow of my illustrious family.  **The Rochester and Berkeley Years** At Rochester, I came with the same emotions as many of the entering freshman: everything was new, exciting and a bit overwhelming, but at least nobody had heard of my brothers and cousins. I enrolled in a two-year, introductory physics sequence that used *The Feynman Lectures in Physics* as the textbook. The *Lectures* were mesmerizing and inspirational. [Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html) made physics seem so beautiful and his love of the subject is shown through each page. Learning to do the problem sets was another matter, and it was only years later that I began to appreciate what a magician he was at getting answers.  In my sophomore year, I became increasingly interested in mathematics and declared a major in both mathematics and physics. My math professors were particularly good, especially relative to the physics instructor I had that year. If it were not for the Feynman Lectures, I would have almost assuredly left physics. The pull towards mathematics was partly social: as a lowly undergraduate student, several math professors adopted me and I was invited to several faculty parties.  The obvious compromise between mathematics and physics was to become a theoretical physicist. My heroes were Newton, Maxwell, [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/index.html), up to the contemporary giants such as Feynman, [Gell-Mann](https://www.nobelprize.org/nobel_prizes/physics/laureates/1969/index.html), [Yang](https://www.nobelprize.org/nobel_prizes/physics/laureates/1957/index.html) and [Lee](https://www.nobelprize.org/nobel_prizes/physics/laureates/1957/index.html). My courses did not stress the importance of the experimental contributions, and I was led to believe that the “smartest” students became theorists while the remainder were relegated to experimental grunts. Sadly, I had forgotten Mr. Miner’s first important lesson in physics.  Hoping to become a theoretical physicist, I applied to Berkeley, Stanford, Stony Brook (Yang was there!) and Princeton. I chose to go to Berkeley and entered in the fall of 1970. At that time, the number of available jobs in physics was shrinking and prospects were especially difficult for budding young theorists. I recall the faculty admonishing us about the perils of theoretical physics: unless we were going to be as good as Feynman, we would be better off in experimental physics. To the best of my knowledge, this warning had no effect on either me or my fellow students.  After I passed the qualifying exam, I was recruited by Eugene Commins. I admired his breadth of knowledge and his teaching ability but did not yet learn of his uncanny ability to bring out the best in all of his students. He was ending a series of beta decay experiments and was casting around for a new direction of research. He was getting interested in astrophysics at the time and asked me to think about proto-star formation of a closely coupled binary pair. I had spent the summer between Rochester and Berkeley at the National Radio Astronomy Observatory trying to determine the deceleration of the universe with high red-shift radio source galaxies and was drawn to astrophysics. However, in the next two months, I avoided working on the theoretical problem he gave me and instead played in the lab.  One of my “play-experiments” was motivated by my interest in classical music. I noticed that one could hear out-of-tune notes played in a very fast run by a violinist. A simple estimate suggested that the frequency accuracy,delta vtimes the duration of the note,delta tdid not satisfy the uncertainty relationshipdelta v delta t is bigger or equal to 1. In order to test the frequency sensitivity of the ear, I connected an audio oscillator to a linear gate so that a tone burst of varying duration could be produced. I then asked my fellow graduate students to match the frequency of an arbitrarily chosen tone by adjusting the knob of another audio oscillator until the notes sounded the same. Students with the best musical ears could identify the center frequency of a tone burst that eventually sounded like a “click” with an accuracy ofdelta v delta t is approximately 0.1.  By this time it was becoming obvious (even to me) that I would be much happier as an experimentalist and I told my advisor. He agreed and started me on a beta-decay experiment looking for “second-class currents”, but after a year of building, we abandoned it to measure the Lamb shift in high-Z hydrogen-like ions. In 1974, Claude and Marie Bouchiat published their proposal to look for parity non-conserving effects in atomic transitions. The unified theory of weak and electromagnetic interactions suggested by [Weinberg, Salam](https://www.nobelprize.org/nobel_prizes/physics/laureates/1979/index.html) and [Glashow](https://www.nobelprize.org/nobel_prizes/physics/laureates/1979/index.html) postulated a neutral mediator of the weak force in addition to the known charged forces. Such an interaction would manifest itself as a very slight asymmetry in the absorption of left and right circularly polarized light in a magnetic dipole transition. Gene was always drawn to work that probed the most fundamental aspects of physics, and we were excited by the prospect that a table-top experiment could say something decisive about high energy physics. The experiment needed a state-of-the-art laser and my advisor knew nothing about lasers. I brashly told him not to worry; I would build it and we would be up and running in no time.  This work was tremendously exciting and the world was definitely watching us. Steven Weinberg would call my advisor every few months, hoping to hear news of a parity violating effect. Dave Jackson, a high energy theorist, and I would sometimes meet at the university swimming pool. During several of these encounters, he squinted at me and tersely asked, “Got a number yet?” The unspoken message was, “How dare you swim when there is important work to be done!”  Midway into the experiment, I told my advisor that I had suffered enough as a graduate student so he elevated me to post-doc status. Two years later, we and three graduate students published our first results. Unfortunately, we were scooped: a few months earlier, a beautiful high energy experiment at the Stanford Linear Collider had seen convincing evidence of neutral weak interactions between electrons and quarks. Nevertheless, I was offered a job as assistant professor at Berkeley in the spring of 1978.  I had spent all of my graduate and postdoctoral days at Berkeley and the faculty was concerned about inbreeding. As a solution, they hired me but also would permit me to take an immediate leave of absence before starting my own group at Berkeley. I loved Berkeley, but realized that I had a narrow view of science and saw this as a wonderful opportunity to broaden myself.  **A Random Walk in Science at Bell Labs** I joined Bell Laboratories in the fall of 1978. I was one of roughly two dozen brash, young scientists that were hired within a two year period. We felt like the “Chosen Ones”, with no obligation to do anything except the research we loved best. The joy and excitement of doing science permeated the halls. The cramped labs and office cubicles forced us to interact with each other and follow each others’ progress. The animated discussions were common during and after seminars and at lunch and continued on the tennis courts and at parties. The atmosphere was too electric to abandon, and I never returned to Berkeley. To this day I feel guilty about it, but I think that the faculty understood my decision and have forgiven me.  Bell Labs management supplied us with funding, shielded us from extraneous bureaucracy, and urged us not to be satisfied with doing merely “good science.” My department head, Peter Eisenberger, told me to spend my first six months in the library and talk to people before deciding what to do. A year later during a performance review, he chided me not to be content with anything less than “starting a new field”. I responded that I would be more than happy to do that, but needed a hint as to *what* new field he had in mind.  I spent the first year at Bell writing a paper reviewing the current status of x-ray microscopy and started an experiment on energy transfer in ruby with Hyatt Gibbs and Sam McCall. I also began planning the experiment on the optical spectroscopy of positronium. Positronium, an atom made up of an electron and its anti-particle, was considered the most basic of all atoms, and a precise measurement of its energy levels was a long standing goal ever since the atom was discovered in 1950. The problem was that the atoms would annihilate into gamma rays after only 140×10-9 seconds, and it was impossible to produce enough of them at any given time. When I started the experiment, there were 12 published attempts to observe the optical fluorescence of the atom. People only publish failures if they have spent enough time and money so their funding agencies demand something in return.  My management thought I was ruining my career by trying an impossible experiment. After two years of no results, they strongly suggested that I abandon my quest. But I was stubborn and I had a secret weapon: his name is Allen Mills. Our strengths complemented each other beautifully, but in the end, he helped me solve the laser and metrology problems while I helped him with his positrons. We finally managed to observe a signal working with only ~4 atoms per laser pulse! Two years later and with 20 atoms per pulse, we refined our methods and obtained one of the most accurate measurements of quantum electrodynamic corrections to an atomic system.  In the fall of 1983, I became head of the Quantum Electronics Research Department and moved to another branch of Bell Labs at Holmdel, New Jersey. By then my research interests had broadened, and I was using picosecond laser techniques to look at excitons as a potential system for observing metal-insulator transitions and Anderson localization. With this apparatus, I accidentally discovered a counter-intuitive pulse-propagation effect. I was also planning to enter surface science by constructing a novel electron spectrometer based on threshold ionization of atoms that could potentially increase the energy resolution by more than an order of magnitude.  While designing the electron spectrometer, I began talking informally with Art Ashkin, a colleague at Holmdel. Art had a dream to trap atoms with light, but the management stopped the work four years ago. An important experiment had demonstrated the dipole force, but the experimenters had reached an impasse. Over the next few months, I began to realize the way to hold onto atoms with light was to first get them very cold. Laser cooling was going to make possible all of Art Ashkin’s dreams plus a lot more. I promptly dropped most of my other experiments and with Leo Holberg, my new post-doc, and my technician, Alex Cable, began our laser cooling experiment. This brings me to the beginning of our work in laser cooling and trapping of atoms and the subject of my Nobel Lecture.  **Stanford and the future** Life at Bell Labs, like Mary Poppins, was “practically perfect in every way”. However, in 1987, I decided to leave my cozy ivory tower. Ted Hänsch had left Stanford to become co-director of the Max Planck Institute for Quantum Optics and I was recruited to replace him. Within a few months, I also received offers from Berkeley and Harvard, and I thought the offers were as good as they were ever going to be. My management at Bell Labs was successful in keeping me at Bell Labs for 9 years, but I wanted to be like my mentor, Gene Commins, and the urge to spawn scientific progeny was growing stronger.  Ted Geballe, a distinguished colleague of mine at Stanford who also went from Berkeley to Bell to Stanford years earlier, described our motives: “The best part of working at a university is the students. They come in fresh, enthusiastic, open to ideas, unscarred by the battles of life. They don’t realize it, but they’re the recipients of the best our society can offer. If a mind is ever free to be creative, that’s the time. They come in believing textbooks are authoritative but eventually they figure out that textbooks and professors don’t know everything, and then they start to think on their own. Then, I begin learning from them.”  My students at Stanford have been extraordinary, and I have learned much from them. Much of my most important work such as fleshing out the details of polarization gradient cooling, the demonstration of the atomic fountain clock, and the development of atom interferometers and a new method of laser cooling based on [Raman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1930/index.html) pulses was done at Stanford with my students as collaborators.  While still continuing in laser cooling and trapping of atoms, I have recently ventured into polymer physics and biology. In 1986, Ashkin showed that the first optical atom trap demonstrated at Bell Labs also worked on tiny glass spheres embedded in water. A year after I came to Stanford, I set about to manipulate individual DNA molecules with the so-called “optical tweezers” by attaching micron-sized polystyrene spheres to the ends of the molecule. My idea was to use two optical tweezers introduced into an optical microscope to grab the plastic handles glued to the ends of the molecule. Steve Kron, an M.D./Ph.D. student in the medical school, introduced me to molecular biology in the evenings. By 1990, we could see an image of a single, fluorescently labeled DNA molecule in real time as we stretched it out in water. My students improved upon our first attempts after they discovered our initial protocol demanded luck as a major ingredient. Using our new ability to simultaneously visualize and manipulate individual molecules of DNA, my group began to answer polymer dynamics questions that have persisted for decades. Even more thrilling, we discovered something new in the last year: identical molecules in the same initial state will choose several distinct pathways to a new equilibrium state. This “molecular individualism” was never anticipated in previous polymer dynamics theories or simulations.  I have been at Stanford for ten and a half years. The constant demands of my department and university and the ever increasing work needed to obtain funding have stolen much of my precious thinking time, and I sometimes yearn for the halcyon days of Bell Labs. Then, I think of the work my students and post-docs have done with me at Stanford and how we have grown together during this time.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1997*, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1998  This autobiography/biography was written at the time of the award and later published in the book series [*Les Prix Nobel/*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)[*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*/*[*The Nobel Prizes*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/nobel-prizes.html). The information is sometimes updated with an addendum submitted by the Laureate.  Copyright © The Nobel Foundation 1997  **Addendum, September 2017**  Professor Steven Chu is the William R. Kenan, Jr, Professor of Physics and Professor of Molecular and Cellular Physiology at Stanford University, California, United States. Chu is the co-recipient of the Nobel Prize in Physics 1997, received numerous other awards and 31 honorary degrees. Since 2000, he has devoted an increasing portion of his scientific career to the search for new solutions to our energy and climate challenges. He was appointed the Secretary of Energy in President [Obama](https://www.nobelprize.org/nobel_prizes/peace/laureates/2009/obama-facts.html)‘s government in January 2009, a position he held until April 2013.  As the first scientist to hold a Cabinet position in the United States, he helped identify and recruit over a dozen outstanding scientists and engineers into the Department of Energy. While at the DOE, he began several initiatives, including the ARPA-E (Advanced Research Projects Agency – Energy), the Energy Innovation Hubs, and the Clean Energy Ministerial meetings, and was personally tasked by President Obama to assist BP in stopping the Deepwater Horizon oil leak.  From 2004 until the end of 2008, Chu was the Director of the DOE’s Lawrence Berkeley National Laboratory, and was also Professor of Physics and Professor of Molecular and Cell Biology at the University of California, Berkeley. Prior to those positions, he was the Theodore and Francis Geballe Professor of Physics and Applied Physics Departments at Stanford University (1987-2008). During his time at Stanford, he twice chaired the Department of Physics and helped start Bio-X, a multi-disciplinary initiative that brings together the physical and biological sciences with engineering and medicine, and the Kavli Institute for Particle Astrophysics and Cosmology. From 1978-1987, Chu worked at AT&T Bell Laboratories, including four years as Head of the Quantum Electronics Research Department.  While at Bell Labs, Chu led the group that showed how to first cool and then trap atoms with light. The “optical tweezers” atom trap is also widely used in biology. Other contributions include the demonstration of the magneto-optic trap, the most widely used atom trap today. At Stanford, he developed the theory of laser cooling of actual, multilevel atoms (also developed independently by [Claude Cohen-Tannoudji](https://www.nobelprize.org/nobel_prizes/physics/laureates/1997/cohen-tannoudji-facts.html) and Jean Dalibard), and demonstrated the first atomic fountain/fountain atomic clock. For this work, he was a co-recipient of the Nobel Prize in Physics 1997. Chu and his research group also introduced atom interferometry based on optical pulses of light, a technique that has remained the most precise form of atom interferometry. Chu and his group pioneered the use of optical tweezers to manipulate and study individual DNA molecules, and were the first to use FRET (Fluorescence Resonance Energy Transfer) to study induced conformational changes, unfolding and refolding of active enzymes, and molecular interactions between individual biological molecules.  In May 2013, he returned to Stanford in the Physics and Molecular and Cellular Physiology Departments. In addition to his continuing work marshalling scientists and resources to address the energy and climate change challenges, he has begun a new research program synthesizing and applying new nanoparticle probes for biology and biomedical research. He is also working on new approaches to lithium ion batteries, PM2.5 air filtration and other applications of nanotechnology and electrochemistry. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q3 | **What do you most enjoy about science?** |
|  | “What I enjoy about it is you’re trying to figure out how the world works. I love the idea of cumulative knowledge – by experiment, observation and quantitative analysis you get closer to what you think is going on. And that accumulates over time. It’s not just one person or one set of time but over many hundreds of years.” |
| Q5 | **Who has inspired you?** |
|  | “There are numerous people that I met when I was in college, in graduate school and beyond. People whose general interest in science really drives them well beyond job security or even getting Nobel Prizes.  For example, [Charles Townes the inventor of the laser](https://www.nobelprize.org/prizes/physics/1964/townes/facts/) died at 99, almost 100. And up until the last year or two of his life he was working. I remember calling him up when he was about 95 or 96. I wanted to talk to him about something – it was a Saturday afternoon – and I called home and his wife answered. I said, ‘Oh Frances is Charles there?’ And she said, ‘Steve! It’s Saturday! He’s at work, he’s never home Saturday.’ And I was also at work – I still work on Saturdays.  This is part of why science is so wonderful – the people who do it really love doing it.” |
| Q18 | **Is it important to do work that has an impact on society?** |
|  | “As a citizen I hope that all people, whether they are scientists or not would think about what is happening in the world around them.  Scientists have a special place in the sense that they have perhaps a higher ability to understand many of the issues that society faces where science gives guidance. Science doesn’t say you have to do this and you have to do that. But science does tell you – if for example the ozone layer is being threatened by certain chemicals in the atmosphere – what’s the cause of it, what’s happening and then society has to respond to it. The same is true of health and clean water, clean air and finally of climate change.” |
| Q18 | **Why is water such an important issue right now?** |
|  | “Water is necessary for life. We among the planets on our solar system have a very special place. Seventy percent of the earth’s surface is water but that water is salt water. What really sustains life is fresh water.  Fresh water is produced by the heat of the sun, evaporation, rain or snow – and our fresh water resources are being much more strained in part because of the rising population. In addition to the rising population, is rising wealth so more and more people can afford and demand more meat in their diet. That means you have to grow more food but also more food for livestock that then use that food to generate meat for people to eat. This all puts a strain on the water supplies.” |
| Q18 | **How is climate change shaping water on earth?** |
|  | “Over the last couple of decades there’s much more evidence that the climate is changing and a large factor is due to humans. It’s something where as time goes by we’re finding that the earth has a very hair-trigger response to slight changes.  Already with a one degree rise we see weather in the last couple of decades has dramatically turned. But that’s just the tip of what we’re going to be seeing. Most of the impacts will be later in this century and in the next several hundred years.  For example glaciers, particularly in Antarctica, are beginning to slide. The ice is beginning to roll down, sort of like a slow-motion avalanche when once it’s going you can’t stop it until you’ve reestablished a friction grounding line. Even if you decrease the temperature it will require decades in order for it to slow down and stop. This is something we did not expect.  We also know as a matter of history, not climate models, that in a world one degree warmer than today the average sea level was six to nine metres higher than today. We used to think that it would take several thousand years [to reach that], now we think it’s going to be a few hundred years.  A six to nine metre rise in sea level would mean that central London is underwater. Ten percent of the world’s population lives within ten metres of sea level. Because that’s now expected to happen much faster that means you’re going to get displaced people from sea water rises.” |
| Q18 | **What can we do to tackle this problem?** |
|  | “There’s an increasing number of scientists and engineers who recognise this is such a problem that they’re beginning to shift careers, as I have done. I have tried to encourage other scientists – if you have any knowledge or talent and think you can do something – to invent better batteries for electric vehicles, better energy storage, better distribution systems.” |
| Q9 | **Has receiving the Nobel Prize helped you in this work?** |
|  | “One could say it gives you more voice, but in the end what gives you the voice is not a prize you might have but what you’re saying. That’s the beautiful thing about science.” |

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| **Physics\_1999-** | |
| **ID** | 0075 |
| **Biographical** | I was born on April 1, 1933 in Constantine, Algeria, which was then part of France. My family, originally from Tangier, settled in Tunisia and then in Algeria in the 16th century after having fled Spain during the Inquisition. In fact, our name, Cohen-Tannoudji, means simply the Cohen family from Tangiers. The Algerian Jews obtained the French citizenship in 1870 after Algeria became a French colony in 1830.  My parents lived a modest life and their main concern was the education of their children. My father was a self-taught man but had a great intellectual curiosity, not only for biblical and talmudic texts, but also for philosophy, psychoanalysis and history. He passed on to me his taste for studies, for discussion, for debate, and he taught me what I regard as being the fundamental features of the Jewish tradition – studying, learning and sharing knowledge with others.  As a child, I was very lucky to escape the tragic events which marked this century. The arrival of the Americans in Algeria, in November of 1942, saved us from the nazi persecutions that were spreading throughout Europe at the time. I completed my primary and secondary school education in Algiers. And I was also lucky enough to finish high school in very good conditions and to leave Algiers for Paris, in 1953, before the war in Algeria and the stormy period that preceded the independence.  I came to Paris because I was admitted to the Ecole Normale Supérieure. This French “grande école”, founded during the French Revolution about 200 years ago, selects the top high school students who do well in the selective final examination. The four years at this school, from 1953 to 1957, were indeed a unique experience for me. During the first year, I attended a series of fascinating lectures in mathematics given by Henri Cartan and Laurent Schwartz, in physics by [Alfred Kastler](https://www.nobelprize.org/nobel_prizes/physics/laureates/1966/index.html). Initially, I was more interested in mathematics but Kastler’s lectures were so stimulating, and his personality so attractive, that I ended up changing to physics.  In 1955, when I joined Kastler’s group to do my “diploma” work, the group was very small. One of Kastler’s first students, Jean Brossel, who had returned four years before from M.I.T. where he had done research work with Francis Bitter, was supervising the thesis work of Jacques Emile Blamont and Jacques Michel Winter.  We were a small group, but the enthusiasm for research was exceptional and we worked hard. Brossel and Kastler were in the lab nearly day and night, even on weekends. We had endless discussions on how to interpret our experimental results. At the time, the equipment was rather poor and we did what we could without computers, recorders and signal averagers. We measured resonance curves point by point with a galvanometer, each curve five times, and then averaged by hand. We were, somehow, able to get nice curves and exciting results. I think that what I learned during that period was essential for my subsequent research work and key personalities such as Alfred Kastler and Jean Brossel certainly had a significant role in it.  We were going together, once a week, to attend the new lectures given in Saclay by Albert Messiah on quantum mechanics, by Anatole Abragam on NMR and by Claude Bloch on nuclear physics. I can still remember the stimulating atmosphere of these lectures.  During the summer of 1955, I also spent two months at the famous Les Houches summer school in the Alps. This school has contributed largely to the development of theoretical physics in France. At that time, the school offered an intense training in modern physics with about six lectures a day, for two months, and the lecturers were [J. Schwinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html), [N. Ramsey](https://www.nobelprize.org/nobel_prizes/physics/laureates/1989/index.html), G. Uhlenbeck, [W. Pauli](https://www.nobelprize.org/nobel_prizes/physics/laureates/1945/index.html), A. Abragam, A. Messiah, C. Bloch … to mention a few.  After finishing my “diploma” studies, I still had to get through the final examination “Agrégation” before leaving Ecole Normale as a student. The “Agrégation” is a competitive examination for teaching posts in high schools. The preparation consists of theoretical and experimental courses as well as some pedagogical training. You give a lecture attended by other students and a professor and after, there is a moment of general debate and constructive criticism in view of perfecting your lecture. Kastler, I remember, participated in the pedagogical training and he taught us how to organize and present our lecture.  Well, about this time I met Jacqueline who became my wife in 1958. She has shared with me all the difficult and happy times of life. She has been able to pursue her own career as a high school physics and chemistry teacher, to raise our three children Alain, Joëlle and Michel, to be part of the daily life of a researcher which can sometimes be very difficult and demanding. We have had, as many, our share of family tragedy and losing our oldest son Alain was a great misfortune to us all. Alain died in 1993, of a long illness, at the age of 34.  After the “Agrégation”, I left the Ecole Normale and did my military service which was very long (28 months) because of the Algeria war. I was, though, assigned part of the time to a scientific department supervised by Jacques Emile Blamont. We were studying the upper atmosphere with rockets releasing sodium clouds at the sunset. By looking at the fluorescence light reemitted by the sodium atoms excited by the sunlight, it was possible to measure the variations with the altitude of various parameters such as the wind velocity or the temperature.  Then, in the beginning of 1960, I came back to the laboratory to do a Ph.D. under the supervision of Alfred Kastler and Jean Brossel with a research post at the CNRS (French National Center for Scientific Research). The lab had by then been expanded. Bernard Cagnac was finishing his thesis on the optical pumping of the odd isotopes of mercury and I was trying, with Jean-Pierre Barrat, to derive a master equation for the optical pumping cycle and to understand the physics of the off-diagonal elements of the density matrix (the so-called atomic “coherences”). Our calculations predicted the existence of “light shifts” for the various Zeeman sublevels, a curious phenomenon we did not expect at all. I decided to try to see this effect. Cagnac left me his experimental set up during Christmas vacations and I remember getting the first experimental evidence on Christmas Eve of 1960. I was very excited and both Kastler and Brossel were very happy indeed. Kastler called the effect the “Lamp shift”, since it is produced by the light coming from a discharge lamp. Nowadays, it is called light shift or a.c. Stark shift. I built a new experimental set up to check in detail several other predictions of our calculations, especially the conservation of Zeeman coherences during the optical pumping cycle. I submitted my Ph.D. in December of 1962. The members of the committee were Jean Brossel, Pierre Jacquinot, Alfred Kastler and Jacques Yvon.  Shortly after my Ph.D. Alfred Kastler urged me to accept a teaching position at the University of Paris. I followed his advice and started to teach at the undergraduate level. At about this time, there was a new reform in the University system: the so-called “troisième cycle” that consisted of teaching a graduate level with a flexible program. Jean Brossel asked me to teach quantum mechanics. He was teaching atomic physics, Alfred Kastler and Jacques Yvon statistical physics, Pierre Aigrain and [Pierre-Gilles de Gennes](https://www.nobelprize.org/nobel_prizes/physics/laureates/1991/gennes-facts.html) solid state physics.  We had the best students of the Ecole Normale attending these lectures, so I set up a small group where every year a new student would join in and do a post-graduate thesis or a Ph.D. In 1967, I was asked to teach quantum mechanics at a lower level (second cycle). The book “Quantum Mechanics” originated from this teaching experience and was done in collaboration with Franck Laloë and Bernard Diu.  Understanding atom-photon interactions in the high intensity limit where perturbative treatments are no longer valid was one of the main goals of our research group. This led us to develop a new approach to these problems where one considers the “atom + photons” system as a global isolated system described by a time-independent Hamiltonian having true energy levels. We called such a system the “dressed atom”. Although the quantum description of the electromagnetic field used in such an approach is not essential to interpret most physical effects encountered in atomic physics, it turned out that the dressed atom approach was very useful in providing new physical insights into atom-photon interactions. New physical effects, which were difficult to predict by standard semiclassical methods, were appearing clearly in the energy diagram of the dressed atom when examining how this energy diagram changes when the number of photons increases. We first introduced the dressed atom approach in the radio-frequency range while Nicole Polonsky, Serge [Haroche](https://www.nobelprize.org/nobel_prizes/physics/laureates/2012/), Jacques Dupont-Roc, Claire Landré, Gilbert Grynberg, Maryvonne Ledourneuf, Claude Fabre were working on their thesis. One of the new effects which were predicted and observed was the modification, and even the cancellation of the Landé factor of an atomic level by interaction with an intense, high frequency radio-frequency field. This effect presents some analogy with the *g-*2 anomaly of the electron spin except that it has the opposite sign: the*g-*factor of the atomic level is reduced by virtual absorption and reemission of RF photons whereas the factor of the electron spin is enhanced by radiative corrections.  We devoted a lot of efforts to the interpretation of this change of sign and this led us, years later (with Jacques Dupont-Roc and Jean Dalibard), to propose new physical pictures involving the respective contributions of vacuum fluctuations and radiation reaction. And while this was going on, we had some very stimulating discussions with Victor Weisskopf who has always been interested in the physical interpretation of the *g*-2 anomaly.  The dressed atom approach has also been very useful in the optical domain. Spontaneous emission plays an important role as a damping mechanism and as a source of fluorescence photons. Serge Reynaud and I applied this approach to the interpretation of resonance fluorescence in intense resonant laser beams. New physical pictures were given for the Mollow triplet and for the absorption spectrum of a weak probe beam, with the prediction and the observation of new Doppler free lines resulting from a compensation of the Doppler effect by velocity dependent light shifts. The picture of the dressed atom radiative cascade also provided new insights into photon correlations and photon antibunching. New types of time correlations between the photons emitted in the two sidebands of the Mollow triplet were predicted in this way and observed experimentally at the Institut d’Optique in Orsay, in collaboration with [Alain Aspect](https://www.nobelprize.org/prizes/physics/2022/aspect/facts/).  An important event in my scientific life has been my appointment as a Professor at the Collège de France in 1973. The Collège de France is a very special institution created in 1530, by King François I, to counterbalance the influence of the Sorbonne which was, at that time, too scholastic and where only latin and theology were taught. The first appointed by the King were 3 lecturers in Hebrew, 2 in Greek and 1 in Mathematics. This institution survived all revolutions and remains, to this day, reputed for its flexibility. Today there are 52 professors in all subjects, and lectures are open to all, for there is no registration and no degrees given. We professors are free to choose the topics of our lectures. The only rule is that these lectures must change and deal with different topics every year, which is very difficult and demanding. It is, however, very stimulating because this urges one to broaden one’s knowledge, to explore new fields and to challenge oneself. No doubt that without such an effort I would not have started many of the research lines that have been explored by my research group. I am very grateful to Anatole Abragam who is at the origin of my appointment at the Collège de France. Part of this teaching experience incited the two books on quantum electrodynamics and quantum optics written with Jacques Dupont-Roc and Gilbert Grynberg.  In the early 1980s, I chose to lecture on radiative forces, a field which was very new at that time. I was also trying with Serge Reynaud, Christian Tanguy and Jean Dalibard to apply the dressed atom approach to the interpretation of atomic motion in a laser wave. New ideas were emerging from such an analysis related to, in particular, the interpretation of the mean value, the flucalations and the velocity dependence of dipole forces in terms of spatial gradients of dressed state energies and of spontaneous transitions between these dressed states.  When in 1984 I was given the possibility to appoint someone to the position of Associate Director for my laboratory, at the Collège de France, I offered the post to Alain Aspect and then invited him to join me in forming, with Jean Dalibard, a new experimental group on laser cooling and trapping. A year later, Christophe Salomon who came back from a postdoctoral stay in JILA with Jan Hall, decided to join our group. This was a new very exciting scientific period for us. We began to investigate a new cooling mechanism suggested by the dressed atom approach and that resulted from correlations between the spatial modulations of the dressed state energies in a high intensity laser standing wave and the spatial modulations of the spontaneous rates between the dressed states. As a result of these correlations, the moving atom is running up potential hills more frequently than down. We first called such a scheme “stimulated blue molasses” because it appears for a blue detuning of the cooling lasers, contrary to what happens for Doppler molasses which require a red detuning. In fact, this new scheme was the first high intensity version of what is called now “Sisyphus cooling”, a denomination that we introduced in 1986. We also observed, shortly after, the channeling of atoms at the nodes or antinodes of a standing wave. This was the first demonstration of laser confinement of neutral atoms in optical-wavelength-size regions.  A few years later, in 1988, when sub-Doppler temperatures were observed by Bill Phillips, who had been collaborating with us, we were prepared with our background in optical pumping, light shifts and dressed atoms, to find the explanation of such anomalous low temperatures. In fact, they were resulting from yet another (low intensity) version of Sisyphus cooling. Similar conclusions were reached by [Steve Chu](https://www.nobelprize.org/nobel_prizes/physics/laureates/1997/index.html) and his colleagues. At the same time, we were exploring, with Alain Aspect and Ennio Arimondo, the possibility of applying coherent population trapping to laser cooling. By making such a quantum interference effect velocity selective, we were able to demonstrate a new cooling scheme with no lower limit, which can notably cool atoms below the recoil limit corresponding to the recoil kinetic energy of an atom absorbing or emitting a single photon. These exciting developments opened the microKelvin and even the nanoKelvin range to laser cooling, and they allowed several new applications to be explored with success.  These applications will not be described here since they are the subject of the Nobel Lecture which follows this presentation. The purpose here was merely to give an idea of my scientific itinerary and to express my gratitude to all those who have helped me live such a great adventure: my family, my teachers, my students and my fellow colleagues all over the world.  I dedicate my Nobel Lecture to the memory of my son Alain. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0076 |
| **Biographical** | I was born on 5 November 1948 in Wilkes-Barre, Pennsylvania, just across the river from the town of Kingston, where my parents lived with my one and a half year old sister, Maxine. My parents had come to this small Pennsylvania town from places and backgrounds that were far apart and yet quite similar.  My mother, Mary Catherine Savino (later, Savine), was born in the southern Italian village of Ripacandida in 1913. Among her earliest memories are riding into her grandfather’s vineyards in a horse-drawn cart. Her father emigrated to the US and brought the family to Altoona, Pennsylvania in 1920. Her new American schoolmates teased her for her inability to speak English and taunted her as a “Wop” for her Italian heritage. She resolved to excel, and so she did, graduating near the top of her class from Altoona High School.  My father, William (Bill) Cornelius Phillips, was born in Juniata, a community on the edge of Altoona, in 1907. His father was a carpenter and his mother operated a boarding house to augment the family income. His grandfather was a barrel-maker, who would demonstrate the quality of his product by jumping onto the finished barrel in front of the customer. Dad could trace his heritage to ancestors from Wales who fought in the American Revolution.  My father and mother were each the first in their families to go to college, each attending Juniata College, a small school in Huntingdon, Pennsylvania, founded and strongly influenced by the pacifist Church of the Brethren. My father and mother graduated from Juniata in 1930 and 1936, respectively, but never met until a Juniata professor who knew them both suggested to my father that he might call a young Juniata alumna and ask her out. This Italian Catholic young woman and this Welsh-American Methodist young man met, fell in love, got married, earned Masters degrees and became professional social workers in the hard coal country of Pennsylvania.  I grew up surrounded by family and friends, church and school, and physical and mental activity. I clearly remember the value my parents placed on reading and education. My parents read to us and encouraged us to read. As soon as I could read for myself, walking across town to the library became a regular activity. Almost as far back as I can remember, I was interested in science. I assembled a collection of bottles of household substances as my “chemistry set” and examined almost anything I could find with the microscope my parents gave me. Although they had no particular knowledge or special interest in science, they supported mine. Science was only one of the passions of my childhood, along with fishing, baseball, bike riding and tree climbing. But as time went on, Erector sets, microscopes, and chemistry sets captured more of my attention than baseball bats, fishing rods, and football helmets. In 1956, my family moved from Kingston to Butler, near Pittsburgh. I remember that during that time I decided that science was going to be my life work, and sometime during the late 1950s, I came to appreciate, in a very incomplete and naive way, the simplicity and beauty of physics.  My brother Tom was born in 1957 – a concrete confirmation, my sister and I believed, of the power of prayer. We had been praying for a sibling, unaware that our parents could decide, and had decided, that two children were enough. Apparently our prayers were effective. The result was a thrill and a blessing for all of us. Another blessing was my being placed into an experimental “accelerated” class. There, dedicated and concerned teachers taught us things that were not part of the ordinary elementary school curriculum, like French and advanced mathematics. When my family moved to Camp Hill, near Harrisburg, in 1959, interested teachers continued to provide me with advanced instruction, and when I entered the 7th grade of Camp Hill High School in 1960, it was in another accelerated program.  During this time, I had a laboratory in the basement of our family home. Ignorant and heedless of the dangers of asbestos, electricity, and ultraviolet light, I spent many hours experimenting with fire, explosives, rockets and carbon arcs. But life was not all science. I ran for the track team and played for the tennis team at school. During the summer, I spent all day either on the tennis courts or in the community swimming pool, and considered the advantages of life as a tennis bum.  While my parents were not directly involved in my scientific interests, they tolerated my experiments, even when the circuit breakers all tripped because of my overloads. They were always encouraging, and there was never any lack of intellectual stimulation. Dinner table conversations included discussions of politics, history, sociology, and current events. We children were heard and respected, but we had to compete for the privilege of expressing our opinions. In these discussions our parents transmitted important values about respect for other people, for their cultures, their ethnic backgrounds, their faith and beliefs, even when very different from our own. We learned concern for others who were less fortunate than we were. These values were supported and strengthened by a maturing religious faith.  In high school, I enjoyed and profited from well-taught science and math classes, but in retrospect, I can see that the classes that emphasized language and writing skills were just as important for the development of my scientific career as were science and math. I certainly feel that my high school involvement in debating competitions helped me later to give better scientific talks, that the classes in writing style helped me to write better papers, and the study of French greatly enhanced the tremendously fruitful collaboration I was to have with Claude Cohen-Tannoudji’s research group.  The summer after my junior year in high school, I worked at the University of Delaware doing sputtering experiments. It was a great experience and I learned an important truth from Jim Comas, the graduate student who supervised me. “An experimental physicist,” he told me, “is someone who gets paid for working at his hobby.”  Another important part of my high school experience was meeting Jane Van Wynen. Her family had moved from Maine when we were in ninth grade, but we largely ignored each other until our senior year when, during a school trip to the New York World’s Fair during its closing days in 1965, I became suddenly aware of her considerable charms. She was not so immediately convinced that I had any charms of interest to her, but my natural tenacity paid off, and we started dating.  In the fall of 1966 I started my studies at Juniata College, as my mother and father, my Aunt Betty, and my sister had before me, and as my younger brother, Tom would later. Juniata had a foreign language requirement, which could be satisfied by studying two years of a language or by passing a test. I passed the test in French, whereupon the chairman of the French department, who knew my sister, a French major in her senior year, suggested that I enroll in an advanced French literature class. Being a naive freshman, I did. The professor lectured in French, we read classic French literature and wrote our exams in French – not what I was used to in high school! I got a “C” on my first test and realized that college was not going to be as easy as high school. I finished the course with an “A”, and learned an important lesson: I would have to work hard at Juniata.  Physics with calculus was a challenge as well, but a true joy. Ray Pfrogner, who taught that first course, revealed a beauty and a unity in physics and mathematics that, until then, I had lacked the tools to appreciate. Some evenings he invited us students to showings of films of [Richard Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html)‘s classic public lectures on “The Character of Physical Law.” These events included popcorn that Pfrogner popped himself. Feynman’s breezy yet incisive style on occasional evenings and Pfrogner’s clear expositions every other morning fueled my passion for physics.  My passion for Jane was also increasing during this time, fueled by daily letters, weekly phone calls and infrequent visits to her school, Penn State University. It is a passion that has matured and deepened but remained undiminished over the years. Our separation during our college years meant that I did not have a highly active social life, leaving lots of time for physics.  During my first year at Juniata, Wilfred Norris, the Physics Department chairman, invited me to start on the laboratory course normally taken by third-year students – a series of classic physics experiments, which I did under his supervision. Later, I started doing serious research under Norris’s direction, rebuilding an X-band electron spin resonance (ESR) spectrometer and trying to resolve discrepancies in the literature about ESR linewidths.  In my senior year I spent a semester doing ESR at Argonne National Laboratories, working with Juan McMillan and Ted Halpern. There, I experienced full-time research, performed by a team of professionals who would discuss what the important problems were, decide what to do, how to do it, and then go into the lab and do it. I loved it!  Back at Juniata for my final semester, I was applying to graduate schools. First on my list was Princeton – because I had heard its graduate program was superb and because a visitor to Juniata had told me that a physics student from my school would never be accepted to Princeton! I was accepted, but a visit to Princeton left me unconvinced that I wanted to go there. From the lobby of the Princeton physics building, I called Dan Kleppner at the Massachusetts Institute of Technology (MIT).  Dan had seen my application to MIT, including my experience in magnetic resonance, and had invited me to visit his group and consider working on a hydrogen maser experiment. So I visited MIT (and Harvard for good measure); I was struck by the pleasant camaraderie, and the friendly yet electric atmosphere that Dan had created in his group. That emotional reaction, and Jane’s desire to return to New England, more than any purely scientific considerations, made me decide to go to MIT. I never regretted that decision, or any of the other decisions I made afterwards based on considerations of the heart.  During a hectic several weeks in 1970, Jane and I graduated from our respective colleges, married, honeymooned and moved to Boston. At MIT I started working with Fred Walther on the high-field hydrogen maser, another X-band magnetic resonance spectrometer. I learned how to do electronics, machining, plumbing and vacuum – all skills I have found essential in experimental research. I also learned from Dan, and from the others in his group, a way of thinking about physics intuitively, and a way of inquiring about a problem that has shaped the way I approach physics to this day. The style of open and lively discussion of physics problems that I found in Dan’s group is one that I have tried to emulate in my own group at NIST. I also try to follow the principle Dan taught by example: that one can do physics at the frontiers, competing with the best in the world, and do it with openness, humanity and cooperation.  For my thesis research I measured the magnetic moment of the proton in H2O. Through this project I met others in the community of precision measurements and fundamental constants – in particular, Barry Taylor and Ed Williams at the National Bureau of Standards. By the time I completed that measurement (which is, at least for the moment, still the best of its kind), tunable dye lasers had become commercially available and had found their way into our lab. I decided that I should learn more about these new toys and, with Dan’s encouragement, embarked on an experiment to study the collisions of laser-excited atoms. I finally wrote up both experiments for my thesis and defended it in 1976.  I accepted a Chaim Weizmann fellowship to work on projects of my own choosing at MIT for another two years. During that time, I continued to work on collisions with Dave Pritchard and Jim Kinsey; I also started work on Bose-Einstein condensation (BEC) in spin-polarized hydrogen with Dan and Tom Greytak. We were filled with optimism in the early days of that experiment, but today, 22 years later, BEC of hydrogen is still “just around the corner.” Nevertheless, the innovations achieved by that group, long after I left, along with the developments in laser cooling recognized by this year’s Nobel Prize, were crucial in showing the way to the eventual success of BEC in alkali vapors.  At the party celebrating my thesis in 1976, Dan Kleppner said it was fortunate that I had done the second experiment, using lasers, because otherwise I would probably have ended up going to the National Bureau of Standards (NBS). In 1978 I accepted a position at NBS (later renamed the National Institute of Standards and Technology-NIST) in Barry Taylor’s division, working with Ed Williams and Tom Olsen on precision measurements of the proton gyromagnetic ratio and of the Absolute Ampere. These were exciting projects, but my experience with lasers and atomic physics had also earned me the opportunity to devote part of my time to exploring ways of improving measurement capabilities using those tools. I used that opportunity to pursue laser cooling, and the story of how that went is told in the accompanying Nobel Lecture.  In 1979, shortly after Jane and I moved to Gaithersburg, we joined Fairhaven United Methodist Church. We had not been regular church-goers during our years at MIT, but Ed and Jean Williams invited us to Fairhaven and there we found a congregation whose ethnic and racial diversity offered an irresistible richness of worship experience. Later that year, our first daughter, Catherine, now known as Caitlin, was born. In 1981 Christine was born. Our children have been an unending source of blessing, adventure and challenge. Their arrival, at a time when both Jane and I were trying to establish ourselves in new jobs, required a delicate balancing of work, home, and church life. Somehow, our faith and our youthful energy got us through that period.  At NBS, with some borrowed equipment and some extra money that Barry Taylor, in his inimitable fashion, obtained from somewhere, I got started with laser cooling. Support from the Office of Naval Research allowed Hal Metcalf to spend time at NBS in those early days. I had worked with Hal a little at MIT, and I knew that his unbounded enthusiasm and his effervescent creativity were priceless qualities. My collaborating with Hal on laser cooling was the first and one of the most important among many valuable interactions with colleagues who came to NIST, or whom I met elsewhere. I have mentioned many of these in my Lecture, and I want to emphasize again how much they have contributed to the development of laser cooling, and particularly, how important the senior group members, Kris Helmerson, Paul Lett, Steve Rolston, and Chris Westbrook, have been. I also want to recall the words of Bengt Nagel in his formal remarks to Steve Chu, Claude Cohen-Tannoudji and myself on 10 December 1997 in Stockholm. He said that we were being recognized as leaders and representatives of our groups. The three of us feel very strongly that this Prize honors all of those wonderful colleagues who contributed so much to the development of laser cooling.  Since the announcement of the award of the 1997 Nobel Prize in Physics, I have been honored to receive greetings and congratulations from colleagues and friends all over the world, as well as from many people whom I did not know. One such greeting came, not to me but to my children, from Susan Hench Bowis. She had read newspaper accounts of the announcement and recalled to my teenage daughters that she had been 17 when in 1950 her father, [Philip Hench](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1950/index.html), had been awarded the Nobel Prize in Physiology or Medicine. He had been far from home at the time of the announcement, as I had been, and, like Caitlin, Susan Hench had been away at school. Transatlantic telephone calls were not common in those days, and so when she eventually made contact and congratulated her father, it was by cable. He cabled back to her, “Prouder of you, my darling, than of any prize.” Surely the Nobel Prize is the highest award a scientist could hope to receive, and I have received it with a sense of awe that I am in the company of those who have received it before. But no prize can compare in importance to the family and friends I count as my greatest treasures. |
| **Autobiographical** |  |
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| **Biographical** | My parents were born and brought up in New York City. My father was trained as an electrical engineer and my mother was an elementary school teacher. They were the children of Jewish immigrants who had come to the United States from England and Lithuania in the late 1800’s. One of my great grandfathers had actually settled in the United States considerably earlier. When I was born on January 20, 1931, my parents lived in a small suburban town, Rye, New York, just outside New York City. My father commuted by train to his job at a small but growing electrical manufacturing company in the city. During the great economic depression of the early 1930’s we moved to the city for a few years to save money, but eventually moved back to Rye, where I received my early education. As time went on our family circumstances improved as my father advanced in his company, which was expanding rapidly, and eventually became its president.  As a child I was fascinated by living things in the fields and along the coast line near our home. I was constantly roaming around collecting frogs, fish, salamanders, snakes and worms. Starting at the age of six, I spent every summer away from home at various children’s camps in New England, giving me further opportunities to explore this interest.  My other childhood passion was railways. I managed to accumulate an extensive collection of railway timetables covering the entire U.S.A. and became a young travel expert. When I was a very young child my father gave me a set of spring-operated “wind-up” trains. The first thing I did was to insert the tracks into the electric socket in our kitchen. A shower of sparks flew all over the room. Fortunately my parents were indulgent and everyone laughed about the incident.  As a young teenager I became very interested in meteorology. I kept my own weather records and subscribed to the daily weather map issued by the U.S. weather bureau. One day I asked my father about a book in his library entitled *The Mysterious Universe* by Sir James Jeans. He indicated that no one really understood what was in the book. I immediately picked up the book and began to read it. There was a beautiful discussion of the cosmology known at that time, which I found totally fascinating. I think that this book really sparked my interest in physics.  The high school in Rye had an excellent program. There was emphasis on acquiring the necessary basic skills in writing and mathematics through extensive exercises but we were also taught to think for ourselves. I owe a considerable debt of gratitude to my teachers. Of course most young boys during that time wanted to be sports heroes and I was no exception. I was a reasonably good short distance runner and so was active on our school track team, as well as a participant in our high school football program, but there was no chance that I would ever be a sports hero.  Following graduation from high school in 1948, I attended Harvard University where I became a physics major. Having grown up in a small town, I found Harvard to be an enormously enriching experience. Students in my class came from all walks of life and from a great variety of geographical locations. I still stay in touch with many of my college friends. At one time during my college years I considered the possibility of a career in medicine. With this in mind I took some of the pre-medical courses in addition to my physics major. I especially enjoyed the course in organic chemistry, but in spite of my early interests, I did not find the biological sciences fascinating. Therefore I gave up the idea of a career in medicine and continued with my studies of physics. My main extracurricular activity was the Harvard Yacht Club. In June 1950 a group of us sailed in the Bermuda race from Newport, Rhode Island, to Hamilton, Bermuda. It was a wonderful adventure.  After 3 1/2 years at Harvard, I had enough credits to graduate in January 1952. In April 1952, I entered the U.S. Army for 22 months and served at various posts in the continental United States during the final stages of the Korean War. One night during this period I was serving as corporal of the guard. One of the guards was a young soldier named Herbert Fried. It turned out that he had been a graduate student at the University of Connecticut with Professor Paul Zilsel who specialized in the theory of superfluidity. We had a wonderful discussion about superfluid helium 4. Later on Herbert Fried became a Professor of Theoretical Physics at Brown University.  Following my honorable discharge from the army, I entered the University of Connecticut in February 1954, partly as a result of my discussion with Herbert Fried, and partly because my parents had moved to Connecticut, so it was now my home state. The one and one-half year stay at the University of Connecticut was extremely beneficial. It gave me the chance to study physics in a relatively relaxed setting and to learn about experimental physics. My first project was to build an ionization gauge control circuit for Professor Edgar Everhart’s Cockcroft-Walton accelerator. In those days vacuum tubes were the active components in electronic circuits. I can still recall the warm orange glow of the vacuum tube filaments and the cool blue glow of the thyratron tubes. In assembling and trouble shooting my circuit, I can also still remember all the 300 volt electric shocks from the vacuum tube power supply.  While at the University of Connecticut, I met my lifelong friend John Reppy who was later to become my colleague in our Cornell low temperature group. John was doing experimental research on superfluid liquid helium with Professor Charles Reynolds. It was Professor Reynolds who really excited my interest in superfluidity and low temperature physics.  In addition to John Reppy’s prowess as an experimental physicist, he was a rock climber and mountaineer, par excellence. He somehow persuaded me to overcome my natural fear of heights and took me on some wonderful climbs in the Grand Tetons of Wyoming and the Black Hills of South Dakota in the American west. I still enjoy hiking in the mountains.  Eventually I completed my requirements for the Master of Science degree at the University of Connecticut, after which I enrolled in the Ph.D. program in physics at Yale University in the summer of 1955. My summer project at Yale was to build a mercury jet stripper for the Heavy Ion Linear Accelerator then under construction. By removing more electrons from an ion, one could increase its net charge and thus accelerate it to higher energies. Electrons from the ions were removed rather efficiently when the ions were passed through a supersonic jet of mercury atoms. Also during my first summer at Yale I met Russell Donnelly who was finishing his Ph.D. thesis on rotating superfluid helium in the Yale low temperature group with Professor Cecil T. Lane. Russ was a talented experimentalist with tremendous enthusiasm for physics. He has had a distinguished career and is now a Professor at the University of Oregon. In addition to my work on the accelerator, I enjoyed helping Russ with his experiments that summer. In a very short time, I learned a great deal about experimental low temperature physics and the life of an experimental physicist. As time went on my growing fascination with low temperature physics led me to the decision that this would be my area of specialization in graduate school. Fortunately, Professor Henry A. Fairbank of the Yale low temperature group had a position for me. Henry was an excellent mentor and a helpful and understanding thesis adviser. At that time, the isotope 3He was first becoming available. My thesis topic involved research on liquid 3He and is discussed in my Nobel lecture. I look back upon graduate school as being a very happy period in my life. The chance to be thoroughly immersed in physics and to be surrounded by friends pursuing similar goals was a marvelous experience. It was totally rewarding to observe exciting new effects in an apparatus that I had designed and constructed with my own hands.  In January 1959, I completed my research at Yale and joined the Cornell University faculty. My responsibilities were to set up a research laboratory in low temperature physics and to teach courses in the physics department. I was also responsible for the operation of our helium liquifier. Shortly after arriving at Cornell I met my wife, Dana, who was a Ph.D. student in nutrition and biochemistry. She was born and raised in Thailand. Her father originally came from Copenhagen and her mother was a native Thai. For more than 36 years she has been a wonderful companion. Without her loving support my career would certainly have been far less successful. We now have two grown sons who, with their wives, joined us at the Nobel celebration in Stockholm. Over the years I worked my way up through the ranks to the position of Professor in the Cornell physics department. Meanwhile our low temperature group increased in size with the addition in the 1960’s of Professors John D. Reppy, who had also been a graduate student at Connecticut, and later at Yale, and [Robert C. Richardson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1996/index.html) who joined us from Duke University. More recently Professor Jeevak Parpia has joined our group. Over the years our program has been very successful.  Highlights, in addition to the work on superfluid 3He, include the discovery of the tri-critical point on the phase separation curve of liquid 3He-4He mixtures by graduate student Erlend Graf, John D. Reppy and myself, the discovery of the antiferromagnetic ordering in solid 3He by graduate student William P. Halperin, Robert C. Richardson and their associates, and the discovery of nuclear spin waves in spin polarized atomic hydrogen gas as part of a collaboration between myself and Jack H. Freed of our chemistry department. In addition, John Reppy and his students conducted extensive investigations of persistent currents in superfluid 4He and 3He. His experiment with graduate student David Bishop provided a striking example of the [Kosterlitz](https://www.nobelprize.org/nobel_prizes/physics/laureates/2016/kosterlitz-facts.html)–[Thouless](https://www.nobelprize.org/nobel_prizes/physics/laureates/2016/thouless-facts.html) transition in superfluid 4He films. For this work John was awarded the 1981 Fritz London Memorial Prize. Jeevak Parpia has recently performed some very exciting studies of superfluid 3He in confined geometries. Other prizes awarded to members of the group include the 1976 Sir Francis Simon Memorial Prize of the British Institute of Physics and 1981 Oliver Buckley Prize of the American Physical Society. Both of these prizes were awarded to [Douglas D. Osheroff](https://www.nobelprize.org/nobel_prizes/physics/laureates/1996/index.html), Robert C. Richardson and myself for the discovery of superfluid 3He. In addition, Robert Richardson, John Reppy and myself have been elected to the National Academy of Sciences and the American Academy of Arts and Sciences. One of the most rewarding aspects of an academic career is the opportunity to work with graduate students, and to watch them develop after leaving graduate school. My fellow laureate, Doug Osheroff, is a prime example of a scientist who was extremely successful as a graduate student but who later had a distinguished career at AT & T Bell Laboratories and at Stanford University. Most of our other students have had very responsible and rewarding careers in science and technology. It is a special pleasure to thank my students and my colleagues for their role in our success. |
| **Autobiographical** |  |
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| **Biographical** | Ethnically, I come from a mixed family. My father was the son of Jewish immigrants who left Russia shortly after the turn of the century, and my mother was the daughter of a Lutheran minister whose parents were from what is now Slovakia. Mostly, however, I grew up in a medical family. My father’s father and all his children either became physicians or married them. My parents had met in New York where my father was a medical intern and my mother was a nurse. At the end of World War II, my parents settled in Aberdeen, a small logging town on the west coast of Washington State, where medical doctors were in short supply. Surrounded by natural beauty, it was a perfect place to raise a family, and I was the second of five children.  To this day I grow pale at the sight of blood, and never for a moment considered a career in medicine. Despite this, my father, who was usually engrossed in his medical career, inspired in me passions for both photography and gardening, which were his hobbies when time permitted, as they are mine. Natural science interested me intensely from a very early age. When I was six I began tearing my toys apart to play with the electric motors. From then on, my free hours were occupied by a myriad of mechanical, chemical and electrical projects, culminating in the construction of a 100 keV X-ray machine during my senior year in high school.  My projects often involved an element of danger, but my parents never seemed too concerned, nor did they inhibit me. Once a muzzle loading rifle I had built went off in the house, putting a hole through two walls. On another occasion a make-shift acetylene ‘miners’ lamp blew up on my chemistry bench in the basement, embedding shards of glass in the side of my face, narrowly missing my right eye. With blood running down my face, I came up the stairs cupping my hands to keep the blood off the carpet. My mother was by then at the top of the stairs. Knowing my propensity for practical jokes, she exclaimed loudly “If you’re kidding I’ll kill you! ” As usual, my father lectured me about safety as he sewed the larger wounds closed, and there was always an unspoken understanding that that particular phase of my experimentation was over.  In high school I was a good student, but only really excelled in physics and chemistry classes. While I liked physics much more than chemistry, the chemistry teacher, William Hock, had spent quite a bit of time telling us what physical research was all about (as opposed to my experimentation), and that effort made a deep impression on my young mind. My interest in experimentation helped me to develop excellent technical skills, but I did not feel motivated to do independent reading in those areas of physics or chemistry associated with my projects. I was intellectually rather lazy, and in high school I would always take one free class period so that I could get my homework out of the way, freeing the evenings for my many projects.  My parents were generous, and the home for me was filled with scientific toys and gadgets. In addition, their children were allowed to attend any university to which they could get admitted. I chose Caltech over Stanford to avoid a continuing comparison of my academic record with that of my older brother, then a Stanford undergraduate.  It was a good time to be at Caltech, as [Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html) was teaching his famous undergraduate course. This two-year sequence was an extremely important part of my education. Although I cannot say that I understood it all, I think it contributed most to the development of my physical intuition. The Feynman problem sets were very challenging, but I had the good fortune to know Ernest Ma, who was an undergraduate one year ahead of me. Ernest would never tell me how to solve problems, but would give obscure hints when I got stuck, at least they seemed obscure to me at the time.  It was a shock to suddenly have to work so hard in my studies. I had the most trouble in math, and only through considerable trauma did I gradually improve my performance from a grade of C+ to A+ over a three-year period. Years later, when Caltech was offering me a faculty position, I confided that I did not have a very illustrious career as an undergraduate. To this remark the division chair replied “That’s OK Doug, we are not hiring you to be an undergraduate.”  The pressure at Caltech was extreme, and I am not sure I would have survived had I not joined a group of undergraduates working with Gerry Neugebauer on his famous infra-red star survey during my junior year. This experience made me recognize how satisfying research could be, and how different it was from doing endless problem sets. In my senior year, in order to get out of a third term of senior physics lab, I also began working in David Goodstein’s low temperature lab (David was in Italy). Two professors, Don McCullum from U.C. Riverside and Walter Ogier from Pamona College, were spending their sabbatical leaves there trying to reach a temperature of 0.5K by pumping on a helium bath in which the superfluid film had been carefully controlled. They filled my mind with the wonders of the low temperature world, and I decided I would go into solid state physics.  I chose to attend Cornell for graduate school largely because it was so far away from the Pasadena smog. In the end, it was a good choice, and a good time to be at Cornell. Soon after my arrival I met two people who were to become very important in my life. While still looking for housing, I met Phyllis Liu, a pretty young woman from Taiwan, who had also just arrived in Ithaca. We dated a bit, but then she found herself too busy with her studies for such diversions. We met again three years later, and were married in August, 1970, two weeks after she obtained her Ph.D. The other person was [David Lee](https://www.nobelprize.org/nobel_prizes/physics/laureates/1996/index.html), the head of the low temperature laboratory at Cornell and the professor under whom I was to work as a teaching assistant my first year. Dave seemed to think that I was bright, and encouraged me to join the low temperature group.  Low temperature physics seemed even more exciting at Cornell than it had been at Caltech. New technologies and interesting physics made the field easy to choose, and I found myself thoroughly enjoying every minute of my work. In the spring of my fourth year Dave Lee asked me to talk to the Bell Labs recruiter, who came to campus in the fall and spring of each year. I was not ready to graduate, but we talked a bit, especially about making tiny electrical plugs to be used throughout the Bell Telephone system. It seemed interesting to me, although not really physics. In the fall, Dave suggested I start interviewing in earnest. I first talked with General Electric, who seemed to have no jobs whatsoever. I then talked to Bell Labs again, but this time to a new recruiter, Venky Narayanamurti, who had recently received his Ph.D. in physics at Cornell. Venky was enthusiastic about what I was doing, and felt that I might be able to get a postdoc doing Raman spectroscopy. I didn’t confess that I knew nothing about the subject.  We discovered our mysterious phase transitions in my Pomeranchuk cell in November 1971, and almost by magic, Venky called me up in early December with good news. The hiring freeze which had been in place for almost two years at Bell had been lifted. How soon could I be ready to come down for a job interview? I told Venky that we had stumbled on to something that was pretty exciting, and we fixed the date: January 6, 1972.  At Bell Labs, a job interview began with a thesis defence, and it could at times turn nasty. I was lucky that no one questioned my association of the A and B features with the solid. In particular, Dick Werthamer was in the audience, and he had done early work on the p-wave BCS state soon to be associated with the B phase. I think my enthusiasm carried the day, and ultimately Bell Labs offered me not a postdoc position in Raman spectroscopy, but a permanent position which would allow me to continue my studies on 3He.  Phyllis and I moved to New Jersey in September, 1972; Phyllis to a postdoc position at Princeton University, and I to Bell Laboratories at Murray Hill. This was the golden era at Bell Labs. The importance of the transistor, invented in the research area there, made management extremely supportive of basic research. The only requirement was that work done should be ‘good physics’ in that it changed the way we thought about nature in some important way. I joined the Department of Solid State and Low Temperature Research under the direction of C. C. Grimes, and began purchasing the equipment I would need to continue what I by then knew were studies of superfluidity in 3He. Some instrumentation was even purchased before I arrived in New Jersey. Yet I knew it would take at least a year to set up my laboratory, and I feared that most of the important pioneering work would be done before my own lab became operational.  I was surprised to find that by the time my laboratory did become operational, few of the studies that interested me had been done. Indeed, there seemed to be some question as to whether or not these new phases were all p-wave BCS states. In addition, theorists [Phil Anderson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/index.html) and Bill Brinkman at Bell Labs had become interested in the theory of superfluid 3He. This set the stage for what was to be an extremely productive period in my career. Over a five year period, beginning in 1973, we measured many of the important characteristics of the superfluid phases which helped identify the microscopic states involved. We found the superfluid phases to be almost unbelievably complex, and at the same time extremely well described by the BCS theory and extensions to that theory developed during that period.  In about 1977 I began to feel pressure from Bell Laboratories management to go on to study other physical systems. I decided to study solid 3He, my original thesis topic, and at the same time Gerry Dolan and I began a modest program to test some of the ideas that [David Thouless](https://www.nobelprize.org/nobel_prizes/physics/laureates/2016/thouless-facts.html) had discussed on electron localization in disordered one-dimensional systems. This latter study had to fit within the extremely slow time scale of the solid 3He work. By late 1979, both of these efforts had succeeded beyond my wildest expectations. We discovered antiferromagnet resonance in nuclear spin ordered solid 3He samples which we grew from the superfluid phase directly into the spin-ordered solid phase. At the same time, the low temperature group at the University of Florida also discovered these resonances, but because we cooled our samples by adiabatic nuclear demagnetization of copper rather than Pomeranchuk cooling, only we were able to form and study single crystals, and could thus identify the allowed magnetic domain orientations. In the end, Mike Cross, Daniel Fisher and I were able to determine the symmetry of the magnetic sub-lattice structure, and correctly guessed the precise ordered structure, later confirmed by polarized neutron scattering. The frequency shifts resulting from this antiferromagnetic resonance have made solid 3He an extremely useful model magnetic system, and to understand them theoretically, we had borrowed some of the same formalism which [Leggett](https://www.nobelprize.org/nobel_prizes/physics/laureates/2003/index.html) used to understand the frequency shifts in superfluid 3He.  At almost the same time that Cross, Fisher and I made our breakthrough in our solid 3He studies, Dolan and I discovered the log(T) temperature dependence to the electrical resistivity in disordered 2D conductors which Phil Anderson and his ‘gang of four’ had just predicted would exist, as a result of what they termed ‘weak localization’. I did not continue the work on weak localization, as I only had one cryostat, and to do so would have meant that I could not continue my studies on nuclear spin ordering in solid 3He, since the two sets of experiments would have vastly different time scales. Somewhat ironically, I got a second cryostat two years later.  In 1987, after fifteen years, I left Bell Laboratories to accept a position at Stanford University. I had received informal offers of university positions periodically while at Bell Labs, but always found Bell to be the ideal place to do research. The combination of in-house support for basic science and first rate collaborators made Bell Labs unbeatable as an environment for doing research. However, my wife recognized in me a teacher waiting to be born. In addition, she was not happy with her job in New Jersey, and we agreed that she would apply for positions elsewhere. When she received offers from two biotech companies in California, Amgen and Genentech, I suggested that she accept the Genentech offer and that I would start talking to Stanford and U.C. Berkeley. Stanford, which has a small physics department, had just begun a search for a low temperature physicist. Ultimately, I received offers from both institutions, and chose Stanford because we liked the atmosphere better, and it was a better commute for Phyllis.  At Stanford my students and I have continued work on superfluid and solid 3He, studying how the B superfluid phase is nucleated from the higher temperature A phase and diverse properties of magnetically ordered solid 3He in two and three dimensions. In addition, we have developed a program to study the low temperature properties of amorphous solids. Our work has shown that interactions between active defects in these systems create a hole in the density of states vs. local field, just as is seen in spin-glasses. In amorphous materials, it may be possible to measure the size of coupled clusters of such defects, something which has been difficult in spin-glasses.  I have thoroughly enjoyed all aspects of university life, except for having to apply for research support. In particular, I have been fortunate to have had excellent graduate students, and to be able to teach bright undergraduates. Of course, with undergraduates one always has a few students who do not appreciate the professor’s efforts. In 1988, after teaching my first large lecture course, one student wrote in his course evaluation: “Osheroff is a typical example of some lunkhead from industry who Stanford University hires for his expertise in some random field.” Despite this minority opinion, in 1991 Stanford presented me their Gores Award for excellence in teaching. From 1993-1996 I served as Physics Department chair, and stepped down in September 1996, hoping to spend more time with my graduate students. The day I learned I was to receive the Nobel Prize, after just two and a half hours sleep the night before, I taught my class on the physics of photography, although the lecture was not on photographic lenses, but the discovery of superfluidity in 3He. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| Q12 | **I just would like to start off the interview for the official website. Welcome Professor Osheroff to this interview. And really start off with asking you something about your childhood. It seems like you really liked experimenting at the time. There are some amazing stories that we have been able to read about. Can you tell us some?** |
|  | Douglas Osheroff: It’s absolutely true that I led a wild childhood. I guess it started at age six when I tore the locomotive for this electric set I’d just gotten for Christmas apart to get the electric motor out. I think my parents didn’t scold me. I think my father was quite fascinated with my fascination. As time went on … He was a physician in town, and his patients would obviously give him things to give to me. I was one of five children. I don’t know if they all got quite that much attention, but some of things, like a box of parts from the telephone company and a box of magnets and stuff like this, and I just found all of this stuff so fascinating. |
| Q6 | **There was once an explosion and you came running up with cuts in your face?** |
|  | Douglas Osheroff: This was, I can’t remember who told me, you could buy calcium carbide at a hardware store. The idea was to put it in moles’ runways. It generates acetylene gas when the calcium carbide gets in contact with water, and that would drive out the moles. But you could take a soda pop bottle, fill it mostly full of water, drop in one of these grains of calcium carbide, and then jam the rubber stopper with a glass tube that was pulled to a fine point so there was just a very small orifice. You had to wait until all the oxygen was out of the bottle, and then you would light this and you can see an intense white flame. This is essentially a miner’s lamp. It was fun. But it would go for, I don’t know, a minute or something or less and then you’d have to open it up and put another one in. I thought I would make one differently that would run for hours. I took a 500-millilitre beaker and filled it full of calcium carbide. I had a long burette, so it would drip water into this thing, and then there was a delivery tube. I didn’t think about the fact that there was such a much larger volume in this thing. I waited what seemed like an appropriate period of time and then I lit this. I got a blue flame rather than this brilliant white flame. I instinctively knew enough to move my head like this. This thing blew up. I went like this and it blew up. I had glass in the side of my face, which would have gone in my eye if I hadn’t been luckier. My mother was up fixing dinner and she hears this explosion downstairs and she comes to the top of the stairs – I was in the basement. I’m coming up the stairs cupping my hands to keep the blood from dripping on the carpet. I was so famous for practical jokes that she of course couldn’t trust anything I did, so she says, “If you’re kidding I’ll kill you!” I was old enough to drive and I drove myself down to my father’s office and he sewed up the largest of the cuts. This had happened so many times before. |
| Q12 | **But they went with it? They were okay? They really encouraged you though?** |
|  | Douglas Osheroff: Yes. After that I stopped playing with calcium carbide. There was an unwritten agreement between us: if I did something really stupid I would simply stop doing that. I was usually quite happy because there were so many things and there wasn’t enough time anyway. |
| Q4 | **You were very young when this amazing discovery was made early in your twenties. Were you aware of what you were on to, together with Professor Lee and Professor Richardson, or was it sheer hard work and you knew there was something, but did you know exactly what you were coming up?** |
|  | Douglas Osheroff: Let me go back a little bit if I can. When I was a kid, again there five of us, and we would go for walks along long deserted logging roads that wound their way into the Hemlock Forest that surrounded Aberdeen. We would imagine that we were pioneers, explorers, that we were the first humans that had ever been on the roads. A contradiction in terms I suppose, but I’ve always wanted to be an explorer, and when I went to Cornell, Bob Richardson gave a talk on a new refrigeration technology that gave the promise of allowing man to look at nature in a new and different realm. I wanted to be one of the people. That was when I decided I would go into low temperature physics. I went in with the idea that there would be some excitement associated with it and everything. But my fifth year of graduate study, the experiment I was doing was based on some very, what appeared to be very, exciting work by one of our competitors. When I went into first reproduce that, then go beyond it, we found that that work was all wrong and that the actual effect was easily calculated. I kept looking for small deviations from the existing theory. What I like to say is if I continued on that experiment I probably would be a taxi cab driver of New York City or something like that.  But luckily one day there were two other students that had patiently been waiting for the only NMR iron core electromagnet in the lab, so high enough homogenating stability to do NMR. Eventually they went to Dave Lee and Bob Richardson and they agreed that I had to relinquish this magnet which I’d been monopolising for three months. It was only after that that I did a curiosity driven experiment, which in fact gave the first evidence that we’d seen a new phase transition. The refrigeration technology was a mixture of liquid and solid helium three, and we didn’t know whether the phase transition, this curve that we saw, was giving evidence of was in the liquid or the solid. The first publications we made we were wrong. I mean all the data was correct, but the interpretation was all wrong. We’d said it was all in the solid, but it wasn’t. This was exactly what I’d felt I’d been born for and I wasn’t going to just stop. I kept trying to come up with better and better ways of probing what was going on inside this mixture at this very, very low temperature where one couldn’t see what was happening. |
| Q9 | **But you knew you had something amazing there, but did you expect that you would get the prize?** |
|  | Douglas Osheroff: When we did the work I think that our attitude was that this other person that had done this wrong experiment that got me into the parameter space where we could make the discovery, if he’d made that discovery we felt he would have gotten the Nobel Prize. But we didn’t think that we would, at least that was my personal feeling. I think it was probably 1976 we were awarded the Simon Memorial Prize, which is an international prize, a British prize. I guess it was only shortly after that that people started telling me that they had nominated me for the Nobel Prize. Now of course you’re not supposed to do that, but I think a lot of people do. For years I would hear this and get nervous every October, and how many years can you get nervous every October? I think eventually I made a conscious decision I would simply ignore this whole business of it, it seemed unlikely after 20-some years that it would happen. But having made the discovery, in any event it opened enormous doors for me and I had lots of opportunities. Just going to Bell Laboratories was a fantastic thing for me. |
| Q10 | **What was so amazing with Bell, just in some brief words, because it seems like a number of laureates have been working at Bell’s and had a good connection to Bell? What was so amazing for you there?** |
|  | Douglas Osheroff: For one thing you know you were surrounded by so many of the best scientists, for me physicists, in the country, indeed in the world. The reason I left Bell Labs, because I don’t think I ever could have done that on my own, my wife had always thought of me as a frustrated professor waiting to be born. I would go around New Jersey giving demonstration lectures on low temperature physics to high school kids and middle school, even grade school kids. |
| Q17 | **The students seem to be very happy as well; you have won an award for excellency in teaching. How did you feel about that?** |
|  | Douglas Osheroff: It happened fairly early actually. I came to Stanford and there was an intermediate lab sequence for physics majors that had actually been – the structure of this thing was more or less worked out by someone else – but it was all low temperature, all based on physics at low temperatures. I took over this thing. Everyone said they thought I had a really light teaching load, but all of this was an incredible amount of work. I guess I created this course a little bit, tailored it for what I felt I wanted to do. The kids seemed to really enjoy it a lot. They worked awfully hard. I taught that course for ten years. It’s very difficult teaching anything for ten years. You have to really, really love it to do that. At some point I would like to go back and teach that again. |
| Q2 | **On a daily basis, how do you keep the creativity going? How do you get that energy to encourage the students or is it just environment in itself that creates that?** |
|  | Douglas Osheroff: No. I have to say that the environment at Stanford is certainly a wonderful environment, but for me, I don’t have theorists who are waiting on. Even if I worked back at Bell Labs right now, the field helium three physics is a mature field and I’m a little bit surprised that I’m still in it I suppose. But I still find it fascinating and I still find it very good training for students. |
| Q18 | **I would like to ask you, you have told us and I have read that you have been part of the Columbia Accident Investigation Team, the accident that happened year 2003. You had said briefly that it was good for the team, the investigation team, to have a Nobel Laureate on, it’s your speculation. But you came to really have a very, very important role in this investigation. Will you tell us the way you saw your role and what it actually came to be?** |
|  | Douglas Osheroff: Yes. People, as soon as they heard that I had been offered a spot on the board, people immediately said, “Oh you will be the Richard Feynman of the Columbia Accident Investigation Board.” I quickly said, “I can’t fill those shoes.” But in some sense I suppose I did end up being kind of the Richard Feynman of the thing in that I was the only one that did an experiment, but it was a very different experiment. Feynman basically, some people said he was put up to it, but if he was he was a wonderful showman and I think he made his point extremely well, dumped this rubber O ring in ice water and showed that it became hard. This is nothing that the engineers at Morton Thiokol hadn’t known back, even the morning before the accident.  First let me say I joined late and so most everything was being covered. People were looking at the organisational aspects of the accident. I was a member of a group that was trying to establish with as much certainty as possible the physical origins of the accident. Every time I tried to do something I found that I was infringing on someone else’s turf so to speak and it was a bit complicated. |
| Q17 | **But you even took and made some experiments at home to prove that what you were suggesting was true?** |
|  | Douglas Osheroff: Yes. The first thing I did is decide what I should really do is look over people’s shoulder to make sure that what they’re doing is reasonable and complete and all of this stuff. It was a very professional bunch of investigators, most of whom had a lot of expertise in safety investigations or knew NASA like the back of their hand or whatever. They were all people who had much more reason to be there than I did. I did that for a while. Then I said, “Is there something that’s not being covered here?” I realised that no one was worrying about why the foam fell off in the first place, and so I started looking at that and doing a bunch of calculations and stuff like that. It didn’t look like NASA’s model made any sense, just because the heat couldn’t propagate through the foam fast enough to do what it had to do. But then we said, “Well, maybe the heat is being generated internally by vibrations and things.” Then I asked a different question: If in fact you suddenly start building up pressure inside the foam, how does that pressure propagate through to the surface? Does it do that in a manner which is consistent with throwing off foam? I designed a very simple experiment. It cost me about $100. I mean some of the stuff we had lying around, but if you had to buy it all to start with maybe it would have been $300 or something. We did this experiment. We got a very clear answer, which was that the pressure created a plainer fracture which propagated up and intersected a section normal to the surface. All the motion of the foam was normal to the surface and that wouldn’t throw any foam off. We did this experiment under various different conditions over and over again; always got the same result. |
| Q21 | **What did NASA say to your findings and has it been established, or are you seen as the bad boy, so to speak, of having insisted that this is the problem?** |
|  | Douglas Osheroff: No, I don’t think I was the bad boy because of that. I was the bad boy because I think Nobel Laureates have a tendency to talk to the press a lot more than for instance Air Force Generals, who are in the business of doing safety investigations. The thing that really got me in trouble was talking to the press. There was one particular interview, which was August 1st, that’s my birthday, and anyone can figure out who it was, but the reporter had asked me a simple question, which was, Did I think that we were writing a good report? because it was supposed to come out in less than a month? I said, Well, it depends. I think the people that wrote the Rogers Commission Report thought they had written a good report. But then over time I think they could see that NASA’s regard for safety had relaxed back to the pre-Challenger accident level. Then surely when this accident occurred they must have concluded that in fact the report had not done what they hoped it would do. I think the only way we can have written a good report is if we get NASA to change their culture in a way that will not relax back. That was the first time that NASA realised, evidently, that that was going to be in our report. |
| Q18 | **Do you approve of men in space so to speak, to send people up in a space shuttle, or do you think it should be done in a different way until all the safety measures have been taken care of?** |
|  | Douglas Osheroff: I think ‘approve’, I would not use that term. I think that there’s no moral or ethical questions here. I think the astronauts realise the dangers involved. I think that if you look at the survival rate of astronauts it’s probably higher than the survival rate of test pilots, particularly those that are testing military aircraft. It is a risky business. I think for various reasons it is politically not tolerable for astronauts to die in the line of work. That’s a reality and I don’t think that we’re going to change. These are all heroes and how can we stand by and watch heroes die, and particularly stand by and watch heroes die because of a blind spot that NASA had in their management? I think it’s not so bad that there were two accidents, but the fact that both of these accidents could and should have been avoided, I think that is the point that’s been hard for NASA to live with and to understand. |

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| **Physics\_1999-** | |
| **ID** | 0079 |
| **Biographical** | I was born on June 26, 1937 in Georgetown University Hospital in Washington, DC. My parents, Lois Price Richardson and Robert Franklin Richardson, lived in Arlington, VA. My sister and only sibling, Addie Ann Richardson, was born on May 6, 1939, also in Georgetown University Hospital.  My earliest memories are of the apartment building in Arlington where my mother, sister, and I lived during the years of World War II while my father was away in the US Army. He was an officer in the Signal Corps. We lived across the street from the fire department and became accustomed to the blast of the siren at all hours of the day and night. It is fortunate that we lived so close to the fire department because one morning while my mother was visiting neighbors my sister set the apartment on fire while playing with the gas stove. Little damage was done, though I am certain that my mother was thoroughly embarrassed.  My father was a native Virginian. Branches of his family could be traced back to the early colonial times. His father, Robert Coleman Richardson, after whom I was named, owned a general store in a small rural village, Penola, VA. My father attended Roanoke College for two years during the Great Depression. When his mother became seriously ill, he left college because of the increased family expenses. He became interested in electricity and began work as a ‘lineman’ for the Chesapeake and Potomac Telephone Company in Richmond, VA.  My mother’s family was from North Carolina. She was an orphan, practically from birth, and was shuttled among relatives in North Carolina. As was a common practice in the rural South, she was taught at home by various aunts. She attended only one year of public school before going off to college. The one year of high school was in Reidsville, NC in 1918. She attended various colleges – Gulf Park College, the University of Alabama, the University of Mississippi, and the University of Virginia. She was one of the first women to attend the latter and obtained a Master’s Degree in History there. During her college career she was brought in to the large and warm family of Ernest H. Mathewson in Richmond, and thus gained three brothers and two sisters. The Mathewsons were known by my sister and me as our other grandparents during our youth.  My parents met in Richmond and were married there in 1935. Shortly thereafter, my father was transferred by the telephone company to their branch in Washington, DC. As an army reservist my father was called to active duty during World War II and again during the Korean War. During his service for the latter he was assigned to the Pentagon so that it did not become necessary for him to leave home. During his second tour of duty with the army he took advantage of the educational benefits associated with the ‘G.I. Bill of Rights’ to finish college. He graduated from the University of Maryland in 1955.  I do not remember having any special scientific interests during childhood but I did love school. In 1946, when I was in the fourth grade, my family moved from the apartment building we had lived in during the war years. My father bought a new house in one of the housing tract developments so common to the postwar suburbs of American cities. We still lived in Arlington, VA. My new elementary school, Walter Reed, was overcrowded. The fourth and fifth grades met in the same room with the same teacher. I paid as much attention to the fifth grade instruction as the fourth. I especially loved the history lessons because Mrs. Walton, our teacher, was a remarkable storyteller. During the summer between fourth and fifth grade, I went to summer school just to have something to do. The teacher of the summer session was confused about my grade status and inadvertently promoted me to the sixth grade. The Arlington County School system accepted her decision. So I skipped a grade. Had I remained in the same grade, one of my classmates in Walter Reed School would have been Warren Beatty (of film star fame), whose family had just moved to our neighborhood in Arlington.  With my parent’s encouragement, I became very active in the Boy Scouts. Scouting did not exist in rural Virginia, where my father grew up. In his youth, he had always envied boys from larger cities who could be in scouting. My involvement gave him, vicariously, the scouting experience he had missed. With his help, I became an Eagle Scout in the minimum amount of time permitted by the rules. I especially enjoyed the outdoor activities of scouting, hiking, camping, and even birdwatching.  I spent the enjoyable summers of my high school years working as a counselor in Camp Letts, a Boy Scout Camp on the western shore of the Chesapeake Bay in Maryland. I was a nature counselor. I spent my days leading tours on nature trails through the camp. My ankles were covered with a minor poison ivy rash from June through August. In the evenings I led groups in ‘stargazing;’ and one morning each week I led a ten-mile canoe trip through the Maryland marshland to look at birds. I liked the canoe trips best. We would arrive at the entrance of the marsh just at sunrise when the maximum number of birds would be out feeding. The marshes had large water birds like egrets and herons, three kinds of wrens, more than twenty different warblers, vireos, plus large birds of prey like hawks and eagles. It was possible in a single morning for a scout to spot enough birds on a single trip to qualify for the birdwatching merit badge. I learned where all of the birds hung out and how to tell them by their songs. Although I am color blind, I memorized their descriptions in the bird manual. I would describe subtle pastel features of warblers and vireos flitting about in the tree tops 60 feet above the ground to the amazement of even the adult scout leaders. There is a famous painting by James Audobon of a bald eagle diving toward an osprey just after the osprey has caught a fish. Each summer I was fortunate enough to see that scene re-enacted at least once. It made a special impression on the groups I led because I showed them a copy of the painting before we left on the trips.  My high school class at Washington-Lee High School had 925 students in it. I graduated, as I recall, in a six-way tie for 19th place. There was nothing exceptional about the math and science training at Washington-Lee. The idea of ‘advanced placement’ had not yet been invented. I did not take a calculus course until my second year of college. The biology and physics courses were very old fashioned. The idea of a ‘photon’ was said to be controversial. This in 1953! I was taught that absolute zero is the temperature at which all motion stops. It is most fortunate that the statement was wrong. Otherwise 3He could not become a superfluid.  I entered Virginia Polytechnic Institute, also called Virginia Tech, in the Fall of 1954. In those days, the Reserve Officers Training Corps program was compulsory for all physically fit entering students at VPI. Moreover, all ROTC students lived in a cadet corps with fairly rigorous military discipline. I surprised myself by really enjoying life in the VPI Corps of Cadets. I learned an easy and democratic camaraderie. As we were assigned to live in cadet companies in alphabetical order, my closest friends were those in the bottom third of the alphabet.  In class, I started out as an electrical engineer but soon became bored and impatient with the mechanical drawing course and the rote application of a single principle, Kirchoff’s Laws, in a five-hour course. I tried to become a chemistry major but ran into great difficulty in a course called quantitative analysis because of my color-blindness. I could not tell when the color of the indicator solution turned from pink to blue unless I made a very strong over-concentration of acid or base. When I complained to the professor he told me that I was very fortunate to discover my disability early in my college career because I certainly was not suited to be a chemist.  Finally, I turned to physics as a major. I was not an especially diligent student but nevertheless obtained a reasonable education in physics. I graduated with a B average and fourth in a group of about 9 physics majors. My education through the Cadet Corps was at least as valuable as that in formal class training. I was a leader in several campus organizations. The rigorous honor code at VPI in those days was almost exhilarating. We were all very proud of it. I never saw anyone cheat on a test in my years there.  In summers, while in college, I had a very interesting job with the National Bureau of Standards. I worked in the Electricity Division calibrating electrical resistance standards which power companies sent to NBS once each year. The NBS program for summer students was quite wonderful. First, we were well paid. Next, we actually did useful research. Finally, we attended a weekly seminar series which was given at our level of understanding. In my spare time at NBS, I read the scientific literature on electrical instrumentation and even met some of the authors of some of the classic articles. The experience at NBS gave me some notion of what a scientific research career could be.  After graduating from college, I had a vague idea of going to a graduate program in business – with hopes of becoming an executive in a large corporation. First, though, I felt that I had not quite given physics and research a chance so I decided to remain at VPI for one more year to obtain a Master’s Degree before going off to military service as an Army Officer. The project I worked on was the measurement of the lifetime of photo-excited carriers in germanium. In the process I had to build a great deal of equipment because Tom Gilmer, my advisor, had just come to VPI to a practically empty lab. Tom was a good mentor, but he was very busy as department chairman and VPI professors had quite a large teaching load. I learned a great deal about how to do things with my own hands – operate a lathe, solder, make simple electronic circuits, etc. I knew about keeping a lab book from my summer jobs at NBS. In that year I became a good deal more confident that I could learn physics at advanced levels, but still was not in any way special. I think I was still fourth in the group of graduate students. With the feeling that I would probably be a mediocre physicist, at best, I left VPI with the intent of attending a Masters in Business Administration, MBA, program after finishing military service.  A great piece of good fortune fell for me during my year of graduate work at VPI. The Army ran short of money. Thus, rather than having to spend two years on active duty, I was only assigned for six months of active duty in the US Army Ordnance Corps between November 1959 and May 1960. This was a time well after the Korean War and well before the Vietnam War. There was no likelihood of actually having to see any combat. At Aberdeen Proving Ground, the Ordnance Corps training base, I took courses in how to manage a platoon which would do things like repairing jeeps and tanks. I hated the course and the being in the Army. Wearing a uniform and the military discipline did not bother me; I had become used to both while in the VPI Cadet Corps. But I did not enjoy the training in how to run a small business – for that’s what a repair platoon in the Ordnance Corps was. Therefore, I decided to return to graduate school to obtain a Ph. D. in physics.  I had no opportunity while in the Army to take tests like the Graduate Record Exam to qualify me for admission to one of the top graduate schools – like MIT, Harvard, or Cornell. Besides, I probably would not have been admitted even if I had taken the tests. Therefore, I looked for smaller research universities with strong specialties. In my graduate research project, I had made a simple liquid nitrogen dewar, and found the area of low temperature physics to be interesting. I had read some articles about the work going on at Duke University so decided to apply there. I received a warm letter from Horst Meyer, a new Assistant Professor at Duke, encouraging me to come to work for him. The letter was very flattering – the first strong encouragement I had ever received about my potential as a physicist. Therefore, I entered Duke in the Fall of 1960 as a full-time graduate student.  I had a glorious time at Duke. I made strong friendships which have been maintained through the rest of my life. I met my wife, Betty McCarthy, there. One of only two physics majors in her class at Wellesley College, Betty was also a graduate student in Physics. We were married in 1962 and our daughters Jennifer and Pamela were born in Durham, NC, in 1965 and 1966.  Horst was a very conscientious mentor. He taught me a great deal of the craft of low temperature technology he had learned as a research associate at the Clarendon Laboratory in Oxford. In all of the subsequent years he has been a valued friend. We had the best of two worlds in our low temperature group at Duke in those days. Bill Fairbank had been there but left before I arrived. Much of the old equipment and the residue of the experimental technology from Bill Fairbank remained. Horst brought a different set of techniques with him and we had our choice of which way to do things – for example the use of wood’s metal to attach vacuum cans along with Epiezon J-oil for thermal contact were the Oxford technique. Indium O-rings and vacuum grease were the Fairbank method. Both had advantages.  Horst put me on a good problem – the NMR study of the exchange interaction in solid 3He. Earle Hunt came to Duke as a research associate with Horst and taught me about the new methods for pulsed NMR-spin echos and all of that. The combination of training with Horst and Earle put me in business for practically the rest of my research career.  I finished my thesis in 1965 and remained at Duke for another year as a research associate in order to clean up some of the loose ends of the research and to look for a good job. In the latter, I was fortunate indeed. Cornell University, with its special funding as an Interdisciplinary Laboratory (IDL) had decided to expand its effort in low temperature physics. In the Spring of 1966 the Laboratory of Atomic and Solid State Physics invited me to join them to work with [Dave Lee](https://www.nobelprize.org/nobel_prizes/physics/laureates/1996/index.html) and John Reppy on very low temperature helium research. As far as I was concerned, there could be no better career opportunity.  I moved my family to Ithaca in October 1966 and have remained there ever since. I received sound career advice from Dave and John from the day I arrived. The research environment at Cornell has been superb with an unbroken string of talented graduate students, close colleagues in both theory and experiment, and a team of technical support specialists who helped make everything work. During my thirty years at Cornell I even learned how to teach undergraduate physics courses, an activity which my wife and I enjoy a great deal. After our daughters entered Junior High School, Betty turned to teaching physics at Cornell also. She is now a Senior Lecturer.  My children grew to adulthood in Ithaca. It is a wholesome college town with few of the problems of large cities. Jennifer went to college back at Duke and later attended a Master of Fine Arts in Creative Writing program at Columbia University. Jenny married James Merlis in June 1994. We had a beautiful wedding reception among my large rhododendron bushes in our back garden. In addition to her writing and other activities, she now plays violin in an all female rock band called Splendora.  Pamela went to college at Cornell. After graduation, she went to the New York School of Interior Design for a year and then decided to become a nurse. She returned home to take the science courses she had skipped at Cornell. She spent a year at our local community college taking chemistry, biology, anatomy, etc., displaying a surprising scientific talent. After the year at home she went to Vanderbilt University where she entered a Masters of Nursing program. In November of 1994 – after one year in the Vanderbilt nursing program – she died tragically, of heart failure. Though she had been born with a heart defect, her death came without warning.  In an effort to drag ourselves out of our grief and despondency over losing Pam, we have taken on a major family project in the past year: the production of an introductory college physics text book. Betty is the co-author of the book, with Alan Giambattista of Cornell; and I have been working on a companion CD ROM. When completed, the work will be published by McGraw Hill. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q6 | **I just thought I would start off by asking you … During the banquet, the speech that your co-laureate was holding, Professor David Lee, he said that it was a very exciting time there in the beginning of the 1970s, those months that you were working together on the experiments. How would you describe those days, those months?** |
|  | Robert C. Richardson: I have to agree with Dave. It was a very, very exciting and surprising. Dave Lee recruited me to go to Cornell to work on experiments that were designed to do something entirely different, but they were related. It was to try to cool liquid and solid helium to very low temperatures and look for a phase transition in solid helium-3, and we were doing these experiments. I came and helped start a part of the program and my speciality was nuclear magnetic resonance. Then we had this fantastic graduate student named Douglas Osheroff that came and took over the project. In the American tradition the graduate student’s hands are the ones that have to do the work and I’m not permitted to take the soldering iron out of the graduate students’ hands. It’s just a joy to interact with Doug. When these unusual and bizarre experimental results came along, we would just stay up sometimes until six in the morning and then I’d have a lecture to give in a class at eight o’clock. I would just be thinking about it and worrying about where it was going and what it meant. In the spring of 1972, I can remember just not feeling tired at all, but just elated because night after night it would be thinking and talking. We had a spectacular *aurora borealis* in our part of New York in the summer of 1972 and Doug and I would talk about it in the morning as we were going home. Looking up in the sky, it was as though there was some special reward that was coming because of the excitement that we were having in it. |
| Q2 | **The working together, the co-working between the three of you, the working together, how important is that do you think to really move on and be creative?** |
|  | Robert C. Richardson: There are certain areas of science where people work in very, very large teams. In high energy physics there might be hundreds of people because they’re building very big detectors and things. Our work was still manageable, that is there were little pieces of equipment like this that could get made on the lathe by the graduate students themselves and developed techniques for making bellows move and creating magnetic field gradients and so forth. A big team’s not important but having a constant interaction to look back and forth, Yes, let’s do this and Let’s call this that, it really made it fun. I cannot imagine how it would be any fun at all to be working alone. It was more fun in discussion. |
| Q2 | **I can understand. How did you come up with the idea to become a scientist? Was it something that you wanted right from a small age, or was it something that happened during your student years?** |
|  | Robert C. Richardson: That’s a very interesting and complicated question. I went to a public school in Virginia in a Washington suburb. It was a very good school. I made pretty good grades. I went to a public college in my state, at Virginia, Virginia Polytechnic Institute. I had a vague thought, I can do these math things, I’ll be an engineer maybe. I started taking engineering and I really found that there was too much repetition in that and my favourite subjects were English literature and history and mathematics, so maybe I’ll be a chemistry major. I started taking chemistry courses, some were required. I was taking one course and I’d made very good grades, without really having to work very hard by the way, in chemistry. I was taking one course called quantitative analysis where you’re supposed to figure out just how much of a certain compound is in a mixture. In order to get the right result, you had to do something called titration where you would release a certain amount, a drop at a time, a colour indicator, phenolphthalein into the solution, and tell exactly when it turned from pink to blue. I’m colour blind and I couldn’t see when it turned from pink to blue. This was in 1955, I guess. |
| Q16 | **But you had tried for Cornell already at that time?** |
|  | Robert C. Richardson: No, it was just that was one of the ones I looked at. I looked at Harvard and Princeton and Stanford and all those fancy places. At Duke university they had this program in low temperature physics and low temperatures had a lot of appeal for me because it seemed like … It’s like explorers. If you’re one of the first explorers to go to Antarctica, you can have a mountain named after you maybe, and that was going to very low temperatures, that sort of opportunity. They had a very famous theorist named Fritz London and a spectacular experimentalist named William Fairbank, so I would try to go there. I got a warm letter from a very nice man named Horst Meyer who said he would love to have me come work with him and I said, OK, that will be fun and I can see all these great people at Duke. When I got to Duke after I was out of the army I discovered that Fitz London had been dead for five years and Bill Fairbank, the famous experimentalist, had gone to Stanford. But I went to work with Horst Meyer and we became great friends and started a thesis project that made the prediction about this nuclear magnetic phase transition in helium-3 at low temperatures. That was that, that led Dave Lee to come see me and say, Hey, why don’t you come with me and work with me at Cornell and we’ll see if we can do that work?’ Then that led to the work super fluid helium-3, so I had a lot of happy accidents. But if you had ever asked me even when I was in college, Are you going to spend your life as a professor doing research in a very specialised field? I would have said, No, you’re crazy that’s not what I want to do. |
| Q3 | **What made you stay on as a teacher?** |
|  | Robert C. Richardson: Because it was fun. I enjoyed working with the students and had summer programs for high school students and I discovered I was more of a ham actor than I thought. I loved to give these big luxury demonstration courses and make things go pop and sparks and tell corny jokes of the kind of professors tell. The students would politely laugh and maybe even remember them in the future. The whole purpose of the luxury demonstration course is to persuade to seduce the students into thinking that there is something there that might be worth understanding and pursue a little bit further. |
| Q10 | **It sounds like you chose exactly what you wanted, but yet if you had gone into industry, it might have been more money for you personally, it might have been more money for research, applied research, but you stayed on. Has it been difficult to get money to do research, more basic research at the university? How have you worked on that field?** |
|  | Robert C. Richardson: The people that especially follow the small group research path have increasingly had to scramble and the competition for the most open-ended research with say the National Science Foundation is very stiff, but roughly only one person in three that writes the proposal gets funded. I think that young people starting up right now have a harder time than I did at the beginning. But generally people say, OK, I have to write three proposals to get one funded, that’s what I’ll do. |
| Q1 | **Do you encourage them as well to do that if they come and ask your advice?** |
|  | Robert C. Richardson: We give a lot of advice in mentoring young people. One good trend is a lot more of multi-party, multi-disciplinary research. We have groups of people who get together and think about research in a given topic area and get together and figure out how they can make contributions to the whole and a pattern of research. That’s great fun. |
| Q9 | **That’s great that it’s done so well, and I am sure it was also good for the publishing of the book that you were a Nobel Laureate. Am I wrong? Does it come with certain obligations, expectations to have won the prize?** |
|  | Robert C. Richardson: Now there is another piece of it too, that I have worked in a very successful graduate school. At almost every major university in the United States there is a former student, maybe not of me but of somebody close to me, that knew me and interacted with me and would be willing to actually ‘Hey, the Richardsons were involved in this book’ and would take a look at it and give a hearing, so that helped too. |
| Q9 | **Does it come with obligations to have won the prize, to have the prize?** |
|  | Robert C. Richardson: Some people think so and some not, but I felt I did. I mean I was 59 when I won the prize and I felt that the system, the country, a science report and my university had given me a lot and I’d had a very happy and successful career. I’ve tried to return that now and have participated and I accepted an administrative job at Cornell. I’m the senior research officer at Cornell and I’ve been on a number of panels and boards and science policy groups in the time since then. |
| Q8 | **To round up this interview I just would like to ask a more personal question. I have read that you are a great fan of the outdoor activities. You were a scout; you like to watch birds. Can you tell us a little bit about that?** |
|  | Robert C. Richardson: I guess that was the closest part of being involved in science. I was a boy scout and I came along when you had to be 12 to be a boy scout and I became an eagle scout, the highest rank in the minimum time; appointed great pride to my mother who encouraged me. She encouraged me a lot of things. I took piano lessons from the age of six until I graduated from high school. I was not a great pianist, but I enjoyed it. Three summers that I was in high school I had summer jobs in a boy scout camp on the Chesapeake Bay, Camp Roosevelt. I was a nature counsellor and I loved it. My job was to take kids on bird hikes and that part of Maryland on the Chesapeake Bay has a lot of marshes and we’d go on canoe trips and be in the marshes at sunrise in the morning to be able to see the birds. I learned where the birds would be and they became like friends. I learned the birds by their songs and I would point to the top of an oak tree there and I’d say, Do you see that, that’s a red eyed vireo and you can tell the difference between the red eyed and the white eyed vireo. We had binoculars, but you couldn’t see the difference, they have a different colour ring around it. I would then imitate the song and we saw wonderful things. There’s a very famous painting by Audubon, the great naturalist in America, of an osprey, that’s a fish hawk holding a fish between his claws, or just dropping one and an American eagle swooping down on him. One of the tricks that the eagle had to get food was he’d wait for the osprey to catch a fish and then he would swoop down from above and catch it. We saw that. |
| Q15 | **Is it important to you to be able to combine the two, your love for the nature and your obvious interest in the research and finding out more about what we are doing here on this earth?** |
|  | Robert C. Richardson: Yes, that’s true. There’s that, that’s part of life and how it fits. If I had come along maybe four or five years later after it was clear what the discovery [Watson and Crick](https://www.nobelprize.org/prizes/medicine/1962/summary/) had made in DNA, I might have decided to go into biology, but from my point of view at the time I was going to college, biology seemed more like a library science, cataloguing this and that and the other whereas the physical sciences had more clearly defined steps one could take in both research and in the applications. |
| Q1 | **Just to round off, any advice to young students. Out in the nature and back in to the laboratory very quickly to do the research. What shall they do?** |
|  | Robert C. Richardson: My advice for young children and parents is to encourage them to be very broad and to have enough of their training in the key courses like mathematics at an early age, so that the options aren’t cut off. A central thing in flexibility and career is learning mathematics and pursuing that and recognising that it’s not just being able to fill out your tax forms and income statements and so forth. A key path in creativity is understanding mathematics and having mathematical training at an early age. Then after that, my recommendation is to find something you like and pursue it with all your heart and have the guts to change when you change your mind. |

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| **Physics\_1999-** | |
| **ID** | 0080 |
| **Biographical** | G**ood Schools, Books, a Love of Mechanics, and You Must Earn a Living** About 1900 my parents came to the United States as children from what was then the Polish area of Russia. As Jews, their families left Russia to escape the poverty and the antisemitism. My parents grew up in poor areas of New York City, my father Oscar Perl in the East Side district of Manhattan and my mother Fay Rosenthal in the Brownsville district of Brooklyn. Their educations ended with high school – my father going to work as a clerk and then salesman in a company dealing in printing and stationary, and my mother working as a secretary and then bookkeeper in a firm of wool merchants.  My parents were determined to move into the middle class. By the time my sister, Lila Perl, and I were born in the 1920’s, my father had established a printing and advertising company called Allied Printing. For many years, Allied Printing was a precarious business. I remember conversations at the dinner table about the problem of meeting the upcoming Friday payroll. However, Allied Printing brought the four of us into the middle class and kept us in the middle class thru the Depression of the 1930’s. We lived in the better neighborhoods of the borough of Brooklyn, not the fanciest neighborhoods, but quite good neighborhoods, and so we went to quite good schools.  These schools and the attitude of my parents towards these schools were important in preparing me for the work of an experimental scientist. Going to school and working for good marks, indeed working for very good marks, was a serious business. My parents regarded school teachers as higher beings, as did many immigrants. School principals were gods to be worshiped but never seen by children or parents. Parents never visited the school to talk about the curriculum or to meet with their child’s teacher. A parent being called to the school because their child had misbehaved was as serious as a parent being called to the police station because their child had robbed a bank. The remoteness of my parents from the schools, so unfashionable today, was often painful for me, but I learned early to deal with an outside and sometimes hard world. Good training for research work! The experimenter dealing with nature faces an outside and often hard world. Natures’ curriculum cannot be changed.  The curricula were unsophisticated, with a great deal of time wasted on penmanship and geography in the early grades and repetitions of the trivial history of New York City in higher grades. But there were also serious courses. In my high school, two foreign languages had to be studied, four years of English was required, and that meant mostly grammar and composition. I was able to take four years of mathematics and a year of physics. Whatever the course, whether the course was boring or interesting to me, whether I was talented in mathematics or not talented in languages, my parents expected A’s. This was good training for research, because large parts of experimental work are sometimes boring or involve the use of skills in which one is not particularly gifted.  For example, I am not a good craftsman. Until recently when I could use computer-based drafting programs, my drawings always looked messy, with uneven lines and ragged lettering. I could never get an “A” in drafting in college. Yet drawing the apparatus to be built for my experiments has always been a crucial part of my experimental work.  There was compensation for the unsophisticated curriculum; with good marks one could “skip” school years. The normal progression was to begin the eight years of elementary school at six years of age, and then to take four years of high school, leading to graduation at eighteen. But classrooms were crowded, and there were no worries about the proper social level of a student; a good student could skip a year or more in elementary school. I was sixteen when I graduated from James Madison High School in Brooklyn in 1942. My sister, who is now a well known writer in the United States, moved through school even faster – she graduated at fifteen and one-half.  Along with my parents insistence, soon internalized, that I do very well in school, went my love of reading and my love of mechanics. I read everything: fiction, history, science, mathematics, biography, travel. There were two free public libraries within walking distance of my home; I remember taking six books home from every visit, the limit set by the library. This reading had only partial approval from my parents. They wanted me to play more sports because they were acutely sensitive to their children being one hundred percent American, and they believed that all Americans played sports and loved sports. They felt that too much reading interfered with my going outside to play sport. I loved rainy days when I did not have to go outside, and to the present I still feel very content on a rainy day.  Two books are burned in my memory, Lancelot Hogben’s Mathematics for the Millions and his Science for the Citizen. I borrowed them from the library again and again. I made summaries of them. I could not understand Hogben’s introduction to calculus so I copied that section completely. I don’t know why it never occurred to me or my parents to buy the books. We could have well-afforded them, but somehow buying books was a waste of money. Naturally, I have compensated in my adult years by owning very large numbers of books.  Another thing we could have afforded was to buy me an Erector construction set. The Erector construction set was the United States equivalent of Meccano or Märklin construction sets in England and Europe. But the cousin I played with every Saturday had an Erector set, and one Erector set per extended family was considered quite enough. He also had electric trains. I loved to build with the Erector set, I loved to build toys and models out of wood, I loved to draw mechanical devices, even those I could not build. I loved to read the magazines, *Popular Mechanics* and *Popular Science*. I loved all things mechanical; cars, trucks, derricks trains, and steam ships. I was in love with mechanics, and I still am.  Before leaving this subject I must mention that since I never owned an Erector set as a child, I have compensated in my adult years by collecting old European, English, and American construction sets; and even by devising and starting prototype production of a modern wooden construction set called BIG-NUT.  I was also interested in chemistry, but my parents were not willing to buy me a chemistry set. I had some chemicals but when I bought sulfuric acid and nitric acid, my father confiscated the acids on the grounds of safety. As every child knows, chemistry with nothing stronger than vinegar soon becomes dull. Strangely for a person who became a physicist, I was not interested in amateur radio or in building radios. I don’t know why. This was the 1930’s when vacuum tubes and variable condensers made radio building quite mechanical.  In spite of very good school marks, a love of books (particularly in science and mathematics), and a great love of mechanics, I never thought of becoming a scientist. That was because as the children of immigrants, my sister and I were taught that we must use our education to “earn a good living” In fact, we didn’t have to be taught that. It was obvious to us. Our home life was physically comfortable, and in some ways emotionally supportive, but it was also rigid and stifling. We knew that we had to earn our own livings to escape from home and Brooklyn. A good living in the Jewish middle class meant that a girl should become a teacher or nurse; a boy should become a doctor, dentist, lawyer, or accountant. I did not think about going into business because the difficulties of the Depression years did not make business a good way to earn a living.  Although I won the physics medal when I graduated from high school, I did not think of becoming a physicist or any kind of scientist. My parents and I knew about a few scientists, certainly Pasteur, and perhaps Einstein, but we did not know that it was possible for a man to earn a living as a scientist.  **Engineering Studies, the War, a Practicing Engineer, and What You are Interested in is called Physics** We did know that a man could earn a living as engineer. And so in choosing a profession for me, my parents and I took into account my love of mechanics, and science and mathematics. We put aside my becoming a doctor, dentist, lawyer, or accountant in favor of my becoming an engineer. This was an unusual choice for a Jewish boy in the early 1940’s because there was still plenty of antisemitism in engineering companies. I enrolled in the Polytechnic Institute of Brooklyn, now Polytechnic University, and began studying chemical engineering.  There were several reasons for choosing chemical engineering. Chemistry was a very exciting field in the late 1930’s and early 1940’s. Chemistry was bringing to our lives synthetic materials such as nylon. The slogan of the radio program, *Dupont’s Cavalcade of America*, was “Better things for better living through chemistry”. Furthermore, Allied Printing had prospered through my father’s hard work, and through the inclusion of a few chemical companies among his customers. He became friends with buyers in several of these companies, and they told him about the expansion of their companies. There would always be a good job in chemical engineering.  One of the first courses I took in college was general physics, using the textbook by Hausman and Slack. The course was all about pulleys and thermometers; physics seemed a dead field compared to chemistry. So, just as I was blind to the fascination of physics in high school, I was once again blind to its fascination in college. I ignored physics, and continued studying chemistry and chemical engineering.  My studies were interrupted by the war. I wanted to join the United States Army, but I was not yet eighteen and my parents would not give me permission. However, they agreed to me joining the United States Merchant Marine, I was allowed to leave college and become an engineering cadet in the program at the Kings Point Merchant Marine Academy. The training ship was wonderful – it had a main reciprocating steam engine, and direct steam driven pumps and auxiliary machinery; a paradise of mechanics. But when I went to sea for six months as part of the training, I was on a Victory ship with a sealed turbine and electrically driven auxiliary machinery. Very boring. Therefore, when the war ended with the atom bomb, I left the merchant marine and went to work for my father while waiting to return to college. I knew so little about physics that I didn’t know even vaguely why the bomb was so powerful.  I didn’t get right back to college. The draft was still in force in the United States. I was drafted, and spent a pleasant year at an army installation in Washington, DC, doing very little. Finally, I returned to the Polytechnic Institute and received a summa cum laude bachelor degree in Chemical Engineering in 1948.  The skills and knowledge I acquired at the Polytechnic Institute have been crucial in all my experimental work: the use of strength of materials principles in equipment design, machine shop practice, engineering drawing, practical fluid mechanics, inorganic and organic chemistry, chemical laboratory techniques, manufacturing processes, metallurgy, basic concepts in mechanical engineering, basic concepts in electrical engineering, dimensional analysis, speed and power in mental arithmetic, numerical estimation (crucial when depending on a slide rule for calculations), and much more.  Upon graduation, I joined the General Electric Company. After a year in an advanced engineering training program, I settled in Schenectady, New York, working as a Chemical Engineer in the Electron Tube Division. I worked in an engineering office in the electron tube production factory. Our job was to troubleshoot production problems, to improve production processes, and occasionally to do a little development work. We were not a fancy R&D office. I worked on speeding-up the production of television picture tubes, and then on problems of grid emission in industrial power tubes. These tasks led to a turning point in my life.  I had to learn a little about how electron vacuum tubes worked, so I took a few courses in Union College in Schenectady specifically, atomic physics and advanced calculus. I got to know a wonderful physics professor, Vladimir Rojansky. One day he said to me “Martin, what you are interested in is called physics not chemistry!” At the age of 23, I finally decided to begin the study of physics.  **Graduate Study in Physics, I.I. Rabi, and Learning the Physicist’s Trade** I entered the physics doctoral program in Columbia University in the autumn of 1950. Looking back, it seems amazing that I was admitted. True, I had a summa cum laude bachelor degree, but I had taken only two courses in physics: one year of elementary physics and a half-year of atomic physics. There were several reasons I could do this 1950; it could not have been done today. First, graduate study in physics was primitive in 1950, compared to today’s standards. We did not study quantum mechanics until the second year, the first year was devoted completely to classical physics. The most advanced quantum mechanics we ever studied was a little bit in Heitler, and we were not expected to be able to do calculations in quantum electrodynamics.  Second, there was no thought of advising or course guidance by the Columbia Physics Department faculty – students were on their own. I was arrogant about my ability to learn anything fast. By the time I realized I was in trouble, but the time I realized that many of my fellow students were smarter than me and better trained then me, it was too late to quit. I had explained the return to school to my astonished parents by telling them that physics was what Einstein did. They thought if Einstein, why not Martin; I could not quit. I survived the Columbia Physics Department, never the best student, but an ambitious and hard-working student. I was married and had one child. I had to get my Ph.D and once more earn a living.  Just as the Polytechnic Institute was crucial in my learning how to do engineering; just as Union College and Vladimir Rojansky were crucial in my choosing physics; so Columbia University and my thesis advisor, [I.I. Rabi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/index.html), were crucial in my learning how to do experimental physics. I undertook for my doctoral research the problem of using the atomic beam resonance method to measure the quadrupole moment of the sodium nucleus. This measurement had to be made using an excited atomic state, and Rabi had found a way to do this.  As is well known, Rabi was not a “hands-on” experimenter. He never used tools or manipulated the apparatus. I learned experimental techniques from older graduate students and by occasionally going to ask for help or advice from Rabi’s colleague, Polykarp Kusch. I hated to go to Kusch, because it was always an unpleasant experience. He had a loud voice which he deliberately made louder so that the entire floor of students could hear about the stupid question asked by a graduate student.  Thus as in the course work, I was on my own in learning the experimenter’s trade. I learned quickly, as I tell my graduate students now, there are no answers in the back of the book when the equipment doesn’t work or the measurements look strange.  I learned things more precious than experimental techniques from Rabi. I learned the deep importance of choosing one’s own research problems. Rabi once told me that he would worry when talking to Leo Szilard that Szilard would propose some idea to Rabi. This was because Rabi wouldn’t carry out an idea suggested by someone else, even though he had already been thinking about that same idea.  I learned from Rabi the importance of getting the right answer and checking it thoroughly. When I finished my measurement of the quadrupole moment, I was eager to publish and to get on with earning a living. But Rabi had heard that a similar measurement had been made by an optical resonance method in France. He wrote to the French physicists to see if they had a similar answer. He didn’t telephone or cable; he calmly wrote. I waited nervously. Six or eight weeks later he received the answer that they had a similar answer; then, I was allowed to publish. It is far better to be delayed, it is better to be second in publishing a result, than to publish first with the wrong answer.  It was Rabi who always emphasized the importance of working on a fundamental problem, and it was Rabi who sent me into elementary particle physics. It would have been natural for me to continue in atomic physics, but he preached particle physics to me – particularly when his colleagues in atomic physics were in the room. I think that most of that public preaching may have been Rabi’s way of deliberately irritating his colleagues.  **Michigan, Bubble Chambers, and On my Own with L.W. Jones** When I received my Ph.D. in 1955, I had job offers from the Physics Departments at Yale, the University of Illinois, and the University of Michigan. At that time, the first two Physics Departments had better reputations in elementary particle physics, and so I deliberately went to Michigan. I followed a two-part theorem that I always pass on to my graduate students and post doctoral research associates. Part 1: don’t choose the most powerful experimental group or department – choose the group or department where you will have the most freedom. Part 2: there is an advantage in working in a small or new group – then you will get the credit for what you accomplish.  At Michigan I first worked in bubble chamber physics with [Donald Glaser](https://www.nobelprize.org/nobel_prizes/physics/laureates/1960/index.html). But I wanted to be on my own. When the Russians flew SPUTNIK in 1957, I saw the opportunity, and jointly with my colleague, Lawrence W. Jones, we wrote to Washington for research money. We began our own research program, using first the now-forgotten luminescent chamber and then spark chambers. This brings me to the story I tell in my Nobel lecture on the discovery of the tau lepton.  **It was Good Fortune …** Looking back to to my early years in Brooklyn, at the Polytechnic Institute, and at the General Electric Company, I am astonished to be writing a biographical note as a Nobel Laureate. I have tried to tell how it happened, yet I realize that I have left out the most crucial element: good fortune. It was good fortune to be a child during the Depression years and a youth during the war years. I lived in a country united by the belief that hard work and perseverance could get one through great difficulties. I saw right triumph. The progression of my career coincided with the growth of universities and the tremendous expansion in federal support for basic research, Academic jobs were relatively easy to get and hold, research funds were relatively easy to get. All good fortune. Of course, my ultimate good fortune was that the tau existed.  Life is much harder for the young women and men who are in science in present times. But they are smarter and better trained than I was at their ages; they know more and have better equipment. I wish them good fortune. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0081 |
| **Biographical** | I was born in Paterson, New Jersey on March 16, 1918, the youngest of four children. My parents, Israel and Gussie (Cohen), had met and married in New York City after emigrating to the United States from the same small town in Russia. A paternal relative in Russia, the Rabbi Isaac Jacob Reines (1839-1915), was famous for his role in founding the Religious Zionist movement, Mizrachi. Manually very skilled and to some extent a frustrated machinist, my father worked as a weaver before World War I, started a silk mill business after the war, and eventually moved to Hillburn, New York, where he ran a general store. My early childhood memories center around this typical American country store and life in a small American town, including 4th of July celebrations marked by fireworks and patriotic music played from a pavilion bandstand. As a child, I enjoyed building things and participating in group singing in school. Music, and singing in particular, was to become a central lifelong interest of mine. The first stirrings of interest in science that I remember occurred during a moment of boredom at religious school, when, looking out of the window at twilight through a hand curled to simulate a telescope, I noticed something peculiar about the light; it was the phenomenon of diffraction. That began for me a fascination with light.  My early education was strongly influenced by my older siblings. Our home had many books due principally to the educational interests of my sister and two brothers, all of whom where serious students engaged in professional studies; my sister became a doctor of medicine and my brothers became lawyers. Among my activities was membership in the Boy Scouts; I rose each year through the ranks, eventually achieving the rank of Eagle Scout and undertaking leadership roles in the organization. My scientific interests also blossomed during this time in the Boy Scouts, where I began to build crystal radios “from scratch.” By this time the family had returned to New Jersey, and I was a student at Union Hill High School. In school, I was initially more attracted to literary interests and did not do as well in science studies. However, by my junior and senior years in high school this situation turned around aufficiently to point me in the direction of science. I was strongly encouraged by a science teacher who took an interest in me and presented me with a key to the laboratory to allow me to work whenever I wanted. I also served as Editor-in-Chief of the high school year book. In response to the year book query to students about their principal ambition, my entry was: “To be a physicist extraordinaire.”  When time arrived to select a college for study in science or engineering, I initially aimed to go to MIT, and was accepted and advised to apply for a scholarship based on my grades. However, I had a chance encounter with an admissions officer of Stevens Institute of Technology, who so impressed me by his erudition and enthusiasm for the school that I changed course and entered Stevens Institute. There, in addition to engineering studies, I participated in the dramatic society and in a dance group performance. But the college activity that I engaged in which was to have a long-standing attraction to me was singing in the chorus, where I performed solo roles in major pieces, including Händel’s “Messiah”. My voice and ear for music were sufficiently highly regarded that I was encouraged by the leader of the chorus to take lessons with a well-known voice coach at the Metropolitan Opera. Since, as a student, I could not afford to pay for lessons, they were eventually provided to me free of charge. Between college and graduate school, I even thought briefly about pursuing a professional singing career, but ultimately decided against it.  The interests in music and drama that I developed in college have persisted throughout my life. Years later, while working in Los Alamos, I sang solos with the town chorus and performed with the dramatic society; my dramatic roles included the lead role in “Inherit the Wind.” I also sang in performances of Gilbert and Sullivan operettas in Los Alamos. My discovery of Gilbert and Sullivan had also occurred while I was in college, and I have enjoyed occasionally entertaining colleagues and friends with G & S lyrics. The peak of my musical endeavors occurred during the period I lived in Cleveland, when I performed with the chorus of the Cleveland Symphony Orchestra under the direction of Robert Shaw and orchestra conductor George Szell.  I received my undergraduate degree in engineering in 1939 and a Master of Science degree in mathematical physics in 1941 at Steven Institute of Technology. It was during this period in 1940, that I married Sylvia Samuels. We have two children, Robert G., who currently lives in Ojo Sarco, New Mexico, and Alisa K. Cowden, of Trumansburg, New York, and six grandchildren.  I continued with graduate studies at New York University, where I worked for a time in experimental cosmic ray physics under the direction of S.A. Korff, and wrote a theoretical Ph.D thesis on “The Liquid Drop Model for Nuclear Fission” under R.D. Present. Even before completing my thesis in 1944, I was recruited as a staff member under [Richard Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html) in the Theoretical Division at the Los Alamos Scientific Laboratory, to work on the Manhattan Project. During my participation in the Manhattan Project and subsequent research at Los Alamos, encompassing a period of fifteen years, I worked in the company of perhaps the greatest collection of scientific talent the world has ever known. About a year after I arrived I became a Group Leader in the Theoretical Division and, later, the director of Operation Greenhouse, which consisted of a number of Atomic Energy Commission experiments on Eniwetok atoll. In addition to my work on the results of bomb tests conducted at Eniwetok, Bikini and the testing grounds in Nevada, I directed my efforts during this period to the basic understanding of the effects of nuclear blasts, including a study of the air blast wave coauthored with John von Neumann. In 1958, I was a delegate to the Atoms for Peace conference in Geneva.  In 1951, I took a sabbatical-in-residence from my duties at Los Alamos to think about the physics I would pursue in the coming years. It was during this time that I decided to attempt the observation of the neutrino. The idea of searching for the elusive neutrino had, in fact, occurred to me as early as 1947, but the opportunity did not present itself. I was now determined to do it, and formed an extremely fruitful collaboration with Clyde Cowan, another Los Alamos staff member. We initially considered the use of a nuclear bomb test as the source of neutrinos, but soon decided that the reactor at Hanford, Washington, would be better. After the first hints of a result at Hanford in 1953, we were informed by John Wheeler about the new Savannah River reactor facility being built in South Carolina. The conditions at Savannah River were ideal for this experiment and, in 1955, Cowan and I transferred the operation there. In 1956 we observed the electron antineutrino. Shortly after that, Cowan left Los Alamos and our collaboration came to a natural end. I turned my attention for a while to gamma ray astronomy and soon began the first in a continuous series of experiments at the Savannah River site to study the properties of the neutrino.  I left Los Alamos in 1959 to become Professor and Head of the Department of Physics of the (then) Case Institute of Technology in Cleveland, Ohio. During my seven years at Case, I built a group working in reactor neutrino physics, double beta decay, electron lifetime studies, searches for nucleon decay, and a very ambitious experiment in a gold mine in South Africa that made the first observation of the neutrinos produced in the atmosphere by cosmic rays. The primary goals of the experimental program were elucidation of the properties of the neutrino and probing of the limits of fundamental symmetry principles and conservation laws, such as the conservation of charge, baryon number and lepton number. Most of these experiments required the reduction of the cosmic ray muon flux in order to be successful, and the group necessarily became expert in the operation of deep underground laboratories. The projects also drew us into developing innovative detector techniques, including the use of large liquid scintillator and water Cherenkov detectors.  This line of research continued when I went, and brought my research group with me, to the new University of California, Irvine campus in 1966 to become the founding Dean of the School of Physical Sciences. I served as Dean until 1974, when I stepped down to return to full time teaching and research. I was appointed Distinguished Professor of Physics at UCI in 1987 and became Professor Emeritus in 1988. I have also served as Professor of Radiological Sciences in the College of Medicine at UCI. The “Neutrino Group” at Irvine has been actively involved in a wide range of neutrino and elementary particle physics experiments, including its role in the IMB (Irvine-Michigan-Brookhaven) proton decay experiment. This group has continued the program of reactor neutrino experiments, has been the first to observe double beta decay in the laboratory, and was awarded the 1989 Bruno Rossi prize in High Energy Astrophysics by the American Astronomical Society for its joint observation (with the Kamiokande Experiment in Japan) of neutrinos from supernova 1987A. The detection of the supernova neutrinos was a particularly gratifying outcome of the IMB experiment. Our group had always been aware of the possibility of seeing neutrinos from stellar collapse in our large detectors, and several of the previous detectors had been adorned with signs identifying each of them as a “Supernova Early Warning System.”  Over the years, a number of other intriguing experimental ideas and areas of investigation have been the objects of my attention, and I have devoted some time and effort to exploring the inherent possibilities. These include: the search for relic neutrinos; the “neutrino Mössbauer effect”, in which a photon is replaced by a neutrino; the measurement of the gravitational constant, G, the most poorly measured non-nuclear fundamental constant by several orders of magnitude; a spherical lens space telescope; attempting to set more stringent limits on violation of the Pauli Exclusion Principle; exploration of the brain using ultra-sound; and a variety of new detector ideas. These scientific concepts, goals and challenges continue to excite and stimulate my interest. |
| **Autobiographical** |  |
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| **Biographical** | It appears that I was born in hospital in Lethbridge, Alberta, Canada on July 15, 1918. My first memories are of a farm near Milk River where I lived with my mother and father and my sister, Alice Evelyn, and a variety of farm and domestic animals. My father, Israel Bertram Brockhouse, had homesteaded with other members of his family in 1910. He had spent his years to that time in the United States after being brought to this continent at two years of age from the family’s native Yorkshire. My mother, Mable Emily (Neville) Brockhouse had grown up in Illinois, the product of uncounted generations of North American English people. As the years went on there were two other children born: Robert Paul, who died in infancy, and Gordon Edgar who became much later a railroad civil engineer. In the winter of 1926-27 our family moved to Vancouver B.C. and it was in that city my sister and brother and I grew up.  My sister entered the school system in a normal way. But I had been a somewhat nominal attendee of the one-room elementary school a couple of miles from our farm and my preparation for the system was somewhat mixed. I must have learned to read and to do simple arithmetic at a very early age because I cannot remember ever learning these subjects. But in other ways I was much behind my potential classmates. But the fine Vancouver schools I attended (Central and then Lord Roberts elementary schools and King George High School – and the Sunday School of St. John’s United Church) soon took care of this. So I had what I believe to be a good basic education, except for social and organizational defects probably arising from the facts that I found school work easy and that I was younger than most of my classmates.  There were other people of course who had influence on me. These included my two aunts: Edith (Neville) Murphy in Chicago and Maude (Brockhouse) Smith in western Canada. My older cousin Wilbert B. Smith may have inspired an early interest in radio technology.  Our family finances were somewhat precarious so I carried newspapers for most of my teens. But the Great Depression made things worse and in 1935 our family moved by train to Chicago in the hope of bettering the situation. I had completed High School by this time and took some evening courses at Central YMCA College (now Roosevelt University). I was interested in the technical aspects of radios and learned to repair and design and build them. This and my facility with mathematics was, I suppose, what pointed me eventually in the direction of physics. For part of our time in Chicago I worked as a lab assistant in a small electronic firm, Aubert Controls Corporation. But the company failed in the recession of 1937. In 1938 our family decided to return to Vancouver and we drove across the continent, all of us I think enjoying the experience.  In Chicago I had begun to repair radios as a small business and I continued this in Vancouver. My parents ran a small grocery store but neither enterprise was really successful. I had always been interested in politics but now I began to take part as an active member of the leftist party of the era, the CCF. My adherence to the CCF continued for many years, in fact until I became an employee of the Dominion Government in the shape of the Chalk River Laboratory. (I understood then (and still do) that there is something dishonourable in a democratic society for a Government employee being other than politically neutral). I was profoundly anti-totalitarian and hence anticommunist so that when World War II erupted I was motivated from many sides to join the military. On September 26, 1939 I enlisted in the Royal Canadian Navy with the design of becoming a Radio Telegrapher. In the event I spent some months at sea as a seaman and ASDIC operator but spent most of my six years in the Navy servicing ASDIC equipment at a shore base. In 1944 I was enrolled in a six-month course in Electrical Engineering at Nova Scotia Technical College and then as a newly-minted Electrical Sub-Lieutenant assigned to the test facilities at the National Research Council in Ottawa. It was there that I met Doris Miller, the girl who later became my wife.  The war having ended, in late August 1945 I was drafted home to Vancouver and was discharged from the Navy on September 11 1945, under the principle “first in – first out”. The Department of Veterans’ Affairs was ready to supply finances for either a small land-holding or for training or education. Thus the way was clear for me to start immediately at the University of British Columbia. My preparation was such that the obvious choices for my course of study was either Electrical Engineering or Physics and I chose to enroll in Physics and Mathematics. I did very well in my first year, actually winning a scholarship. The university life was probably not typical because many of us were older than would normally have been the case. It was not all study, I operated also a (very) small business which eased our financial problems and I owned a motorcycle for transportation and enjoyment.  In the summer of 1946 after taking a summer class for extra credit, I took a vacation on my motorcycle, going all the way to Ottawa via Chicago. This was probably a decisive step in my life because I took up with Dorie again. With time short I returned with my motorcycle by train to Vancouver. Just before Christmas of 1946 my father died. He had long been troubled with a heart condition so his death was not a surprise. In the spring of the year Alice married so our family was now considerably changed. I had received some University credit for my irregular courses in mathematics and electricity and together with overload credits I was able to complete my B.A. program in April. I had been offered a summer job in the Nationel Research Council laboratory (the electrical standards section) so off I went to Ottawa again. There Dorie and I became engaged to be married.  It had been arranged that I should return to Vancouver to take a Master’s degree course but instead I went to the Low Temperature Laboratory of the University of Toronto. This was one of the two Universities in Canada to offer Ph. D. programs at that time (the other was McGill in Montreal). Being already 29 years of age I was very anxious to embark on my physics career. Furthermore, partly no doubt for financial reasons, DVA was very keen that I do my studies in Canada. So I started work under the guidance of Professors Hugh Grayson-Smith and James Reekie on the effects of stress and temperature on ferro-magnetism and finished a Master’s program in the then normal period of eight months. In May, Dorie and I were married in the village of Kirkfield, the old home of her family. For the remainder of the summer we lived in Ottawa, Dorie continuing as a film technician at the National Film Board while I worked as a summer student in the acoustics section of the National Research Council. The more passive part of my education was now complete. The instruction via course-work which I received at UBC and Toronto was probably as good as I could reasonably have expected. Certainly I remember almost all the teachers and courses with fondness. Partly because my mind was “already formed” I suppose, I did not become comfortable with Quantum Mechanics and indeed never did so. The classical nature of the small researches I performed contributed to what was probably an “old-fashioned outlook” even at the time. And now I was forced to assume full responsibility for my future – and the future of my new family.  The Low-Temperature Laboratory at Toronto was long-established and reasonably well-equipped. But at this point my supervisors both left to assume more senior positions at other institutions. Furthermore the third faculty member in the Lab also left. So I was left essentially unsupervised and should also have moved – except that we were now expecting the birth of our first child. But happily, as we thought, Sir Edward Bullard, an expert in earthmagnetism, was coming to head the Department – and to assume direction of my thesis work. If he had stayed for longer than he did then possibly I would have changed my field and worked on the earth-magnetism problems then very current and in which I had some interest. But he left to assume a high position in the U.K. so ultimately I had to do the best I could while receiving every possible help from the Department.  My thesis subject was a contribution to Solid State physics which involved experiments at both low and high temperatures. There were a few books on the general subject, two excellent ones being by Frederick Seitz and by [N.F. Mott](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/index.html) and H. Jones. These I to a considerable extent devoured. I had had lectures on the subject from Grayson-Smith and had a small correspondence with him. I had courses in Thermodynamics, Statistical Mechanics and Theory of Errors. I took a course on Nuclear Theory from my friend Melvin Preston, who was then at Toronto. So I was not too badly prepared in a general way for work on the periphery of Nuclear Energy, when the chance to work at Chalk River was offered to me.  In August 1950 I went up to Deep River, in the van carrying our belongings, while Dorie (and baby Ann) stayed with her parents in a cottage on Balsam Lake near Kirkfield. There I met Don Hurst in whose (neutron physics) group I was to work and saw the house on Hillcrest Ave which was assigned to us. In a short while Dorie (and Gordon-soon-to-be) and Ann joined me. There was still some work to do on my thesis so I would be very busy for the next months. But in October Gordie was born and I passed my Ph. D. exam and we were set for the next period of our lives.  We had originally thought of staying for only a few years and then going on, probably to a University. In the event we stayed for twelve years and four more children. As I progressed we moved (twice) to a better house as was the custom. Despite my long and irregular hours each of us had a social life and one together and we have kept in touch with some of our acquaintance then to this day. Since the work I did then represents a major part of the content of my lecture I will here be brief; I have reviewed it elsewhere – the major advance at this time in early 1951 was the realization that phonons could be studied by studying inelastic scattering and that evocative experiments to do so might be feasible at Chalk River.  The first actual experiments studied the scattering of neutrons by highly absorbing elements, in the process verifying the famous Breit-Wigner formula. This work (on scatterers Cd, Sm and Gd) was done in collaboration with Myer Bloom and D.G. Hurst and was published in Physical Review (1951) and in the Canadian Journal of Research (1953). The apparatus was later much mod)fied and used to study the inelastic scattering from several materials (Aluminium, Graphite and Diamond) by absorption methods. This was the first quantitative experiment in slow neutron spectroscopy and was published in Physical Review. Other experiments by absorption methods were done about the same time at Harwell by R.D. Lowde and P.A. Egelstaff; that by Ray Lowde was particularly significant as it went far to establish the concept “spin wave” on a microscopic basis.  Preparations were underway to attempt proper (differential) studies of inelastic scattering and some almost futile attempts had been made, when our work was terminated by an accident to the NRX high flux reactor which was the source of the neutrons we used. This occurred in November, 1952 and I did not resume actual experiments at NRX until the summer of 1954. Fortunately, I was invited to go to Brookhaven National Laboratory and was able to spend most of one year there with my family, returning to Deep River in February, 1954. The time was very profitable for me, I worked on several experiments, with collaborators and without. But I did not do any spectroscopic work though I met Donald Hughes and Harry Palevsky, now also thinking about inelastic scattering and in particular thinking about the “Cold Neutron” or (Beryllium) Filter-Chopper method. And I met Leon Van Hove and learned about the new generalized (time-dependent) correlations which Noel K. Pope and I were later to put to good use.  After NRX was available to us again in August 1954, things progressed rapidly. Because of the efforts of David G. Henshaw and Jack Freeborn, we had metal monochromators of greatly improved efficiency compared with the NaCl crystals which we were using in 1952. Alec T. Stewart was rapidly getting the Be/Pb Filter-Chopper apparatus together and the primitive Triple-Axis spectrometer was functioning. So I was able to present a paper with substantial (if primitive) results at the New York meeting of the American Physical Society at the end of January, 1955. Publications followed soon after, in Physical Review and in the Canadian Journal of Research.  In 1956 we were able to complete the first true Triple-Axis crystal spectrometer, though only for operation at constant incoming energy. The flexibility of operation and the accuracy of the results were both greatly improved. The “Constant Q Method” was invented in 1958 and at about the same time a new apparatus allowing operation with variable incoming energy was installed at the new high-flux reactor NRU. (Ed Glaser and William McAlphin played crucial roles in these developments.) With the considerable improvements in both the neutron flux and the operating conditions afforded by NRU the subject entered a new phase in 1959. The Triple-Axis spectrometer thereby reached nearly full development. Visitors from other countries were now arriving to spend time working in the group. (The first such visitor was P.K. Iyengar from India who with several others became a life-long friend.) From about 1958 on the interest shifted, from the neutron physics and the methods and the validity of the theory, to the specific results and interpretation for the specific speciment material.  In 1956 also Alec Stewart completed the Filter-Chopper apparatus. This was an equipment similar in general to that of Hughes and Palevsky; it was used in experiments on Aluminium and Vanadium, both chosen for the same good technical reasons that others chose to work on them. When Stewart left to become a professor at Dalhousie University I converted the instrument to the first “Rotating Crystal Spectrometer” – a bad choice of name as it should have been termed “Spinning Crystal”. This instrument was used principally to study liquids and polycrystals, as was its improved successor at the NRU reactor.  Three other major technological initiatives were taken. Filters of (large, perfect) single-crystals (quartz), preferably cooled to low temperatures, enabled major improvement in the ratio of slow neutrons to fast in the primary beam and thus in the signal to background ratio. The “Beryllium Detector” method was developed by enabling the Triple-Axis spectrometer to accept Beryllium polycrystalline filters in the scattered beam and thus, with incoming neutrons of variable energy, to get energy distributions in a different and sometimes advantageous manner – an inverse of the Filter-Chopper method. Finally profitable uses of the new material, pyrolitic graphite, were found – as filters and as crude monochromators.  As time went on I began to receive invitations to attend Conferences and colloquia. In 1957 I made my first trip to England and Europe. Aside from several seminars, I gave a paper in September at a Conference on the Physics and Chemistry of Liquids; held in Varenna on Lago di Como in Italy. My last stop was at a gathering in Stockholm of neutron scattering people. After giving my paper on the first day I became ill with “flu” and spent the next few days of my trip to Europe in hospital. Nevertheless the trip was very inspirational and rewarding. In October 1960, this time accompanied by Dorie, I made another trip to Europe, and gave papers at two [IAEA](https://www.nobelprize.org/nobel_prizes/peace/laureates/2005/index.html) Conferences in Vienna. One of these was the first of the IAEA Conferences on Inelastic Scattering that played such a large role in the development of the subject.  In 1958 our group was joined by A.D.B. (David) Woods, who from then on was my closest collaborator. Numerous people spent periods of time in the group. Of these I must mention William Cochran who collobarated in the project to study the lattice vibrations in alkali halide crystals and in the course of this work developed his well-known “shell model” for the atoms in these and other crystals. Following this, his student from Cambridge University, Roger A. Cowley, commenced his own long association with the group. In 1961 Gerald Dolling arrived after studies at Cambridge and Harwell (with G. L. Squires); he is the only person among those mentioned who is still active in the group.  Other colleagues at Chalk River and visistors there were important to my program. These included: I.L. (Dick) Fowler, Harris McCrady, Walther Woytowich, C.W. Crawford, C.E.L. Gingell, William Howell, G.R. DeMille, Guiseppe Caglioti, T. Arase, R.G. Johnson, K.R. Rao, M. Sakamoto, Hiroshi Watanable, Leo N. Becka, Roger N. Sinclair, B.A. Dasannacharya, R.H. (Bob) March, A.E. (Ted) Dixon, R. Sherman Weaver, J. Bergsman.  In 1962 I took up a position as Professor of Physics at McMaster University, in Hamilton, Ontario. The research program that I had embarked on eleven years before had been successful beyond expectations and the field was becoming well established. For over fifteen years it had been my intention to take up a University career and in my mid-forties it seemed that “now” was the time if I were to do so. McMaster had a “swimming-pool” reactor which promised to make the transition easier on the research side. For social reasons I preferred not to join a mega-university or live in a mega-city, partly because I thought that it would be better for our family of six children. Dorie was supportive of these ideas. So off we went in the summer of 1962, first to a house in Dundas and soon after to the house in Ancaster in which we still live.  Chalk River had been very good to us. And now the Laboratory facilitated our transfer and encouraged my plans to continue a research program based at McMaster and to use the reactor there for training students and for preliminary work on experiments to be carried out at Chalk River. This arrangement was I think very successful all through the 1960s and early ’70s and indeed has been carried on by others since that time. At McMaster a talented group of students put together a neutron diffractometer and a triple-axis instrument and these were available from 1965 on – and indeed are still in use. For the first years we used existing equipment at Chalk River but about 1971 we installed our own spectrometer at NRU and the smaller group now working with me used it (as did others) until I completely left neutron scattering about 1979.  Deep River was also good to us. Five of our six children were born in Deep River Hospital. (Gordon Peter, Ian Bertram, James Christopher, Alice Elizabeth and Charles Leslie.) Our contacts with friends made then have remained deep. But there was one matter for distress – in babyhood Jamie developed hyperactive and autistic behaviour and in 1961 he was placed in Smith Falls Hospital School where he remained until, somewhat improved, we brought him home to Ancaster in 1967. He was sent to special schools in Hamilton; since then he has worked in a sheltered workshop. Of late years he has lived with other afflicted persons in a supervised apartment. Our other five children have all gone on to successful careers; Charles, a molecular biologist, is the only scientist among them. We now have eight grandchildren in four families.  At McMaster I lived the normal life of a Professor of Physics. Each year I usually taught two courses (mostly Solid State Physics, Thermodynamics and Statistical Mechanics) and carried out the other duties required of me. Eleven people won wheir Ph.D. degrees under my supervision: S.H. Chen, J.M. Rowe, E.C. Svensson, S.C. Ng, A.P. Miller, E.D. Hallman, J.R.D. Copley, A.P. Roy, W.A. Kamitakahara, H.C. Teh, A. Larose. About half of them found their careers in neutron scattering. The research of the group consisted of studies of the phonons in crystals and their temperature behaviour, especially in single crystals of metallic alloys. There were also several Master’s projects, one of which should be mentioned: the highly quantitative study by R.R. Dymond of the reflective behaviour of maltreated copper monochromators. The contributions of several other men should be mentioned, including G.A. DeWit, William Scott, James Couper, E. Roger Cowley, A.K. Pant, Jake Vanderwal and David Macdonald.  But my greatest debt is to my wife of 46 years and my family, whose support and encouragement were indispensable and total. And following this, my colleagues and I owe gratitude to the technologists who engineered and maintained the reactors which provided the neutrons employed in the work – and to Don Hurst who introduced me to the subject – and to the National Research Council of Canada, who supported the program at McMaster over many years – and, finally, to the people of Canada, who supported all these and us.  From 1960 on I suffered, at intervals of a few years, serious health problems of several varieties. These were kept under control by our medical allies and by the support of Dorie and our families. My work was not affected much in formal ways though undoubtedly some aspects did suffer. Throughout my career my father-in-law (Sidney L. Miller) maintained a cottage on Balsam Lake, north of Toronto; this was a considerable blessing for all of us. In addition we did a little camping from time to time, until I developed a bad back. And music – consert, opera, records – have always been part of our life.  During the 1970s I gradually realigned my intellectual interests. One avenue I explored was what might loosely be termed “philosophy of physics”. Another (intersecting) route was concerned with energy supply and the economics and ethics thereof. And there were others. In my explorations I entertained the hope that I would find some interesting niche in which to work. But I also realized the extreme importance of reaching general points of view, if this were at all possible. In this quest I struggled with new descriptions of the furniture of the world. Not much of what I sought was found and not much of that was made public – though I did give some seminars and some talks to service clubs and the like. Perhaps the new impetus to action, given by the amazing event of the Nobel Prize and its accompaniments, will move me on to produce something more well-defined. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
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| **Biographical** | I was born on September 23, 1915 to my parents, David H. and Daisy B. Shull, in the section of Pittsburgh, Pennsylvania, known as Glenwood, which obviously relates to their selection of my middle name. I was preceded by an older sister, Evalyn May, and an older brother, Perry Leo, so that I grew up as the baby in the family. Both my father and mother had origins in rural, central Pennsylvania, in farming sections of Perry County. After moving with his then family to the big city, Pittsburgh, my father started a small business that evolved into a hardware store and an associated home repair service.  My early years of growth were entirely normal and happy ones and I had the usual collection of friends and buddies, who were often seen on the ball field or on roller skates. Grade schooling was nearby, a few blocks from our home, and this led later on to junior high school in the adjoining Hazelwood section but still within walking distance of our home. Following this, I had decided to go to Schenley High School for the remaining three years of school work and this required a more troublesome commute of 45 minutes by public street car. My first interest in physics as a career speciality came during my senior year at Schenley when I took the physics course taught by Paul Dysart. Somewhat older than the usual high school teacher and with a PhD degree in his background, he was a very impressive teacher who delighted in demonstrations from his laboratory and in explaining the principles behind them. Thereafter my original interest in aeronautical engineering was in heavy competition with physical science.  It seemed natural, in view of limited family financial straights, that I should continue into college study by living at home and commuting to the Carnegie Institute of Technology (now Carnegie Mellon University). Carnegie Tech was also located in the Schenley Park district of Pittsburgh so that essentially the same commute was called for and it offered good, reputable curricula in the engineering and physical sciences. I was pleased when offered admission to the fall term of 1933 and particularly so when given a half-tuition scholarship in view of my good high school record. Once there, my interest in physics as a major subject sharpened quckly, helped along no doubt by the brilliant lectures in my freshman physics course given by Harry Hower, the chairman of the Physics Department. Hower was more aptly labeled an optical and illuminating engineer than a physicist, because of his extensive consulting activities in coastal lighthouse lens design and other architectural problems, but his lectures were delightful, inspiring and not often-to-be-missed by his students.  Shortly after my admission at Carnegie Tech, a family crisis developed when my father died unexpectedly in January, 1934. By this time, my sister had married and, with her husband, were living at home along with my brother (who had just finished college as an art major), my mother and myseelf. My brother decided to forego his art teaching and operate my father’s business and this continued until I had finished Carnegie Tech in 1937. The four years spent there were entirely pleasurable ones, in spite of the time-consuming commute, and I enjoyed the association with my fellow students and professors in the department. I was able to work in the summer periods at jobs both on and off campus and this helped to meet my rather minimal expenses during the year. Among the professors, I valued very much the friendly encouragement and counsel offered by Emerson Pugh during my junior and senior years, leading to my continuance into graduate school at New York University in the fall of 1937.  New York University was then a very large university, perhaps the largest in the nation, with several distributed, more or less autonomous, campuses. I was located with the Physics Department at the University Heights campus in the upper Bronx section of New York City and my teaching assistantship provided living subsistence, teaching meaning laboratory course help and problem assignment grading. We graduate students were encouraged at an early stage to join and help in one of the half dozen or so ongoing research projects within the department. I became associated with a nuclear physics group headed by Frank Myers and Robert Huntoon, who were in the process of building a 200 keV Cockcroft-Walton generator for accelerating deuterons. Much valuable experience was obtained with this exposure by Craig Crenshaw, another graduate student, and myself and we were able to help in the initial experiment with this accelerator, a study of the D-D nuclear reaction.  During the third year of my graduate study, the Department decided that it could support the construction of a new 400 keV Van de Graaff generator to be used for accelerating electrons. Frank Myers took on this responsibility with me as his assistant and the thought that it could be used to repeat the electron-double-scattering (EDS) experiment as a possible thesis topic for me. This EDS type of experiment loomed important at the time because it was considered a direct test that electrons have a spin or polarization. Several earlier experiments had given either negative or inconclusive results and it seemed worthwhile that the experiment be performed again under new conditions. The construction and testing of the new facility went smoothly and I turned to getting ready for my thesis EDS experiment. By this time, Frank Myers had decided to take his overdue sabbatical leave with Robert Van de Graaff at MIT. I was fortunate in getting Richard Cox, a senior professor in the department, to supervise and offer expert and friendly advice on my efforts. Finally after four months of data collection and analysis, the experiment was successful and I was able to prepare a thesis and take my PhD degree in June 1941.  Among the other research programs being pursued by the NYU department was the study of neutron interactions with materials as started by Alan Mitchell and carried on by Martin Whitaker. Using a Ra-Be neutron source surrounded by a paraffin howitzer, a modest beam of thermalized neutrons was available for experimentation and, during my period at the Heights, this was directed towards a search for the expected paramagnetic scattering from certain materials. Theoretical prediction of this had been given by O. Halpern and M. Johnson and their students in the Department. I was familiar with this problem through my contemporary graduate student William Bright who worked with Whitaker on the experiment and indeed found myself working on the same problem a decade later.  I have neglected to mention an important event that occurred in my first year in New York City. Through my good friend Craig Crenshaw, I was introduced to a young lady, Martha-Nuel Summer, who had recently come from South Carolina to the graduate school at Columbia University to study early American History. Our friendship flourished during the years of our professional studies and we married shortly after I took my degree and had a job in waiting. She has remained my loving companion to the present and along the way we have been favored by three fine sons, John, Robert and William, who have beautiful families of their own.  I had arranged for a position at Beacon, NY with the research laboratory of The Texas Company, and Martha and I set up housekeeping there in July 1941. This laboratory addressed problems associated with the production and use of petroleum fuels and lubricants and included a small group of physicists. I was asked to study the microstructure of catalysts using gas adsorption and x-ray diffraction and scattering as tools for characterizing the physical structure of these materials. These catalysts were used in the production of high-performance aviation fuel and this area of investigation became increasingly important after the US entry in the World War in December 1941. Of singular significance to the scientific community in the first year of our wartime activity was the growth of the Manhattan Project dealing with the development of an atomic weapon. Many scientists had been drawn into this, including a number of my old colleagues and professors from graduate school. I was encouraged to join them and would have done so except that The Texas Company would not agree to my wartime job change. The matter was finally settled in their favor by an adjudication hearing at an area manpower board and I stayed in Beacon through the war years.  My work at Beacon was interesting and challenging and gave me the opportunity of learning things about diffraction processes, crystallography and the new field of solid state physics. Through visits and early meetings of the American Society for X-ray and Electron Diffraction, I was able to know established personages such as Warren, Buerger, Fankuchen, Zachariasen, Ewald, Harker, Gingrich and Donnay. Once the war was over, my interest in participating in the exciting new developments in nuclear physics within the Manhattan Project returned, and I paid a visit to the Clinton Laboratory (now Oak Ridge National Laboratory) in Tennessee. The activity there fascinated me very much and I convinced Martha that we should move there, which we did in June 1946 along with our one and a half year old son.  It was arranged that I would work with Ernest Wollan, who had been at the Laboratory since its formation during the war period and who had just assembled a rudimentary two-axis spectrometer for obtaining neutron diffraction patterns of crystals and materials. Wollan had shown me his first powder diffraction pattern on my earlier visit and I was delighted to be able to join him in exploring how neutron patterns could be used to supplement those obtained with x-rays or electrons. Our collaboration on common problems was to continue for nearly a decade until I left Oak Ridge in 1955 for academic life at Massachusetts Institute of Technology. I regret very much that Wollan’s death in 1984 precluded his sharing in the Nobel honor that has been given to Brockhouse and me since his contributions were certainly deserving of recognition.  I was attracted to MIT by the prospects of teaching and of training graduate research students at the soon-to-be-completed MITR-I research reactor on campus. This reactor was among the early group of condensed volume reactors using isotopically enriched fuel which were being explored in that period. Together with occasional post-doctoral students and a regular flow of graduate thesis students, our group carried on investigations using neutron radiation from this reactor in many fields until my retirement from MIT in 1986. These studies included: internal magnetization in crystals, development of polarized beam technology, dynamical scattering in perfect crystals, interferometry, and fundamental properties of the neutron. The opportunity of being at MIT with its fine faculty and excellent students has certainly been most stimulating and satisfying. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0084 |
| **Biographical** | I was born November 28, 1950 in New York City, the son of Alan and Betty Joan Hulse. My parents tell me that I quickly showed an unusual level of curiosity about the world around me as a child, and that this transformed itself into an interest in science at a very early age. For my part, I certainly recall that science was a defining part of my approach to life for as far back as I can remember. My parents fostered and supported this interest, and I thank them very much for being my first and, by far, most uncritically supportive funding agency. I ran through a seemingly endless series of interests involving chemistry sets, mechanical engineering construction sets, biology dissection kits, butterfly collecting, photography, telescopes, electronics and many other things over the years.  The door to a whole range of new experiences opened for me when my father started building a summer house on land given to us by my Aunt Helen in Cuddebackville, New York, about two hours northwest of the city. Eventually, this became a year-round house for my grandparents when they retired and it is where my parents live now that they are retired. I remember spending weekends and summers helping my father put in place walls, rafters, siding and everything else that goes into a house. Among other things, it produced an early familiarity with tools and a do-it-yourself approach which has stood me in good stead over the years. My parents’ friends and relatives were apparently not too sure that I should have been given such freedom to work with power tools at an early age, but fortunately I came through the experience with all of my fingers intact. Cuddebackville was also important to me as a place where a city kid could see nature, and as a practical place to work on my bigger projects.  My parents not only supported my interests at home but also suffered along with me (and, most likely, much more than me) when some of my first experiences with school proved to be less than positive. Though I had some elementary school teachers with whom I got along well, there were some real problems with others who found me and my intense interest in science difficult to understand and deal with.  Entering the Bronx High School of Science in 1963 was thus very important to me as it was there that I found myself in a school environment which explicitly emphasized what I found most interesting in life. Yet, as in the years before and after, while schoolwork was an important job to be done my interests in science tended to be expressed most clearly by my home projects. My biggest home project while at Bronx Science was building an amateur radio telescope up at my parents’ house in Cuddebackville. I particularly enjoyed building antennas of various types, relying on an amateur radio antenna design book as a guide. The electronics were an odd mix of old television parts, military surplus power supplies, receivers and the like combined with other components I built myself. Unfortunately, the telescope never did work particularly well in terms of detecting radio sources (a little outside technical advice probably would have made a big difference in there somewhere), but I did enjoy myself and I learned a lot in the process.  At the end of high school, I had my first big career decision to make. While I had by then begun to focus more on physics and astronomy amongst the sciences, I also enjoyed designing and building electronic equipment. This lead me to consider electrical engineering as well but, in the end, I decided that a degree in physics was probably the best fit to my interests.  My college choices were limited by the fact that paying for college would have placed an inordinate financial burden on my parents. Fortunately, I was admitted to Cooper Union, a tuition-free college in lower Manhattan. From 1966 to 1970, I lived at home in the Bronx with my parents and commuted to Cooper each day on the New York subway system. Along, with the usual course work, Cooper provided me with my first experience with a new interest, computers. Cooper had an IBM 1620 available for the students to use and, while there were no courses on programming it, there were the instruction manuals. The first project that I selected by way of teaching myself FORTRAN was to use the computer to do orbit simulations, perhaps an early omen of things to come.  After receiving my bachelor’s degree in physics from Cooper Union in 1970, I started graduate school at The University of Massachusetts in Amherst. While I knew that I eventually wanted to do my thesis research in astronomy, preferably radio astronomy, I once again leaned towards a broader background and decided to get my doctorate in physics rather than astronomy. I went to UMass not only because its graduate program offered this type of flexibility, but also because it was located not too far from New York in a rather beautiful part of rural western Massachusetts.  The five years I spent in Amherst are some of those which I remember most clearly from my past. Graduate school was an entirely new environment, with new experiences and challenges. The demands were such that, for the first time, I focused almost exclusively on my academic career, with my other outside interests tempered by the demands of the moment.  After passing my Ph.D. qualifying examinations, I turned to finding a thesis project. This represented at long last a convergence of my outside and career interests, as I finally started working in radio astronomy again, now as a career rather than as a hobby. The rest of that story is told in my Nobel lecture.  After completing my Ph.D. in 1975, I had a post-doctoral appointment at the National Radio Astronomy Observatory in Charlottesville, Virginia from 1975 to 1977. While I still enjoyed doing pulsar radio astronomy, from the moment I arrived at NRAO I was increasingly preoccupied with the lack of long-term career prospects in astronomy. While I had some confidence that I could find another position of some sort after NRAO, it was not at all clear to me when, where, and how I would be able to settle down with some reasonable expectation of stability in my career. I certainly knew of astronomers who had been obliged to roam from place to place for many years and the potential for such repeated major dislocations in my personal life was more than I could quite tolerate. In particular, I had the classic problem of how a two-career couple could stay in reasonable geographical proximity, since my friend, Jeanne Kuhlman, was then doing her graduate work at the University of Pennsylvania. I therefore decided to try falling back on my broader interests and my physics Ph.D., exercising the option which I had left myself when I started at UMass.  While even with this broader view not many good career opportunities seemed available, I did discover from an advertisement in Physics Today that the Princeton University Plasma Physics Laboratory (PPPL) was hiring. Not only did controlled fusion seem an interesting and diverse field, but the lab was located in Princeton, not too far from Jeanne in Philadelphia.  After interviewing at PPPL, I was offered a position with the plasma modeling group, based on my physics and computer background. Starting at the lab in 1977, my first task was developing new computer codes modeling the behavior of impurity ions in the high temperature plasmas of the controlled thermonuclear fusion devices at PPPL. I had never really done computer modeling before and the art and science of computer modeling is one of the most valuable things which I have learned in the 16 years which I have now been at the lab.  The multi-species impurity transport code which ultimately grew out of this initial work at PPPL is still in use to this day. It models the behavior of the different charge states of an impurity element under the combined influences of atomic and transport processes in the plasma. I oriented my development of this code very much towards its practical use by spectroscopists and other experimentalists in interpreting their data and one of my greatest satisfactions has been that this code has become widely used over the years both at PPPL as well as at other fusion laboratories. My own research with this code included determining transport coefficients for impurity ions by modeling spectroscopic observations of their behavior following their injection into the plasma. In connection with modeling impurity behavior, I also worked on investigating the atomic processes themselves, for example, by helping to elucidate the importance of charge exchange reactions between neutral hydrogen and highly charged ions as an important recombination process for impurities in fusion plasmas. In a rather different sort of contribution, I more recently developed a computer data format which has been adopted by the International Atomic Energy Agency as a standard for the compilation and interchange of atomic data for fusion applications.  While I am still involved in supporting this impurity transport modeling code at PPPL, my more active area of work in the past few years has been modeling the transport of electrons in the plasma as revealed by pellet injection experiments. The pellets involved here are pellets of solid hydrogen, injected at high velocity into the plasma. The relaxation of the plasma electron density profile after a pellet has deposited its mass inside the plasma provides an important way of observing plasma transport in action. For this work, I wrote an electron particle transport code which focused on modeling the experimentally observed density profile evolutions using theoretically motivated, highly non-linear forms for the particle diffusion coefficients.  In another recent new direction, I have been working to establish a new effort at PPPL in advanced computer modeling environments. The objective of this research is the development of novel approaches to creating modular computer codes which will make it much easier to develop and apply computer models to an extended range of applications in research, industry and education. I have been pursuing this work in the context of cooperative research and development agreements with an industrial partner, taking advantage of this new type of collaborative arrangement recently made possible between government sponsored research laboratories and the private sector.  By now, it is surely clear that my interest in science has never been so much a matter of pursuing a career per se, but rather an expression of my personal fascination with knowing “How the World Works”, especially as it could be understood directly with hands-on experience. This central motivation has been expressed over the years not only in my career but also in a wide range of hobbies. Notable amongst these “hobbies” have always been interests in various areas of science beyond whatever I was professionally employed in at any given time. For example, I have most recently been considering that much of what I have found so interesting about both the natural and man-made world has involved how individual, often autonomous, elements combine to make a functioning whole, either by design or by self-organization. I have thus started to be interested in various aspects of the new so-called “sciences of complexity”, especially as they can be explored using computer modeling.  My list of more traditional hobbies and recreational activities has also changed over time. Many activities which I formerly enjoyed, such as amateur radio and woodworking, have been eventually dropped simply because I realized that I did not have enough time and energy to pursue everything I might enjoy doing. A current list of my activities would include nature photography, bird watching (and observing the beauty and drama of nature in general), target shooting, listening to music, canoeing, crosscountry skiing, and other outdoor activities.  I do not pretend to be anything like an accomplished expert in all of the many things that I have ever been or am presently involved in doing. My most fundamental urge has always been just to spend time on what I found the most interesting, trying of course to match this up somehow with the more practical demands of life and a career. In this sense I have come to realize that at times I must not have always been the easiest person to have had as a student, or as an employee, and I therefore appreciate the efforts of those who helped me to accommodate myself to these practical demands, or often, who worked to help accommodate the practical demands to me.  I would like to close on the thought that some of the most enjoyable moments of my life have always involved sharing my various interests with those others who understood them (and me) the best. Thus special thanks go to my parents, to Jeanne Kuhlman, and to all of the good friends that I have had over the years. |
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| **Physics\_1999-** | |
| **ID** | 0085 |
| **Biographical** | I was born on March 29, 1941, in Philadelphia, Pennsylvania, the second son of Joseph Hooton Taylor and Sylvia Evans Taylor. When I was seven we moved back to the family farm in Cinnaminson Township, New Jersey, then operated by my paternal grandfather. We were three children, joined later by three more, plus two Evans cousins; like the farm’s peaches and tomatoes, the eight of us grew and ripened in a healthy and carefree environment on the eastern bank of the Delaware River. Among my fondest boyhood memories are collecting stone arrowheads left on that land by its much-earlier inhabitants, and erecting, together with my brother Hal, numerous large, rotating, ham-radio antennas, high above the roof of the three-story Victorian farmhouse. With one such project we managed to shear off the brick chimney, flush with the roof, much to the consternation of our parents. That incident was one of many practical lessons of my youth, not all absorbed in the most timely fashion, involving ill-advised shortcuts toward some goal.  Both the Evans and Taylor families have deep Quaker roots going back to the days of William Penn and his Philadelphia experiment. My parents were living examples of frugal Quaker simplicity, twentieth-century style; their very lives taught lessons of tolerance for human diversity and the joys of helping and caring for others. Our house was large, open, and friendly. To my knowledge it has never been (nor indeed can be) locked. In our school years, Hal and I filled most of the third floor with working ham-radio transmitters and receivers. Our rigs were mostly built from a mixture of post-war surplus equipment and junk television sets. We learned by experience that when you need high voltage, the power company’s 6,000-to-120-volt transformers work admirably in reverse; and that most amplifiers will oscillate, especially if you don’t want them to.  I was educated mostly at Quaker institutions, in particular Moorestown Friends School and Haverford College. In school, mathematics was my first academic love. Somewhat backward high-school introductions to chemistry and physics (I failed to recognize them as such at the time) did not dampen any enthusiasm for science, they just gave me more time for sports, then a greater passion. Soccer, basketball, baseball, golf, and tennis claimed much of my energy through the Haverford years. Concurrently, though, I began discovering the delights of what science is really about. A fascinating senior honors project in physics allowed me to combine a working knowledge of radio-frequency electronics with an awakening appreciation of scientific inquiry, and to build a working radio telescope. My principal references were an old friend, *The Radio Amateur’s Handbook*, and an early book on radio astronomy by Pawsey and Bracewell. This thoroughly enjoyable honors project cannot really qualify as research – everything I accomplished had been done by others, years before – but it provided excellent lessons in problem-solving of various kinds. It also delivered a valid reason for selecting something I had been hoping to find: a desirable field of physics in which to pursue graduate studies.  My academic work in the Harvard departments of Astronomy, Physics, and Applied Mathematics was the hardest I ever remember working, at least during my first year there. I suppose every beginning graduate student feels that he or she has something to prove; anyway, I certainly did. But my thesis research in radio astronomy was, once again, thoroughly enjoyable. My mentor, Alan Maxwell, knew the field and its participants well. He gave me plenty of flexibility, provided inroads and introductions when I needed them, and taught me (among many other things) the importance of clear, well-crafted writing in a scientific paper. Ron Bracewell again played an unwitting role; his 1965 book *The Fourier Transform and its Applications* came out just in time to give me some crucial insights necessary for analyzing the data for my thesis. It also prepared me for understanding the signalprocessing techniques that later became important in my study of pulsars.  I have noticed in recent years that many budding scientists worry much more than I ever did about what the future may bring: how to get into the best university, work with the biggest names, find the best post-doctoral fellowship, and secure the ideal university position. My own psychological bent, insofar as it has influenced any professional decisions, is to pursue a path promising enjoyment along the way, without looking too far ahead. Perhaps related to my Quaker upbringing, I’ve always valued personal involvement in a difficult task over appeals to eminence or authority; I like the challenge of re-examining a problem from fresh perspectives. Ultimately, I believe that in important matters we are mostly self-taught, but in a way that is strongly reinforced by cooperative human relationships. I have worked in two extremely stimulating intellectual environments, first at the University of Massachusetts and more recently at Princeton. I’m fortunate to have associated with some uniquely gifted individuals who have been especially compatible co-seekers of diverse truths and pleasures: among them Dick Manchester, Russell Hulse, Peter McCulloch, Joel Weisberg, Thibault Damour, Dan Stinebring, students too numerous to name, and especially my dearly beloved wife, Marietta Bisson Taylor. |
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| **Physics\_1999-** | |
| **ID** | 0086 |
| **Biographical** | |  |  | | --- | --- | | **Born** |  | |  | August 1 1924 in Dabrovica, Poland Naturalized French citizen in 1946 | |  |  | | **Studies** |  | |  | Lycée Saint Louis in Paris | |  | Lycée de Montpellier | | 1945-1947 | Ecole des Mines (Mining school) in Paris | |  |  | | **Degrees** |  | | 1948 | Bachelor of Science. Mining engineer. | | 1954 | Ph. D. Physics. Experimental research in Nuclear Physics at College de France | |  |  | | **Positions** |  | | 1948-1959 | Centre National de la Recherche Scientifique (CNRS) | | 1959-1991 | Centre Européen pour la Recherche Nucléaire (CERN) | |  |  | | **Research** |  | | 1960 | Participated in the first exact measurement of the magnetic momentum of the muon | | 1961-1967 | Development of various types of nonphotographic scintillation chambers | | 1962-1967 | Nuclear structure studied by reactions (p+2p) | | 1968 | Introduction of proportional multiwire chambers | | 1974 | Introduction of spherical drift chambers for studies of proteins by X-ray diffraction (Orsay) | | 1979-1989 | Introduction of multistage avalanche chambers and application of photon counters for the imaging ionizing radiations | | 1985-1991 | Participated in experiments at Fermilab (USA). Introduction of chambers based on luminescent avalanches. Development of instrumentation for biological research using b-ray imaging (Centre Médical Universitaire de Genève. | |
| **Autobiographical** |  |
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| **Interview** |  |
| Q10 | **Professor Georges Charpak, welcome to Stockholm and to this Nobel interview. You are one of the very few French scientists who have been awarded a Nobel Prize. Why is that so?** |
|  | Georges Charpak: I know a lot of very good scientists in France and I don’t know the rules of the games, since it was a good surprise to have the Nobel Prize. And it so happened because what I did was important for the activity of other physicists who had had the Nobel Prize using the instruments they had made, but I don’t feel it is the most important thing I have done. |
| Q4 | **What is the most important thing you have done?** |
|  | Georges Charpak: I’ve worked on many detectors, some were very elegant and useless, and didn’t have a Nobel Prize, so this one was not the most elegant, but it was useful. |
| Q6 | **You have also written a biography, *My Name is Grisha*. Where does this name come from?** |
|  | Georges Charpak: I am born in Ukraine, it is now Ukraine, it was on the border. So I had a very agitated life at the beginning of my life, because I was born in a place where there had been ten years of ethnic fights and revolutionary fights. |
| Q3 | **So how come you got interested in science?** |
|  | Georges Charpak: I was not interested in science. I was interested in everything. I was reading Jules Verne, Alexander Dumas, even Lenin, I wanted to change the world. I was interested in the world. And France was a place where you have a kind of intellectual evolution between the two halves. You had the fascism coming up, the anti-fascists fighting … |
| Q6 | **Where were you during the war?** |
|  | Georges Charpak: I spent one year in jail, in the south of France, and one year in Dachau, which is a concentration camp … |

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| **Physics\_1999-** | |
| **ID** | 0087 |
| **Biographical** | P. G. de Gennes was born in Paris, France, in 1932. He majored from the École Normale in 1955. From 1955 to 1959, he was a research engineer at the Atomic Energy Center (Saclay), working mainly on neutron scattering and magnetism, with advice from A. Herpin, A. Abragam and J. Friedel (PhD 1957). During 1959 he was a postdoctoral visitor with C. Kittel at Berkeley, and then served for 27 months in the French Navy. In 1961, he became assistant professor in Orsay and soon started the Orsay group on *supraconductors*. Later, 1968, he switched to liquid crystals. In 1971, he became Professor at the Collège de France, and was a participant of STRASACOL (a joint action of Strasbourg, Saclay and College de France) on *polymer physics*.  From 1980, he became interested in interfacial problems, in particular the *dynamics of wetting*. Recently, he has been concerned with the physical chemistry of *adhesion*.  P.G. de Gennes has received the Holweck Prize from the joint French and British Physical Society; the Ampere Prize, French Academy of Science; the gold medal from the French CNRS; the Matteuci Medal, Italian Academy; the Harvey Prize, Israel; the Wolf Prize, Israel; The Lorentz Medal, Dutch Academy of Arts and Sciences; and polymer awards from both APS and ACS.  He is a member of the French Academy of Sciences, the Dutch Academy of Arts and Sciences, the Royal Society, the American Academy of Arts and Sciences, and the National Academy of Sciences, USA. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0088 |
| **Biographical** | I was born in Chicago, Illinois on March 28, 1930, the second of two children of Selig and Lillian Friedman, nee Warsaw, who were immigrants from Russia. My father came to the United States in 1913 and later served in the U.S. Army Artillery Corps in World War I. After the war he was employed by the Singer Sewing Machine Co. and later established his own business, repairing and selling used commercial and home sewing machines. My mother arrived in the United States in 1914 on one of the last voyages of the Lusitania. She supported herself until she was married by working in a garment factory. My parents had little formal education, except for courses in English after they arrived in the United States, but were self taught and had wide ranging interests. My father was an avid reader, having interests in science and political history, and our home was filled with books. My mother, who had a lovely singing voice, loved music and, in particular, opera. The education of my brother and myself was of paramount importance to my parents, and in addition to their strong encouragement, they were prepared to make any sacrifice to further our intellectual development. When there were financial difficulties they still managed to provide us with music and art lessons. They greatly respected scholarship in itself, but they also impressed upon us that there were great opportunities available for those who were well educated. I received my primary and secondary education in Chicago. As I very much liked to draw and paint as a child, I entered a special art program in high school, which was very much like being in an art school imbedded in a regular high school curriculum. While I always had some interest in science, I developed a strong interest in physics when I was in high school as a result of reading a short book entitled *Relativity*, by [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/index.html). It opened a new vista for me and deepened my curiosity about the physical world. Instead of accepting a scholarship to the Art Institute of Chicago Museum School and against the strong advice of my art teacher, I decided to continue my formal education and sought admission to the University of Chicago because of its excellent reputation and because [Enrico Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html) taught there. I was fortunate to have been accepted with a full scholarship. As my parents had limited means, my university training would not have been possible without such help. After finishing my requirements in an highly innovative and intellectually stimulating liberal arts program (established by Robert M. Hutchins who was then President of the University), I entered the Physics Department in 1950, receiving a Master’s degree in 1953 and a Ph.D. in 1956. It is difficult to convey the sense of excitement that pervaded the Department at that time. Fermi’s brilliance, his stimulating, crystal clear lectures that he gave in numerous seminars and courses, the outstanding faculty in the Department, the many notable physicists who frequently came to visit Fermi, and the pioneering investigations of pion proton scattering at the newly constructed cyclotron all combined to create an especially lively atmosphere. I was indeed fortunate to have seen the practice of physics carried out at its “very best” at such an early stage in my development. I also had the great privilege of being supervised by Fermi, and I can remember being overwhelmed with a sense of my good fortune to have been given the opportunity to work for this great man. It was a remarkably stimulating experience that shaped the way I think about physics. My thesis project was an investigation in nuclear emulsion of proton polarization produced in scattering from nuclei at cyclotron energies. The objective was to determine whether the polarization resulted from elastic or inelastic scattering. Professor Fermi tragically died in 1954 after a short illness. What an immense loss it was to all of us. My thesis work was not yet completed, and John Marshall kindly took over my supervision and signed my thesis. After I received my Ph.D., I continued working as a post-doc at the University of Chicago nuclear emulsion laboratory, which was then led by Valentine Telegdi. That year Val Telegdi and I did an emulsion experiment in which we searched for parity violation in muon decay. We were one of the first groups to observe this surprising effect which had been suggested by [T.D. Lee and C.N. Yang](https://www.nobelprize.org/nobel_prizes/physics/laureates/1957/index.html). Val was not only an excellent mentor but he was instrumental in getting me my first real job with Robert Hofstadter.  In 1957, I joined Hofstadter’s group at the High Energy Physics Laboratory at Stanford University as a Research Associate. This was where I learned counter physics and the techniques of electron scattering. While there I did a number of experiments studying elastic and inelastic electrondeuteron scattering. In an experiment to measure a weighted sum-rule for inelastic electron deuteron scattering which was related to the n-p interaction I had to confront the problem of making radiative corrections to inelastic spectra, and I developed a technique which proved to be valuable in my later work. Henry Kendall independently developed a similar technique and later we combined efforts to develop a radiative corrections program for our deep inelastic scattering work at SLAC. It was in Hofstadter’s group that I began my long collaboration with Henry Kendall who was also a member of the group. During this period I became acquainted with Richard Taylor, who was just finishing his thesis in another group, and with other future collaborators in the deep inelastic program at SLAC, Dave Coward and Hobey DeStacbler. One of the highlights of this period was attending the wonderfully informal and informative high energy physics seminars in the home of W.K.H. Panofsky, who was Director of the Laboratory.  In 1960, I was hired as a faculty member in the Physics Department of the Massachusetts Institute of Technology. When I arrived I joined David Ritson’s research group. A short time later he accepted a position at Stanford University and I inherited a small group. With these resources I soon began working on collaborative effort to measure muon pair production at the Cambridge Electron Accelerator (CEA) in order to test the validity of Quantum Electro-Dynamics. Henry Kendall joined my group in 1961 and we have been collaborators at MIT since that time. The last measurement we did at the CEA was a measurement of the deuteron form factor at the highest momentum transfers that could be reached at that accelerator to get some limits on the size of relativistic effects and meson currents.  In 1963, Henry Kendall and I started a collaboration with W.K.H. Panofsky, Richard Taylor and other physicists from the Stanford Linear Accelerator Center and the California Institute of Technology to develop electron scattering facilities for a physics program at the Stanford Linear Accelerator, a 20 GeV electron linac that was being constructed under the leadership of Panofsky. This required that we both travel between MIT and SLAC on a regular basis. The MIT Physics Department gave us special support by reducing our teaching responsibilities. We soon set up a small MIT group at SLAC and for extended periods of time one of us was always there. We had a rare opportunity. We were part of a group of physicists who were provided a new accelerator, given the support to design and construct optimal experimental facilities, and had the opportunity to participate in the exploration of a new energy range with electrons. From 1967 to about 1975 the MIT and SLAC groups carried out a series of measurements of inelastic electron scattering from the proton and neutron which provided the first direct evidence of the quark sub-structure of the nucleon. It was a very exciting time for all of us. This program is described in detail in the adjoining Physics Nobel Lectures.  As the program at SLAC was nearing completion we joined a collaborative effort at Fermilab involving a number of institutions to build a beam line and a single-arm spectrometer in the Meson Laboratory. During the latter half of the 1970’s this collaboration carried out a series of experiments to investigate elastic scattering, [Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html) scaling and production mechanisms in inclusive hadron scattering. When this work was completed, our group joined another collaboration to build a large neutrino detector at Fermilab. The objective of this program was to study the weak neutral currents in measurements of inclusive neutrino and anti-neutrino nucleon scattering, which were done in the first half of the 1980’s. These investigations confirmed the predictions of the Standard Model.  In 1980, I became Director of the Laboratory for Nuclear Science at MIT and then served as Head of the Physics Department from 1983 to 1988. During the time I was in these administrative positions I managed to maintain a foothold in research, which greatly eased my transition back to full-time teaching and research in 1988. While it was a very interesting period in my life, I was happy to get back to more direct contact with students in the classroom and in my research projects. Currently, our MIT group is participating in the construction of a large detector to study electron-positron annihilations at the Stanford Linear Collider and has also been engaged in design work for a detector for the Superconducting Super Collider, which is now under construction.  Over the years I have served on a number of program and scientific policy advisory committees at various accelerators. I also was a member of the Board of the University Research Association for six years, serving as Vice President for three years. I am currently a member of the High Energy Advisory Panel for the Department of Energy and also Chairman of the Scientific Policy Committee of the Superconducting Super Collider Laboratory.  Experimental high energy physics research is a group effort. I have been very fortunate to have had outstanding students and colleagues who have made invaluable contributions to the research with which I have been associated. I thank them not only for their contributions, but also for their friendship.  My life has been enhanced by my marriage to Tania Letetsky-Baranovsky who has broadened my horizons and has been an unfaltering source of support. She has endured with cheerful resignation my many absences when I have had to travel to distant particle accelerators. There are four grown children in our family, Ellena, Joel, Martin, and Sandra who pursue their activities in various parts of the country.  With regard to my non-vocational activities, in addition to getting much pleasure from various cultural activities, such as theater, music, ballet, etc., I enjoy painting and study Asian ceramics. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0089 |
| **Biographical** | I was born on December 9, 1926 in Boston, Massachusetts. My parents were Henry P. Kendall, a Boston businessman, and Evelyn Way Kendall, originally from Canada.  I lived in Boston until the early 1930s when the family – there were five, for by then I had a younger brother and a younger sister – moved to a small town outside Boston, where the three of us grew up and where I still live.  I went briefly to a local grade school but was held back by a reading disability which was cured after I was moved to a school some miles distant. From age 14 to 18, most of the period of World War II, I spent at Deerfield Academy, a college preparatory school. My academic work was poor for I was more interested in non-academic matters and was bored with school work. I had developed – or had been born with – an active curiosity and an intense interest in things mechanical, chemical and electrical and do not remember when I was not fascinated with them and devoted to their exploration. Father was a great encouragement in these projects except when they involved hazards, such as the point, at about age 11, when I embarked on the culture of pathogenic bacteria. He also instilled in both me and my brother a love and respect for the outdoors, especially the mountains and the sea.  I entered the US Merchant Marine Academy in the summer of 1945. I was there, in basic training, when the first atom bombs were exploded over Japan. I was unaware of the human side of these events and only recall a feeling that some of the last secrets of nature had been penetrated and that little would be left to explore. I spent the winter of 1945-46 on a troop transport on the North Atlantic (a most interesting experience), returning to the Academy for advanced training in the spring of 1946. I resigned in October, 1946, to start as a freshman at Amherst College. Although a mathematics major at college, my interest in physics was great and I did undergraduate research and a thesis in that field. But history, English and biology were all most attractive and there was a period, early on, when any one of these might have ended up as the major subject. Non-college enterprises, in the summers particularly, absorbed considerable time. I and a Deerfield friend became interested in diving and two summers were spent in organizing and running a small diving and salvage operation. We wrote our first books after that; one on shallow water diving, another on underwater photography, with a considerable success for both. These activities, mostly self-taught, were a good introduction to two skills very helpful in later experimental work: seeing projects through to successful conclusions and doing them safely.  On the urging of Karl Compton, a family friend and then President of MIT, I applied for, and was accepted at that institution’s school of physics in 1950. The years at graduate school were a continuing delight – the first sustained immersion in science at a full professional level. My thesis, carried out under the supervision of Martin Deutsch, was an attempt to measure the Lamb shift in positronium, a transient atom discovered by Deutsch a few years before. The attempt was unsuccessful but it served as a very interesting introduction to electromagnetic interactions and the power of the underlying theory.  The two years after receiving the PhD degree were spent as a National Science Foundation Postdoctoral Fellow at MIT and at Brookhaven National Laboratory, followed by a trip west to join the research group of Robert Hofstadter and the faculty of the Stanford University physics department. Hofstadter was engaged in the study of the proton and neutron structure that was later to bring him [the Nobel Prize](https://www.nobelprize.org/nobel_prizes/physics/laureates/1961/index.html), work that even at the time was clearly of the greatest interest and importance. The principal facility used in this research was a 300 ft. linear electron accelerator, a precursor to the 2 mile machine at the Stanford Linear Accelerator Center (SLAC), later built in the hills behind the University. Here I met and worked with Jerome Friedman, got to know Richard Taylor, then a graduate student in another group and W.K.H. Panofsky, the driving force behind SLAC. Friedman, Taylor and I were later to join in the long series of measurements on deep inelastic scattering at SLAC.  As in the college years, absorbing non-physics matters claimed a portion of my leisure time: mountaineering and mountain photography. Stanford and the San Francisco Bay area offered a number of skilled climbs as well as Yosemite Valley not far away. After two years of rock and mountain climbing, I was invited on the first-of several expeditions to the Andes. Later there have been trips to the Himalayas and the Arctic, with cameras of increasing size to capture some of the astonishing beauty of those remote places. Many of the friends made during those years have remained through life.  After five years at Stanford I moved back to MIT as a member of the faculty. Friedman had gone there a year earlier and we reestablished our collaboration. By 1964, the joint work with Taylor, by then a research group leader at SLAC, was initiated. This collaboration was surely the most enjoyable of any physics I have ever done. It was a pleasure shared by most people in the effort and well recognized at the time. All three of us have remained, up to the present, in the universities we were at then. I have been involved in research in later years, after the SLAC effort wound down in the middle 1970s, at the proton accelerator at Fermilab and since 1981, again at SLAC. The most interesting physics for me has always been the searches for new phenomena or new effects. With colleagues I have searched for limits to quantum electrodynamics, heavy electrons, parity breakdown in electron properties, and other such things. Unfortunately, the ever-growing size, scale, and duration of particle experiments, as well as the much larger collaborations, have made such programs less and less congenial to me over the years, circumstances that disturb many in the physics community.  At the start of the 1960s, troubled by the massive build-up of the superpower’s nuclear arsenals, I joined a group of academic scientists advising the U.S. Defense Department. The opportunity to observe the operation of the Defense establishment from the “inside,” both in the nuclear weapons area and in the counterinsurgency activities that later expanded to be the U.S. military involvement in South East Asia proved a valuable experience, helpful in later activities in the public domain. It was clear that changing unwise Government policies from inside, especially those the Government is deeply attached to, involves severe, often insurmountable, problems.  In 1969, I was one of a group founding the Union of Concerned Scientists (UCS), and have played a substantial role in its activities in the years hence. UCS is a public interest group, supported by funds raised from the general public, that presses for control of technologies which may be harmful or dangerous. The organization has had an important national role in the controversies over nuclear reactor safety, the wisdom of the US Strategic Defense Initiative, the B2 (Stealth) bomber, and the challenge posed by fossil fuel burning and possible greenhouse warming of the atmosphere, among others. I have been Chairman of the organization since 1974. The activities of the organization are part of a slowly growing interest among scientists to take more responsibility for helping society control the exceedingly powerful technologies that scientific research has spawned. It is hard to conclude that scientists are in the main responsible for the damage and risks that are now so apparent in such areas as environmental matters and nuclear armaments; these have been largely the consequence of governmental and industrial imperatives, both here and abroad. Yet it seems clear that without scientists’ participation in the public debates, the chances of great injury to all humanity is much enhanced. In my view, the scientific community has not participated in this effort at a level commensurate with the need, nor with the special responsibilities that scientists ineluctably have in this area.  This expenditure of effort and the sense of responsibility to help achieve control of aberrant technologies which drives it, stems in no small measure from the example set by my Father, who, throughout his life, spent a great deal of time and no small amount of energy on quiet, *pro bono* work. He was not alone among his own friends – nor among his own contemporaries – in this; it has been a tradition in New England of very long standing. In continuing to pursue such objectives, my expectation is that the challenges facing both me and the Union will be made substantially easier by the award of the Nobel Prize. This is perhaps the most attractive part of having gained this exceptional honor. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0090 |
| **Biographical** | Medicine Hat is a small town in Southwestern Alberta founded just over 100 years ago in a valley where the Canadian Pacific Railway crossed the South Saskatchewan River. I was born there on November 2, 1929 and raised in comfortable if somewhat Spartan circumstances. My father was the son of a Northern Irish carpenter and his Scottish wife who homesteaded on the Canadian prairies; my mother was an American, the daughter of Norwegian immigrants to the northern United States who moved to a farm in Alberta shortly after the first World War. During my early years our family of three was part of a large family clan headed by my Scottish grandmother. I attended schools named after English Generals and Royalty – Kitchener, Connaught, Alexandra.  Although I read quite a bit and found mathematics easy, I was not an outstanding student. In high school I did reasonably well in mathematics and science thanks to some talented and dedicated teachers.  I was nearly ten years old when World War II began. That conflict had a great effect on our town, and on me. In rapid succession the town found itself host to an R.A.F. flight training school, a prisoner of war camp and a military research establishment. The wartime glamor of the military, the sudden infusion of groups of sophisticated and highly-educated people, and new cultural opportunities (the first live symphonic music I ever heard was played by German prisoners of war) all transformed our town and widened the horizons of the young people there. I developed an interest in explosives and blew three fingers off my left hand just before hostilities ended in Europe. The atomic bomb that ended the war later that summer made me intensely aware of physicists and physics.  Higher education was highly prized in the society of a small prairie town and I was expected to continue on to university. After some difficulties over low grades in some high school subjects, I was admitted to the University of Alberta in Edmonton. I registered in a special program emphasizing mathematics and physics and gradually became interested in experimental physics, continuing my studies towards a Masters degree at the same institution. My thesis research was a rather primitive effort to measure double b-decay in an aging Wilson cloud chamber. Between sessions at the University, I spent two summers as a research assistant at the Defense Research Board installation near Medicine Hat working with Dr. E.J. Wiggins, who encouraged me to continue my studies either in eastern Canada or in the United States.  Those were interesting years, and during this time I met, courted and married Rita Bonneau – a partnership which has enriched my life in every way. Together we decided to try California, and I was accepted into the graduate program at Stanford, while she found work teaching in a military school in order to support us both. The first two years at Stanford were exciting beyond description – the Physics Department at Stanford included [Felix Bloch](https://www.nobelprize.org/nobel_prizes/physics/laureates/1952/index.html), Leonard Schiff, [Willis Lamb](https://www.nobelprize.org/nobel_prizes/physics/laureates/1955/index.html), [Robert Hofstadter](https://www.nobelprize.org/nobel_prizes/physics/laureates/1961/index.html), and W.K.H. (Pief) Panofsky who had just arrived from Berkeley. I found that I had to work hard to keep up with my fellow students, but learning physics was great fun in those surroundings. At the end of the second year I joined the High Energy Physics Laboratory where the new linear accelerator was just beginning to do experiments. My thesis work was accomplished there under Prof. Robert F. Mozley, on a rather difficult experiment producing polarized g-rays from the accelerator beam and then using those g-rays to study p-meson production.  In 1958 I was invited to join a group of physicists at the École Normale Supérieure in Paris who were planning experiments at an accelerator (similar to the linac at Stanford) which was under construction in Orsay. I stayed in France for about three years working on the experimental facilities for the accelerator, and then participated in some electron scattering experiments. My wife began a new career there as a librarian at the Orsay laboratory, a career which was interrupted for a while when our son, Ted, was born in 1960. We returned to the United States in 1961 but a continuing connection to French physics and physicists has been a significant element in my life since that time – including a Doctorate (Honoris Causa) very kindly conferred upon me in 1980 by the Université de Paris-Sud.  Upon our return to the United States, I joined the staff of the Lawrence Berkeley Laboratory at the University of California. After less than a year in Berkeley, I moved back to Stanford where work on the construction of Stanford Linear Accelerator Center (SLAC) was just beginning. At SLAC, I started working on the design of the experimental areas for the new accelerator. By 1963 I had joined the group considering the requirements for electron scattering apparatus in the larger of two experimental areas. I worked closely with Pief Panofsky, and with collaborators from the California Institute of Technology and the Massachusetts Institute of Technology. I spent the next decade helping to build equipment and taking part in various electron scattering experiments, a number of which are the subject of the 1990 Nobel lectures. This was a period of intense activity, but also one of intense enjoyment for me. I was surrounded by people I liked and admired, and deeply involved in experiments which generated interest in laboratories and universities all over the world. I count myself extremely fortunate to have been at SLAC at that time.  I became a member of the SLAC faculty in 1968. In 1971, I was awarded a Guggenheim fellowship and spent an interesting sabbatical year at CERN, where I was impressed by the great progress that European science had made in the decade since I had worked in France.  Well before my trip to CERN, colleagues in the group at SLAC had become interested in testing some of the invariance properties of the electromagnetic interaction, a field which would absorb our efforts for most of the 1970s. When Charles Prescott joined the group in 1970, he began a serious study of ways to test parity conservation in the interaction between an electron and a nucleon. The electroweak theories of [Weinberg and Salam](https://www.nobelprize.org/nobel_prizes/physics/laureates/1979/index.html) predicted levels of nonconservation that looked extremely hard to measure. We attempted an experiment with the existing Yale polarized source, but the measurements did not reach the desired level of sensitivity. I was not very encouraging to my colleagues who wished to pursue the experiment to higher levels of accuracy. After the theoretical work of Veltman and van’t Hooft and the discovery of neutral currents at CERN (during the year I was there) and at NAL (now Fermilab), the interest in experiments on parity conservation greatly intensified. In 1975 a new method for producing polarized electrons was discovered by a group in Colorado which included E.L. Garwin of SLAC. In 1978, after building a source for the linac based on the new method, we were able to demonstrate a violation of parity in close agreement with the electroweak predictions.  After the parity experiments, our group presented two proposals for large experimental facilities at PEP, the *e*+*e*– collider then being built at SLAC. Both those proposals were rejected. The group was finally successful in proposing a relatively small PEP detector, but I did not take part in that experiment.  In 1981, I received an Alexander von Humboldt award which allowed me to spend most of the 1981-82 academic year at DESY in Hamburg. In 1982 I returned to SLAC as Associate Director for Research, a post I held until 1986 when I resigned to return to research. Since that time I have spent quite a bit of time in Europe and I am presently playing a very small role in the H1 detector preparations at HERA. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0091 |
| **Biographical** | I was born August 27, 1915 in Washington, D.C. My mother, daughter of German immigrants, had been a mathematics instructor at the University of Kansas. My father, descended from Scottish refugees and a West Point graduate, was an officer in the Army Ordnance Corps. His frequently changing assignments took us from Washington, DC to Topeka, Kansas, to Paris, France, to Picatinny Arsenal near Dover, New Jersey, and to Fort Leavenworth, Kansas. With two of the moves I skipped a grade and, encouraged by my supportive parents and teachers, I graduated from high school with a high academic record at the age of 15.  My early interest in science was stimulated by reading an article on the quantum theory of the atom. But at that time I did not realize that physics could be a profession. My parents presumed that I would try to follow my father’s footsteps to West Point, but I was too young to be admitted there. I was offered a scholarship to Kansas University but my parents again moved – this time to New York City. Thus I entered Columbia College in 1931, during the great depression. Though I started in engineering, I soon learned that I wanted a deeper understanding of nature than was then expected of engineers so I shifted to mathematics. By winning yearly competitive mathematics contests, I was honored in my senior year by being given the mathematics teaching assistantship normally reserved for graduate students. At the time I graduated from Columbia in 1935, I discovered that physics was a possible profession and was the field that most excited my curiosity and interest.  Columbia gave me a Kellett Fellowship to Cambridge University, England, where I enrolled as a physics undergraduate. The Cavendish Laboratory in Cambridge was then an exciting world center for physics with a stellar array of physicists: J.J. Thomson, Rutherford, Chadwick, Cockcroft, Eddington, Appleton, Born, Fowler, Bullard, Goldhaber and Dirac. An essay I wrote at Cambridge for my tutor, Maurice Goldhaber, first stimulated my interest in molecular beams and in the possibility of later doing my Ph. D. research with I.I. Rabi at Columbia.  After receiving from Cambridge my second bachelors degree, I therefore returned to Columbia to do research with Rabi. At the time I arrived Rabi was rather discouraged about the future of molecular beam research, but this discouragement soon vanished when he invented the molecular beam magnetic resonance method which became a potent source for new fundamental discoveries in physics. This invention gave me the unique opportunity to be the first graduate student to work with Rabi and his associates, Zacharias, Kellogg, Millman and Kusch, in the new field of magnetic resonance and to share in the discovery of the deuteron quadrupole moment.  Following the completion of my Columbia thesis, I went to Washington, D.C. as a Carnegie Institution Fellow, where I studied neutron-proton and proton-helium scattering.  In the summer of 1940 I married Elinor Jameson of Brooklyn, New York, and we went to the University of Illinois with the expectation of spending the rest of our lives there, but our stay was short lived. World War II was rampant in Europe and within a few weeks we left for the MIT Radiation Laboratory. During the next two years I headed the group developing radar at 3 cm wavelength and then went to Washington as a radar consultant to the Secretary of War. In 1943 we went to Los Alamos, New Mexico, to work on the Manhattan Project.  As soon as the war ended I eagerly returned to Columbia University as a professor and research scientist. Rabi and I immediately set out to revive the molecular beam laboratory which had been abandoned during the war. My first graduate student, William Nierenberg, and I measured a number of nuclear magnetic dipole and electric quadrupole moments and Rabi and I started two other students, Nafe and Nelson, on a fundamental experiment to measure accurately the atomic hydrogen hyperfine separation. During this period Rabi and I also initiated the actions that led to the establishment of the Brookhaven National Laboratory on Long Island, New York, where in 1946 I became the first head of the Physics Department.  In 1947 I moved to Harvard University where I taught for 40 years except for visiting professorships at Middlebury College, Oxford University, Mt. Holyoke College and the University of Virginia. At Harvard I established a molecular beam laboratory with the intent of doing accurate molecular beam magnetic resonance experiments, but I had difficulty in obtaining magnetic fields of the required uniformity. Inspired by this failure, I invented the separated oscillatory field method which permitted us to achieve the desired accuracy with the available magnets. My graduate students and I then used this method to measure in many different molecules a number of molecular and nuclear properties including nuclear spins, nuclear magnetic dipole and electric quadrupole moments, rotational magnetic moments of molecules, spin-rotational interactions, spin-spin interactions, electron distributions in molecules, etc. Although we studied a wide variety of molecules we concentrated on the diatomic molecules of the hydrogen isotopes since these molecules were most suitable for comparing theory and experiment. During this period I also consulted with various groups that were applying the separated oscillatory field method to atomic clocks and I analyzed the precautions which must be taken to avoid errors. Although our original molecular beam research was only with the magnetic resonance method, we later built a separated oscillatory fields electric resonance apparatus and used it to study polar molecules.  In an effort to attain even greater accuracy and to do so with atomic hydrogen, the simplest fundamental atom, Daniel Kleppner, a former student, and I invented the atomic hydrogen maser. We then used it for accurate measurements of the hyperfine separations of atomic hydrogen, deuterium and tritium and for determining the extent to which the hyperfine structure was modified by the application of external electric and magnetic fields. We also participated with Robert Vessot and others in converting a hydrogen maser to a clock of unprecedented stability.  While these experiments were being carried out with some of my graduate students, I worked with other students and associates to apply similar precision methods to beams of polarized neutrons. At the Institut Laue-Langevin in Grenoble, France, we measured accurately the magnetic moment of the neutron, set a low limit to the electric dipole moment of the neutron as a test of time reversal symmetry and discovered and measured the parity non-conserving rotations of the spins of neutrons passing through various materials.  Concurrently with my molecular and neutron beam research, I was also teaching and involved with other scientific activities. I was director of the Harvard Cyclotron during its construction and early operation and participated in proton-proton scattering experiments with that cyclotron. I was later chairman of the joint Harvard-MIT committee managing the construction of the 6 GeV Cambridge Electron Accelerator and used that device for various particle physics experiments including electron-proton scattering. For a year and a half I was on leave from Harvard as the first Assistant Secretary General for Science (Science Advisor) in NATO where I initiated the NATO programs for Advanced Study Institutes, Fellowships and Research Grants. For sixteen exciting years I was on leave half time from Harvard as President of Universities Research Association which exercised its management responsibilities for the construction and operation of the Fermilab accelerator through two outstanding laboratory directors, Robert R. Wilson and Leon Lederman.  Although I am primarily an experimental physicist, theoretical physics is my hobby and I have published several theoretical papers including early discussions of parity and time reversal symmetry, the first successful theory of the NMR chemical shifts, theories of nuclear interactions in molecules and the theory of thermodynamics and statistical mechanics at negative absolute temperatures.  I officially retired from Harvard in 1986, but I have remained active in physics. For one year I was a research fellow at the Joint Institute for Laboratory Astrophysics at the University of Colorado and I now periodically revisit JILA as an Adjunct Research Fellow. Subsequent to our year in Colorado, I have been visiting professors at The University of Chicago, Williams College and the University of Michigan. I continue writing and theoretical calculations in my Harvard office and with my collaborators we are continuing our neutron experiments at Grenoble.  After Elinor died in 1983, I married Ellie Welch of Brookline, Massachusetts and we now have a combined family of seven children and six grandchildren. We enjoy downhill and cross country skiing, hiking, bicycling and trekking as well as musical and cultural events.  I have greatly enjoyed my years as a teacher and research physicist and continue to do so. The research collaborations and close friendships with my eighty-four graduate students have given me especially great pleasure. I hope they have learned as much from me as I have from them. |
| **Autobiographical** |  |
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| **Interview** |  |
| Q3 | **Why do you want to continue? (working)** |
|  | Norman F. Ramsey: Because it’s very interesting. I want to know the answers. We’re trying to make various investigations. We’re studying symmetry of the neutron and looking for an electric dipole moment to the extent to which it may or may not be shaped like an American football or like a sphere. And it’s a very interesting thing; I want to know. And we in theory have been sort of competing on those theories of whether there should be one there, or shouldn’t be one there. Now we’re convinced that there should be one there, but we haven’t seen it yet. But that’s a puzzle for them. |
| Q5 | **That’s fantastic. The people you’re working with, are they former students of yours?** |
|  | Norman F. Ramsey: Yes, some are former students. I’ve been working particularly with a somewhat international group at Grenoble, France. I would say Michael Pendlebury is one of the principle and Philip Harris and some of the people from Grenoble. I started the experiments with graduate students. |
| Q4 | **Is it that you pick up some information?** |
|  | Norman F. Ramsey: That’s right. I mean the very first experiment I did and that was looking disparity, which is symmetry, whether nature would know the difference between left and right handed. And everybody said ‘It won’t know the difference’. But I said ‘Well, it’s still worth doing a test’. We didn’t find it, but it turns out there was a failure of parity. That’s a good test to do. |
| Q3 | **What was it in your childhood maybe that made you want to become a scientist?** |
|  | Norman F. Ramsey: I don’t know. I mean at the time I was a child I didn’t know too much about science is a thing you, a career you go in to, but on the other hand one of my favourite magazines was *Popular Mechanics*. And I used to make up some of the things that they did there and read with interest what was then the frontier of science as it was then reported. And it fascinated me, but at the time I was in college, even physics wasn’t really recognised as a subject, so it was only after I graduated from college that I shifted to physics actually as opposed … well for a while I was in mathematics, which I enjoyed very much. But it was always, my basic interest was always there. I mean physical things have always interested me. I was curious and still am. |
| Q9 | **What do you think when you were rewarded the Nobel Prize, 1989, had you expected it?** |
|  | Norman F. Ramsey: No I had not. Almost every year a group of us would wonder not whether I’m going to get the prize, but whether who’s going to get the prize. But that particular year 1989 my wife and I had been on a mountain trip in the Himalayas. We took a trip from Kashmir to Ladakh passing over a couple of passes at 17,000 feet high, which was higher than any mountain I’d been On, much less any pass, and it was a fascinating trip. We didn’t speculate about the prize from here. I got back home and even then I had a little bit of learning about it because we’d been away the previous year actually visiting a professor. And we kept the telephone, but it was turned out this company only kept the telephone in my wife’s name not my name. So the chairman of the committee couldn’t find me. |
| Q9 | **How long a time did it take?** |
|  | Norman F. Ramsey: They released it to the newspapers at seven and the New York Times science editor found me immediately. I mean he knew where I was. But I learned later that they had even made a mistake. They had guessed maybe I might be in Washington DC. There was a Norman Ramsey there. And the chairman of the committee called him and called that number and asked if the young man who answered it his father was there. And he said ‘Yes he is’. ‘Well we’d like to speak to him’. He said ‘But well my father’s sound asleep. It’s now six a.m.’ and they said ‘We want to tell him he’s received half the Nobel Prize in Physics’. And this young man said ‘That’s very interesting since my father’s an economist’. |
| Q9 | **So what was the feeling then when you got it? Were you really happy?** |
|  | Norman F. Ramsey: I was delighted. My first reaction even after I got the call from the New York Times, my first questioning was, he told me ‘What do you think about getting half the Nobel Prize in Physics’. I said ‘That’s wonderful, but are you sure?’ But then he named the other people who were getting it that year and they were very good. I was delighted to be in their company and that made it sound as if it was a real thing. |
| Q6 | **Could you tell us a little bit about you know what is has been and what you see in the future?** |
|  | Norman F. Ramsey: It has had really many, many more than I would have expected at the time I was doing the work, which was work on magnetic resonance. Magnetic resonance was a brand new thing before even I did the work because before I received the prize I was working with [I I Rabi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/rabi-facts.html) at Columbia. And had the very good luck of starting to work with him about two months before he invented the magnetic resonance method, which was a new thing. Immediately I and several others started working on it and the first experiments worked. Then the second experiment didn’t really seem to work right and you know you’re always disappointed when it doesn’t work right. But usually that’s more exciting because then it means there’s something different we found. Yes we were also looking to interactions in molecules, which was not what we were looking for originally. And so in addition to measuring the magnetic moment of the nucleus, we were also measuring the interactions that was in the molecules. So the chemists were almost immediately very interested in it.  And since then it has steadily expanded other things, part of which I have been involved in and part of which other people have been involved in. I mean one of the ones that turned out, because of the interaction in the molecule, particularly in the case of Adams, it’s a very sort of constant of nature. And it’s determined by quantum mechanics; it’s a very interesting thing. Quantum mechanics has things are fixed. You can be sure what they are. So quantum mechanically you could be sure that this constant would stay fixed, for that mean measuring that is a very good basis for atomic clocks because you need for that, and it’s really even different in principle for more previous clocks. I mean previous clocks like pendulum clocks; it depends on the mechanic who makes the pendulum. Does he make it the right length?  And in the case of atomic clocks, it’s nature who makes the device that determines the time, so that it’s universally you can be sure, unless we make a terrible mistake. If we make an atomic clock in the United States and someone makes it in England, it better be the same within our experimental area. And then we’ve steadily improved it. So that turned out, it makes that time determine that way rather more fun in the middle you know. And secondly it also is true that you could measure it much more accurately. So it’s both happy circumstance that the most accurate measurement, so that for example the second, the unitive time of the second is now determined, is defined in terms of oscillation and a caesium atom, which we were concerned with at one time.  I think it’s a rapidly moving field now. It looks as there will be even better definitions of a second. That atom was accurate to about what we call one part in 10 to the 15th. That’s one part in one followed by 15 zeros. So it’s very accurate. But there are things for which you need even greater accuracy. It’s the best test of the theory of relativity are now done with atomic clocks. And you can make extreme tests for that. Radio astronomy is now a very powerful tool, but to make the telescopes have good resolution, you want one to be in one side of the earth and the other on the other side of the earth. But you have to have good clocks to match the two. So there are many, many applications for it. |
| Q4 | **GPS is another area?** |
|  | Norman F. Ramsey: GPS, everybody who buys for $100 a GPS receiver doesn’t actually get an atomic clock. He gets a good crystal clock. But the satellites which are going around which give the signals that he synchronises to, those are atomic clocks. And then particularly the central stations, which keep the time for the whole system, are atomic clocks. |
| Q2 | **Maybe that’s one of, as you said, you know the reason you’re still so curious, is that what you need to have, the curiosity?** |
|  | Norman F. Ramsey: That’s right. You need to be very curious and willing to work hard, but also think freshly I mean. |
| Q2 | **What are you doing then if something doesn’t work?** |
|  | Norman F. Ramsey: That’s one of the things I talked about. I think all of us who do scientific research, almost all of us have our successes and our failures. And the key difference between the people I think who on the whole succeed and the others is what happens after you’ve had one of your failures? I mean you think you have a good thing, an important thing to look for and well it can’t be there; it isn’t there, you can’t find it. It’s a disappointment. Or somebody else does the experiment first, that’s a sad thing too. But the people who are really good in the field, say ‘Fine, I guess we lost that one. We’ll try something different we hope that’s even better’. |
| Q14 | **So the scope for doing new beautiful discoveries are still out there obviously?** |
|  | Norman F. Ramsey: They’re still out there. They will be different in nature. I mean yes, no one will ever discover again that at least in the energy levels we’re doing with if the electrons circulate around a nucleus or something like that. That was done by our people, but how it does it? For example, when I decided to do experiments of [Rutherford](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1908/rutherford-facts.html) and others who found that was the motion. It wasn’t quantise. Now we know that you have to do with quantum mechanics. It’s probably different mechanics in fact and that’s developed in more recent years. And we have the interesting time I think in physics now that in some respects we can account for an amazing amount of material. I mean our ordinary universe that we see at normal energies, at normal accuracies, we really understand quite thoroughly. And it requires quantum mechanics to do this, but it works. |
| Q18 | **That’s fascinating. My last question would be, do you believe that scientists have any special responsibility, particularly scientists who have received a Nobel Prize, for bringing messages of importance to politicians and decision makers?** |
|  | Norman F. Ramsey: I think they do and I agree with you that probably specially Nobel Prize, a number of us do that. Unfortunately, sometimes they are not so responsive and I am afraid at the present moment our government is not as responsive as it was a few years ago, but I hope that will change. But I still think it’s still important to do. This is true for example the things on the environment. I mean it’s very clear that the atmosphere is heating up and that at least a significant factor of that is due to people. The problem of CO2 in the atmosphere is important, but some of the officials in our government don’t recognise it. So even though they have scientific committees that call this to attention and say it is something we should do, and somehow they sort of get some other advisers. The trouble is you can always hire somebody who can give you advice in a different direction if you want. And then they tend to modify their reports in that direction. So there are real problems in that regard. But the answer is yes, we should and I have. I mean actually in my case I also felt very strongly about not getting involved in the Iraq war, but that didn’t affect the government’s position. |

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| **Physics\_1999-** | |
| **ID** | 0092 |
| **Biographical** | My father, Georg, had studied law at the Universität Berlin for some years, and in the first World War had been an artillery officer. He was of a philosophical bend of mind and a man of independent opinions. In the depth of the depression he just managed to make a living in real estate. When the family fortunes had shrunk to ownership of a heavily mortgaged apartment building located in an overwhelmingly Communist part of Berlin, it seemed reasonable to move into one of the apartments ourselves as nobody paid any rent. Cannons were deployed on the streets on occasion and the class war had entered the class rooms. After a few bloody noses administered by a burly repeater, I shifted my interests from roaming the streets more towards playing with rudimentary radio receivers and noisy and smelly experiments in my mother’s kitchen. In the spring of 1933 my mother, a very energetic lady, saw to it that, at the age of ten, I entered the Gymnasium zum Grauen Kloster, the oldest Latin school in Berlin, which counted Bismarck amongst its Alumni. This involved a stiff entrance examination and I was admitted on a scholarship. My father at that time expressed the opinion that I probably would be happier as a plumber. However, he apparently didn’t quite believe this himself. Thus, in years before, he had bought me an erector set and books on the lives of famous inventors and Greek mythology, and when I was ill he had given me the encyclopedia to read. I supplemented the school curriculum with do-it-yourself radio projects until I had hardly any time left for my class work. Only tutoring from my father rescued me from disaster. Reading popular radio books deepened my interest in physics. While physics was taught at the Kloster only in the later grades, in the public library I read books with titles such as “Umsturz im Weltbild der Physik” and learned about the Balmer series and Bohr’s energy levels of the hydrogen atom. My teachers at the Kloster were excellent, I remember in particular Dr. Richter, who taught Latin and Greek, and Dr. Splettstoesser, who taught biology and physics. Richter liked to expand on the classical works, which we were reading in class. I spent most of the ample breaks in related intense discussions with a group of classmates, Heppke, Hubner, Landau and Leiser while others engaged in boxing matches. Splettstoesser was a working scientist who spent Summers as a visitor with a marine biology institute on the Adriatic. I jumped a term and graduated in the spring of 1940.  Having received a notice from the draft board, I found it wise to volunteer for the anti-aircraft artillery and a motorized unit. I was not able to serve as a radio man but was assigned to a gun crew and never rose above the rank of senior private. Sent to relieve the German armies at Stalingrad, my battery was extremely lucky to escape the encirclement. A few months later I was even more lucky to be ordered back to Germany to study physics under an army program at the Universität Breslau in 1943. After one year of study, I was sent to the Western Front and captured in the Battle of the Bulge. I spent a year in an American prisoner of war camp in France and was released early in 1946. Supporting myself with the repair and barter of prewar radios, I took up my study of physics again at the Universität Göttingen. Here I attended lectures by Pohl, Richard Becker, Hans Kopfermann and Werner Heisenberg; Max v. Laue and Max Planck attended the physics colloquia. At the funeral of Planck I was chosen to be one of the pall bearers. At the university, I greatly enjoyed repeating the Frank-Hertz experiment, the Millikan oil drop, Zeemann effect, Hull’s magnetron, Langmuir’s plasma tube and other classic modern physics experiments in an excellent laboratory class run by Wolfgang Paul. In one of his Electricity & Magnetism classes Becker drew a dot on the blackboard and declared “Here is an electron…” Having heard in another class that the wave function of an electron at rest spreads out over all of space, and having read about ion trapping in radio tubes in my teens set me to wonder how one might realize Becker’s localization feat in the laboratory. However, that had to wait a while. In 1948, in Kopfermann’s Institute, which was heavily oriented towards hyperfine structure studies, I completed an experimental Diplom-Arbeit (master’s thesis) on a Thomson mass spectrograph under Peter Brix. The results were published in “Die photographischen Wirkungen mittelschneller Protonen II,” the first paper of which I was a (co)author. Soon thereafter, I began work on my doctoral thesis under Hubert Kruger in the same Institute. Well prepared by a series of excellent Institute seminars on the NMR work of Bloch and of Purcell, we were able to successfully compete with workers at Harvard University. In 1949 we discovered Nuclear Quadrupole Resonance and reported it in our paper “Kernquadrupolfrequenzen in festem Dichloraethylen.” My doctoral thesis had the title “Kernquadrupolfrequenzen in kristallinen Jodverbindungen.” This work led to an invitation to join Walter Gordy’s well known microwave laboratory at Duke University as postdoctoral associate.  At Duke I had the pleasure of making the acquaintance of James Frank, Fritz London, Lothar Nordheim and Hertha Sponer. I advised Hugh Robinson, a graduate student of Gordy’s in an NQR experiment, did my own research and also contributed some NMR expertise to an experiment by Bill Fairbank and Gordy on spin statistics in 3He/4He mixtures, gaining some very useful low temperature experience in this brief collaboration. Through Gordy’s and Nordheim’s good offices I was able to receive a visiting assistant professor appointment at the University of Washington with a charge to advise Edwin Uehling’s students during his sabbatical and to do independent research. I had built my first electron impact tube during a brief interlude in 1955 in George Volkoffs laboratory at the University of British Columbia. Prior to that I had attempted a paramagnetic resonance experiment on free atoms in Gottingen and succeeded in doing so at Duke. During seminars at Göttingen on the magnetic resonance techniques of Rabi and of Kastler, it had occurred to me that because of the analogy between an atom and a radio dipole antenna, (a), a*lignment* of the atom should show up in its optical absorption cross section, and (b), electron impact should produce *aligned* excited atoms. I put these two ideas to good use in 1956 in Seattle in an experiment entitled “Paramagnetic Resonance Reorientation of Atoms and Ions Aligned by Electron Impact.” In this paper I first pointed out the usefulness of *ion trapping for high resolution spectroscopy* and mentioned the 1923 Kingdon trap as a suitable device. This work also brought me into close contact with spin exchange between electron and target atom, which gave me the idea for my 1958 experiment “Spin Resonance of Free Electrons Polarized by Exchange Collisions.” However, first I had to learn how to produce polarized atoms, which could then transfer their orientation to trapped electrons. Falling back on buffer gas techniques developed in my 1955 Duke paper “Atomic Phosphorus Paramagnetic Resonance Experiment,” I quickly demonstrated in my 1956 Seattle paper “Slow Spin Relaxation of Optically Polarized Sodium Atoms” how to efficiently produce and monitor a polarized atom cloud. Trapping the electrons in a neutralizing ion cloud slowly diffusing in the buffer gas, I was able to carry out the spin resonance experiment. My optical transmission monitoring scheme proved also very useful in the development of rubidium vapor magnetometers and frequency standards by Earl Bell and Arnold Bloom at Varian Associates, in which I acted as a consultant. The rubidium frequency standard is still the least expensive, smallest and most widely used commercial atomic frequency standard. The thesis “Experimental Upper Limit for the Permanent Electric Dipole Moment of Rb85 by Optical Pumping Techniques” of my first graduate student, Earl Ensberg, also made use of these novel optical pumping schemes and was finished in 1962. These early results were improved orders of magnitude by my doctoral student Philip Ekstrom in his 1971 thesis “Search for Differential Linear Stark Shift in Cs133 and Rb85 Using Atomic Light Modulation Oscillators.”  I was not satisfied with the plasma trapping scheme used for the electrons and asked my student, Keith Jefferts, to study ion trapping in an electron beam traversing a field free vacuum space between two grids. Also, I began to focus on the magnetron/Penning discharge geometry, which, in the Penning ion gauge, had caught my interest already at Göttingen and at Duke. In their 1955 cyclotron resonance work on photoelectrons in vacuum Franken and Liebes had reported undesirable frequency shifts caused by accidental electron trapping. Their analysis made me realize that in a pure electric quadrupole field the shift would not depend on the location of the electron in the trap. This is an important advantage over many other traps that I decided to exploit. A magnetron trap of this type had been briefly discussed in J.R. Pierce’s 1949 book, and I developed a simple description of the axial, magnetron, and cyclotron motions of an electron in it. With the help of the expert glassblower of the Department, Jake Jonson, I built my first high vacuum magnetron trap in 1959 and was soon able to trap electrons for about 10 sec and to detect axial, magnetron and cyclotron resonances. About the same time, my Göttinger colleague, Otto Osberghaus, sent me a research report on the Paul rf ion cage. This trap had very desirable properties for atomic ions and it did not require a magnetic field. Therefore, I asked my student, Fouad Major, to experiment with a simplified cylindrical version of such a trap in the hope that it might be useful in hfs resonance experiments on hydrogenic helium ions. The early results were very encouraging and Jefferts also switched to the Paul trap. In 1962, Jefferts and Major both finished their Doctoral Theses entitled respectively “Alignment of Trapped H2+ Molecular Ions by Selective Photodissociation” and “The Orientation of Electrodynamically Contained He4 Ions.” As a continuation of the latter, a new postdoc, Norval Fortson, Major and I published the 1966 paper “Ultrahigh Resolution DF=0 ± 13He+ HFS Spectra by an Ion Storage-Exchange Collision Technique.” My own attempts to detect the polarization of the electrons acquired from a polarized beam of alkali atoms in my Penning (magnetron) trap, described in a 1961 research report to the NSF “Spin Resonance of Free Electrons,” were not so quickly successful. However in this work I was much impressed by seeing the beam of sodium atoms traversing my glass apparatus in the reflected light from a sodium vapor street lamp adapted as illuminating light source. Only a later concerted effort by Gräff and Werth at Bonn, reinforced by Major and Fortson, as visitors, made a similar spin resonance experiment work in 1968.  In the 1966 paper with Fortson and Major, I also proposed to develop an infrared laser based on ions in an rf trap. To this end my student, David Church, completed a thesis in 1969 entitled “Storage and Radiative Cooling of Light Ion Gases in RF Quadrupole Traps.” In this work we demonstrated a race-track-shaped trap and cooled the ions by coupling to a resonant LC circuit. In parallel work my student, Stephan Menasian, in 1968, with some help from G.R. Huggett, succeded in cooling Hg+ ions in a race-track-trap with a helium buffer gas and in detecting them by optical absorption. Jefferts’ research on hfs spectra of H2+ was continued in Seattle by my postdoc Charles Richardson and later by Menasian in his 1973 doctoral thesis “High Resolution Study of the (1, 1/2, 1/2) – (1, 1/2,3/2) HFS Transition in H2+.” The resolution in the 3He+ hfs work was greatly enhanced in work with my colleague Fortson and my postdoc Hans Schuessler. Realizing in 1961 that precision measurements of the electron magnetic moment would require a large magnetic field and that Becker’s electron localization feat might be approximated in a Penning trap, I began to consider other avenues for magnetic resonance experiments. Some success in the electron work, achieved with the help of my new student, Fred Walls, was described in our 1968 paper “‘Bolometric’ Technique for the RF Spectroscopy of Stored Ions.” I reviewed the work on ions and electrons up to 1968 in two articles “Radiofrequency Spectroscopy of Stored Ions.”  The able assistance of two postdocs, [David Wineland](https://www.nobelprize.org/nobel_prizes/physics/laureates/2012/) and my former student Phil Ekstrom, made the isolation of a single electron become a reality in 1973 with our paper “Monoelectron Oscillator.” Measuring its magnetic moment was another story. At Göttingen in the late forties I had attended a seminar given by Helmut Friedburg, a doctoral Student of Wolfgang Paul, on focussing spins with a magnetic hexapole. This may be viewed as a refinement of the Stern-Gerlach effect. In subsequent discussions with fellow students a rumor of a Stern-Gerlach experiment for electrons was brought up, and also Bohr’s and Pauli’s thesis that such experiments were impossible in principle. Though it greatly piqued my interest, I could not understand this thesis. Stimulated by a 1927 paper of Brillouin on the subject, I followed another of the guiding principles formulated by Bohr: “In my Institute we take nothing absolutely serious, including this statement.” In 1973 I proposed, together with Ekstrom, to monitor spin and cyclotron quantum numbers of the lone electron by means of the “continuous Stern-Gerlach effect” in an abstract “Proposed g-2/dvz Experiment on Stored Single Electron or Positron.” My new postdoc Robert Van Dyck, Philip Ekstrom and myself reported the first such experiment in our 1976 paper “Axial, Magnetron, and Spin-Cyclotron Beat Frequencies Measured on Single Electron Almost at Rest in Free Space (Geonium).” This work also already made use of the important technique of side band cooling of the electron. The demonstration of sideband cooling had eluded us in earlier attempts undertaken together with Walls and later with Wineland. Encouraged by the success of the monoelectron oscillator I had also published in 1973 an abstract “Proposed 1014D*v* v Laser Fluorescence Spectroscopy on Tl+ Mono-Ion Oscillator.” Unfortunately, this proposal infuriated one of the agencies funding our research to the degree that they terminated their support almost immediately. I was rescued by a prize from the Humboldt Foundation and an invitation by Gisbert zu Putlitz to initiate the proposed laser spectroscopy project in his Institute at the Universität Heidelberg. As the fruit of these efforts a paper “Localized visible Ba+ mono-ion oscillator” by Neuhauser, Hohenstatt, Toschek and myself appeared in 1980.  In 1981 Van Dyck, my doctoral student Paul Schwinberg and myself extended the electron work to its antiparticle in our paper “Preliminary Comparison of the Positron and Electron Spin Anomalies” and I reviewed it in an article “Invariant Frequency Ratios in Electron and Positron Geonium Spectra Yield Refined Data on Electron Structure.” In 1986 we published a detailed paper “Electron Magnetic Moment from Geonium Spectra: Early Experiments and Background Concepts” and in 1987 our collaboration reported a 4 parts in 1012 resolution in the g factor for electron and positron in “New High-Precision Comparison of Electron and Positron g Factors.” A very promising scheme to detect cyclotron excitation through the small relativistic mass increase accompanying it was published in a 1985 paper “Observation of Relativistic Bistable Hysteresis in the Cyclotron Motion of a Single Electron” together with my postdoc, Gerald Gabrielse, and William Kells, a visitor from Fermi Lab.  Two years after the Heidelberg pioneering work an individual magnesium ion was isolated in Seattle with my postdoc Warren Nagourney and my student Gary Janik. The latter’s thesis bore the title “Laser Cooled Single Ion Spectroscopy of Magnesium and Barium.” “Shelved optical electron amplifier: Observation of quantum jumps,” was published in 1986 with my colleague Nagourney, and Jon Sandberg, an exceptional undergraduate assistant. The paper introduced a new technique which has made optical spectroscopy on an individual ion possible with record resolution and reproducibility. To date the best resolution has been realized at NIST by a group headed by my former collaborator Wineland. Peter Toschek who had made important contributions to the visible ion work in Heidelberg has built up a thriving laboratory for monoion-spectroscopy at the Universität Hamburg. With Herbert Walther a collaboration almost came off in 1974. Walther, with his large staff and excellent facilities in Munich, has since developed his own expertise in the field and made outstanding contributions to it. Gabrielse, now a full professor at Harvard, has assembled a large group and is trapping and cooling antiprotons at CERN.  In the 1988 paper “A Single Atomic Particle Forever Floating at Rest in Free Space: New Value for Electron Radius” I have surveyed the field and suggested new avenues for its extension. More precise measurements of the g factor of the electron may well be the most promising approach to study its structure. No less important, a trapped individual atomic ion may reveal itself as a timekeeping element of unsurpassed reproducibility. The research effort in Seattle continues on troth projects. The National Science Foundation has supported my research since 1958 without interruption. Initially the Army Office of Ordnance Research and the Office of Naval Research did also provide support for many years.  I am married to Diana Dundore, a practising physician. I have a grown son, Gerd, from an earlier marriage to Irmgard Lassow who is deceased.  I do regular hatha yoga exercises, enjoy waltzing, hiking in the foothills, reading, listening to classical music, and watching ballet performances. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0093 |
| **Biographical** | I was born on August 10, 1913 in Lorenzkirch a small village in Saxony, as the forth child of Theodor and Elisabeth Paul nee Ruppel. All in all we were six children. Both parents were descendants from Lutheran ministers in several generations. I grew up in München where my father has been a professor for pharmaceutic chemistry at the university. He had studied chemistry and medicine having been a research student in Leipzig with [Wilhelm Ostwald](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1909/index.html), the Nobel Laureate 1909. So I became familiar with the life of a scientist in a chemical laboratory quite early. Unfortunately, my father died when I was still a school boy at the age of fifteen years. But my interest in sciences was awaken, even my parents were very much in favour of a humanistic education. After finishing the gymnasium in München with 9 years of Latin and 6 years of ancient greek, history and philosophy, I decided to become a physicist. The great theoretical physicist, Arnold Sommerfeld, an University colleague of my late father, advised me to begin with an apprenticeship in precision mechanics. Afterwards, in the fall 1932, I commenced my studies at the Technische Hochschule München. Listening to the very inspiring physics lectures by Jonathan Zenneck with lots of demonstrations – 6 full hours a week – I felt being on the right track.  After my first examination in 1934 I turned to the Technische Hochschule in Berlin. I was lucky in finding in Hans Kopfermann a teacher with a feeling for the essentials in physics but also a very liberal man, who had taken a fatherly interest in me. He, a former Ph.D. student of James Franck, had just returned from a three years stay at the Niels Bohr Institute in Copenhagen, working in the field of hyperfine spectroscopy and nuclear moments. All in all I worked 16 years with him.  As a theorist Richard Becker taught at the TH Berlin whom I met later at the University of Göttingen again. Both men had the strongest influence on my scientific thinking. But it was not only the scientific aspect. In the Germany of these days just as important was the human and the political attitude. And I am still a little bit proud having been accepted by these sensitive men in this respect. Here are the roots for my later engagement in the anti nuclear weapon discussion and for having signed the declaration of the so-called “Göttinger Eighteen” in 1957 with its important consequences in german politics.  In 1937 after my diploma exam with Hans Geiger as examinator I followed Kopfermann to the University of Kiel where he had just been appointed Professor Ordinarius. For my doctor thesis I had chosen the determination of the nuclear moments of Beryllium from the hyperfine spectrum. I developed an atomic beam light source to minimize the Doppler effect. But just before the decisive measurements I was drawn to the air force a few days before the war started. Fortunately, a few month later I got a leave of absence to finish my thesis and to take my doctor exam at the TH Berlin. In 1940 I was exempted from military service. I joined again the group around Kopfermann which 2 years later moved to Gottingen. There in 1944 I became Privatdozent at the University.  In these years I worked in mass spectrometry and isotope separation together with W. Walcher. When we heard of the development of the betatron by D. Kerst in the United States and also of a similar development by Gund at the Siemens company, Kopfermann saw immediately that scattering experiments with high energy electrons would enable the study of the charge structure of nuclei. He convinced me to turn to this new very promising field of physics and I soon participated in the first test measurements at the 6 MeV betatron at the Siemens laboratory. Later after the war we succeeded in getting this accelerator to Gottingen.  But due to the restriction in physics research imposed by the military government I turned for a few years my interest to radiobiology and cancer therapy by electrons in collaboration with my colleague G. Schubert from the medical faculty.  Besides we performed some scattering experiments and studied first the electric disintegration of the deuteron, and not to forget for the first time we measured the Lamb shift in the He-spectrum with optical methods.  In 1952 I was appointed Professor at the University of Bonn and Director of the Physics Institute, with very good students waiting for a thesis advisor. I was very lucky that my best young collaborators followed me 0. Osberghaus, H. Ehrenberg. H.G. Bennewitz, G. Knop and H. Steinwedel as a “house theoretician”. Here we started new activities: molecular beam physics, mass spectrometry and high energy electron physics. It was a scanty period after the war. But in order to become in a few years competitive with the well advanced physics abroad we tried to develop new methods and instruments in all our research.  In this period these focusing methods in molecular beam physics with quadrupole and sextupole lenses having already started in Gottingen with H. Friedburg, were further developed and enabled new types of experiments. The quadrupole mass spectrometer and the ion trap were conceived and studied in many respects by research students. And with the generous support of the Deutsche Forschungsgemeinschaft we have built a 500 MeV *electron* synchrotron, the first in Europe working according to the new principle of strong focusing. It was followed in 1965 by a synchroton for 2500 MeV. My colleagues H. Ehrenberg, R.H. Althoff and G. Knop were sharing this success with me.  In recent years my interest turned to neutron physics with a new device, a magnetic storage ring for neutrons.  U. Trinks and K.J. Kügler and later my two sons Lorenz and Stephan, joined me in our experiments with stored neutrons at the ILL in Grenoble. My experience in accelerator physics brought me in close contact to CERN. I served there from the very early days on as an advisor. Having spent the year 1959 in Genève I became director of the nuclear physics division for the years 1964 – 67. I was for several years member and later chairman of the Scientific Policy Committee and for many years scientific delegate of Germany in the CERN-Council. For a short period I was chairman of ECFA, the European Committee for Future Accelerators.  Together with my friends W. Jentschke and W. Walcher in 1957 we started the German National Laboratory DESY in Hamburg which I joined as chairman of the directorate 1970 – 73. For several years I was chairman of its scientific council. In the same positions I served in the first years of the Kernforschungsanlage Jülich.  In 1970 I spent some weeks as Morris Loeb lecturer at Harvard University. 1978 I was lecturing as distinguished scientist at the FERMI Institute of the University of Chicago and in a similar position at the University of Tokyo. Since 1981 I am Professur Emeritus at the Bonn University.  In the past decades of recovery of German Universities and Physics research I was engaged in many advisory bodies. I have served as a referee and later as member of senate to the Deutsche Forschungsgemeinschaft. I was member and chairman of several committees: for reforming the university structure and for research planning of the federal government.  Ten years ago I was elected President of the Alexander von Humboldt Foundation which since 130 years fosters the international collaboration among scientists all over the world in the universal spirit of its patron Humboldt.  I was married for 36 years to the late Liselotte Paul, nee Hirsche. She shared with me the depressing period during and after the war and due to her optimistic view of life she gave me strength and independence for my profession. Four children were born to us, two daughters, Jutta and Regine, an historian of art and a pharmacist, and two sons, Lorenz and Stephan, both being physicists. Since 1979 I am married to Dr. Doris Walch-Paul, teaching medieval literature at the University of Bonn. |
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| **Physics\_1999-** | |
| **ID** | 0094 |
| **Biographical** | New York City in the period of 1922 to 1979 provided the streets, schools, entertainment, culture and ethnic diversity for many future scientists. I was born in New York on July 15, 1922 of immigrant parents. My father, Morris, operated a hand laundry and venerated learning. Brother Paul, six years older, was a tinkerer of unusual skill. I started my schooling in 1927 at PS 92 on Broadway and 95th Street and received my Ph.D. in 1951 about one mile north, at Columbia University. In between there were neighborhood junior and senior high schools and the City College of New York. There I majored in chemistry but fell under the influence of such future physicists as Isaac Halpern and my high school friend, Martin J. Klein. I graduated in 1943 and proceeded promptly to spend three years in the U.S. Army where I rose to the rank of 2nd Lieutenant in the Signal Corps. In September of 1946 I entered the Graduate School of Physics at Columbia, chaired by [I.I. Rabi.](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/index.html)  The Columbia Physics Department was constructing a 385 MeV Synchrocyclotron at their NEVIS Laboratory, located in Irvington-on-the-Hudson, New York. Construction was aided by the Offce of Naval Research and “NEVIS” eventually proved to be an extremely productive laboratory, as judged by physics results and students produced.  I joined that project in 1948 and worked with Professor Eugene T. Booth, the director of the-cyclotron project. My thesis assignment was to build a Wilson Cloud Chamber. Rabi invited many experts to Columbia to assist the novice staff in what was, for Columbia, a totally new field. Gilberto Bernardini came from Rome and John Tinlot came from Rossi’s group at MIT. Somewhat later, Jack Steinberger was recruited from Berkeley. After receiving my Ph.D. in 1951 I was invited to stay on, which I did, for the next 28 years. Much of my early work on 1 ions was carried out with Tinlot and Bernardini.  In 1958, I was promoted to Professor and took my first sabbatical at CERN where I organized a group to do the “g-2” experiment. This CERN program would continue for about 19 years and involve many CERN physicists (Picasso, Farley, [Charpak](https://www.nobelprize.org/nobel_prizes/physics/laureates/1992/index.html), Sens, Zichichi, etc.). It was also the initiation of several collaborations in CERN research which continued through the mid-70s.  I became Director of the Nevis Labs in 1961 and held this position until 1978. I have been a guest scientist at many labs but did the bulk of my research at Nevis, Brookhaven, CERN and Fermilab. During my academic career at Columbia (1951 – 1979) I have had 50 Ph.D. students, 14 are professors of physics, one is a university president and the rest with few exceptions, are physicists at national labs, in government or in industry. None, to my knowledge, is in jail. In 1979, I became Director of the Fermi National Accelerator Laboratory where I supervised the construction and utilization of the first superconducting synchrotron, now the highest energy accelerator in the world.  I have three children with my first wife, Florence Gordon. Daughter Rena is an anthropologist, son Jesse is an investment banker and daughter Rachel a lawyer. I now live with my second wife Ellen at the Fermilab Laboratory in Batavia, Illinois, where we keep horses for riding and chickens for eggs. I have been increasingly involved in development via scientific collaboration with Latin America, with science education for gifted children and with public understanding of science. I helped to found and am on the Board of Trustees of the Illinois Mathematics and Science Academy, a three year residence public school for gifted children in the State of Illinois. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q14 | **Professor Leon Lederman, welcome to Stockholm and to this Nobel interview. You have won the Nobel Prize in 1988 and this was for your research in elementary particle physics and the last century was really a very exciting time in physics and some people maybe say that all the major discoveries in physics, they are there. Do you think so?** |
|  | Leon M. Lederman: I don’t know. There’s certainly some discoveries that still have to be done, in other words there are some things we don’t understand and we don’t know how profound these are. You know, 100 years ago, there was also a feeling that all the discoveries were done, but there were some funny things and the question was are these minor problems that will be solved by the powerful knowledge that came from Uten and Maxwell, from the 1600s right through the end of 1900s, I mean, well, the end of the 1800s. Was all of that knowledge powerful knowledge enough and so that the small things that we didn’t understand would be fixed? And many people thought they would be fixed. Instead, they were indications of the major revolution. |
| Q22 | **How deep is the ocean?** |
|  | Leon M. Lederman: We should hasten to say that, whereas the field I’m talking about, which is sort of cosmology and particle physics, that field is different from the rest of physics because the rest of physics, the physics of complexity, that will go on forever, there’s no question about that because there’s essentially an infinite amount of complexity, I believe, and these days some of that is called biology which of course between us is a branch of physics nowadays because it’s based on molecules. |
| Q18 | **Yes. You wrote the popular science book called “The God Particle”. What do you think is the connection between the particle and the God?** |
|  | Leon M. Lederman: Well, the God particle, the name was given by a publisher who wanted to make a lot of money and I must admit I was sympathetic to his desire, so the name was really when I was writing the book, it was called the Higgs particle. At the end, the commercial interest said no-one ever heard of Higgs but everyone has heard of God so we’ll call it the god particle but the god in my book is not a theological god, it’s more of a philosophical god, it’s really a metaphor for nature. Nature is very puzzling, nature has to be understood and Einstein often made that connection, he called it in German “der Alter”, the old man, you know, how did he and I prefer she in my book, as a she and I thought if it would be ever made into a movie then Margaret Thatcher could play God, that would be a perfect role. |
| Q3 | **You’re working a lot with the science and public and with public education in science. Is it since you have been awarded the Nobel Prize?** |
|  | Leon M. Lederman: Well, let’s see, when did I start? Well, you know, in the US we invented the best job in Western civilisation, which is being a professor at a university because you do research and you teach, if you insist, and teaching was always something I liked to do, so I was always a teacher as well as a researcher. |
| Q18 | **Why do you think it is so? Why is science so important?** |
|  | Leon M. Lederman: Because science is changing our lives. The engine for change comes out of science and technology, so today it’s cell phones and computers and laptops and internet and in 10 years, you can think of something, an equivalent set of words which will change the way we live. Science changes the way we live, it has huge economic consequences. You may know that in developing countries, a 5th grade education – which includes science – is the best contraceptive ever invented and since population’s a major human problem, science influences that, so science is the driving influence for change and today, we’re very aware that there is a battle, if you like, or some people like to call it a war between civilisation and rigid belief systems, fundamentalism which does not allow for any diversity of opinions on how the world works and so science is in conflict and I think that’s a conflict we cannot avoid and that conflict takes place in the schools of the world. |
| Q10 | **So you don’t think that the schools, for example the schools in the US, do the right job?** |
|  | Leon M. Lederman: Oh, they’re doing a terrible job in my opinion because the US, you know, there are experts that measure science understanding and I think the US is not much worse than France or maybe Sweden. I think most countries do not educate non-scientists properly so that they feel comfortable with science. The teachers have that problem because teachers themselves especially teachers of young children, primary school teachers, are totally ignorant about how to teach science and what science is; how to teach mathematics and what mathematics is. And that’s terrible because children are born scientists, right? They do everything that scientists do. They test how strong things are, they measure the falling bodies, they’re balancing themselves, they’re doing all kinds of things to learn the physics of the world around them, so they’re all perfect scientists. They ask questions, they drive parents crazy with why, why, why. And then somehow they go into school and the school system crushes their curiosity and converts them to timidity and to the same fear of science that the teachers have. |
| Q10 | **Do you see that part of this is a result of how science works or how scientists are working?** |
|  | Leon M. Lederman: Well, science itself is the acquisition of knowledge, so you can’t blame science because you would be saying I don’t want to know. Science says here are how things work. Technology is the application of science in society and that’s in the hands of not the scientists but more of the politicians, the citizens and their leaders, and in democratic society elected officials who have to make decisions because not everything we know how to do, we should do.  There may be things we don’t want to do because they have bad effects and we’ve learned a lot about the problems of pollution and environmental contamination and we’re now much wiser about this and the US in its occasional wisdom developed something called the Office of Technology Assessment, which was an office to look at technologies and to warn us that some technologies may not be beneficial. But then another Congress came and said we don’t need such advice and so wisdom is not always part of political life. |
| Q18 | **But you have been fighting the science illiteracy for many years now. Do you have any results of your work?** |
|  | Leon M. Lederman: Of course. But I don’t know how to measure them because I think it’s a long term thing. You know, first you should start in the schools, that’s a long term right, because it takes 20 years before the child who begins in school can make use of the knowledge that you give them, but you have to start early and we learn from the experts on children’s minds and brains and cognition science. We’ve learned that it’s very important to begin training of mathematics and science very early so that’s where you’ll begin, but then you have to continue through the entire education always bringing in the importance of science, so that if the student is going to be a lawyer or a business man or journalist or whatever they’re going to be, these days they have to have a basic understanding of how science works and a comfort level with science so that if there’s something new that happens and they don’t know it, they can find out. They learn how to go to internet and get information or they can go to their library, now they call them information centres, and find out.  That’s the basic training we need, is that people have to be comfortable with new ideas and new possibilities and that applies to future politicians. I mean, it’s just a chilling thought that politicians with so much power don’t understand science at all and they have to reach out to somebody near them, hopefully, who has more of an understanding. That can work if they can choose wisely among their advisors but I think there’s no substitute for some basic, just as there’s no substitute for understanding a language, you know. You need the grammar and the vocabulary before you can enjoy Shakespeare or do things that require that. It’s the same thing with the scientific knowledge.  And then, of course, you don’t want to neglect the people that are already out of school and so you need to use television and cinema, radio, museums, are extremely important, all of these channels for informal education. So we need programmes which don’t glamorise athletes and movie stars but maybe do something about scientists, you know. We need a programme which tells people how scientists work and what they do and what science can do and what science can’t do . |

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| **Physics\_1999-** | |
| **ID** | 0095 |
| **Biographical** | Having been born in 1932, at the peak of the great depression, I grew up in difficult times. My parents worked extraordinarily hard to give us economic stability but at the same time they managed to instill in me two qualities which became the foundation of my personal and professional life. One is an unbounded sense of optimism; the other is a strong feeling as to the importance of using one’s mind for the betterment of mankind.  My interest in Physics really began at the age of 12 when I entered the Bronx High School of Science in New York. That school has become famous for the large number of outstanding individuals it has produced including among them four Nobel Laureates in Physics. The four years I spent there were certainly among the most exciting and stimulating of my life, mostly because of the interaction with other students having similar background, interest and ability. It’s rather amazing how important the interaction with the one’s peers can be at that age in determining one’s direction and success in life.  Upon graduating from high school the path to follow was fairly obvious. The Columbia Physics Department at that time was unmatched by any in the world. Largely a product of the late [Professor I.I. Rabi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/index.html), it was a-department which was to provide the ambiance for six Nobel Prize pieces of work in widely diverse fields during the next thirteen years. And, in addition, it was the host for a period of time to another half dozen or so future Nobel Laureates either as students or as post-doctoral researchers. I know of no other institution either before or since that has come close to that record.  Thus, it was that I became an undergraduate at Columbia in 1949, to stay there through my graduate years and take up a faculty position as Assistant Professor in 1958. I became an Associate Professor in 1960 and a Professor in 1963.  In order for me to put my life into perspective, I must mention four individuals who have given it meaning, direction and focus. Foremost among these is my wife Marilyn whom I married 35 years ago and who has provided the one most enduring thread throughout these years. Without her constant encouragement and enthusiasm there would have been far less meaning to my life. The second is of course Jack Steinberger. Jack was my teacher, my mentor and my closest colleague during my years at Columbia. Whatever taste and judgement I have ever had in the field of Particle Physics came from Jack. Third of course is [T.D. Lee](https://www.nobelprize.org/nobel_prizes/physics/laureates/1957/index.html). He was the inspirer of this experiment and the person who has served as a constant sounding board for any ideas I have had. He has also become, I am proud to say, a dear personal friend. And finally, my close collaborator Leon Lederman. If there is any one person who has served as the sparkplug for high energy physics in the U.S. it has been Leon. I am proud to have been his collaborator.  In 1966, after having spent 17 years at Columbia, I decided to move West to Stanford, where a new accelerator was just being completed. During the ensuing years I was involved in two major research efforts. The first of these investigated the charge asymmetry in the decay of the long-lived neutral kaon. The second of these, which was quite unique, succeeded in producing and detecting relativistic hydrogen-like atoms each made up of a pion and a muon.  During the 1970’s, lured in part by the new industrial revolution in “Silicon Valley” I decided to try my hand at a totally new adventure. Digital Pathways, Inc. of which I am currently the Chief Executive Officer is a company dedicated to the secure management of data communications. Although it is difficult to predict the future I still have all the optimism that I had back when I first grew up in New York-life can be a marvelous adventure.  *(added in 1991):* A new change in my career occurred in February 1991 when I became Associate Director, High Energy and Nuclear Physics, at Brookhaven National Laboratory. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0096 |
| **Biographical** | I was born in Bad Kissingen (Franconia) in 1921. At that time my father, Ludwig, was 45 years old. He was one of twelve children of a rural ‘Viehhändler’ (small-time cattle dealer). Since the age of eighteen he had been cantor and religious teacher for the little Jewish community, a job he still held when he emigrated in 1938. He had been a bachelor until he returned from four years of service in the German Army in the first World War. My mother was born in Nuremberg to a hop merchant, and was fifteen years the younger. Unusual for her time, she had the benefit of a college education and supplemented the meagre income with English and French lessons, mostly to the tourists which provided the economy of the spa. The childhood I shared with my two brothers was simple; Germany was living through the post-war depression.  Things took a dramatic turn when I was entering my teens. I remember Nazi election propaganda posters showing a hateful Jewish face with crooked nose, and the inscription “Die Juden sind unser Ungluck”, as well as torchlight parades of SA storm troops singing “Wenn’s Juden Blut vom Messer fliesst, dann geht’s noch mal so gut”. In 1933, the Nazis came to power and the more systematic persecution of the Jews followed quickly. Laws were enacted which excluded Jewish children from higher education in public schools. When, in 1934, the American Jewish charities offered to find homes for 300 German refugee children, my father applied for my older brother and myself. We were on the SS Washington, bound for New York, Christmas 1934.  I owe the deepest gratitude to Barnett Faroll, the owner of a grain brokerage house on the Chicago Board of Trade, who took me into his house, parented my high-school education, and made it possible also for my parents and younger brother to come in 1938 and so to escape the holocaust. New Trier Township High School on the well-to-do Chicago North Shore, enjoyed a national reputation, and, with a swimming pool, athletic fields, cafeteria, as well as excellent teachers, offered horizons unimaginable to the young emigrant from a small German town.  The reunited family settled down in Chicago. We were helped to acquire a small delicatessen store which was the basis of a very marginal income, but we were used to a simple life, so this was no problem. I was able to continue my education for two years at the Armour Institute of Technology (now the Illinois Institute of Technology) where I studied chemical engineering. I was a good student, but these were the hard times of the depression, my scholarship came to an end, and it was necessary to work to supplement the family income.  The experience of trying to find a job as a twenty-year-old boy without connections was the most depressing I was ever to face. I tried to find any job in a chemical laboratory: I would present myself, fill out forms, and have the door closed hopelessly behind me. Finally through a benefactor of my older brother, I was accepted to wash chemical apparatus in a pharmaceutical laboratory, G.D. Searl and Co., at eighteen dollars a week. In the evenings I studied chemistry at the University of Chicago, the weekends I helped in the family store.  The next year, with the help of a scholarship from the University of Chicago, I could again attend day classes, so that in 1942 I could finish an undergraduate degree in chemistry.  On 7 December 1941, Japan attacked the United States at Pearl Harbor. I joined the Army and was sent to the MIT radiation laboratory after a few months of introduction to electromagnetic wave theory in a special course, given for Army personnel at the University of Chicago. My only previous contact with physics had been the sophomore introductory course at Armour. The radiation laboratory was engaged in the development of radar bomb sights; I was assigned to the antenna group. Among the outstanding physicists in the laboratory were Ed Purcell and Julian Schwinger. The two years there offered me the opportunity to take some basic courses in physics.  After Germany surrendered in 1945, I spent some months on active duty in the Army, but was released after the Japanese surrender, to continue my studies at the University of Chicago. It was a wonderful atmosphere, both between professors and students and also among the students. The professors to whom I owe the greatest gratitude are [Enrico Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html), W. Zachariasen, Edward Teller and Gregor Wentzel. The courses of Fermi were gems of simplicity and clarity and he made a great effort to help us become good physicists also outside the regular class-room work, by arranging evening discussions on a widespread series of topics, where he also showed us how to solve problems. Fellow students included [Yang](https://www.nobelprize.org/nobel_prizes/physics/laureates/1957/index.html), [Lee](https://www.nobelprize.org/nobel_prizes/physics/laureates/1957/index.html), Goldberger, Rosenbluth, Garwin, [Chamberlain](https://www.nobelprize.org/nobel_prizes/physics/laureates/1959/index.html), Wolfenstein and Chew. There was a marvellous collaboration, and I feel I learned as much from these fellow students as from the professors.  I would have preferred to do a theoretical thesis, but nothing within reach of my capabilities seemed to offer itself. Fermi then asked me to look into a problem raised in an experiment by Rossi and Sands on stopping cosmic-ray muons. They did not find the expected number of decays. After correcting for geometrical losses there was still a missing factor of two, and I suggested to Sands that this might be due to the fact that the decay electron had less energy than expected in the two-body decay, and that one might test this experimentally. When this idea was not followed, Fermi suggested that I do the experiment, instead of waiting for a theoretical topic to surface. The cosmic-ray experiment required less than a year from its conception to its conclusion, in the end of the summer of 1948. It showed that the muon’s is a three-body decay, probably into an electron and two neutrinos, and helped lay the experimental foundation for the concept of a universal weak interaction.  There followed an interlude to try theory again at the Institute for Advanced Study in Princeton, where Oppenheimer had become director. It was a frustrating year: I was no match for Dyson and other young theoreticians assembled there. Towards the end I managed to find a piece of work I could do, on the decay of mesons via intermediate nucleons. I still remember how happy Oppenheimer was to see me come up with something, at last.  In 1949, Gian Carlo Wick, with whom I had done some work on the scattering of polarized neutrons in magnetized iron while still a graduate student at Chicago University, invited me to be his assistant at the University of California in Berkeley. There the experimental possibilities in the Radiation Laboratory, created by E.O. Lawrence, were so great that I reverted easily to my wild state, that is experimentation. During the year there, I had the magnificent opportunity of working on the just completed electron synchrotron of [Ed McMillan](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1951/index.html). It enabled me to do the first experiments on the photoproduction of pions (with A.S. Bishop) to establish the existence of neutral pions (with W.K.H. Panofsky and J. Stellar) as well as to measure the pion mean life (with O. Chamberlain, R.F. Mozley and C. Weigand).  I survived only a year in Berkeley, partly because I declined to sign the anticommunist loyalty oath, and moved on to Columbia University in the summer of 1950. At its Nevis Laboratory, Columbia had just completed a 380 MeV cyclotron; this, for the first time, offered the possibility of experimenting with beams of T mesons. In the next years I exploited these beams to determine the spins and parities of charged and neutral pions, to measure the pi– pi0 mass difference and to study the scattering of charged pions. This work leaned heavily on the collaboration of Profs. D. Bodansky and A.M. Sachs, as well as of several Ph.D. students: R. Durbin, H. Loar, P. Lindenfeld, W. Chinowsky and S. Lokanathan.  These experiments all utilized small scintillator counters. In the early fifties, the bubble-chamber technique was discovered by [Don Glaser](https://www.nobelprize.org/nobel_prizes/physics/laureates/1960/index.html), and in 1954 three graduate students, J. Leitner, N.P. Samios and M. Schwartz, and myself began to study this technique which had not as yet been exploited to do physics. Our first effort was a 10 cm diameter propane chamber. We made one substantial contribution to the technique, that was the realization of a fast recompression (within ~10 ms), so that the bubbles were recompressed before they could grow large and move to the top. This permitted chamber operation at a useful cycling rate. The first bubble-chamber paper to be published was from our experiment at the newly built Brookhaven Cosmotron, using a 15 cm propane chamber without magnetic field. It yielded a number of results on the properties of the new unstable (strange) particles at a previously unattainable level, and so dramatically demonstrated the power of the new technique which was to dominate particle physics for the next dozen years. Only a few months later we published our findings on three events of the type Sigma0-> Delta0 + gamma, which demonstrated the existence of the Sigma0 hyperon and gave a measure of its mass. This experiment used a new propane chamber, eight times larger in volume, and with a magnetic field. This chamber also introduced the use of more than two stereo cameras, a development which is crucial for the rapid, computerized analysis of events, and has been incorporated into all subsequent bubble chambers.  In the decade which followed, the same collaborators, together with Profs. Plano, Baltay, Franzini, Colley and Prodell, and a number of new students, constructed three more bubble chambers: a 12″ H2 chamber as well as 30″ propane and H2 chambers, developed the analysis techniques, and performed a series of experiments to clarify the properties of the new particles. The experiments I remember with the most pleasure are:  – the demonstration of parity violation in D decay, 1957; – the demonstration of the ß decay of the pion, 1958; – the determination of the p0 parity on the basis of angular correlation in the double internal conversion of the g rays, 1962; – the determination of the w and j decay widths (lifetimes), 1962; – the determination of the S0 – D0 relative parity, 1963; – the demonstration of the validity of the DS = DQ rule in K0 and in hyperon decays, 1964.  This long chain of bubble-chamber experiments, in which I also enjoyed and appreciated the collaboration of two Italian groups, the Bologna group of G. Puppi and the Pisa group of M. Conversi, was interrupted in 1961, in order to perform, at the suggestion of Mel Schwartz, and with G. Danby, J.M. Gaillard, D. Goulianos, L. Lederman and N. Mistri, the first experiment using a high-energy neutrino beam now recognized by the Nobel Prize, and described in the paper of M. Schwartz.  In 1964, CP violation was discovered by Christensen, [Cronin](https://www.nobelprize.org/nobel_prizes/physics/laureates/1980/index.html), [Fitch](https://www.nobelprize.org/nobel_prizes/physics/laureates/1980/index.html) and Turlay. Soon after I found myself on sabbatical leave at CERN, and proposed, together with Rubbia and others, to look for the interference between K0s and K0L amplitudes in the time dependence of K0 decay. Such interference was expected in the CP violation explanation of the results of Christensen et al., but not in other explanations which had also been proposed. The experiment was successful, and marked the beginning of a set of experiments to learn more about CP violation, which was to last a decade. The next result was the observation of the small, CP-violating, charge asymmetry in K0L leptonic decay, in 1966. Measurement of the time dependence of this charge asymmetry, following a regenerator, permitted a determination of the regeneration phase; this, together with the earlier interference experiments, yielded, for the first time, the CP-violating phase jh+ – and, in consequence, as well as the observed magnitudes of the CP-violating amplitudes in the two-pion and the leptonic decays, certain checks of the superweak model. The same experiment also gave a more sensitive check of the DS = DQ rule, an ingredient of the present Standard Model.  In 1968, I joined CERN. Charpak had just invented proportional wire chambers, and this development offered a much more powerful way to study the K0 decay to which I had become addicted. Two identical detectors were constructed, one at CERN together with Filthuth, Kleinknecht, Wahl, and others, and one at Columbia together with Christensen, Nygren, Carithers and students. The Columbia beam was long, and therefore contained no Ks but only KL, the CERN beam was short, and therefore contained a mixture of Ks and KL. It was contaminated by a large flux of L0, and so was also a hyperon beam, permitting the first measurements of L0 cross-sections as well as the Coulomb excitation of L0 to S0, a difficult and interesting experiment carried out chicfly by Steffen and Dydak. The most important result to come from the Columbia experiment was the observation of the rare decay KL -> µ+µ– with a branching ratio compatible with theoretical predictions based on unitarity. Previously, a Berkeley experiment had searched in vain for this decay and had claimed an upper limit in violation of unitarity. Since unitarity is fundamental to field theory, this result had a certain importance.  The CERN experiment, which extended until 1976, produced a series of precise measurements on the interference of Ks and KL in the two-pion and leptonic decay modes, thus leading us to obtain highly precise results on the CP-violating parameters in K0 decay. I believe the experiment was beautiful, and take some pride in it, but the results were all in agreement with the superweak model and so did little towards understanding the origin of CP violation.  In 1972, the K0 collaboration of CERN, Dortmund and Heidelberg was joined by a group from Saclay, under R. Turlay, to study the possibilities for a neutrino experiment at the CERN SPS then under construction. The CDHS detector, a modular array of magnetized iron disks, scintillation counters and drift chambers, 3.75 m in diameter, 20 m long, and weighing 1200 t, was designed, constructed, and exposed to different neutrino beams at the SPS during the period 1977 to 1983. It provided a large body of data on the charged-current and neutral-current inclusive reactions in iron, which permitted first of all the clearing away of a number of incorrect results, e.g. the “high-y anomaly” produced at Fermilab, allowed the first precise and correct determination of the Weinberg angle, demonstrated the existence of right-handed neutral currents, provided measurements of the structure functions which gave quantitative support to the quark constituent model of the nucleon, and, through the Q2 evolution of the structure functions, gave quantitative support to QCD. The study of multimuon events gave quantitative support to the GIM model of the Cabibbo current through its predictions on charm production.  In the CDHS experiment we were about thirty physicists. Since 1983, I have been spokesman for a collaboration of 400 physicists engaged in the design and construction of a detector for the 100 + 100 GeV e+e– Collider, LEP, to be ready at CERN in the beginning of 1989. In the meantime I had also helped to design an experiment to compare CP violation in the charged and neutral two-pion decay of the K0L. This experiment was the first to show “direct” CP violation, an important step towards the understanding of CP violation.  In 1986, I retired from CERN and became part-time Professor at the Scuola Normale Superiore in Pisa. However, my chief activity continues as before in my research at CERN.  I am married to Cynthia Alff, my former student and now biologist, and we have two marvellous children, Julia, 14 years old, and John, 11 years old. From an earlier marriage to Joan Beauregard, there are two fine sons, Joseph Ludwig and Richard Ned.  I play the flute, unfortunately not very well, and have enjoyed tennis, mountaineering and sailing, passionately.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Physics 1981-1990*, Editor-in-Charge Tore Frängsmyr, Editor Gösta Ekspong, World Scientific Publishing Co., Singapore, 1993  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  Copyright © The Nobel Foundation 1988  **Addendum, June 2005**  In 1988, I was the spokesman of a collaboration of about 350 physicists, preparing the detector we called ALEPH, which we had started to plan in 1981, for the CERN electron-positron collider then under construction called LEP, and which started to operate in 1989. Altogether, about fifteen hundred physicists participated, using four such detectors. LEP results dominated CERN physics, perhaps the world’s, for a dozen or more years, with crucial, precise measurements, which confirmed the Standard Model of the unified electro-weak and strong interactions. The physics scene had changed a lot since the time of my thesis experiment in 1948, which I could do quite alone. For some time I could help, as manager, but also contributing to the detector design and the physics analysis. This came to an end in 1995, partly because I had no new ideas on the physics we might learn, and partly because the challenges became more and more technical, especially in the use of computers, and I could not compete with the younger generation.  Since that time I have enjoyed learning cosmology and astrophysics, and following its progress. This has given me much satisfaction: on the one hand it involved having to learn some basic physics new to me, physics important to cosmology but unimportant in particle physics, such as general relativity and hydrodynamics, on the other hand these have been spectacular years in astrophysics, with the discovery in 1992, and continually improving observational results, of the inhomogeneities of the cosmic microwave background radiation, which give a totally new map of the universe, at a much earlier time than stars or galaxies, much simpler and therefore much easier to learn from, and more precisely. I still come to CERN, the 10 km on my bicycle, every day and sometimes enjoy trying to learn something new. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0097 |
| **Biographical** | I was born in Neuenkirchen, North-Rhine Westphalia, in the Federal Republic of Germany on May 16, 1950, as the fourth child of Anton and Elisabeth Bednorz. My parents, originating from Silesia, had lost sight of each other during the turbulences of World War II, when my sister and two brothers had to leave home and were moved westwards. I was a latecomer completing our family after its joyous reunion in 1949.  During my childhood, my father, a primary school teacher and my mother, a piano teacher, had a hard time to direct my interest to classical music. I was more practical-minded and preferred to assist my brothers in fixing their motorcycles and cars, rather than performing solo piano exercises. At school it was our teacher of arts who cultivated that practical sense and helped to develop creativity and team spirit within the class community, inspiring us to theater and artistic performances even outside school hours. I even discovered my interest in classical music at the age of 13 and started playing the violin and later the trumpet in the school orchestra.  My fascination in the natural sciences was roused while learning about chemistry rather than physics. The latter was taught in a more theoretical way, whereas in chemistry, the opportunity to conduct experiments on our own, sometimes even with unexpected results, was addressing my practical sense.  In 1968, I started my studies in chemistry at the University of Münster, but somehow felt lost due to the impersonal atmosphere created by the large number of students. Thus I soon changed my major to cristallography, that field of mineralogy which is located between chemistry and physics.  In 1972, Prof. Wolfgang Hoffmann and Dr. Horst Böhm, my teachers, arranged for me to join the IBM Zürich Research Laboratory for three months as a summer student. It was a challenge for me to experience how my scientific education could be applied in reality. The decision to go to Switzerland set the course for my future. The physics department of which I became a member was headed by K. Alex Müller, whom I met with deep respect. I was working under the guidance of Hans Jörg Scheel, learning about different methods of crystal growth, materials characterization and solid state chemistry. I soon was impressed by the freedom even I as a student was given to work on my own, learning from mistakes and thus losing the fear of approaching new problems in my own way.  After my second visit in 1973, I came to Rüschlikon for six months in 1974 to do the experimental part of my diploma work on crystal growth and characterization of SrTiO3, again under the guidance of Hans Jörg Scheel. The perovskites were Alex Müller’s field of interest and, having followed my work, he encouraged me to continue my research on this class of materials.  In 1977, after an additional year in Münster, I joined the Laboratory of Solid State Physics at the Swiss Federal Institute of Technology (ETH) in Zürich and started my Ph.D. thesis under the supervision of Prof. Heini Gränicher and K. Alex Müller. I gratefully remember the time at the ETH and the family-like atmosphere in the group, where Hanns Arend provided a continuous supply of ideas. It was also the period during which I began to interact more closely with Alex and reamed about his intuitive way of thinking and his capability of combining ideas to form a new concept.  In 1978, Mechthild Wennemer followed me to Zürich to start her Ph.D. at the ETH, but more importantly to be my partner in life. I had met her in 1974 during our time together at the University of Münster. Since then she has acted as a stabilizing element in my life and is the best adviser for all decisions I make, sharing the up’s and down’s in an unselfish way.  I completed my work on the crystal growth of perovskite-type solid solutions and investigating them with respect to structural, dielectric and ferroelectric properties, and joined IBM in 1982. This was the end of a ten-year approach which had begun in 1972.  The intense collaboration with Alex started in 1983 with the search for a high-TC superconducting oxide; in my view, a long and thorny but ultimately successful path. We both realized the importance of our discovery in 1986, but were surprised by the dramatic development and changes in both the field of science and in our personal lives.  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above. |
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| **Physics\_1999-** | |
| **ID** | 0098 |
| **Biographical** | I was born in Basel, Switzerland, on 20th April 1927. The first years of my life were spent with my parents in Salzburg, Austria, where my father was studying music. Hereafter, my mother and I moved to Dornach near Basle to the home of my grandparents, and from there to Lugano in the italian-speaking part of Switzerland. Here, I attended school and thus became fluent in the Italian language.  My mother died when I was eleven years old, and I attended the Evangelical College in Schiers, situated in a mountain valley in eastern Switzerland. I remained there until I obtained my baccalaureate (Mature) seven years later. This means I arrived in Schiers just before the Second World War started, and left just after it terminated. This was indeed quite a unique situation for us youngsters. Here, in a neutral country, we followed the events of the war worldwide, even in discussion groups in the classes. These college years in Schiers were of significance for my career.  The school was liberal in the spirit of the nineteenth century, and intellectually quite demanding. We were also very active in sports, I especially so in alpine skiing. In my spare time, I became quite involved in building radios and was so fascinated that I really wanted to become an electrical engineer. However, in view of my abilities, my chemistry tutor, Dr. Saurer, eventually convinced me to study physics.  At the age of 19, I did my basic military training in the Swiss army. Upon its completion, I enrolled in the famous Physics and Mathematics Department of the Swiss Federal Institute of Technology (ETH) in Zürich. Our freshman group was more than three times the normal size. We were called the “atombomb semester”, as just prior to our enrollment nuclear weapons had been used for the first time, and many students had become interested in nuclear physics. The basic course was taught by Paul Scherrer and his vivid demonstrations had a lasting effect on my approach to physics. Other courses were in part not as illuminating, so that, despite good grades, I once seriously considered switching to electrical engineering. However, Dr. W. Kanzig, responsible for the advanced physics practicum, convinced me to continue. In the later semesters, [Wolfgang Pauli](https://www.nobelprize.org/nobel_prizes/physics/laureates/1945/index.html), whose courses and examinations I took, formed and impressed me. He was truly a wise man with a deep understanding of nature and the human being. I did my diploma work under Prof. G. Busch on the Hall effect of grey tin, now known as a semimetal, and, prompted by his fine lectures, also became acquainted with modern solid-state physics.  After obtaining my diploma, following my interest in applications, I worked for one year in the Department of Industrial Research (AFIF) of the ETH on the Eidophor large-scale display system. Then I returned to Prof. Busch’s group as an assistant and started my thesis on paramagnetic resonance (EPR). At one point, Dr. H. Granicher suggested I look into the, at that time, newly synthesized double-oxide SrTiO3. I found and identified the EPR lines of impurity present in Fe3+.  In spring of 1956, just before starting the latter work, Ingeborg Marie Louise Winkler became my wife. She has always had a substantial influence in giving me confidence in all my undertakings, and over the past 30 years has been my mentor and good companion, always showing interest in my work. Our son Eric, now a dentist, was born in the summer of 1957, six months before I submitted my thesis.  After my graduation in 1958, I accepted the offer of the Battelle Memorial Institute in Geneva to join the staff. I soon became the manager of a magnetic resonance group. Some of the more interesting investigations were conducted on layered compounds, especially on radiation damage in graphite and alkalimetal graphites. The general manager in Geneva, Dr. H. Thiemann, had a strong personality, and his ever-repeated words “one should look for the extraordinary” made a lasting impression on me. Our stay in Geneva was most enjoyable for the family, especially for two reasons: the charm of the city and the birth of our daughter Silvia, now a kindergarten teacher.  While in Geneva, I became a Lecturer (with the title of Professor in 1970) at the University of Zürich on the recommendation of Prof. E. Brun, who was forming a strong NMR group. Owing to this lectureship, Prof. A.P. Speiser, on the suggestion of Dr. B. Luthi, offered me a position as a research staff member at the IBM Zürich Research Laboratory, Rüschlikon, in 1963. With the exception of an almost two-year assignment, which Dr. J. Armstrong invited me to spend at IBM’s Thomas J. Watson Research Center in Yorktown Heights, N.Y., I have been here ever since. For almost 15 years, research on SrTiO3 and related perovskite compounds absorbed my interest: this work, performed with Walter Berlinger, concerned the photochromic properties of various doped transition-metal ions and their chemical binding, ferroelectric and soft-mode properties, and later especially critical and multicritical phenomena of structural phase transitions. In parallel, Dr. Heinrich Rohrer was studying such effects in the antiferromagnetic system of GdAlO3. It was an intense and also, from a personal point of view, happy and satisfying time. While I was on sabbatical leave at the Research Center, he and [Dr. Gerd Binnig](https://www.nobelprize.org/nobel_prizes/physics/laureates/1986/index.html) started the Scanning Tunneling Microscope (STM) project. Just before leaving for the USA, I had been involved in the hiring of Dr. Binnig. Upon my return to Rüschlikon, I closely followed the great progress of the STM project, especially as from 1972 onwards, I was in charge of the physics groups.  The desire to devote more time to my own work prompted me to step down as manager in 1985. This was possible because in 1982 the company had honored me with the status of IBM Fellow. The ensuing work is summarized in Georg Bednorz’s part of the Lecture. As he describes there, he joined our Laboratory to pursue his diploma work, on SrTiO3 of course! Ever since making his acquaintance, I have deeply respected his fundamental insight into materials, his human kindness, his working capacity and his tenacity of purpose! |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0099 |
| **Biographical** | I was born on 25 December 1906 in Heidelberg as the fifth of seven children of Professor Julius Ruska and his wife Elisbeth (*née* Merx). After graduating from grammar school in Heidelberg I studied electronics at the Technical College in Munich, studies which I began in the autumn of 1925 and continued two years later in Berlin. I received my practical training from Brown-Boveri & Co in Mannheim and Siemens & Halske Ltd in Berlin. Whilst still a student at the Technical College in Berlin I began my involvement with high voltage and vacuum technology at the Institute of High Voltage, whose director was Professor Adolf Matthias. Under the direct tutelage of Dr Max Knoll and together with other doctoral students I worked on the development of a high performance cathode ray oscilloscope. On the one hand my interest lay principally in the development of materials for the building of vacuum instruments according to the principles of construction; on the other it lay in continuing theoretical lectures and practical experiments in the optical behaviour of electron rays.  My first completed scientific work (1928-9) was concerned with the mathematical and experimental proof of Busch’s theory of the effect of the magnetic field of a coil of wire through which an electric current is passed and which is then used as an electron lens. During the course of this work I recognised that the focal length of the waves could be shortened by use of an iron cap. From this discovery the polschuh lens was developed, a lens which has been used since then in all magnetic high-resolution electron microscopes. Further work, conducted together with Dr Knoll, led to the first construction of an electron microscope in 1931. With this instrument two of the most important processes for image reproduction were introduced-the principles of emission and radiation. In 1933 I was able to put into use an electron microscope, built by myself, that for the first time gave better definition than a light microscope. In my Doctoral thesis of 1934 and for my university teaching thesis (1944), both at the Technical College in Berlin, I investigated the properties of electron lenses with short focal lengths.  Since the further technical development of electron microscopes could not be the task of a college institute – whose resources would have been far overstretched – I went to work in industry in the field of electron optics. From 1933 to 1937 I was with Fernseh Ltd in Berlin-Zehlendorf and was responsible for the development of television receivers and transmitters, as well as photoelectric cells with secondary amplification. Convinced of the great practical importance of electron microscopy for pure and applied research I attempted during this time to continue the development of high-resolution electron microscopes with larger materials, this time working with Dr Bodo von Borries. This work was made possible in 1936-7 by Siemens & Halske. In Berlin-Spandau in 1937 we set up the Laboratory for Electron Optics and developed there until 1939 the first customised electron microscopes (the ‘Siemens Super Microscope’). Parallel to the development of this instrument my brother, Dr Med. Helmut Ruska, and his colleagues worked on its application, particularly in the medical and biological fields. In order to promote its usage in different scientific areas as quickly as possible we suggested to Siemens that they set up a visiting institute for research work to be carried out using electron microscopy. This institute was founded in 1940. From this institute, in which we worked together with both German and foreign scientists, around 200 scientific papers were published before the end of 1944. My task consisted in the development and production of the electron microscope, such that by the beginning of 1945 around 35 institutions were equipped with one.  In the years following 1945 I, together with a majority of new colleagues, reconstituted the Institute of Electron Optics in Berlin-Siemensstadt, which had been disbanded due to bombing, so that by 1949 electron microscopes were again being built. This new period of development led in 1954 to ‘Elmiskop 1’, which since then has been used in over 1200 institutions the world over. At the same time I sought the further physical development of the electron microscope by working at other scientific institutions. Thus from August 1947 to December 1948 I worked at the German Academy of Sciences in Berlin-Buch in the Faculty of Medicine and Biology, then from January 1949 as Head of Department at what is today the Fritz Haber Institute of the Max Planck Society in Berlin-Dahlem. Here on 27 June 1957 I was made Director of the Institute for Electron Microscopy, after I had given up my position with Siemens in 1955. I retired on 31 December 1974.  From 1949 until 1971 I held lectures on the basic principles of electron optics and electron microscopy at both the Free University and the Technical University of Berlin. My publications in the area of electron optics and electron microscopy include several contributions to books and over 100 original scientific papers. |
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| **Physics\_1999-** | |
| **ID** | 0100 |
| **Biographical** | I was born in Frankfurt, W. Germany, on 7.20., ’47 as the first of two sons. My childhood was very much influenced by the Second World War, which had only just ended. We children had great fun playing among the ruins of the demolished buildings, but naturally were too young to realize that much more than just buildings had been destroyed.  Until the age of 31, I lived partly in Frankfurt and partly in Offenbach, a nearby city. I attended school in both cities, and it was in Frankfurt that I started to study physics. Already as a child about 10 years of age, I had decided to become a physicist without actually knowing what it involved. While studying physics, I started to wonder whether I had really made the right choice. Especially theoretical physics seemed so technical, so relatively unphilosophical and unimaginative. In those years, I concentrated more on playing music with friends in a beat-band rather than on physics. My mother had introduced me to classical music very early in life, and I believe this played an important role in my subsequent development. Unfortunately, I started playing the violin rather late, at the age of 15 only, but thoroughly enjoyed being a member of our school orchestra. My brother was responsible for my transition from classics to beat by his perpetually immersing me with the sounds of the Beatles and the Rolling Stones, until I finally really liked that kind of music, and even started composing songs and playing in various beat-bands. In this way, I first learned how difficult teamwork can be, how much fun it is to be creative, and how unpredictable the reaction of an audience can be.  My education in physics gained some significance when I began my diploma work in Prof. Dr. W. Martienssen’s group, under Dr. E. Hoenig’s guidance. I realized that actually *doing* physics is much more enjoyable than just learning it. Maybe ‘doing it’ is the right way of learning, at least as far as I am concerned.  I have always been a great admirer of Prof. Martienssen, especially of his ability to grasp and state the essence of the scientific context of a problem. Dr. Hoenig introduced me to experimenting, and exhibited great patience when I asked him very stupid questions in trying to catch up on what I had missed over all the previous years.  In 1969, Lore Wagler became my wife. We had both been studying for quite a long time – Lore is now a psychologist – so only recently did we decide to have children: a daughter born in Switzerland in 1984, and a son born in California in 1986. This was the absolute highlight and most wonderful experience of my whole life. However, fatherhood is not without its sacrifice. For the time being, nearly all my hobbies, like music (singing, playing the guitar and the violin), and sports (soccer, tennis, skiing, sailing and playing golf) have had to take a back seat.  It was in 1978 that Lore – my private psychotherapist – convinced me to accept an offer from the IBM Zürich Research Laboratory to join a physics group. This turned out to be an extremely important decision, as it was here I met Heinrich Rohrer. His way of viewing physics, combined with his humanity and sense of humor, fully restored my somewhat lost curiosity in physics. My years at Ruschlikon, and in IBM Research in general, have been very exciting, not only because of the development of the STM, but also because of the stimulating and pleasant atmosphere created by the people working there, and by those responsible. Working together in a team with Heinrich Rohrer, Christoph Gerber and Edmund Weibel was an extraordinarily delightful experience, and one for which I shall be eternally grateful. It is also extremely gratifying that our work was recognized far afield. We were first awarded the German Physics Prize, the Otto Klung Prize, the Hewlett Packard Prize, the King Faisal Prize, and now the ultimate crown, the Nobel Prize for Physics. Life certainly does not become easier for a scientist once his work has exceeded a certain significance. But while prizes do add some complications, I must admit they also have their compensations!  *(added in 1991:)*  In 1990 I joined the Supervisory Board of the Daimler Benz Holding and presently I am involved in a few political activities.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Physics 1981-1990*, Editor-in-Charge Tor |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0101 |
| **Biographical** | I was born in Buchs, St. Gallen, Switzerland on 6.6., ’33 as the third child, half an hour after my twin sister. We were fortunate to enjoy a carefree childhood with a sound mixture of freedom, school and farm work. In 1949, the family moved to Zürich and our way of life changed from country to town. My finding to physics was rather accidental. My natural bent was towards classical languages and natural sciences, and only when I had to register at the ETH (Swiss Federal Institute of Technology) in autumn 1951, did I decide in favor of physics. In the next four years, Professors G. Busch, [W. Pauli](https://www.nobelprize.org/nobel_prizes/physics/laureates/1945/index.html), and P. Scherrer taught me the rudiments. In autumn 1955, I started work on my Ph.D. Thesis and it was fortuitous that Jörgen Lykke Olsen trusted me to measure the length changes of superconductors at the magnetic-field-induced superconducting transition. He had already pioneered the field with measurements on the discontinuity of Young’s modulus. Following in his footsteps, I lost all respect for angstroms. The mechanical transducers were very vibration sensitive, and I learned to work after midnight, when the town was asleep. My four graduate years were a most memorable time, in a group of distinguished graduate students always receptive for fun, and including the interruptions by my basic training courses in the Swiss mountain infantry.  In summer 1961, Rose-Marie Egger became my wife, and her stabilizing influence has kept me on an even keel ever since. Our honeymoon trip led us to the United States where I spent two post-doe years working on thermal conductivity of type-II superconductors and metals in the group of Professor Bernie Serin at Rutgers University in New Jersey. Then in the summer of 1963, Professor Ambros Speiser, Director of the newly founded IBM Research Laboratory in Rüschlikon, Switzerland, made me an offer to join the physics effort there. Encouraged by Bruno Lüthi, who later became a Professor at the University of Frankfurt, and, at the time, strongly recommended the hiring of Gerd Binnig, I accepted to start in December 1963, after having responded to the call of the wild in the form of a four-month camping trip through the USA.  My first couple of years in Rüschlikon were spent studying mainly Kondo systems with magnetoresistance in pulsed magnetic fields. End of the sixties, Keith Blazey interested me to work on GdAlO3, an antiferromagnet on which he had done optic experiments. This started a fruitful cooperation on magnetic phase diagrams, which eventually brought me into the field of critical phenomena. Encouraged by [K. Alex Müller](https://www.nobelprize.org/nobel_prizes/physics/laureates/1987/index.html), who had pioneered the critical-phenomena effort in our Laboratory, I focused on the bicritical and tetracritical behavior and finally on the random-field problem. These were most enjoyable years, during which so many patient colleagues taught me physics. I left them with some regret, when I ventured with Gerd to discover new shores. We found them. Thank you, Gerd.  In 1974/75, I spent a sabbatical year with Professor Vince Jaccarino and Dr. Alan King at the University of California in Santa Barbara, to get a taste of nuclear magnetic resonance. We solved a specific problem on the bicritical point of MnF2, their home-base material. We traded experience, NMR and critical phenomena. Rose-Marie and I also took the opportunity at the beginning and end of my sabbatical to show the USA to our two daughters, Doris and Ellen, on two extended camping trips from coast to coast.  In all the years with IBM Research, I have especially appreciated the freedom to pursue the activities I found interesting, and greatly enjoyed the stimulus, collegial cooperation, frankness, and intellectual generosity of two scientific communities, namely, in superconductivity and critical phenomena. I should also like to take this opportunity to thank the many, many friends, teachers, and seniors who have contributed towards my scientific career in any way whatsoever, and most particularly my mother for her unstinting aid and assistance, especially when times were difficult. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
| **Interview** |  |
| Q4 | **Heinrich Rohrer, welcome to Stockholm and to this interview with Nobelprize.org. You were awarded the 1986 Nobel Prize in Physics together with Ernst Ruska and Gerd Binnig for your joint work in developing microscopy. And in particular you and Gerd Binnig received the prize for your design of the scanning tunnelling microscope. Now this is a device that enables us to look at the surface of objects at the atomic scale, by means of a probe which skims over the surface and records variations in the topography below it with minute current fluctuations. What did this device enable us to see for the first time that we hadn’t seen before?** |
|  | Heinrich Rohrer: As you say surface structures in a different way. I mean you had some ideas about certain surface structures, but you never saw it in, so to speak, in real space. So it’s just like if you would trace a surface with your finger, you see. So you see it in real space and then of course … actually you see the electrons on the surface. It’s electronic wave functions and so you also can see certain properties. You can test the hardness of a surface. |
| Q4 | **Right. And in those early days of using it, what were the first surprises that came up? Because as you said, you sort of had an idea and then you could actually see. What surprises turned up?** |
|  | Heinrich Rohrer: I think the first surprise was that we really could do it with atomic resolution, you see. Actually that you can do it with atomic resolution that’s inherent in the approach we took. But we didn’t expect it that it could make such fine fingers. But then every point of a needle ends up with one atom. And this very atom at the end that’s then your finger, you see, and that’s this atom, it has the size of an atom and so you can feel other atoms. And that was a pleasant surprise that you could do it relatively easy. |
| Q2 | **On the subject of being able to do it, it seems unimaginably complex to make such a thing because you are talking about atoms almost in contact with other atoms, there must be no vibration. You are talking about a probe which is, as you say, one atom thick at its tip. The mind boggles. How did you have the confidence to even imagine that one could do such a thing, to make such an instrument?** |
|  | Heinrich Rohrer: Let’s say we didn’t see any obstacle which could not be overcome. And of course there were quite a few fortuitous developments in the whole thing, but somehow luck was on our side. But I think in science you need luck, you see. And if you don’t have the luck in a specific case, then you do something different. You might have luck in something else. So, you see it was less difficult finally than everybody thought. That’s why everybody thought you could not do it and that’s why nobody did it.  And that’s, I would say, a crucial thing in science. You see, everything is new because other people think you cannot do it, or because other people do not appreciate that it could become important. And we live of novelty in science, so whenever you do something new, you have to overcome certain beliefs that this cannot be done, that it is not interesting and so on. |
| Q10 | **Oh really? So the physicists are treated a little bit differently there?** |
|  | Heinrich Rohrer: Yes, you see people who work on technology, I think that’s usually a joint effort of many people whereas the physics are … we did this type of physics where everybody could do his own project on his own, maybe with some help of a technician or of a post doc or of a PHD student. Whereas in technology you have to make concentrated efforts towards something and there of course a certain freedom gets lost. You still, even in technology you have the freedom to solve a problem your way, you see. But it naturally sits in a certain framework whereas in the physics everybody had to come up with his own idea what he was going to do. |
| Q16 | **And you changed field a few times along the way? How did you make the decisions about which fields to pursue? Was that IBM directed or were they just your?** |
|  | Heinrich Rohrer: No, that’s all the research of member are responsible for their own field and finding their own topics, you see. They are their own, how do you say?  **Masters.**  Heinrich Rohrer: They have to make their own work you see. So I was working on a set of problems before and I thought somehow I came as far as I could go without learning completely new things; that’s in critical phenomena. And that was a very interesting topic and it was more or less at the top of the interest. And so by making everything smaller, inhomogenities plays a more important role.  So if you have an oxide which has a few holes then the conduction of electricity through this oxide is maybe a little bit larger. Now if you have 100 holes, then you have the same type if you make another batch of oxide, you might have 98 holes. And then maybe the conductivity will be different simply by two percent. So the fluctuation from piece to piece is still small. But if you make things smaller and smaller then you might have batches where you just have two or three holes, you see. Now maybe this one has two holes and this one has three holes and this one has one hole, so then the holes effect a conductivity most, so then in one case you would have a tremendous conductivity and in the other case you would have a conductivity which is two times less or something like that. And that is how the fluctuations from piece to piece which cannot be tolerated in miniaturisation. And so that’s an idea to work along these lines and nobody did it really in IBM and it was an important problem. Now in this case it was inhomogeneity of /- – -/ oxide, but you can have many different ways where inhomogeneities play an important role and the smaller something becomes the more the role of the inhomogeneity can get the disasters you see. |
| Q16 | **Right, well I want to return to the theme of miniaturisation in general a little later, but thinking about the decades at the IBM research labs where you moved through various fields and changed. Do you think that as well as being, wanting to move on because you had reached the pinnacle of success in a particular field, there was also an element of feeling that as a newcomer, but an experienced newcomer to another field, you could make a significant contribution fast?** |
|  | Heinrich Rohrer: I think that’s generally true and unfortunately we do not practise that. You see, if you look at Nobel Prize work, that’s very often done at a young age. |
| Q5 | **Did you recognise it immediately? Was it apparent from the beginning that it was just the perfect partnership?** |
|  | Heinrich Rohrer: No, because I think he recognised it earlier than me, but you see I’m older so I’m a little bit more maybe conservative in many respects.  But I think it simply worked out. And that was the first team really in physics to my knowledge. That was the first time two guys got together and did something. |
| Q9 | **And in 1986, basically the technique had been proved and very rapidly you were awarded the Nobel Prize on the back of lots of other prizes as well. Did that change everything very radically for you, or did life continue just pretty much the same?** |
|  | Heinrich Rohrer: I think the recognition we got already before, I mean let’s say the recognition in the scientific community, the esteem, then of course a Nobel Prize, that’s maybe a discontinuity in the whole thing … but surely changed a few things. I mean ok, you are then a Nobel Laureate and that’s already a bit different, how do you say? |
| Q18 | **And STM was one of the sort of building blocks of what I suppose one might call the nanoscience revolution. It opened up a whole new area to view and I’d like to ask you a little bit about the word I used there, revolution, because nanoscience is seen as a revolution. Is that a correct term? Is it changing the way we look at the world?** |
|  | Heinrich Rohrer: I think the revolution has to have a little bit more than just to scale. It’s true that it was getting to a smaller and smaller scale, that was always the beginning of a revolution. I mean, from exploring the world since Columbus you see, that was the world scale and then came the industrial revolution that was the micro scale to let’s say down to the micrometre as a precision scale. And then came the IT revolution, the information technology revolution and that was in the scale of micrometre. But you see it wasn’t the micro revolution. It was really the information technology revolution. And the other one was the industrial revolution. So I would hate to say the nanorevolution. It has to have a little bit more substance. I have my expectations what could happen, but I don’t have a word for it, you see.  So now for nano just getting smaller alone does not make the revolution. I mean also for the microtechnology you had to have a transistor which really changed in the way you could do things.  And I think also on the nanotechnology you have to have a few more ideas, the way you will approach new things. We know a little bit what could be the end of it, but how to do it we don’t know yet. |
| Q14 | **Is one point that having reached the nano scale, the atomic scale, we’ve really reached the limits of miniaturisation? We can’t really go smaller.** |
|  | Heinrich Rohrer: I would guess so. Yes. And so that’s I think you hit the point, you see. And we are already very close. We have covered of the miniaturisation over the last 50 years. We have covered two thirds, or three quarters. And there is just a little bit to go, the difficult bit to the nano. So just getting smaller is not all. You see I think nano has to offer a little bit different things such as being smaller in order to become a revolution. And I think nano offers really completely new prospects in many ways. |
| Q14 | **That’s a nice drawing to a close of things. I’d just like to ask one last question which is based around the idea of moving fields. Obviously, you’ve been successful in moving from field to field. What do you think, is it possible to answer, nanoscience would require now in terms of an influx of new people? Are there people who you would like to see move into the field of studying properties at the atomic level?** |
|  | Heinrich Rohrer: The young people and I think they do it.  Heinrich Rohrer: That was a meeting. You see, that was also an interesting aspect when we started with the whole thing when the STM came out, you see it was mainly young people. There weren’t many established ones you see. The surfer scientist, the established surfer scientist, they stayed surfer scientists. They didn’t go into the STM, only later. But now it’s quite interesting to see when you go to a meeting. I was at the last meeting, a big meeting that was the conference in Basel in 2005 and I was very pleasantly surprised by the young people, you see. There were so many young people, young guys with their enthusiasm and in particular a lot of young women, who get involved in nanoscience and not just in the biological place, biological area, they get involved in real hard let’s say mechanical engineering, or electrical engineering in context with nanoscience. I think that for me is a very encouraging sign when also young women get really enthusiastic about something. I think it’s in good hands. |

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| **Physics\_1999-** | |
| **ID** | 0102 |
| **Biographical** | |  | | --- | | Born 28th June 1943 in Schroda (Posen), German nationality. | |  | | **February 1962** | | Abitur in Quakenbruck. | |  | | **April 1962 to March 1969** | | Technical University Braunschweig Diploma in Physics. | | Title of diploma work: “Lifetime Measurements on InSb” (Prof. F. R. Keßler). | |  | | **May 1969 to Nov. 1980** | | University Würzburg (Prof. Dr. G. Landwehr) | | Thesis work about: “Galvanomagnetic Properties of Tellurium in Strong Magnetic Fields” (Ph.D. in 1972). | | Habilitation in 1978. | | The most important publication related to the Nobel Prize appeared in: Phys. Rev. Letters *45*, 494 (1980). | | Research work at the Clarendon Laboratory, Oxford (1975 to 1976) and High Magnetic Field Laboratory, Grenoble (1979 to 1980). | |  | | **Nov. 1980 to Dec. 1984** | | Professor at the Technical University, München. | |  | | **Since January 1985** | | Director at the Max-Planck-Institut für Festkörperforschung, Stuttgart. | |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q4 | **Welcome, very nice to see you, professor Klitzing. It was back in 1985 you received the Nobel Prize. I just would like to start off by asking you how did you go about it, when you had this hypothesis and you wanted to prove it? How long time did it take and how did you go about it?** |
|  | Klaus von Klitzing: Ok, then we have to really to go back how I selected physics to study and how did I go into the semi-conductor physics. At the time when I started at university in 1962, semi-conductor physics, lasers, just very modern topics, and I decided into go into this modern area. I was fascinated by the scientific topic and I studied in Braunschweig and you should know that Braunschweig, there is a National Bureau of Standards in Germany, the metrology institute, called Physikalisch-Technische Bundesanstalt and as a young student, I always worked between the terms at this Physikalisch-Technische Bundesanstalt. This was very important also for my discovery because I identified that it is very important for this Institute for metrology because my discovery has something to do with fundamental constant, these very precise measurements. I was already sensitive as a young student with these questions. Then I have done research and then you are doing the diploma work, in Germany the PhD and then the habitation, if you want to go into the professor direction, but then I suddenly discovered, ok, do we have enough positions at universities to find a job? There were no positions and then I tried to get jobs in the industry and they said oh no, you are over educated, you will be unhappy in industry. Fortunately there was this so called [Heisenberg](https://www.nobelprize.org/prizes/physics/1932/heisenberg/facts/) programme that a scientist had the possibility to do what he wants to do, can select the institute, the research centre, he will get every month his salary and then he has to give some lectures but he is absolutely free to do research and I decided to go to the best laboratories to do my experiments. I was working the semi-conductors and have used high magnetic fields and there was one centre in Grenoble in France and I decided to go to this laboratory and to continue my research. |
| Q4 | **But you knew then already what you wanted to study? You had this idea?** |
|  | Klaus von Klitzing: Yes, I had always semi-conductor physics, I wanted to understand modern micro electronics, how can I improve devices. I had contacts also in the industry but from the fundamental physics, I wanted to know how can I make faster switches, how can the electron move in the semi-conductors. It was really more or less by accident that I went into this direction because during my research I discovered if I clean a sample, that change the properties, so if you clean the surface, the electrical behaviour was different. Then you see, ok, the surface is very important and then I focused on the very thin layer of the surface and this is connected to modern micro electronics. All electronic properties are in a very, very thin layer and in this way, I went in a direction where I never expected that I would discover something fundamental. I was just interested to understand how I can improve something, how can I understand something and I had experience for about 10 years in this field. I collected a lot of information in my head and anyone who reads literature had the chance to discover the quantized Hall effect for which I got the Nobel Prize, because it was published, the data, but if you have not the experience. I worked with different companies, Siemens company in Germany, /- – -/ company in England and I saw that if I used different samples, I saw the same phenomena. Then I concluded, ok, there’s something fundamental in the pen of the source of the material and then I can fix it really. In the night of 5th of February at two o’clock in the morning, I decided to look at some special feature which I have seen for many, many years and then I developed a very simple theory which was so simple that couldn’t believe that this is working. It was too simple because in a semi-conductor you have impurities, you have dirty effects and it’s very complicated normally. I used low approximation to understand or to analyse something and I found that the very simple equation works, that there are no corrections, no deviations, so within five minutes I knew there is something interesting. This was not the main road, this was just one type of experiment which was used to characterise our system. |
| Q9 | **What did it mean to you professionally, that you got the prize a few years later? You were quite a young man still, at that age, one of the youngest to receive a prize.** |
|  | Klaus von Klitzing: This is a problem. A lot of scientists told me, oh be careful, this is danger if you get this prize because the Nobel Prize really is the highest prize you will get in science and it can go down only or you can keep the level. I decided now I have this position to generate an atmosphere for young students to develop, to have creativity. Immediately I decided you cannot go still up to a higher level so just transfer it to the young students because I could develop because my professor gave me the freedom and always supported me. I never worried about my future because I knew as long as I’m doing good research, somebody will help me. I will have this atmosphere also for my students, so this was my decision. |
| Q10 | **So, it’s important to have the funding to do the basic researches. Is that something that you are convinced of?** |
|  | Klaus von Klitzing: Yes, this is very important because there’s obviously discussion about basic research and at the [Max Planck](https://www.nobelprize.org/prizes/physics/1918/planck/facts/) Society we have not a problem because Max Planck Society, they favour basic research and the most important thing is that you have some new knowledge that just developed during your research. Only the very first one is a success. You have never second prize or third prize in basic research. You should be at the very front and you should work for 100% as a scientist, so there is no compromise between science and then some other things, you have to work 120%. |
| Q11 | **Max Planck Institute is mainly funded by the German state. You have already said you think it’s very important that independent research institutes are there. How do you see the development in the future for scientific research in Europe specifically or maybe even directly in Germany?** |
|  | Klaus von Klitzing: I think the Max Planck Society is internationally known for excellent science and is accepted doing good science and if I’m asked what’s the reason for this I always say the independence. We are financed by the government. In Germany half of the central government and half by the different states but they have no direct influence on the research in the institute, so we decide, we select our directors and highest quality is always the most important thing. As a Max Planck director, I’m absolutely free to select my topic, so I can do brain research if I believe that I can contribute to this. We have some variation every six years, compare them between different Max Planck Institutes and so on but to give them the freedom is very important but you need money and the politicians always try to get some influence on the scientific topics in the institute. Even at present we are fighting against this influence. They will say ok, perhaps for political reason you have to select more female directors or something. If the quality is there, there is no problem but for outside to have some special boundary conditions, it becomes difficult so if you have to co-operate with industry or with universities, we will do this. It’s there on the same level. For both sides, we have some profit out of this but if somebody says, ok now because universities should be supported with basic research, you should combine Max Planck Institute with universities. If we decide this is ok, we’ll do it, but to have decision from outside, this will be always dangerous and we are trying to avoid this. You spoke about Europe – I’m trying to generate also in Europe some European research currency. There’s some discussion because basic research is not in the frameworks of the European programme, these programmes are for industry. There are a lot of boundary conditions, you need partners from different countries, you cannot optimise from the quality of your research, you have to optimise this by other parameters and I’m trying to generate something like Max Planck Institute, on the European level, to have something where the highest quality is the first criteria for supporting some research and I hope that in next years we will generate a European research council where only the quality of research counts. |
| Q9 | **I would like to ask you a more personal question. In which way did the prize affect your personal life and your family’s life? Was it to good or bad? Did it put much more pressure on you maybe, work wise?** |
|  | Klaus von Klitzing: There are different influences. First of all my children or my wife, they don’t believe what’s in the newspaper they can read because they have the experience that the newspaper … Because I decided the family is one side and my business is the other side and I didn’t want to have my family included in all the discussions. Then the press generates some information which are not true, this was very bad side effect. It’s also some education, that you should not believe everything what is printed, you have to read different source to get the true information. Now for me, I decided to behave in the same way as before. Even today, I never accept some invitation where only the fact that I’m a Nobel Prize winner is the reason that I’m invited for some exhibition or something like that or after dinner speech also; even if they give money, I decide no. If this is the reason, I will not accept this. I decided to be as normal as possible and therefore I like to have these discussions also with students, these Lindau meetings. The Nobel Prize winners and the students meet in order to demonstrate that even Nobel Prize winners are normal persons. I remember when I first time I saw a Nobel Prize winner at a distance of 20 metres as a student, there was something, a different person, and I never tried to contact him. I think I will encourage people that we are normal persons, we know something in special areas but we also don’t know everything because this is one of our problems, that a lot of people expect that we should know everything and that we should problems which we are not able to solve or everyone else is able to solve, this is the same way. In this way I can survive. |

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| **Physics\_1999-** | |
| **ID** | 0103 |
| **Biographical** | I was born in the small town of Gorizia, Italy, on 31 March, 1934. My father was an electrical engineer at the local telephone company and my mother an elementary school teacher. At the end of the World War II most of the province of Gorizia was overtaken by Yugoslavia and my family fled to Venice first and then to Udine.  As a boy, I was deeply interested in scientific ideas, electrical and mechanical, and I read almost everything I could find on the subject. I was attracted more by the hardware and construction aspects than by the scientific issues. At that time I could not decide if science or technology were more relevant for me.  After completing High School, I applied to the Faculty of Physics at the rather exclusive Scuola Normale in Pisa. My previous education had been seriously affected by the disasters of the war and the subsequent unrest. I badly failed the admission tests and my application was turned down. I forgot about physics and I started engineering at the University of Milan (Politecnico). To my great surprise and joy a few months later I was offered the possibility of entering the Scuola Normale. One of the people who had won the admission contest had resigned! I am recollecting this apparently insignificant fact since it has determined and almost completely by accident my career of physicist. I moved to Pisa, where I completed the University education with a thesis on cosmic ray experiments. They have been very tough years, since I had to greatly improve my education, which was very deficient in a number of fundamental disciplines. At that time I also participated under my thesis advisor Marcello Conversi to new instrumentation developments and to the realization of the first pulsed gas particle detectors.  Soon after my degree, in 1958 I went to the United States to enlarge my experience and to familiarize myself with particle accelerators. I spent about one and a half years at Columbia University. Together with W. Baker, we measured at the Nevis Syncro-cyclotron the angular asymmetry in the capture of polarized muons, demonstrating the presence of parity violation in this fundamental process. This was his first of a long series of experiments on Weak Interactions, which ever since has become my main field of interest. Of course at that time it would have been quite unthinkable for me to imagine to be one day amongst the people discovering the quanta of the weak field!  Around 1960 I moved back to Europe, attracted by the newly founded European Organization for Nuclear Research, where for the first time the idea of a joint European effort in a field of pure Science was to be tried in practice. The Syncro-cyclotron at CERN had a performance significantly superior to the one of the machine in Nevis and we succeeded in a number of very exciting experiments on the structure of weak interactions, amongst which I would like to mention the discovery of the beta decay process of the positive pion, p+ = p0 + e + v and the first observation of the muon capture by free hydrogen, µ–+ p = n + v.  In the early sixties John Adams brought to operation the CERN Proton Syncrotron. I moved to the larger machine where I continued to do some weak interaction experiments, like for instance the determination of the parity violation in the beta decay of the lambda hyperon.  During the summer of 1964 [Fitch and Cronin](https://www.nobelprize.org/nobel_prizes/physics/laureates/1980/index.html) announced the discovery of CP violation. This has been for me a tremendously important result and I abandoned all current work to start a long series of observations on CP violation in K0 decay and on the KL-KS mass difference. Unfortunately the subject did not turn out to be as prolific as in the case of the previous discovery of parity violation and even today, some thirty years afterwards we do not know much more about the origin of CP-violation than right after the announcement of the discovery.  I returned again to more orthodox weak interactions a few years later, when together with David Cline and Alfred Mann we proposed a major neutrino experiment at the newly started US laboratory of Fermilab. The operational problems associated with a limping accelerator and a new laboratory made very difficult, albeit impossible for us during the Summer of 1973 to settle definitively the question of the existence of neutral currents in neutrino interactions, when competing with the much more advanced instrumentation of Gargamelle at CERN. Instead, about one year later we could cleanly observe the presence of all-muons events in neutrino interactions and to confirm in this way one of the crucial predictions of the GIM mechanism, hinting at the existence of charm, glamorously settled only few months later with the observation of the Y/J particle.  In the meantime and under the impulse of Vicky Weisskopf a new, fascinating adventure had just started at CERN with a new type of colliding beams machine, the Intersecting Storage Rings, in which counter-rotating beams of protons collide against each other. This novel technique offered a much more efficient use of the accelerator energy than the traditional method of collisions against a fixed target. From the very first operation of this new type of accelerator, I have participated to a long series of experiments. They have been crucial to perfect the detection techniques with colliding beams of protons and antiprotons needed later on for the discovery of the Intermediate Bosons.  By that time it was quite clear that Unified Theories of the type SU(2) x U(1) had a very good chance of predicting the existence and the masses of the triplet of intermediate vector bosons. The problem of course was the one of finding a practical way of discovering them. To achieve energies high enough to create the intermediate vector bosons (roughly 100 times as heavy as the proton) together with David Cline and Peter Mc Intyre we proposed in 1976 a radically new approach. Along the lines discussed about ten years earlier by the Russian physicist Budker, we suggested to transform an existing high energy accelerator in a colliding beam device in which a beam of protons and of antiprotons, their antimatter twins, are counter-rotating and colliding head-on. To this effect we had to develop a number of techniques for creating antiprotons, confining them in a concentrated beam and colliding them with an intense proton beam. These techniques were developed at CERN with the help of many people and in particular of Guido Petrucci, Jacques Gareyte and Simon van der Meer.  In view of the size and of the complexity of the detector, physics experiments at the proton-antiproton collider have required rather unusual techniques. Equally unusual has been the number and variety of different talents needed to reach the goal of observing the W and Z particles. International cooperation between many people from very different countries has been proven to be a very successful way of achieving such goals.  **Addendum, 1991**  For eighteen years, I have dedicated one semester per year to teaching at Harvard University in Cambridge, Mass., where I have been appointed professor in 1970, spending the rest of my time mostly in Geneva, where I was conducting various experiments, especially the UA-1 Collaboration at the proton-antiproton collider until 1988.  On 17 December 1987, the Council of CERN decided to appoint me Director-General of the Organization as from 1st January 1989, for a mandate of five years.  My wife, Marisa, teaches Physics at High School, and we have two children, a married daughter Laura, medical doctor, and a son, André, student in high energy physics. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0104 |
| **Biographical** | I was born in 1925, in The Hague, the Netherlands, as the third child of Pieter van der Meer and Jetske Groeneveld, both of Frisian origin. I had three sisters.  My father was a schoolteacher and my mother came from a teacher’s family. Under these conditions it is not astonishing that learning was highly prized; in fact, my parents made sacrifices to be able to give their children a good education.  I visited the Gymnasium in The Hague and passed my final examination (in the sciences section) in 1943. Because the Dutch universities had just been closed at that time under the German occupation, I spent the next two years attending the humanities section of the Gymnasium. Meanwhile, my interest in physics and technology had been growing; I dabbled in electronics, equipped the parental home with various gadgets and assisted my brilliant and inspiring physics teacher (U.Ph. Lely) with the preparation of numerous demonstrations.  From 1945 onwards, I studied “Technical Physics” at the University of Technology, Delft, where I specialized in measurement and regulation technology under C.J.D.M. Verhagen. The physics taught in this newly created subsection of an old and established engineering school, although of excellent quality, was of necessity somewhat restricted and I have often felt regrets at not having had the intensive physics training that many of my colleagues enjoyed. Nevertheless, if I have at times been able to make original contributions in the accelerator field, I cannot help feeling that to a certain extent my slightly amateur approach in physics, combined with much practical experience, was an asset.  After obtaining my engineering degree in 1952, I worked in the Philips Research Laboratory, Eindhoven, mainly on high-voltage equipment and electronics for electron microscopes. In 1956 I moved to Geneva to join the recently founded European Organization for Nuclear Research (CERN), where I have been working ever since on many different projects, in an agreeable and stimulating international atmosphere.  To start with, my work (under the leadership of J.B. Adams and C.A. Ramm) was concerned mainly with technical design: poleface windings, multipole correction lenses for the 28 GeV synchrotron and their power supplies. My interest in matters more directly concerned with the handling of particles was growing, in the meantime, stimulated by many contacts with people understanding accelerators. After working for a year on a separated antiproton beam (1960), I proposed a high-current, pulsed focusing device (“horn”) aimed at increasing the intensity of a beam of neutrinos, then at the centre of interest at CERN and elsewhere. The design of this monster, together with the associated neutrino flux calculations kept me busy until 1965, when I joined a small group, led by F.J.M. Farley, preparing the second “g-2” experiment for measuring the anomalous magnetic moment of the muon. I designed the small storage ring used and participated at all stages of the experiment proper, including part of the data treatment. This was an invaluable experience; not only did I learn the principles of accelerator design, but I also got acquainted with the lifestyle and way of thinking of experimental high-energy physicists.  From 1967 to 1976 I returned to more technical work when I was responsible for the magnet power supplies, first of the Intersecting Storage Rings (ISR) and then of the 400 GeV synchrotron (SPS). I kept up with accelerator ideas, however, and worked (during my ISR period) on a method for the luminosity calibration of storage rings and on stochastic cooling. The latter was, of course, aimed at increasing the ISR luminosity, but practical application seemed difficult at the time, mainly because the high beam intensity in the ISR would have made the cooling very slow. After developing a primitive theory (1968) I therefore did not pursue this subject. However, the work was taken up by others and in 1974 the first experiments were done in the ISR.  In 1976, Cline, McIntyre, Mills, and Rubbia proposed to use the SPS or the Fermilab ring as a pp collider. Accumulation of the needed antiprotons would clearly require cooling. At this time, my work on the SPS power supplies had just come to an end; I joined a study group on the pp project and an experimental team studying cooling in a small ring (ICE). The successful experiments in this ring and the work by Sacherer on theory and by Thorndahl on filter cooling showed that p accumulation by stochastic stacking was feasible. The collider project was approved and I became joint project leader with R. Billinge for the accumulator construction. Since then, I have worked with the group that commissioned and improved the ring and that is now preparing the construction of a second ring to increase the p stacking rate by an order of magnitude. As a spin-off from this work, I proposed the stochastic extraction method that is now used (in a much improved form) in the Low-Energy Antiproton Ring (LEAR).  In the meantime, in 1966, while skiing with friends in the Swiss mountains, I met my wife-to-be Catharina M. Koopman and after a very brief interval we decided to marry. This was certainly one of the best decisions I ever made; my life has since been far more interesting and colourful. We have two children: Esther (1968) and Mathijs (1970). |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0105 |
| **Biographical** | I was born in Lahore (then a part of British India) on the 19th of October 1910, as the first son and the third child of a family of four sons and six daughters. My father, Chandrasekhara Subrahmanya Ayyar, an officer in Government Service in the Indian Audits and Accounts Department, was then in Lahore as the Deputy Auditor General of the Northwestern Railways. My mother, Sita (neé Balakrishnan) was a woman of high intellectual attainments (she translated into Tamil, for example, Henrik Ibsen’s *A Doll House*), was passionately devoted to her children, and was intensely ambitious for them.  My early education, till I was twelve, was at home by my parents and by private tuition. In 1918, my father was transferred to Madras where the family was permanently established at that time.  In Madras, I attended the Hindu High School, Triplicane, during the years 1922-25. My university education (1925-30) was at the Presidency College. I took my bachelor’s degree, B.Sc. (Hon.), in physics in June 1930. In July of that year, I was awarded a Government of India scholarship for graduate studies in Cambridge, England. In Cambridge, I became a research student under the supervision of Professor R.H. Fowler (who was also responsible for my admission to Trinity College). On the advice of [Professor P.A.M. Dirac](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/index.html), I spent the third of my three undergraduate years at the Institut för Teoretisk Fysik in Copenhagen.  I took my Ph.D. degree at Cambridge in the summer of 1933. In the following October, I was elected to a Prize Fellowship at Trinity College for the period 1933-37. During my Fellowship years at Trinity, I formed lasting friendships with several, including Sir Arthur Eddington and Professor E.A. Milne.  While on a short visit to Harvard University (in Cambridge, Massachusetts), at the invitation of the then Director, Dr. Harlow Shapley, during the winter months (January-March) of 1936, I was offered a position as a Research Associate at the University of Chicago by Dr. Otto Struve and President Robert Maynard Hutchins. I joined the faculty of the University of Chicago in January 1937. And I have remained at this University ever since.  During my last two years (1928-30) at the Presidency College in Madras, I formed a friendship with Lalitha Doraiswamy, one year my junior. This friendship matured; and we were married (in India) in September 1936 prior to my joining the University of Chicago. In the sharing of our lives during the past forty-seven years, Lalitha’s patient understanding, support, and encouragement have been the central facts of my life.  After the early preparatory years, my scientific work has followed a certain pattern motivated, principally, by a quest after perspectives. In practise, this quest has consisted in my choosing (after some trials and tribulations) a certain area which appears amenable to cultivation and compatible with my taste, abilities, and temperament. And when after some years of study, I feel that I have accumulated a sufficient body of knowledge and achieved a view of my own, I have the urge to present my point of view, ab initio, in a coherent account with order, form, and structure.  There have been seven such periods in my life: stellar structure, including the theory of white dwarfs (1929-1939); stellar dynamics, including the theory of Brownian motion (1938-1943); the theory of radiative transfer, including the theory of stellar atmospheres and the quantum theory of the negative ion of hydrogen and the theory of planetary atmospheres, including the theory of the illumination and the polarization of the sunlit sky (1943-1950); hydrodynamic and hydromagnetic stability, including the theory of the Rayleigh-Bénard convection (1952-1961); the equilibrium and the stability of ellipsoidal figures of equilibrium, partly in collaboration with Norman R. Lebovitz (1961-1968); the general theory of relativity and relativistic astrophysics (1962-1971); and the mathematical theory of black holes (1974- 1983). The monographs which resulted from these several periods are:  1. An Introduction to the Study of Stellar Structure (1939, University of Chicago Press; reprinted by Dover Publications, Inc., 1967).  2a. Principles of Stellar Dynamics (1943, University of Chicago Press; reprinted by Dover Publications, Inc., 1960).  2b. ‘Stochastic Problems in Physics and Astronomy’, *Reviews of Modern Physics*, 15, 1 – 89 (1943); reprinted in *Selected Papers on Noise and Stochastic Processes* by Nelson Wax, Dover Publications, Inc., 1954.  3. Radiative Transfer (1950, Clarendon Press, Oxford; reprinted by Dover Publications, Inc., 1960).  4. Hydrodynamic and Hydromagnetic Stability (1961, Clarendon Press, Oxford; reprinted by Dover Publications, Inc., 1981).  5. Ellipsoidal Figures of Equilibrium (1968; Yale University Press).  6. The Mathematical Theory of Black Holes (1983, Clarendon Press, Oxford).  However, the work which appears to be singled out in the citation for the award of the Nobel Prize is included in the following papers:  ‘The highly collapsed configurations of a stellar mass’, *Mon. Not. Roy. Astron. Soc.*, 91, 456-66 (1931).  ‘The maximum mass of ideal white dwarfs’, *Astrophys. J.*, 74, 81 – 2 (1931).  ‘The density of white dwarfstars’, Phil. Mag., 11, 592 – 96 (1931).  ‘Some remarks on the state of matter in the interior of stars’, *Z. f. Astrophysik*, 5, 321-27 (1932).  ‘The physical state of matter in the interior of stars’, *Observatory*, 57, 93 – 9 (1934)  ‘Stellar configurations with degenerate cores’, *Observatory*, 57, 373 – 77 (1934).  ‘The highly collapsed configurations of a stellar mass’ (second paper), *Mon. Not. Roy. Astron. Soc.*, 95, 207 – 25 (1935).  ‘Stellar configurations with degenerate cores’, *Mon. Not. Roy. Astron. Soc.*, 95, 226-60 (1935).  ‘Stellar configurations with degenerate cores’ (second paper), *Mon. Not. Roy. Astron. Soc*., 95, 676 – 93 (1935).  ‘The pressure in the interior of a star’, *Mon. Not. Roy. Astron. Soc.*, 96, 644 – 47 (1936).  ‘On the maximum possible central radiation pressure in a star of a given mass’, *Observatory*, 59, 47 – 8 (1936).  ‘Dynamical instability of gaseous masses approaching the Schwarzschild limit in general relativity’, *Phys. Rev. Lett.*, 12, 114 – 16 (1964); Erratum, *Phys. Rev. Lett.*, 12, 437 – 38 (1964).  ‘The dynamical instability of the white-dwarf configurations approaching the limiting mass’ (with Robert F. Tooper), *Astrophys. J.*, 139, 1396 – 98 (1964).  ‘The dynamical instability of gaseous masses approaching the Schwarzschild limit in general relativity’, *Astrophys. J.*, 140, 417 – 33 (1964).  ‘Solutions of two problems in the theory of gravitational radiation’, *Phys. Rev. Lett.*, 24, 611 – 15 (1970); Erratum, *Phys. Rev. Lett.*, 24, 762 (1970).  ‘The effect of graviational radiation on the secular stability of the Maclaurin spheroid’, *Astrophys. J.*, 161, 561 – 69 |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0106 |
| **Biographical** | I was born in 1911 in Pittsburgh, Pennsylvania, the son of John MacLeod Fowler and Jennie Summers Watson Fowler. My parents had two other children, my younger brother, Arthur Watson Fowler and my still younger sister, Nelda Fowler Wood. My paternal grandfather, William Fowler, was a coal miner in Slammannan, near Falkirk, Scotland who emigrated to Pittsburgh to find work as a coal miner around 1880. My maternal grandfather, Alfred Watson, was a grocer. He emigrated to Pittsburgh, also around 1880, from Taniokey, near Clare in County Armagh, Northern Ireland. His parents taught in the National School, the local grammar school for children, in Taniokey, for sixty years. The family lived in the central part of the school building; my great grandfather taught the boys in one wing of the building and my great grandmother taught the girls in the other wing. The school is still there and I have been to see it.  I was raised in Lima, Ohio, from the age of two when my father, an accountant, was transferred to Lima from Pittsburgh. Each summer during my childhood the family went back to Pittsburgh during my father’s vacation from work. He was an ardent sportsman and through him I became (and still am) a loyal fan of the Pittsburgh Pirates in the National Baseball League and of the Pittsburgh Steelers in the National Football League.  Lima was a railroad center served by the Pennsylvania, Erie, Nickel Plate and Baltimore & Ohio railroads. It was also the home of the Lima Locomotive Works which built steam locomotives. My brother, Arthur Watson Fowler, a mechanical engineer, worked for Lima Locomotive all his life until his retirement. After 1960 the company produced power shovels and construction cranes. As a boy I spent many hours in the switch yards of the Pennsylvania Railroad not far from my family home. It is no wonder that I go around the world seeking passenger trains still pulled by steam locomotives. In 1973 I travelled the Trans Siberian Railroad from Khabarovsk to Moscow because, among other reasons, the train was powered by steam for almost 2 500 kilometers from Khabarovsk to Chita. It’s not powered by steam but now I can afford to ride on the new Orient Express. It is also no wonder that on my 60th birthday my colleagues and former students presented me in Cambridge, England, with a working model, 3 1/4″ gauge (1/16 standard size) British Tank Engine. I operated it frequently on the elevated track of the Cambridge and District Model Engineering Society. It is my pride and joy. I have named it *Prince Hal*.  I attended Horace Mann Grade School and Lima Central High School. A few of my high school teachers are still alive and I met them at my 50th class reunion in 1979. I was President of the Senior Class of 1929. My teachers encouraged and fostered my interest in engineering and science but also insisted that I take four years of Latin rather than French or German. My family home was located across the street from the extensive playgrounds of Horace Mann School. There were baseball diamonds, tennis courts, a running track and a football field. During my high school days I played on the Central High School football team and won my letter as a senior. Horace Mann was Central’s home football field. During my college days I served as Recreational Director of the Horace Mann playground during the summer. Not far from my home was Baxter’s Woods with a running creek and swimming hole. What a wonderful environment it all was for my boyhood!  On graduation from school I enrolled at the Ohio State University in Columbus, Ohio, in ceramic engineering. I had won a prize for an essay on the production of Portland cement and ceramic engineering seemed a natural choice for me. Fortunately all engineering students took the same courses including physics and mathematics. I became fascinated with physics and when I learned from Professor Alpheus Smith, head of the Physics Department, that there was a new degree offered in Engineering Physics I enrolled in that option at the start of my sophomore year. So also did Leonard I. Schiff, who became a very great theoretical physicist. We were lifelong friends until his death a few years ago.  My parents were not affluent and my summer salary as recreation director did not cover my expenses at Ohio State. For my meals I waited table, washed the dishes and stoked the furnaces at the Phi Sigma Sigma Sorority. I worked Saturdays cutting and selling ham and cheese in an outside stall at the Central Market in Columbus. Early in the morning we put up the stall and unloaded the hams and cheeses from the wholesaler’s truck; late at night we cleaned up and took down the stall. For eighteen hours work I was paid five dollars. I did scrape enough money together to join a social fraternity, Tau Kappa Epsilon. In my junior year I was elected to the engineering honorary society, Tau Beta Pi, and in my senior year I was elected President of the Ohio State Chapter.  My professors at Ohio State solidified my interest in experimental physics. Willard Bennett permitted me to do an undergraduate thesis on the “Focussing of Electron Beams” in his laboratory. From him I learned how different a working laboratory is from a student laboratory. The answers are not known! John Byrne permitted me to work after school hours in the electronic laboratory of the Electrical Engineering Department. I studied the characteristics of the Pentode! It was the best of worlds-the thrills of making real measurements in physics along with practical training in engineering.  On graduation from Ohio State I came to Caltech and became a graduate student under Charles Christian Lauritsen – physicist, engineer, architect and violinist – in the W.K. Kellogg Radiation Laboratory. Kellogg was constructed to Lauritsen’s architectural plans by funds obtained from the American corn flakes king by [Robert Andrews Millikan](https://www.nobelprize.org/nobel_prizes/physics/laureates/1923/index.html). Lauritsen was a native of Denmark and in common with many Scandinavians he loved the songs of Carl Michael Bellman, the 18th century Swedish poet-musician. He tried to teach me to sing Bellman’s drinking songs with a good Swedish accent but I failed miserably except in spirit or should I say spirits. ‘Del Delsasso dubbed me Willy and it stuck’.  Charlie Lauritsen was the greatest influence in my life. He supervised my doctoral thesis on “Radioactive Elements of Low Atomic Number” in which we discovered mirror nuclei and showed that the nuclear forces are charge symmetric-the same between two protons as between two neutrons when charged particle Coulomb forces are excluded. He taught me many practical things-how to repair motors, plumbing, and electrical wiring. Most of all he taught me how to do physics and how to enjoy it. I also learned from my fellow graduate students Richard Crane and Lewis Delsasso. Charlie’s son, Tommy Lauritsen, did his doctoral work under us and the three of us worked together as a team for over thirty-five years. We were primarily experimentalists. In the early days Robert Oppenheimer taught us the theoretical implications of our results. Richard Tolman taught us not to rush into the publication of premature results in those days of intense competition between nuclear laboratories.  [Hans Bethe’s](https://www.nobelprize.org/nobel_prizes/physics/laureates/1967/index.html) announcement of the CN-cycle in 1939 changed our lives. We were studying the nuclear reactions of protons with the isotopes of carbon and nitrogen in the laboratory, the very reactions in the CN-cycle. World War II intervened. The Kellogg Laboratory was engaged in defense research throughout the war. I spent three months in the South Pacific during 1944 as a civilian with simulated military rank. I saw at first hand the heroism of soldiers and seamen and the horrors they endured.  Just before the war I married Ardiane Foy Olmsted whose family came to California over the plains and mountains of the western United States in the Gold Rush around 1850. We are the parents of two daughters, Mary Emily and Martha Summers, whom we refer to as our biblical characters. Martha and her husband, Robert Schoenemann, are the parents of our grandson, Spruce William Schoenemann. They live in Pawlet, a small village in Vermont-the Green Mountain State.  After the war the Lauritsens and I restored Kellogg as a nuclear laboratory and decided to concentrate on nuclear reactions which take place in stars. We called it Nuclear Astrophysics. Before the war Hans Staub and William Stephens had confirmed that there was no stable nucleus at mass 5. After the war Alvin Tollestrup, Charlie Lauritsen and I confirmed that there was no stable nucleus at mass 8. These mass gaps spelled the doom of George Gamow’s brilliant idea that all nuclei heavier than helium (mass 4) could be built by neutron addition one mass unit at a time in his big bang. Edwin Salpeter of Cornell came to Kellogg in the summer of 1951 and showed that the fusion of three helium nuclei of mass four into the carbon nucleus of mass twelve could probably occur in Red Giant stars but not in the big bang. In 1953 Fred Hoyle induced Ward Whaling in Kellogg to perform an experiment which quantitatively confirmed the fusion process under the temperature and density conditions which Hoyle, Martin Schwarzschild and Allan Sandage had shown occur in Red Giants.  Fred Hoyle was the second great influence in my life. The grand concept of nucleosynthesis in stars was first definitely established by Hoyle in 1946. After Whaling’s confirmation of Hoyle’s ideas I became a believer and in 1954/1955 spent a sabbatical year in Cambridge, England, as a Fulbright Scholar in order to work with Hoyle. There Geoffrey and Margaret Burbidge joined us. In 1956 the Burbidges and Hoyle came to Kellogg and in 1957 our joint efforts culminated in the publication of “Synthesis of the Elements in Stars” in which we showed that all of the elements from carbon to uranium could be produced by nuclear processes in stars starting with the hydrogen and helium produced in the big bang. This paper has come to be known from the last initials of the authors as B2FH. A. G. W. Cameron single-handedly came forward with the same broad ideas at the same time.  Fred Hoyle became the Plumian Professor at Cambridge, was knighted by the Queen and founded the Institute of Theoretical Astronomy in Cambridge in 1966. I spent many happy summers at the Institute until Hoyle’s retirement to Cumbria in the Lake District of England. Fred taught me more than astrophysics. He introduced me to English cricket, rugby and association football (we call it soccer). He took me to the Scottish Highlands and taught me how to read an ordnance map as well as how to enjoy climbing the 3000 ft peaks called Munros. I still go climbing somewhere in the British Isles every summer. It keeps me fit and renews my soul.  If has been a long row to hoe. Experimental measurements of the cross section of hundreds of nuclear reactions and their conversion into stellar reaction rates are essential if nucleosynthesis in stars is to be quantitatively confirmed. The Kellogg Laboratory has played a leading role for many years in this effort. I am fortunate that the Nobel Prize was awarded from team work. It is impossible to credit all my colleagues. In experimental nuclear astrophysics Charles Barnes and Ralph Kavanagh have played leading roles. So did Thomas Tombrello and Ward Whaling until they found other fields of interest and promise. In addition Robert Christy and Steven Koonin in theoretical nuclear physics, Jesse Greenstein in observational and theoretical astronomy and Gerald Wasserburg in precision geochemistry on meteoritic and lunar samples have played essential roles. Of my 50 graduate students who have contributed to the field I must single out Donald D. Clayton. His graduate student Stanford Woosley is my grand student and his student Rick Wallace is my great grand student. Nuclear Astrophysics continues to be an active and exciting field. This is clearly evident in my 70th birthday festschrift, “Essays in Nuclear Astrophysics” in which the Cambridge University Press presents the research studies of my colleagues and former students around the world as of 1982. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0107 |
| **Biographical** | I was born 1936 in Waltham, Massachusetts, the son of E. Bright Wilson Jr. and Emily Buckingham Wilson. My father was on the faculty in the Chemistry Department of Harvard University; my mother had one year of graduate work in physics before her marriage. My grandfather on my mother’s side was a professor of mechanical engineering at the Massachusetts Institute of Technology; my other grandfather was a lawyer, and one time Speaker of the Tennessee House of Representatives.  My schooling took place in Wellesley, Woods Hole, Massachusetts (second, third/fourth grades in two years), Shady Hill School in Cambridge, Mass. (from fifth to eighth grade), ninth grade at the Magdalen College School in Oxford, England, and tenth and twelfth grades (skipping the eleventh) at the George School in eastern Pennsylvania. Before the year in England I had read about mathematics and physics in books supplied by my father and his friends. I learned the basic principle of calculus from *Mathematics and Imagination* by Kasner and Newman, and went on to work through a calculus text, until I got stuck in a chapter on involutes and evolutes. Around this time I decided to become a physicist. Later (before entering college) I remember working on symbolic logic with my father; he also tried, unsuccessfully, to teach me group theory. I found high school dull. In 1952 I entered Harvard. I majored in mathematics, but studied physics (both by intent), participated in the Putnam Mathematics competition, and ran the mile for the track team (and crosscountry as well). I began research, working summers at the Woods Hole Oceanographic Institution, especially for Arnold Arons (then based at Amherst).  My graduate studies were carried out at the California Institute of Technology. I spent two years in the Kellogg Laboratory of nuclear physics, gaining experimental experience while taking theory courses; I then worked on a thesis for [Murray Gell-Mann](https://www.nobelprize.org/nobel_prizes/physics/laureates/1969/index.html). While at Cal Tech I talked a lot with Jon Mathews, then a junior faculty member; he taught me how to use the Institute’s computer; we also went on hikes together. I spent a summer at the General Atomic Company in San Diego working with Marshall Rosenbluth in plasma physics. Another summer Donald Groom (then a fellow graduate student) and I hiked the John Muir Trail in the Sierra Nevada from Yosemite Park to Mt. Whitney. After my third year I went off to Harvard to be a Junior Fellow while Gell-Mann went off to Paris. During the first year of the fellowship I went back to Cal Tech for a few months to finish my thesis. There was relatively little theoretical activity at Harvard at the time; I went often to M.I.T. to use their computer and eat lunch with the M.I.T. theory group, led by Francis Low.  In 1962 I went to CERN for a calendar year, first on my Junior Fellowship and then as a Ford Foundation fellow. Mostly, I worked but I found time to join Henry Kendall and James Bjorken on a climb of Mt. Blanc. I spent January through August of 1963 touring Europe.  In September of 1963 I came to Cornell as an Assistant Professor. I received tenure as an Associate Professor in 1965, became Full Professor in 1971 and the James A. Weeks Professor in 1974. I came to Cornell in response to an unsolicited offer I received while at CERN; I accepted the offer because Cornell was a good university, was out in the country and was reputed to have a good folk dancing group, folk-dancing being a hobby I had taken up as a graduate student.  I have remained at Cornell ever since, except for leaves and summer visits: I spent the 1969 – 1970 academic year at the Stanford Linear Accelerator Center, the spring of 1972 at the Institute for Advanced Study in Princeton, the fall of 1976 at the California Institute of Technology as a Fairchild Scholar, and the academic year 1979 – 80 at the IBM Zürich Laboratory.  In 1975 I met Alison Brown and in 1982 we were married. She works for Cornell Computer Services. Together with Douglas Von Houweling, then Director of Academic Computing and Geoffrey Chester of the Physics Department we initiated a computing support project based on a Floating Point Systems Array Processor. I helped write the initial Fortran Compiler for the Array Processor. Since that time I have (aside from using the array processor myself) been studying the role of large scale scientific computing in science and technology and the organizational problems connected with scientific computing. At the present time I am trying to win acceptance for a program of support for scientific computing in universities from industry and government.  I have benefitted enormously from the high quality and selfless cooperation of researchers at Cornell, in the elementary particle group and in materials research; for my research in the 1960’s I was especially indebted to Michael Fisher and Ben Widom.  One other hobby of mine has been playing the oboe but I have not kept this up after 1969.  The home base for my research has been elementary particle theory, and I have made several contributions to this subject: a short distance expansion for operator products presented in an unpublished preprint in 1964 and a published paper in 1969; a discussion of how the renormalization group might apply to strong interactions, in which I discussed all possibilities except the one (asymptotic freedom) now believed to be correct; the formulation of the gauge theory in 1974 (discovered independently by Polyakov), and the discovery that the strong coupling limit of the lattice theory exhibits quark confinement. I am currently interested in trying to solve Quantum Chromodynamics (the theory of quarks) using a combination of renormalization group ideas and computer simulation.  I am also interested in trying to unlock the potential of the renormalization group approach in other areas of classical and modern physics. I have continued to work on statistical mechanics (specifically, the Monte Carlo Renormalization Group, applied to the three dimensional Ising model) as part of this effort. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0108 |
| **Biographical** | My parents, Auke Bloembergen and Sophia Maria Quint, had four sons and two daughters. I am the second child, born on March 11, 1920, in Dordrecht, the Netherlands. My father, a chemical engineer, was an executive in a chemical fertilizer company. My mother, who had an advanced degree to teach French, devoted all her energies to rearing a large family.  Before I entered grade school, the family moved to Bilthoven, a residential suburb of Utrecht. We were brought up in the protestant work ethic, characteristic of the Dutch provinces. Intellectual pursuits were definitely encouraged. The way of life, however, was much more frugal than the family income would have dictated.  At the age of twelve I entered the municipal gymnasium in Utrecht, founded as a Latin school in 1474. Nearly all teachers held Ph.D. degrees. The rigid curriculum emphasized the humanities: Latin, Greek, French, German, English, Dutch, history and mathematics. My preference for science became evident only in the last years of secondary school, where the basics of physics and chemistry were well taught. The choice of physics was probably based on the fact that I found it the most difficult and challenging subject, and I still do to this day. My maternal grandfather was a high school principal with a Ph.D. in mathematical physics. So there may be some hereditary factor as well. I am ever more intrigued by the correspondence between mathematics and physical facts. The adaptability of mathematics to the description of physical phenomena is uncanny.  My parents made a rule that my siblings should tear me away from books at certain hours. The periods of relaxation were devoted to sports: canoing, sailing, swimming, rowing and skating on the Dutch waterways, as well as the competitive team sport of field hockey. I now attempt to keep the body fit by playing tennis, by hiking and by skiing.  Professor L.S. Ornstein taught the undergraduate physics course when I entered the University of Utrecht in 1938. He permitted me and my partner in the undergraduate lab, J.C. Kluyver (now professor of physics in Amsterdam) to skip some lab routines and instead assist a graduate student, G.A. W. Rutgers, in a Ph.D. research project. We were thrilled to see our first publication, “On the straggling of Po-a-particles in solid matter”, in print (*Physica 7*, 669, 1940).  After the German occupation of Holland in May 1940, the Hitler regime removed Ornstein from the university in 1941. I made the best possible use of the continental academic system, which relied heavily on independent studies. I took a beautiful course on statistical mechanics by L. Rosenfeld, did experimental work on noise in photoelectric detectors, and prepared the notes for a seminar on Brownian motion given by J.M.W. Milatz. Just before the Nazis closed the university completely in 1943, I managed to obtain the degree of Phil. Drs., equivalent to a M.Sc. degree. The remaining two dark years of the war I spent hiding indoors from the Nazis, eating tulip bulbs to fill the stomach and reading Kramers’ book “Quantum Theorie des Elektrons und der Strahlung” by the light of a storm lamp. The lamp needed cleaning every twenty minutes, because the only fuel available was some left-over number two heating oil. My parents did an amazing job of securing the safety and survival of the family.  I had always harbored plans to do some research for a Ph.D. thesis outside the Netherlands, to broaden my perspective. After the devastation of Europe, the only suitable place in 1945 appeared to be the United States. Three applications netted an acceptance in the graduate school at Harvard University. My father financed the trip and the Dutch government obliged by issuing a valuta permit for the purchase of US$ 1,850. As my good fortune would have it, my arrival at Harvard occurred six weeks after Purcell, Torrey and Pound had detected nuclear magnetic resonance (NMR) in condensed matter. Since they were busy writing volumes for the M.I.T. Radiation Laboratory series on microwave techniques, I was accepted as a graduate assistant to develop the early NMR apparatus. My thorough Dutch educational background enabled me to quickly profit from lectures by [J. Schwinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html), [J.H. Van Vleck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/index.html), E.C. Kemble and others. The hitherto unexplored field of nuclear magnetic resonance in solids, liquids and gases yielded a rich harvest. The results are laid down in one of the most-cited physics papers, commonly referred to as BPP (N. Bloembergen, E.M. Purcell and R.V. Pound, Phys. Rev. *73*, 679, 1948). Essentially the same material appears in my Ph.D. thesis, “Nuclear Magnetic Relaxation”, Leiden, 1948, republished by W.A. Benjamin, Inc., New York, in 1961. My thesis was submitted in Leiden because I had passed all required examinations in the Netherlands and because C.J. Gorter, who was a visiting professor at Havard during the summer of 1947, invited me to take a postdoctoral position at the Kamerlingh Onnes Laboratorium. My work in Leiden in 1947 and 1948 resulted in establishing the nuclear spin relaxation mechanism by conduction electrons in metals and by paramagnetic impurities in ionic crystals, the phenomenon of spin diffusion, and the large shifts induced by internal magnetic fields in paramagnetic crystals.  During a vacation trip of the Physics Club “Christiaan Huyghens” I met Deli (Huberta Deliana Brink) in the summer of 1948. She had spent the war years in a Japanese concentration camp in Indonesia, where she was born. She was about to start her pre-med studies. When I returned to Harvard in 1949 to join the Society of Fellows, she managed to get on a student hospitality exchange program and traveled after me to the United States on an immigrant ship. I proposed to her the day she arrived and we got married in Amsterdam in 1950. Ever since, she has been a source of light in my life. Her enduring encouragement has contributed immensely to the successes in my further career. After the difficult years as an immigrant wife, raising three children on the modest income of a struggling, albeit tenured, young faculty member, she has found the time and energy to develop her considerable talents as a pianist and artist. We became U.S. citizens in 1958.  Our children are now independent. The older daughter, Antonia, holds M.A. degrees in political science and demography, and works in the Boston area. Our son, Brink, has an M.B.A. degree and is an industrial planner in Oregon. Our younger daughter, Juliana, envisages a career in the financial world. She has interrupted her banking job to obtain an M.B.A. in Philadelphia.  In this family setting my career in teaching and research at Harvard unfolded: Junior Fellow, Society of Fellows 1949 – 1951; Associate Professor 1951- 1957; Gordon McKay Professor of Applied Physics 1957 – 1980; Rumford Professor of Physics 1974 – 1980; Gerhard Gade University Professor 1980 present. While a Junior Fellow, I broadened my experimental background to include microwave spectroscopy and some nuclear physics at the Harvard cyclotron. I preferred the smaller scale experiments of spectroscopy, where an individual, or a few researchers at most, can master all aspects of the problem. When I returned to NMR in 1951, there were still many nuggets to be unearthed. My group studied nuclear quadrupole interactions in alloys and imperfect ionic crystals, discovered the anisotropy of the Knight shift in noncubic metals, the scalar and tensor indirect nuclear spin-spin coupling in metals and insulators, the existence of different temperatures of the Zeeman, exchange and dipolar energies in ferromagnetic relaxation, and a variety of cross relaxation phenomena. All this activity culminated in the proposal for a three-level solid state maser in 1956.  Although I was well aware of the applicability of the multilevel pumping scheme to other frequency ranges, I held the opinion – even after Schawlow and Townes published their proposal for an optical maser in 1958 – that it would be impossible for a small academic laboratory, without previous expertise in optics, to compete successfully in the realization of lasers. This may have been a self-fulfilling prophesy, but it is a matter of record that nearly all types of lasers were first reduced to practice in industrial laboratories, predominantly in the U.S.A.  I recognized in 1961 that my laboratory could exploit some of the new research opportunities made accessible by laser instrumentation. Our group started a program in a field that became known as “Nonlinear Optics”. The early results are incorporated in a monograph of this title, published by W. A. Benjamin, New York, in 1965, and the program is still flourishing today. The principal support for all this work, over a period of more than thirty years, has been provided by the Joint Services Electronics Program of the U. S. Department of Defense, with a minimum amount of administrative red tape and with complete freedom to choose research topics and to publish.  My academic career at Harvard has resulted in stimulating interactions with many distinguished colleagues, and also with many talented graduate students. My coworkers have included about sixty Ph.D. candidates and a similar number of postdoctoral research fellows. The contact with the younger generations keeps the mind from aging too rapidly. The opportunities to participate in international summer schools and conferences have also enhanced my professional and social life. My contacts outside the academic towers, as a consultant to various industrial and governmental organizations, have given me an appreciation for the problems of socio-economic and political origin in the “real” world, in addition to those presented by the stubborn realities of matter and instruments in the laboratory.  Sabbatical leaves from Harvard have made it possible for us to travel farther and to live for longer periods of time in different geographical and cultural environments. Fortunately, my wife shares this taste for travel adventure. In 1957 I was a Guggenheim fellow and visiting lecturer at the École Normale Supérieure in Paris, in 1964 – 1965 visiting professor at the University of California in Berkeley, in 1973 Lorentz guest professor in Leiden and visiting scientist at the Philips Research Laboratories in the Netherlands. The fall of 1979 I spent as Raman Visiting Professor in Bangalore, India, and the first semester of 1980 as Von Humboldt Senior Scientist in the Institut für Quantum Optik, in Garching near Munich, as well as visiting professor at the College de France in Paris. I highly value my international professional and social contacts, including two exchange visits to the Soviet Union and one visit to the People’s Republic of China, each of one-month duration. My wife and I look forward to continuing our diverse activities and to enjoying our home in Five Fields, Lexington, Massachusetts, where we have lived for 26 years. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
| **Interview** |  |
| Q8 | **I have read in your autobiography that you must have been a child with a great desire to learn new things. Your siblings had to drag you away from the books. Was it so? Were you …?** |
|  | Nicolaas Bloembergen: That’s correct, but I liked to read and I always liked challenges. So in fact I chose to study physics because I found it the most difficult topic in my gymnasium, the Latin school I went to in Utrecht. |
| Q10 | **And did that give you what you wanted, obviously, but how was it during your student’s year, did you feel it was a challenge?** |
|  | Nicolaas Bloembergen: It’s still a challenge. I mean, it’s still a difficult topic, but I’m fascinated by the very curious correspondence between mathematics and physical phenomena, and that mathematics can describe so many phenomena with such accuracy. A professor Wigner, [Eugene Wigner](https://www.nobelprize.org/prizes/physics/1963/wigner/facts/), a famous theoretician, he called that connection between mathematics and the real world uncanny. |
| Q12 | **You talk very nicely about your parents in your autobiography as well, the way they were trying to keep you children in good health and so on. Would you describe a little bit about that?** |
|  | Nicolaas Bloembergen: My father went by bicycle and a little cart to get potatoes to feed his family, and he travelled for 50 kilometres or more. And then he exchanged objects for food, and so they did a wonderful job keeping the family alive. |
| Q9 | **Yes. When you look at what you have achieved with your work and the way the laser, and its various application, the way it’s used today, what makes you most proud?** |
|  | Nicolaas Bloembergen: I’ve been lucky that the two topics that have interested me have both lead to very important applications. In the case of nuclear magnetic resonance, the thesis data, in my thesis, concerned what is called nuclear magnetic relaxation in liquids and also some solid materials, but most importantly we measured the relaxation time of protons in water and aqueous solutions, the influence of viscosity and temperature, and those data are now the basis in which MRI pictures can be taken, because the nuclear magnetic moments of protons which are water molecules, it’s about 70% of the body, water, they measure small differences in relaxation time between healthy cells and tumorous cells and whatnot. And in blood vessels. So we are, my data was really very basic, to an application I didn’t foresee, nor did my thesis supervisor, [Purcell](https://www.nobelprize.org/prizes/physics/1952/purcell/facts/), in fact nobody had an idea that MRI would come even as late as 1960. |
| Q18 | **On one of your lectures that I listened in to you also told us about other uses of the laser that I was not totally aware of. Would you just mention a few that might not be so known to the public in general?** |
|  | Nicolaas Bloembergen: As I said, the second item was going into optics and especially non-linear optics, which is the behaviour of light and propagation of light in media at very high light intensities. Those high intensities are only available from laser sources. So I was really interested in what one can do with lasers and I mentioned in my [lecture](https://www.nobelprize.org/prizes/physics/1981/bloembergen/lecture/) that lasers are heavily used in surgery and in optical communications systems. Those are the two large scale applications that affect many people, everywhere in the world, because the optical fibre communications systems which, incidentally, use little lasers of … produced by [INAUDIBLE] inside the fibre, make the world very small, and we can now e-mail to anybody, anywhere in the world, we can dial up the worldwide web and all this information flows over large distances under the Pacific and Atlantic ocean, if necessary, to other points on the earth.  So those are the two big ones, and there are smaller and a little more trivial ones, like read out at supermarket checkout counters. You know, you have a code on each article, it’s read by a little laser and so even with the ambient light it doesn’t matter, the reflection on the code gives the information of what article and how much it cost. And the other, which seems rather trivial, but is very important, is to use laser beams which propagate straight lines over long distances. They are used to lay pipelines, including trivial things like sewer lines. I lived in a suburban neighbourhood and there were no sewers yet, and then in 1972 they laid out the sewer line with laser beams. That saves a lot of manpower, you need only one person with a mirror somewhere to do it. But of course, all the major oil pipelines and gas pipelines, they are all laid out by laser beams, and any big building that goes up, the verticals and horizontals are all checked with laser beams. So in the constructing industry it is a very widespread use, too. |
| Q18 | **Did you think it would have such an impact?** |
|  | Nicolaas Bloembergen: No. For several years, in the early 1960s, my colleague, [Art Schawlow](https://www.nobelprize.org/prizes/physics/1981/schawlow/facts/), with whom I shared the prize in 1981, he said the laser is a solution looking for a problem. He had a sense of humour. And that was really true, and all the applications really came gradually in the next decade. |
| Q18 | **Have you had any thoughts about whether scientists have any responsibility …?** |
|  | Nicolaas Bloembergen: Certainly, but what do you do? You know, science itself is basically neutral, and then you get the problem, should I refuse to help to defend my country? So I was advising government committees on the use of laser beams in trying to shoot down intercontinental missiles. And soon thereafter we realised that it would be very difficult, and then 20 years later in 1981 President Reagan instituted the strategic defence initiative and the idea was to put lasers in the upper atmosphere and just beyond in space, and try to shoot down incoming missiles, supposedly, which might be fired by the Soviet Union. And then we wrote a report because everybody felt that that would not be feasible. Neither scientifically nor technologically.  But many people were afraid to express, well, they expressed their opinion, they weren’t afraid to express their opinion, but then they got the answer you haven’t studied the problem. So the American Physics Society said we’ll go and make a scientific study of the issues involved and then we’ll come out with a public report. But they selected 14 or 15 people and I and Kumar Patel were co-chairmen of that committee and the reason these people were chosen is because they had never gone very open in public of what their political opinions were. And we were not supposed to give any political opinion, just as scientific, purely scientific evaluation. |
| Q10 | **You have travelled wide and far and have had lots of contacts with universities all over the world and with other scientists all over the world. What has it given you, and why has it been important to you to be so global, so international?** |
|  | Nicolaas Bloembergen: Well, a university like Harvard University attracts many foreign students and the brightest of them, and I find it interesting to see people from … you know, I myself was an immigrant, into the United States, and so I had many students from all parts of the world, and it’s nice to visit them later and see how they are doing in their respective careers. So we always enjoyed having an international community in the laboratory, involving Frenchmen, German, Norwegian, Italy, and people from the East, Japan, and especially China, different parts of China, some Taiwan, some Hong Kong, some mainland China, very interesting. |
| Q3 | **Is it important to work with young people, I mean, to keep one’s spirit, one’s creativity, do you enjoy that?** |
|  | Nicolaas Bloembergen: Yes, I mean that is the only way not to go to sleep. During old age. No, that is very important to have this. That I think is the most important aspect of this meeting here in Lindau, that there are all these young people to interact with, too. |
| Q5 | **Next year it will be the year of**[**Einstein**](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/)**. What relationship do you have to him, and why is it important to highlight him, if you look in a broader sense to the public in general?** |
|  | Nicolaas Bloembergen: It’s very important to take opportunities to involve the public with science and the centenary of Einstein papers is certainly a worthy excuse to focus attention on scientific efforts. But Einstein wrote a popular book, Einstein and Infeld, I forgot the title, I read it in German and it was originally written in German, and I was in high school, gymnasium and that fascinated me very much, and he explained in simple terms the ideas behind the relativity theory. |
| Q22 | **Is it difficult in general do you think for the public at large to understand the need for scientific research?** |
|  | Nicolaas Bloembergen: Weill it is, but you have to ask them what would you do without computers, without worldwide web, without, you know, cell phones, and so on. Or even without electricity, yeah? Let’s go back further. And it was all started by physicists. |

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| **Physics\_1999-** | |
| **ID** | 0109 |
| **Biographical** | I was born in Mount Vernon, New York, U.S.A. on May 5, 1921. My father had come from Europe a decade earlier. He left his home in Riga to study electrical engineering at Darmstadt, but arrived too late for the beginning of the term. Therefore, he went on to visit his brother in New York, and never returned either to Europe or to electrical engineering. My mother was a Canadian and, at her urging, the family moved to Toronto in 1924. I attended public schools there, Winchester elementary school, the Normal Model School attached to the teacher’s college, and Vaughan Road Collegiate Institute (high school).  As a boy, I was always interested in scientific things, electrical, mechanical or astronomical, and read nearly everything that the library could provide on these subjects. I intended to try to go to the University of Toronto to study radio engineering, and my parents encouraged me. Unfortunately my high school years, 1932 to 1937, were in the deepest part of the great economic depression. My father’s salary as one of the many agents for a large insurance company could not cover the cost of a college education for my sister, Rosemary, and me. Indeed, at that time few high school graduates continued their education. Only three or four out of our high school class of sixty or so students were able to go to a university.  There were, at that time, no scholarships in engineering, but we were both fortunate enough to win scholarships in the faculty of Arts of the University of Toronto. My sister’s was for English literature, and mine was for mathematics and physics. Physics seemed pretty close to radio engineering, and so that was what I pursued. It now seems to me to have been a most fortunate chance, for I do not have the patience with design details that an engineer must have. Physics has given me a chance to concentrate on concepts and methods, and I have enjoyed it greatly.  With jobs as scarce as they were in those years, we had to have some occupation in mind to justify college studies. A scientific career was something that few of us even dreamed possible, and nearly all of the entering class expected to teach high school mathematics or physics. However, before we graduated in 1941 Canada was at war, and all of us were involved in some way. I taught classes to armed service personnel at the University of Toronto until 1944, and then worked on microwave antenna development at a radar factory.  In 1945, graduate studies could resume, and I returned to the University. It was by then badly depleted in staff and equipment by the effects of the depression and the war, but it did have a long tradition in optical spectroscopy. There were two highly creative physics professors working on spectroscopy, Malcolm F. Crawford and Harry L. Welsh. I took courses from both of them, and did my thesis research with Crawford. It was a very rewarding experience, for he gave the students good problems and the freedom to learn by making our own mistakes. Moreover, he was always willing to discuss physics, and even to speculate about where future advances might be found.  A Carbide and Carbon Chemicals postdoctoral fellowship took me to Columbia University to work with Charles H. Townes. What a marvelous place Columbia was then, under [I.I. Rabi’s](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/index.html) leadership! There were no less than eight future Nobel laureates in the physics department during my two years there. Working with [Charles Townes](https://www.nobelprize.org/nobel_prizes/physics/laureates/1964/index.html) was particularly stimulating. Not only was he the leader in research on microwave spectroscopy, but he was extraordinarily effective in getting the best from his students and colleagues. He would listen carefully to the confused beginnings of an idea, and join in developing whatever was worthwhile in it, without ever dominating the discussions. Best of all, he introduced me to his youngest sister, Aurelia, who became my wife in 1951.  From 1951 to 1961, I was a physicist at Bell Telephone Laboratories. There my research was mostly on superconductivity, with some studies of nuclear quadrupole resonance. On weekends I worked with Charles Townes on our book *Microwave Spectroscopy*, which had been started while I was at Columbia and was published in 1955. In 1957 and 1958, while mainly still continuing experiments on superconductivity, I worked with Charles Townes to see what would be needed to extend the principles of the maser to much shorter wavelengths, to make an optical maser or, as it is now known, a laser. Thereupon, I began work on optical properties and spectra of solids which might be relevant to laser materials, and then on lasers.  Since 1961, I have been a professor of physics at Stanford University and was chairman of the department of physics from 1966 to 1970. In 1978 I was appointed J.G. Jackson and C.J. Wood Professor of Physics. At Stanford, it has been a pleasure to do physics with an outstanding group of graduate students, occasional postdoctoral research associates and visitors. Most especially the interaction with Professor Theodor W. Hansch has been continually delightful and stimulating. Our technicians, Frans Alkemade and Kenneth Sherwin have been invaluable in constructing apparatus and keeping it in operation. My secretary for the past nineteen years, Mrs. Fred – a Jurian, provides whatever order that can be found amidst the chaos of my office. Much of the time, my thoughts are stimulated there by the sounds of traditional jazz from my large record collection.  My wife is a musician, a mezzo soprano and choral conductor. We have a son, Arthur Keith, and two daughters, Helen Aurelia and Edith Ellen. Helen has studied French literature at Stanford, the Sorbonne, and at the University of California in Berkeley, and is now on the staff of Stanford University. Edith graduated from Stanford this year with a major in psychology.  **Addendum, 1991**  I retired from teaching and became Professor Emeritus in 1991. My wife died in an automobile accident in May, 1991. My daughter Helen is now Assistant Professor of French at the University of Wisconsin. From Helen and her sister Edith, I now have four grandchildren. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0110 |
| **Biographical** | Born April 20, 1918, in Lund, Sweden. Parents: [Manne Siegbahn](https://www.nobelprize.org/nobel_prizes/physics/laureates/1924/index.html) and Karin Högbom. Married May 23, 1944, to Anna Brita Rhedin. Three children: Per (1945), Hans (1947) and Nils (1953). Attended the Uppsala Gymnasium; Studied physics, mathematics and chemistry at the University of Uppsala from 1936 until 1942. Graduated in Stockholm 1944. Docent in physics that year. Research associate at the Nobel Institute for Physics 1942 – 1951. Professor of physics at the Royal Institute of Technology in Stockholm from 1951 to 1954. Professor and head of the Physics Department at the University of Uppsala since 1954. Member of the [Royal Swedish Academy of Sciences](http://www.kva.se/), Royal Swedish Academy of Engineering Sciences, Royal Society of Science, Royal Academy of Arts and Science of Uppsala, Royal Physiographical Society of Lund, Societas Scienti arum Fennica, Norwegian Academy of Science, Royal Norwegian Society of Sciences and Letters, Honorary Member of the American Academy of Arts and Sciences, Membre du Comite International des Poids et Mesures, Paris, President of the International Union of Pure and Applied Physics (IUPAP). |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0111 |
| **Biographical** | I was born on September 29, 1931 in Chicago, Illinois, while my father, James Farley Cronin, was a graduate student at the University of Chicago. He was a student of classical languages. My mother, Dorothy Watson, had met my father in a Greek class at Northwestern University. After a brief stay at a small school in Alabama, my father became Professor of Latin and Greek at Southern Methodist University in Dallas, Texas, in September 1939. My primary and secondary education was provided by the Highland Park Public School System. I received my undergraduate degree from Southern Methodist University with a major in physics and mathematics in 1951. In high school my natural interest in science was encouraged by an excellent physics teacher, Mr. Charles H. Marshall. He stressed analytical methods as applied to simple physical systems as well as practical experimental problems.  My real education began when I entered the University of Chicago in September 1951 as a graduate student. I was fortunate to have among my classroom teachers, [Enrico Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html), [Maria Mayer](https://www.nobelprize.org/nobel_prizes/physics/laureates/1963/index.html), Edward Teller, Gregor Wentzel, Val Telegdi, Marvin Goldberger and [Murray Gell-Mann](https://www.nobelprize.org/nobel_prizes/physics/laureates/1969/index.html). I did a thesis in experimental nuclear physics under the direction of Samuel K. Allison. While at Chicago my interest in the new field of particle physics was stimulated by a course given by Gell- Mann, who was developing his ideas about Strangeness at the time.  It was also at the University of Chicago that I met my future wife, Annette Martin, in the summer of 1953. It was a wonderful, happy summer; I had passed my Ph.D. qualifying exams the previous winter, and I realized that I had met my lifetime companion. We were married in September 1954. The stable point in my life became our home. On even the worst days, when nothing was working at the lab, I knew that at home I would find warmth, peace, companionship, and encouragement. As a consequence, the next day would surely be better. Annette, with great patience and good spirit, tolerated my many long absences when experiments were carried out at distant laboratories.  After receiving my Ph.D. in 1955 I had the opportunity to join the group of Rodney Cool and Oreste Piccioni who were working at the Brookhaven Cosmotron, a newly completed 3 GeV accelerator. That period was an exciting time in physics. The famous tau-teta puzzle led to the prediction of parity violation and the experimental demonstration of its violation. The long-lived K meson was discovered at Brookhaven.  When the violation of parity was discovered I began a series of electronic experiments to investigate parity violation in hyperon decays. In early 1958 the Cosmotron suffered a severe magnet failure. As a consequence, we moved our experiment to the Berkeley Bevatron. Here I had the good fortune to meet William Wenzel and Bruce Cork. These physicists had a great influence on me. From their example I learned not to be intimidated by complex pieces of apparatus.  While at Brookhaven I met Val Fitch who was responsible for my coming to Princeton University in the fall of 1958. At Princeton all the work in particle physics was supported through a contract with the Office of Naval Research. The Director of the Laboratory, George Reynolds, was most supportive of my efforts to work independently. There followed for ten years a glorious time for research. I was much involved in the development of the spark chamber as a practical research tool. During this period, with a series of excellent students, we further studied hyperon decays. Then we joined with Val Fitch to study neutral K meson decays which led to the discovery of CP violation.  Following the discovery in the summer of 1964, I spent a year in France working at the Centre d’Etudes Nucleaires at Saclay with Rene Turlay. In addition to the research, I enjoyed learning French and assimilating the culture of another country. One of the greatest joys in my life was giving a lecture in French at the College de France.  On returning to Princeton in 1965, I began with students a series of experiments to study the neutral CP violating decay modes of the long lived neutral K meson. These experiments lasted until 1971. In 1971 I returned to the University of Chicago as Professor of Physics. The fact that the new Fermilab 400 GeV Accelerator was being built near Chicago made this move an attractive one. At Fermilab, with younger associates and students, I carried out experiments on the production of particles at high transverse momentum, and on the production of direct leptons. At present with my colleague at Chicago, Bruce Winstein, I am preparing to study with much greater accuracy some of the CP violating parameters of the neutral K meson.  I now live in Chicago near the campus with my wife Annette, and son Daniel. My oldest daughter Cathryn lives and works in New York City. My daughter Emily attends the University of Minnesota. My mother remained in Dallas, Texas, after the death of my father in 1959. For recreation we have a cabin in the woods in Wisconsin which we visit year-round. In the summer we spend some time in Aspen, Colorado. Our whole family assembles in Chicago at Christmas and usually in Aspen in the summer. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0112 |
| **Biographical** | I was born the youngest of three children, on a cattle ranch in Cherry County, Nebraska, not far from the South Dakota border, on March 10, 1923. This is a very sparsely populated part of the United States and remote from any center of population. It seems incredible by modern standards that by the age of 20 my father, Fred Fitch, had acquired a ranch of more than 4 square miles and had persuaded a local school teacher, Frances Logsdon, to marry and join him in living there. They moved to the ranch just 20 years after the battle of Wounded Knee, which occurred about 40 miles northwest. I mention this because our living close to their reservation made the Sioux Indians very much a part of our environment. My father, while not fluent, spoke their language. They recognized his friendly interest on their behalf by making him an honorary chief.  Not long after my birth my father was badly injured when a horse he was riding fell with him. He subsequently had to give up the physically strenuous activity associated with running a ranch and raising cattle. The family moved to Gordon, Nebraska, a town about 25 miles away, where my father entered the insurance business. All of my formal schooling through high school was in the public schools of Gordon. During this period my parents retained ownership of the ranch but the operation was largely left to others. E.B. White has defined farming as 10% agriculture and 90% fixing something that has gotten broken. My memories of ranching are primarily not the romantic ones of rounding up and branding cattle but rather of oiling windmills and fixing fences.  Probably the most significant occurrence in my education came when, as a soldier in the U.S. Army in WWII, I was sent to Los Alamos, New Mexico, to work on the Manhattan Project. The work I did there under the direction of Ernest Titterton, a member of the British Mission, was highly stimulating. The laboratory was small and even as a technician garbed in a military fatigue uniform I had the opportunity to meet and see at work many of the great figures in physics: [Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html), [Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1975/index.html), [Chadwick](https://www.nobelprize.org/nobel_prizes/physics/laureates/1935/index.html), [Rabi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/index.html), Tolman. I have recorded some of the experiences from those days in a chapter in *All in Our Time*, a book edited by Jane Wilson and published by the Bulletin of Atomic Scientists. I spent 3 years at Los Alamos and in that period learned well the techniques of experimental physics. I observed that the most accomplished experimentalists were also the ones who knew most about electronics and electronic techniques were the first I learned. But mainly I learned, in approaching the measurement of new phenomena, not just to consider using existing apparatus but to allow the mind to wander freely and invent new ways of doing the job.  Robert Bacher, the leader of the physics division in which I worked, offered me a graduate assistantship at Cornell after the war but I still had to finish the work for an undergraduate degree. This I did at McGill University. And then another opportunity for graduate work came from Columbia and I ended up there working with [Jim Rainwater](https://www.nobelprize.org/nobel_prizes/physics/laureates/1975/index.html) for my Ph.D. thesis. One day in his of fice, which he shared at the time with Aage Bohr, he handed me a preprint of a paper by John Wheeler devoted to µ-mesic atoms. This paper emphasized, in the case of the heavier nuclei, the extreme sensitivity of the Is level to the size of the nucleus. Even though the radiation from these atoms had never been observed, these atomic systems might be a good thesis topic. At this same time a convergence of technical developments took place. The Columbia Nevis cyclotron was just coming into operation. The beams of (pi)-measons from the cyclotron contained an admixture of µ-measons which came frome the decay of the (pi)’s and which could be separated by range. Sodium iodide with thallium activation had just been shown by [Hofstadter](https://www.nobelprize.org/nobel_prizes/physics/laureates/1961/index.html) to be an excellent scintillation counter and energy spectrometer for gamma rays. And there were new phototubes just being produced by RCA which were suitable matches to sodium iodide crystals to convert the scintillations to electrical signals. The other essential ingredient to make a gamma-ray spectrometer was a multichannel pulse height analyzer which, utilizing my Los Alamos experience, I designed and built with the aid of a technician. The net result of all the effort for my thesis was the pioneering work on µ-mesic atoms. It is of interest to note that we came very close to missing the observation of the gamma-rays completely. Wheeler had calculated the 2p-1s transition energy in Pb, using the then accepted nuclear radius 1.4 A1/3 fermi, to be around 4.5 MeV. Correspondingly, we had set our spectrometer to look in that energy region. After several frustrating days, Rainwater suggested we broaden the range and then the peak appeared – not at 4.5 MeV but at 6 MeV! The nucleus was substantially smaller than had been deduced from other effects. Shortly afterwards Hofstadter got the same results from his electron scattering experiments. While the µ-mesic atom measurements give the rms radius of the nucleus with extreme accuracy the electron scattering results have the advantage of yielding many moments to the charge distribution. Now the best information is obtained by combining the results from both µ-mesic atoms and electron scattering.  Subsequently, in making precise gamma-ray measurements to obtain a better mass value for the µ-meson, we found that substantial corrections for the vacuum polarization were required to get agreement with independent mass determinations. While the vacuum polarization is about 2% of the Lamb shift in hydrogen it is the very dominant electrodynamic correction in µ-mesic atoms.  My interest then shifted to the strange particles and K mesons but I had learned from my work at Columbia the delights of unexpected results and the challenge they present in understanding nature. I took a position at Princeton where, most often working with a few graduate students, I spent the next 20 years studying K-mesons. The ultimate in unexpected results was that which was recognized by the Nobel Foundation in 1980, the discovery of CP-violation.  At any one time there is a natural tendency among physicists to believe that we already know the essential ingredients of a comprehensive theory. But each time a new frontier of observation is broached we inevitably discover new phenomena which force us to modify substantially our previous conceptions. I believe this process to be unending, that the delights and challenges of unexpected discovery will continue always.  It is highly improbable, a priori, to begin life on a cattle ranch and then appear in Stockholm to receive the Nobel Prize in physics. But it is much less improbable to me when I reflect on the good fortune I have had in the ambiance provided by my parents, my family, my teachers, colleagues and students. I have two sons from my marriage to Elise Cunningham who died in 1972. In 1976 I married Daisy Harper who brought with her three stepchildren into my life.  **Honors and Distinctions**I am a fellow of the American Physical Society and the American Association for the Advancement of Science, a member of the American Academy of Arts and Sciences and the National Academy of Sciences. I hold the Cyrus Fogg Brackett Professorship of Physics at Princeton University and since 1976 have served as chairman of the Physics Department. I received the E. O. Lawrence award in 1968. In 1967 Jim Cronin and I received the Research Corporation award for our work on CP violation and in 1976 the John Price Witherill medal of the Franklin Institute. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0113 |
| **Biographical** | My parents, Lewis Glashow and Bella née Rubin immigrated to New York City from Bobruisk in the early years of this century. Here they found the freedom and opportunity denied to Jews in Czarist Russia. After years of struggle, my father became a successful plumber, and his family could then enjoy the comforts of the middle class. While my parents never had the time or money to secure university education themselves, they were adamant that their children should. In comfort and in love, we were taught the joys of knowledge and of work well done. I only regret that neither my mother nor my father could live to see the day I would accept the Nobel Prize.  When I was born in Manhattan in 1932, my brothers Samuel and Jules were eighteen and fourteen years old. They chose careers of dentistry and medicine, to my parents’ satisfaction. From an early age, I knew I would become a scientist. It may have been my brother Sam’s doing. He interested me in the laws of falling bodies when I was ten, and helped my father equip a basement chemistry lab for me when I was fifteen. I became skilled in the synthesis of selenium halides. Never again would I do such dangerous research. Except for the occasional suggestion that I should become a physician and do science in my spare time, my parents always encouraged my scientific inclinations.  Among my chums at the Bronx High School of Science were Gary Feinberg and Steven Weinberg. We spurred one another to learn physics while commuting on the New York subway. Another classmate, Dan Greenberger, taught me calculus in the school lunchroom. High-school mathematics then terminated with solid geometry. At Cornell University, I again had the good fortune to join a talented class. It included the mathematician Daniel Kleitman who was to become my brother-in-law, my old classmate Steven Weinberg, and many others who were to become prominent scientists. Throughout my formal education, I would learn as much from my peers as from my teachers. So it is today among our graduate students.  I came to graduate school at Harvard University in 1954. My thesis supervisor, [Julian Schwinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html), had about a dozen doctoral students at a time. Getting his ear was as difficult as it was rewarding. I called my thesis “The Vector Meson in Elementary Particle Decays”, and it showed an early commitment to an electroweak synthesis. When I completed my work in 1958, Schwinger and I were to write a paper summarizing our thoughts on weak-electromagnetic unification. Alas, one of us lost the first draft of the manuscript, and that was that.  I won an NSF postdoctoral fellowship, and planned to work at the Lebedev Institute in Moscow with [I. Tamm](https://www.nobelprize.org/nobel_prizes/physics/laureates/1958/index.html), who enthusiastically supported my proposal. I spent the tenure of my fellowship in Copenhagen at the Niels Bohr Institute (and, partly, at CERN), waiting for the Russian visa that was never to come. Perhaps all was for the best, because it was in these years (1958-60) that I discovered the SU(2) x U(1) structure of the electroweak theory. Interestingly, it was also in Copenhagen that my early work on charm with Bjorken was done. This was during a brief return to Denmark in 1964.  During my stay in Europe, I was “discovered” by [Murray Gell-Mann](https://www.nobelprize.org/nobel_prizes/physics/laureates/1969/index.html). He presented my ideas on the algebraic structure of weak interactions to the 1960 “Rochester meeting” and brought me to Caltech. Then, he invented the eightfold way, which kept Sidney Coleman and me distracted for several years. How we found various electromagnetic formulae, yet missed the discovery of the Gell-Mann-Okubo formula and of the Cabibbo current is another story.  I became an assistant professor at Stanford University and then spent several years on the faculty of the University of California at Berkeley. During this time, I continued to exploit the phenomenological successes of flavor SU(3) and attempted to understand the departures from exact symmetry as a consequence of spontane23ous symmetry breakdown. I returned to Harvard University in 1966 where I have remained except for leaves to CERN, MIT, and the University of Marseilles. Today, I am Eugene Higgins Professor of Physics at Harvard.  In 1969, John Iliopoulos and Luciano Maiani came to Harvard as research fellows. Together, we found the arguments that predicted the existence of charmed hadrons. Much of my later work was done in collaboration with Alvaro de Rujúla or Howard Georgi. In early 1974, we predicted that charm would be discovered in neutrino physics or in e+ e– annihilation. So it was. With the discovery of the J/Psi particle, we realized that many diverse strands of research were converging on a single theory of physics. I remember once saying to Howard that if QCD is so good, it should explain the Sigma-Lambda mass splitting. The next day he showed that it did. When we spoke, in 1974, of the unification of all elementary particle forces within a simple gauge group, and of the predicted instability of the proton, we were regarded as mad. How things change!  The wild ideas of yesterday quickly become today’s dogma. This year I have been honored to participate in the inauguration of the Harvard Core Curriculum Program. My students are not, and will never be, scientists. Nonetheless, in my course “From Alchemy to Quarks” they seem to be as fascinated as I am by the strange story of the search for the ultimate constituents of matter.  I was married in 1972 to the former Joan Alexander. We live in a large old house with our four children, who attend the Brookline public schools. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0114 |
| **Biographical** | Abdus Salam was born in Jhang, a small town in what is now Pakistan, in 1926. His father was an official in the Department of Education in a poor farming district. His family has a long tradition of piety and learning.  When he cycled home from Lahore, at the age of 14, after gaining the highest marks ever recorded for the Matriculation Examination at the University of the Punjab, the whole town turned out to welcome him. He won a scholarship to Government College, University of the Punjab, and took his MA in 1946. In the same year he was awarded a scholarship to St. John’s College, Cambridge, where he took a BA (honours) with a double First in mathematics and physics in 1949. In 1950 he received the Smith’s Prize from Cambridge University for the most outstanding pre-doctoral contribution to physics. He also obtained a PhD in theoretical physics at Cambridge; his thesis, published in 1951, contained fundamental work in quantum electrodynamics which had already gained him an international reputation.  Salam returned to Pakistan in 1951 to teach mathematics at Government College, Lahore, and in 1952 became head of the Mathematics Department of the Punjab University. He had come back with the intention of founding a school of research, but it soon became clear that this was impossible. To pursue a career of research in theoretical physics he had no alternative at that time but to leave his own country and work abroad. Many years later he succeeded in finding a way to solve the heartbreaking dilemma faced by many young and gifted theoretical physicists from developing countries. At the ICTP, Trieste, which he created, he instituted the famous “Associateships” which allowed deserving young physicists to spend their vacations there in an invigorating atmosphere, in close touch with their peers in research and with the leaders in their own field, losing their sense of isolation and returning to their own country for nine months of the academic year refreshed and recharged.  In 1954 Salam left his native country for a lectureship at Cambridge, and since then has visited Pakistan as adviser on science policy. His work for Pakistan has, however, been far-reaching and influential. He was a member of the Pakistan Atomic Energy Commission, a member of the Scientific Commission of Pakistan and was Chief Scientific Adviser to the President from 1961 to 1974.  Since 1957 he has been Professor of Theoretical Physics at Imperial College, London, and since 1964 has combined this position with that of Director of the ICTP, Trieste.  For more than forty years he has been a prolific researcher in theoretical elementary particle physics. He has either pioneered or been associated with all the important developments in this field, maintaining a constant and fertile flow of brilliant ideas. For the past thirty years he has used his academic reputation to add weight to his active and influential participation in international scientific affairs. He has served on a number of United Nations committees concerned with the advancement of science and technology in developing countries.  To accommodate the astonishing volume of activity that he undertakes, Professor Salam cuts out such inessentials as holidays, parties and entertainments. Faced with such an example, the staff of the Centre find it very difficult to complain that they are overworked.  He has a way of keeping his administrative staff at the ICTP fully alive to the real aim of the Centre – the fostering through training and research of the advancement of theoretical physics, with special regard to the needs of developing countries. Inspired by their personal regard for him and encouraged by the fact that he works harder than any of them, the staff cheerfully submit to working conditions that would be unthinkable here at the ([International Atomic Energy Agency in Vienna (IAEA)](https://www.nobelprize.org/nobel_prizes/peace/laureates/2005/index.html). The money he received from the Atoms for Peace Medal and Award he spent on setting up a fund for young Pakistani physicists to visit the ICTP. He uses his share of the Nobel Prize entirely for the benefit of physicists from developing countries and does not spend a penny of it on himself or his family.  Abdus Salam is known to be a devout Muslim, whose religion does not occupy a separate compartment of his life; it is inseparable from his work and family life. He once wrote: “The Holy Quran enjoins us to reflect on the verities of Allah’s created laws of nature; however, that our generation has been privileged to glimpse a part of His design is a bounty and a grace for which I render thanks with a humble heart.”  *The biography was written by Miriam Lewis, now at IAEA, Vienna, who was at one time on the staff of ICTP (International Centre For Theoretical Physics, Trieste).* |
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| **Physics\_1999-** | |
| **ID** | 0115 |
| **Biographical** | I was born in 1933 in New York City to Frederick and Eva Weinberg. My early inclination toward science received encouragement from my father, and by the time I was 15 or 16 my interests had focused on theoretical physics.  I received my undergraduate degree from Cornell in 1954, and then went for a year of graduate study to the Institute for Theoretical Physics in Copenhagen (now the Niels Bohr Institute). There, with the help of David Frisch and Gunnar Källén. I began to do research in physics. I then returned to the U.S. to complete my graduate studies at Princeton. My Ph.D thesis, with Sam Treiman as adviser, was on the application of renormalization theory to the effects of strong interactions in weak interaction processes.  After receiving my Ph.D. in 1957, I worked at Columbia and then from 1959 to 1966 at Berkeley. My research during this period was on a wide variety of topics – high energy behavior of Feynman graphs, second-class weak interaction currents, broken symmetries, scattering theory, muon physics, etc. – topics chosen in many cases because I was trying to teach myself some area of physics. My active interest in astrophysics dates from 1961-62; I wrote some papers on the cosmic population of neutrinos and then began to write a book, *Gravitation and Cosmology*, which was eventually completed in 1971. Late in 1965 I began my work on current algebra and the application to the strong interactions of the idea of spontaneous symmetry breaking.  From 1966 to 1969, on leave from Berkeley, I was Loeb Lecturer at Harvard and then visiting professor at M.I.T. In 1969 I accepted a professorship in the Physics Department at M.I.T., then chaired by Viki Weisskopf. It was while I was a visitor to M.I.T. in 1967 that my work on broken symmetries, current algebra, and renormalization theory turned in the direction of the unification of weak and electromagnetic interactions. In 1973, when Julian Schwinger left Harvard, I was offered and accepted his chair there as Higgins Professor of Physics, together with an appointment as Senior Scientist at the Smithsonian Astrophysical Observatory.  My work during the 1970’s has been mainly concerned with the implications of the unified theory of weak and electromagnetic interactions, with the development of the related theory of strong interactions known as quantum chromodynamics, and with steps toward the unification of all interactions.  In 1982 I moved to the physics and astronomy departments of the University of Texas at Austin, as Josey Regental Professor of Science. I met my wife Louise when we were undergraduates at Cornell, and we were married in 1954. She is now a professor of law. Our daughter Elizabeth was born in Berkeley in 1963. |
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| Q2 | **Welcome to Stockholm, and to this Nobel interview, Professor Steven Weinberg. I have talked to some of your colleagues here, and everybody says that you’re the one to blame, or the one that brought light to the community of physicists when you wrote your book on cosmology in the early 1970s, called *Gravitation and Cosmology*. So how did you get interested in cosmology?** |
|  | Steven Weinberg: I don’t really think there is anyone who isn’t interested in cosmology. If you go out and night and look at the stars it’s inevitable that you wonder what all this is. For me, it was a fantastic discovery when I was a young professor, just beginning, that there was a mathematical theory that could be applied to the whole universe. It had been worked out in the 1920s and the 1930s, and the theory of the whole universe was something I had to learn about, so I taught courses at Berkeley on the subject. Gradually I learned enough so that I wanted to put it all together in a book of my own, looking at things in my own way. |
| Q3 | **How come you came into this field, into physics?** |
|  | Steven Weinberg: It started with chemistry. When I was young I had a cousin who had been given a chemistry set. This is a toy, with chemicals, and test tubes that you play with, and he lost interest in it. He went into professional boxing. Perhaps he should have stayed in science, but anyway, the chemistry set came down to me, and I loved it, especially that beautiful wooden box that it came in. I loved playing with chemicals, and learned a little chemistry, of course. You always learn a little bit when you play with these things. I learned that all chemicals behave the way they do because of atoms, and then I wanted to learn about atoms. That was difficult because there was apparently a mysterious theory called quantum mechanics that had been developed in the 1920s. I read popular books by people like George Gamow and James Jeans, and I got very excited, not because I began to understand it, but because it seemed incomprehensible. And I thought if someone … |
| Q4 | **You have also written a popular science book on cosmology called *The First Three Minutes*, which made cosmology maybe more comprehensible for the general public. This was more than 20 years ago. What has changed since then?** |
|  | Steven Weinberg: The field has grown so much. I think this is a golden age now for cosmology. There are observations of not only that there is a radio background, this three degree radiation, but there are faint ripples in it that give evidence of conditions when the universe was a few hundred thousand years old, and our knowledge is getting more and more detailed. Also, there is now much more evidence about how the universe is expanding. It seems that the expansion at first was speeding up, then slowing down and now it’s beginning to speed up again. We have a theory inflation that describes what happened at the very earliest times, which we didn’t have when I wrote my text book, or when I wrote *The First Three Minutes*. It’s been a very exciting time for cosmology. Much more exciting in the last decade at any rate than in my own field of elementary particle physics. |
| Q2 | **What would you regard as the most important observation or evidence for the big bang cosmology?** |
|  | Steven Weinberg: Of course the expansion of the universe. We’ve had that evidence since 1930 or thereabouts. The fact that all the galaxies in the universe are rushing away from each other. This at times has been questioned as an interpretation of the observations, but I think it is more and more solid that the universe in this sense is expanding. Now we know much more about the rate of the expansion and how it’s speeding up and slowing down. That’s the most important evidence, but there is lots of other evidence. For example, the abundance of the elements. Most elements are produced in stars, and that doesn’t have so much to do with cosmology, but the lightest elements, about five or six isotopes of the lightest elements, were produced in the first three minutes and astrophysicists can calculate the abundance of these elements and compare it with what’s observed in the oldest stars. It agrees really marvellously well. That’s a real triumph, I think, of theoretical science. |
| Q4 | **What do you consider the greatest contribution that you made to these theorists, or to science?** |
|  | Steven Weinberg: It’s not in astrophysics or cosmology. I’ve written some papers in cosmology but they’re not of the first importance. My main work has been in the theory of elementary particles, and particularly in the unification of two of the forces of nature, the weak force which causes particles of one type to turn into particles of another type, and the electromagnetic force, which people are familiar with, which is responsible for electricity flowing through wires or for magnets attracting pieces of metal. It turns out that these are both aspects of the same underlying force which now has become called the electroweak force. |
| Q4 | **These were two of the four forces of nature?** |
|  | Steven Weinberg: Yes. I’ve also worked on the third force, the strong force. In fact, my work on the electroweak force grew out of my work on the strong force which is the force that holds quarks together inside the particles inside the nucleus of the atom. In that work, I had developed certain mathematical ideas that go by the name of broken symmetry, and shown how the … Well, I had not originated the idea of broken symmetry but I showed how it could be used to understand features of the strong force. Then it occurred to me suddenly, in 1967, that similar mathematical ideas would apply to the weak force and would allow us to unify it with the electromagnetic force in a very satisfactory theory. Other people, of course, have worked on the strong force and the electroweak force, and out of the work of many physicists came in the 1970s a theory of all the forces of nature, except for gravitation, known as the standard model. |
| Q9 | **This is the one that you have been awarded the Nobel Prize for?** |
|  | Steven Weinberg: The Nobel Prize came for the contribution to the electroweak force. Not for the strong force, where I was not, as far as the strong force is concerned I made contributions which I think were important, but not of the most important contributions. |
| Q4 | **Coming back to cosmology. There is especially one statement in your book on cosmology, the popular one, *The First Three Minutes*, that was cited and also discussed very often. You wrote “the more the universe seems comprehensible, the more it also seems pointless”. Can you elaborate a little on that?** |
|  | Steven Weinberg: That’s not the last sentence in the book. If you look at the book then there’s another paragraph that follows that, that explains what I meant, although perhaps I didn’t explain it very well. What I meant in that statement is that there is no point to be discovered in nature itself. There is no cosmic plan for us. We are not actors in a drama that has been written with us playing the starring role. There are laws, we are discovering those laws, but they’re impersonal, they’re cold. We are the result of billions of years of accidents that have led to us, governed by laws of nature that have no care for us. But then after saying that, I went on and said that if there is no point in nature, we can make a point for ourselves. We can find things to cherish that we value. We can love each other, we can create things that are beautiful, and also one of the things that some of us find to give point to our lives is to learn about nature. It’s not an entirely happy view of human life. I think it’s a tragic view, but that’s not new to physicists. A tragic view of life has been expressed by so many poets, that we are here without purpose, trying to identify something to care about. Even when we find the final laws of nature we won’t know why those are the correct laws of nature. But, although, for example, Shakespeare very often expresses a tragic view of life: “golden lads and girls all must, like chimney sweepers come to dust”. Our tragedy is a little different from his, from the heroes of Shakespeare’s plays. For Lear and Othello, the tragedy is in Shakespeare’s script, and what I like to say is that our tragedy is that there is no script. |

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| **Physics\_1999-** | |
| **ID** | 0116 |
| **Biographical** | Pjotr Leonidovich Kapitsa was born in Kronstadt, near Leningrad, on the 9th July 1894, son of Leonid Petrovich Kapitsa, military engineer, and Olga Ieronimovna née Stebnitskaia, working in high education and folklore research.  Kapitsa began his scientific career in A.F. Ioffe’s section of the Electromechanics Department of the Petrograd Polytechnical Institute, completing his studies in 1918. Here, jointly with N.N. Semenov, he proposed a method for determining the magnetic moment of an atom interacting with an inhomogeneous magnetic field. This method was later used in the celebrated Stern-Gerlach experiments.  At the suggestion of A.F. Ioffe in 1921 Kapitsa came to the Cavendish Laboratory to work with Rutherford. In 1923 he made the first experiment in which a cloud chamber was placed in a strong magnetic field, and observed the bending of alfa-particle paths. In 1924 he developed methods for obtaining very strong magnetic fields and produced fields up to 320 kilogauss in a volume of 2 cm3. In 1928 he discovered the linear dependence of resistivity on magnetic field for various metals placed in very strong magnetic fields. In his last years in Cambridge Kapitsa turned to low temperature research. He began with a critical analysis of the methods that existed at the time for obtaining low temperatures and developed a new and original apparatus for the liquefaction of helium based on the adiabatic principle (1934).  Kapitsa was a Clerk Maxwell Student of Cambridge University (1923-1926), Assistant Director of Magnetic Research at Cavendish Laboratory (1924-1932), Messel Research Professor of the Royal Society (1930-1934), Director of the Royal Society Mond Laboratory (1930-1934). With R.H. Fowler he was the founder editor of the International Series of Monographs on Physics (Oxford, Clarendon Press).  In 1934 he returned to Moscow where he organized the Institute for Physical Problems at which he continued his research on strong magnetic fields, low temperature physics and cryogenics.  In 1939 he developed a new method for liquefaction of air with a lowpressure cycle using a special high-efficiency expansion turbine. In low temperature physics, Kapitsa began a series of experiments to study the properties of liquid helium that led to discovery of the superfluidity of helium in 1937 and in a series of papers investigated this new state of matter.  During the World War II Kapitsa was engaged in applied research on the production and use of oxygen that was produced using his low pressure expansion turbines, and organized and headed the Department of Oxygen Industry attached to the USSR Council of Ministers.  Late in the 1940’s Kapitsa turned his attention to a totally new range of physical problems. He invented high power microwave generators – planotron and nigotron (1950- 1955) and discovered a new kind of continuous high pressure plasma discharge with electron temperatures over a million K.  Kapitsa is director of the Institute for Physical Problems. Since 1957 he is a member of the Presidium of the USSR Academy of Sciences. He was one of the founders of the Moscow Physico-Technical Institute (MFTI), and is now head of the department of low temperature physics and cryogenics of MFTI and chairman of the Coordination Council of this teaching Institute. He is the editor-in-chief of the Journal of Experimental and Theoretical Physics and member of the Soviet National Committee of the Pugwash movement of scientists for peace and disarmament.  He was married in 1927 to Anna Alekseevna Krylova, daughter of Academician A.N. Krylov. They have two sons, Sergei and Andrei. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0117 |
| **Biographical** | I was born in Munich, Germany, in 1933. I spent the first six years of my life comfortably, as an adored child in a closely-knit middle-class family. Even when my family was rounded up for deportation to Poland it didn’t occur to me that anything could happen to us. All I remember is scrambling up and down three tiers of narrow beds attached to the walls of a very large room, and then taking a long train trip. After some days of back and forth on the train, we were returned to Munich. All the grown-ups were happy and relieved, but I began to realize that there were bad things that my parents couldn’t completely control, something to do with being Jewish. I learned that everything would be fine if we could only get to “America”.  In the late spring of 1939, shortly after my sixth birthday, my parents put their two boys on a train for England; we each had a suitcase with our initials painted on it, as well as a bag of candy. They told me to be sure and take care of my younger brother. I remember telling him, “*jetzt sind wir allein*” as the train pulled out.  My mother received her exit permit about a month later (just a few weeks before the war broke out) and was able to join us in England. My father had arrived in England almost as soon as the two of us, but we hadn’t seen him because he was interned in a camp for alien men. The only other noteworthy event in the six or so months we spent in England, awaiting passage to America, occurred one morning in a makeshift schoolroom. At that moment, I suddenly realized that I could read the open page of the (English) school book I had been staring at.  We sailed for America toward the end of December 1939 on the Cunard liner Georgic – using tickets that my father had foresightedly bought in Germany a year and half earlier. The ship provided party hats and balloons for the Christmas and New Year’s parties, as well as lots of lifeboat drills. The grey three-inch gun on the aft deck was a great attraction for us boys.  We arrived in New York in January of 1940. My brother and I started school and my parents looked for work. Soon they became “supers” (superintendents of an apartment building). Our basement apartment was rent free and it meant that our family would have a much-needed second income without my mother having to leave us alone at home. As we got older and things got better, we left our “super” job and my mother got a sewing job in a coat factory; my father’s increasing wood-working skills helped him land a job in the carpentry shop of the Metropolitan Museum of Art. As job pressures on him eased, he found time to hold office in a fraternal insurance company as well as to serve as the president of the local organization of his labor union.  It was taken for granted that I would go to college, studying science, presumably chemistry, the only science we knew much about. “College” meant City College of New York, a municipally-supported institution then beginning its second century of moving the children of New York’s immigrant poor into the American middle class. I discovered physics in my freshman year and switched my “major” from chemical engineering to physics. Graduation, marriage and two years in the U.S. Army Signal Corps, saw me applying to Columbia University in the Fall of 1956. My army experience helped me get a research assistantship in the Columbia Radiation Laboratory, then heavily involved in microwave physics, under [I.I. Rabi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/index.html), [P. Kusch](https://www.nobelprize.org/nobel_prizes/physics/laureates/1955/index.html) and [C.H. Townes](https://www.nobelprize.org/nobel_prizes/physics/laureates/1964/index.html). After a painful but largely successful struggle with courses and qualifying exams, I began my thesis work under Professor Townes. I was given the task of building a maser amplifier in a radio-astronomy experiment of my choosing; the equipment-building went better than the observations.  In 1961, with my PhD thesis complete, I went in search of a temporary job at Bell Laboratories, Holmdel, New Jersey. Their unique facilities made it an ideal place to finish the observations I had begun during my thesis work. “Why not take a permanent job? You can always quit,” was the advice of Rudi Kompfner, then Director of the Radio Research Laboratory. I took his advice, and remained a Bell Labs employee for the next thirty seven years.  Since the large horn antenna I had planned to use for radio-astronomy was still engaged in the ECHO satellite project for which it was originally constructed, I looked for something interesting to do with a smaller fixed antenna. The project I hit upon was a search for line emission from the then still undetected interstellar OH molecule. While the first detection of this molecule was made by another group, I learned quite a bit from the experience.  In order to make some reasonable estimate of the excitation of the molecule, I adopted the formalism outlined by George Field in his study of atomic hydrogen. To make sure that I had it right, I took my calculation to him for checking. One of the factors in that calculation was the radiation temperature of space at the line wavelength, 18-cm. I used 2 K, a somewhat larger value than he had used earlier, because I knew that at least two measurements at Bell Laboratories had indications of a sky noise temperature in excess of this amount, and because I had noticed in Gerhard Hertzberg’s “Spectra of Diatomic Molecules” that interstellar CN was known to be excited to this temperature. The results of this calculation were used and then forgotten. It was not until Dr. Field reminded me of them in December of 1966 that I had any recollection of my momentary involvement with what was later shown (by Field and others) to be observational astronomy’s first encounter with the primordial radiation that permeates our Universe.  In the meantime, others at Bell Labs pressed the horn antenna into service for another satellite project. A new Bell System satellite, TELSTAR, was due to be launched in mid-1962. While the primary earth station at Andover, Maine, was more or less on schedule, it was feared that the European partners in the project would not be ready at launch time, leaving Andover with no one to talk to. As it turned out, fitting the Holmdel horn with a 7-cm receiver for TELSTAR proved unnecessary; the Europeans were ready at launch time. This left the Holmdel horn and its beautiful new ultra low-noise 7-cm traveling wave maser available to me for radio astronomy. This stroke of good fortune came at just the right moment. A second radio astronomer, [Robert Wilson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1978/index.html), came from Caltech on a job interview and was hired. After finishing separate projects, we set to work early in 1963.  In putting our radio astronomy receiving system together we were anxious to make sure that the quality of the components we added were worthy of the superb properties of the horn antenna and maser that we had been given. We began a series of radio astronomical observations, ones that I had proposed so as to make the best use of the careful calibration and extreme sensitivity of our system. Of these projects, the most technically challenging was a measurement of the radiation intensity from our galaxy at high latitudes. This multi-year endeavor, which resulted in our discovery of the cosmic microwave background radiation, is described in Wilson’s Nobel lecture.  When our 7-cm program was accomplished, we converted the antenna to 21-cm observations, including another microwave background measurement, as well as galactic, and intergalactic, atomic hydrogen studies. During this period, I took on a visiting position in Princeton’s Astrophysical Sciences Department, thereby enabling me to propose and supervise graduate student research projects in radio astronomy. Like so many others in similar positions, I feel that I learned far more from my students than I could possibly have taught them.  As time went on, opportunities for front line work that we could do with our facility became rarer. Much larger radio telescopes existed, and they were being fitted with low-noise parametric amplifiers whose sensitivities began to approach that of our maser system. As a result, I began looking for new ways of exploring the radio sky. In those days, the portion of the radio spectrum short-ward of one cm wavelength was not yet available for line radio astronomy owing to equipment limitations. At Bell Laboratories, however, many of the key components required for such work had been developed for communications research purposes. With Keith Jefferts, a Bell Labs atomic physicist, Wilson and I assembled a millimeter-wave receiver which we carried to a precision radio telescope built by the National Radio Astronomy Observatory at Kitt Peak, Arizona, early in 1970. Using this new technique, we discovered and studied a number of interstellar molecular species, thereby revealing the rich and varied chemistry which exists in interstellar space.  Millimeter-wave spectral studies have proven to be a particularly fruitful area for radio astronomy, and are the subject of active and growing interest, involving a large number of scientists around the world. The most personally satisfying portion of this work for me was using molecular spectra to explore the isotopic composition of interstellar atoms – thereby tracing the nuclear processes that produced them. Most notably our 1973 discovery of DCN, the first deuterated molecular species found in interstellar space, enabled me to trace the distribution of deuterium in the galaxy. This work provided us with evidence for the cosmological origin of this unique element, which had earned the nickname “Arno’s white whale”. Of all the nuclear species found in nature, deuterium is the only one whose origin stems exclusively from the explosive origin of the Universe. Because deuterium’s cosmic abundance serves as the single most sensitive parameter in the prediction of cosmic background radiation, these measurements provided strong support for the “Big Bang” interpretation of our earlier discovery.  In addition to my astronomical research, I always had made it my business to engage in technology-related work at Bell Labs. It seemed only reasonable to contribute to the pool of technology from which I drew upon. Similarly, Bell Labs has always contributed to, as well as used, the store of basic knowledge – as evidenced by their hiring of a radio astronomer in the first place.  As time went on, I grew more involved in leading the research of others. In 1972 I became the Head of the Radio Physics Research Department upon the retirement of A.B. Crawford, the brilliant engineer who had designed and built the horn antenna that Wilson and I used in our discovery. In 1976, I became the Director of the Radio Research Laboratory, an organization of some sixty scientists and engineers, engaged in a wide variety of research activities, principally related to the understanding of radio and its communication applications. At the same time, I was able to continue my personal research work in radio astronomy, using a superb millimeter-wave radio telescope we had built at our Crawford Hill facility. Fitted with uniquely-sensitive detectors and a dedicated minicomputer (then still something of a rarity), this facility eliminated the manual controls and constant tinkering with equipment, that I had long been used to.  Early in 1979, my managerial responsibilities increased once again when I was asked to assume responsibility for Bell Labs’ Communications Sciences Research Division. At the same time, I continued the personal research which traced the effects of nuclear processing in the Galaxy through the study of interstellar isotopes, and began working in a new area – the nature and distribution of molecular clouds in interstellar space. Instead of participating as actively as I had in the past, however, I introduced this subject to graduate students who explored it in their PhD theses under my supervision.  Then, toward the end of 1981, an unexpected event imposed an abrupt end to my career as a research scientist. At that time, AT&T and the US Department of Justice decided to settle their anti-trust suit by breaking up the Bell System. In the midst of this process, I received yet another promotion – this time to Vice-President of Research – at a moment when two-thirds of the traditional research-funding base moved off with the newly-divested local telephone companies.  As a result, I found myself facing several issues at once: What sort of research organization did the new AT&T require? How to create this new organization without destroying the world’s premier industrial research laboratory in the process? Would the people in this large and tradition bound organization accept and support the changes needed to adapt to new economic and technological imperatives? Needless to say, such matters kept me quite busy.  In retrospect, the research organization which emerged from the decade following the Bell System’s breakup deployed a far richer set of capabilities than its predecessor. In particular, our work featured a growing software component, even as we strove to improve our hardware capabilities in areas such as light-wave and electronics. The marketplace upheaval brought forth by increased competition helped speed the pace of technological revolution, and forced change upon the research and development institutions of all industrialized nations, Bell Labs included. While change is rarely comfortable, I am happy to say that we not only survived but also grew more capable in the process – seeding much of the information revolution which now pervades the world in which we live.  Except for two or three papers on interstellar isotopes, my tenure as Bell Labs’ Vice-President of Research brought my personal research in astrophysics to an end. In its place, I pursued my interest in the principles which underlie the creation and effective use of technology in our society, and eventually found time to write a book on the subject *Ideas and Information*, published by W.W. Norton in 1989. In essence, the book depicts computers as a wonderful tool for human beings but a dreadful role model for what we humans know as intelligence. In other words, “*If you don’t want to be replaced by a machine, don’t try to act like one!*” The warm reception this book received in the US, and the ten other countries which published it in various translations gave me much satisfaction.  By the early 1990’s, my life had settled into a familiar – if not entirely comfortable – routine. The joy and satisfaction that I found in helping to help shape exciting new ideas was offset by onerous management chores – most notably, my annual task of getting adequate financial support for my organization’s budget requirements from our parent corporation. Beset by competitors who didn’t have research labs of their own to pay for, AT&T’s leaders nonetheless did their best to provide for its “crown jewel”. As one year followed another, I did my best to repay that trust by helping to turn some of our scientific “gems” into profitable jewelry.  And then, I did something that surprised everyone – myself included. I decided to swap my job for something entirely new, moving from the world’s largest corporate R&D organization to California’s Silicon Valley, premier incubator of tiny start-up enterprises.  In retrospect, I can point to a number of contributing factors – most notably obligatory retirement age, then only a few years away. While arbitrary, the notion behind an age cutoff for senior managers had much to recommend it. I couldn’t (and still can’t see) myself ever being happy without something challenging to work on. Since getting another management-related job seemed too much of the same thing, I hit upon the idea of turning what I had been enjoying most into a full time job: helping to shape new ideas, and bring them to practical fruition. The more I thought about it, the more attractive this plan for my post-retirement life became. So attractive, in fact, that I soon decided not to wait much longer to put it into place.  Once decided upon, my transition proved surprisingly easy. At the suggestion of the then Bell Labs President, I soon took on a new job – one in which I was to report what I learned about Silicon Valley and its workings, to my Bell Labs colleagues. Accordingly, I arranged to sit in on presentations made by nascent start-up enterprises to venture capitalists. I felt right at home in short order, peppering presenters with questions and suggestions concerning their technologies and plans for turning their offers into viable businesses.  As time went on, an increasing number of these sessions led to invitations from some of the entrepreneurs to get directly involved with their companies, generally by becoming a member of a Board of Directors, or serving on a Technical Advisory Board. I accepted a few of these invitations, but then opted for something that seemed more flexible: working on an as-needed basis with the investing staff and portfolio companies of a single venture capital firm: New Enterprise Associates. Happily, this relationship has endured, and continues to flourish to the time of this writing, almost ten years after it began. This talented and diverse group of people works as a successful laboratory – finding ways in which small handfuls of creative people might change some aspect of the world.  In my early years with NEA, much of my interest focused on communications-related endeavors, but soon broadened to encompass a wide variety of topics under the general heading of “*Information Technology*“. Most recently, I have found and catalyzed several alternative approaches to energy generation – a field I had all but given up on a decade earlier.  With exciting projects underway, and a never-ending stream of new opportunities, my days are filled with new things to learn, challenging puzzles, and stimulating interactions with collaborators. Needless to say, I have no plans to retire. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q2 | **I just would like you, Professor Penzias, to start off with your great discovery. It has been described as one of the biggest discoveries the last century. It has changed the way we look at the universe. It confirmed the big bang theory. When did you and how did you realise that you were on something so big?** |
|  | Arno Penzias: Original work stemmed from the ideas that I had when going to Bell Labs that we had a small antennae which had unique properties. And in some sense as a physicist you sometimes, you may have one burning ambition to do one thing, but as a student, I had just finished my PhD and I was going to, I’d come to Bell Labs because I was attracted to this antennae. Because it had some very interesting properties. It was small and so because it was small it would not, it was better at looking at extended objects. A larger antennae would be more focussed to the single spot and it would be more sensitive to small objects. But this one was better at looking at more extended objects. So that by itself would not make an enormous difference, but the antennae was also very easy to calibrate and because of its odd shape was also, suit itself to having extraordinarily sensitive receivers. Receivers which would not be possible in a normal antennae configuration.  I got the antennae and I was going to put a 21 centimetre receiver in the antennae. But the antennae when I got inherited, had another receiver, a very sensitive amplifier cooled with liquid helium, which had been put there for a satellite project with 7 centimetres wavelength. In other words a shorter wavelength, the higher frequency. And so I thought, before I threw this thing out, what could I do with that wavelength and with that receiver and that system? The first thing I thought of was study the galaxy in a way that no one else had been able to do. After a period of very careful measurements, not just day to night, but seasonally. During the period we’re doing this other experiment, we finally got to the point where I realised that there is nothing wrong with this experiment that we can find.  But then the only question was what to do with it. And since I’d had a whole bunch of other astronomical results, what I resolved to do was write it up and put it in a section of another paper. And so at least I was reporting it, but I wouldn’t if it turned out to be somebody said, ‘Oh you jerk, you forgot so and so’, I wasn’t going to make any claim for it. So I was going to put it in another paper. I started radio astronomy in the 1950s when [Charles Townes](https://www.nobelprize.org/nobel_prizes/physics/laureates/1964/townes-facts.html), my thesis adviser, thought of starting a radio astronomy group. And as a group we studied a series of papers, which had been put together in an American engineering journal called *The Proceedings for the IEEE*, the same; I think it was called, in them days it was called something else. And it turned out every one of those papers was wrong; every single one.  Radio astronomy except for a few things was just in such a primitive state that we were really very scared of making mistakes. So we did this for a long time and only then in 1965 when a colleague, when I was getting ready to publish this thing in that form in part of another paper, pointed out that there had been a preprint from a scientist that prints in university who had an explanation in terms of what others had called the gamma theory. And at that point we felt we would take the stuff and publish it and very conservatively next to another paper. And this is a whole series of things I wrote about in my [Nobel Lecture](https://www.nobelprize.org/prizes/physics/1978/penzias/lecture/) about who did what and so forth. But then this was the first result this having to do with the how old is the universe.  And since there are so many crackpot ideas in physics, as for physics in those days, when I studied astrophysics at almost exactly at that time, the Hubble constant, the age of the universe; the age of the universe was about one and a half billion years at a time the geologists thought that the earth was five billion years old. So nothing really made a lot of sense in those days. So one had to be very careful and didn’t think that any one theory was going to be the right one.  So what I did and I’ll finish that thought. So we signed up with that one not expecting it to be the only one. In fact it wasn’t, there were two other explanations for it. And it turned out the first one was correct. But certainly it was not, I certainly never had the confidence to believe that some theory, which had been had so many other things wrong with it, was going to be the right one. And that the first one that we bumped into so to speak turned out to be the right explanation. So that was the start of that work. |
| Q17 | **What did it teach you, in your life? I know you were very careful in the beginning and yet there was this amazing discovery that you made?** |
|  | Arno Penzias: Well, that’s systematic. It’s I suppose it is, I’d always felt and I still do it, which is I’m quite meticulous. That’s something to do with my German upbringing I suppose. Early toilet training or whatever, I don’t know. But we really do, everything is done very carefully and absolutely in a precise way. You never do anything else. I remember one time speaking to a friend of mine who’s a psychologist. And I said to him, ‘Well you know I didn’t shine my shoes for, I got out the army and I think it was ten years before I shined my shoes. And I buy a car, I keep it for ten years. I paint it once half way through and then when it’s finally dead after ten years, I throw it away. I never wash it’. And he says ‘But everything inside works doesn’t it Arno?’ And I said ‘Yeah, everything inside works’. So that’s the kind of meticulous approach I’ve taken to science. That is that while I’m willing to be creative in other ways, I feel that measurements what to be done as precisely as possible. |
| Q10 | **You worked at Bells over the years, was that also part of that? Did you have the same kind of way of looking at work when you went in and worked at Bells as the boss at the research?** |
|  | Arno Penzias: That was later. No that was two different things. When I was doing my own work, made my own research, I was always, that was a, my own astronomy research had always been very precise. I continued research even after being promoted several times and even until I became head of the research organisation in 1981. When I got this job at that point my responsibilities got so large that I think after my elevation to vice president, which is having about 1,000 PhDs working in my organisation and a budget of 100s of millions of dollars, it just stopped being fun to do astronomy. I still had a graduate student from Princeton and he got his thesis and that was finished and then I wrote one more paper, maybe two other papers after that. And I decided I just couldn’t be pulled in two directions. If I couldn’t do it well I didn’t want to do it as a hobby.  From that time on the things I did were really quite different. I had this organisation so I wanted to learn computer science, so I wrote a book on computers, which was a different, but it was a different sort of approach when you have to lead people. It’s a different kind of situation because you have to take risks, encourage other people to take chances. So it’s really quite different. In my own life, in the things that I do, I still do those things meticulously. And sometimes I do, in the calculations and such I probably take a rather, actually if I think about it, I take an accurate approach that is my results are I think in the work I’ve done since then, are accurate rather than precise. I step back more, so that try to understand what the important issues are and get an answer because it’s very different from questioning nature. There one has to make a decision based on, typically in a management position, even in what I would call leadership, social plan, in things which have economic or social consequences, you always have to make decisions on incomplete information. And so those are different.  In the case of physics, what we study artificially limited. We study, or at least very limited, very constrained questions and those physicists for instance measure things at either very cold, very hot, under tremendous pressure. So you try to push nature to its extremes in order to make measurements and then make them very precisely so you can get the measurements of nature. You can’t take the economy and twist it and put it into some argument.  I had a, on a totally different example there, in my organisation I had as I said 1,000 or more scientists, 100 department heads. And department heads and scientists would come with their PhDs and to become a department head was a nice thing. You got a secretary, you got promoted. So everyone, the idea, the ambition was to become, not the only ambition but for most people, becoming a department head was a big deal. Had 100 department heads and one of the nice things was we had a woman named Betsy Bailey who was an economist and she was a head of one of our economics departments and for a number of years. And then she left there and a very distinguished economist who went then to become head of the American Federal Aviation Administration. And I always liked having Betsy there because if on the rare occasion that we’d promoted a woman, we would then have two women department heads out of 100. And when the other one left we’d go back to one but then we’d often promote another one, so we’d always say the men and women who are department heads. We never had to say the men and woman, we could say men and women because we had two.  This went on for a while despite our best efforts and never promoted any women. One day the women came to me through a meeting. We had 100 women in my organisation and they said ‘You know why we are never promoted, when we never get the job. We’re always considered. And everybody wants to promote women, but we never get the job and do you know why?’ And I was here on this stage and 100 women you know and I thought I was a good guy. I thought we were doing our best and everything. What possible answer do we have? ‘Because we know why. Because we’re never the best qualified’. And they said why? I said ‘Well you have your best that you said’. ‘We have the same education as men though, but when the job comes up, we’re always considered. And we look at the two sets of experience. And the woman never has as good experience because they never get the really leading edge projects to work on. They’re always working on these other projects’. And because we get projects, nobody wants us to fail, so they seem to always give us the second best project, the safe project because we don’t have enough women, they’re helping us by giving us these safe projects. We never get the other projects. What do we do, how do we fix this?’ And I knew I couldn’t go back to the department heads and say ‘give women riskier projects; doesn’t work.  But this is an example of something that has nothing to do with precision. This is my other side, which is the wheeling and dealing side. And so I said, and I thought of this immediately, I said ‘Write a personal business plan’. ‘Personal business plan, what’s that?’ I said ‘put a horizontal line on a piece of paper. Put down a number which is the value, imagine that your project, whatever it is you’re going to do, is a success and give it a number. It could be dollars, or years of your life. I give five years, say you’re a medical student, I give ten years of my life to cure cancer, whatever. I said give a number, multiply it by the probability and divide it by the cost. So if that’s a big number do it, if it’s a small number don’t do it.  Well, it turned out that’s a very imprecise thing to do. And nobody knows exactly what the value is. And nobody knows exactly what the cost is and you don’t know what the probability is. Every one of those is a guess. But in the process of doing it, they first went to their department heads and the department heads came back and said ‘We don’t know its value’. So they said ‘Go ask the customer who is it going to benefit? Go and ask the beneficiary of this how much it’s worth to the beneficiary?’ So we don’t know who the beneficiary is. And all the women began to say ‘You’ve given me a job whose value you don’t know, who the beneficiary is you don’t know’. They started asking that question and within three years I think we had five or six or ten promotions and then my job today is held by one of the women that got promoted that time. So that’s another side. In some ways that’s an example of what you do when you’re a leader. |
| Q17 | **Was it difficult to make that transition for you from being a scientist to become a person which actually had to make completely different decisions?** |
|  | Arno Penzias: That’s an interesting question; I don’t know. I’ve always thought differently. I’ve never felt maybe from childhood on, I’ve never felt really part of anything. I’ve always felt a little special, a little different. I was born in Germany. I only found out I was different when my parents told me I couldn’t join the Hitler youth. You know they all say ‘Adolf Hitler, heil, heil’. You know I wanted to join because there were all these kids having this great time. And my parents said ‘No you’re not going to join that’.  What does that mean? And a lot of things happened after that. Came to America and being poor and a whole bunch of things. So I’ve always been on the side. So the great benefit of that is always big city, up on the corner of whatever room I’m in and seeing what’s going on. So there’s more pain involved, but there’s an awful lot of perspective and I guess that started it. |
| Q9 | **Was that something that came maybe with age as well where you had achieved what you had achieved, the Nobel Prize?** |
|  | Arno Penzias: I don’t know. I’m not sure. I don’t know where estimation came from. It’s certainly not something that, it’s a mystery to me. But I’ve become a very good, there’s almost nothing that I don’t estimate in some way and with whatever limited information I have. And it’s mysterious. |
| Q14 | **What do you see as the big questions then for example if you look at universe and cosmos, you know what was discussed at the Lindau meeting for example? You know the things we don’t know. Is it worth all the cost and effort when we look out in the universe?** |
|  | Arno Penzias: I think one of the things there is, the effort in most case is individual that is the people are investing years of their life. On the other hand, the most costly things are not necessarily the most important. In the case of particle physics very often it’s not the biggest machine that makes the breakthrough experiments; often the second biggest machine. Because people with the second biggest machine have to be a little more clever, right. It’s like the British empire was started by the second sons, the ones who didn’t inherit because in England you know they have the primogenitary there. The oldest son inherits everything and the others get a good education, have to go off. And the British empire you know at one point the second sons conquered the world. So because you’re in a position where you have to catch up, you maybe do a little bit better. So I’m not sure that it’s always necessary to have big and expensive things.  I was probably the most vocal opponent of the superconducting super collider. This was a machine, a particle physics machine, which was going to be put into Texas. It was huge, a huge thing. I’ve always been against the man space flight program. Some things need to be done on a national scale, but other things don’t have to be. And so I think it’s important for there to be dialogues and those informed dialogues are something that, what to be put into some kind of perspective and there ought to be a budget. |
| Q18 | **Why were you against a man space?** |
|  | Arno Penzias: Because it’s a stunt. You’re risking human lives. I mean I think putting a man on the moon at the time was a good idea and it was socially useful. In a sense it was like the two gorillas. Gorillas will go up and they’ll jump up and down and show their muscles to one another and the one that looks a little tougher than the other one goes away and nobody gets hurt. So that’s a nice thing to do. I mean I’m in the renaissance, you know, occasionally two armies would get together and the champions would have one little fight and then everybody would declare a winner without killing everybody. And I think between the United States and Russia, the fact that we got to the moon in America before the Russians did, said something about whether communism is really the future of the world and so forth. So for the man on the moon project’s ok, but beyond that, the rest of it makes really no sense. |
| Q18 | **So they could rather put robots in space?** |
|  | Arno Penzias: Oh much better. Oh it’s much better. I mean I think it’s stupid and inhuman to put, I mean this poor, they put this teacher some years ago in this rocket and the poor lady burnt to death. Do you remember they had this crash of a rocket? And how could a teacher teach a class from space; what does she see? She looks out a port hole like that. Sit down here at Lindau and have a nice cup of coffee, have some strudel and watch a video camera that’s up in space that’s all, the only difference. And you don’t have to throw up on the way up because of weightlessness. Why would anyone do this? But it benefited people who I think in that case it’s just a bureaucracy feeding its own end. So I’m very much against the space program. |
| Q3 | **Does the scientist have some kind of responsibility to voice this do you think?** |
|  | Arno Penzias: But they do. I mean scientists do. The president of the American physical society, or there’s, I think in the case of the space program man space flight, I think the majority of scientists don’t go along with it, but it’s something every country likes and everyone wants to put up an astronaut in these things. But I think it’s a dreadful, personally I think it’s a dreadful idea. But you know I’m not an expert in this field, but I think it is not something that I really think is valuable. There’s a lot one can do, certainly the space one telescopes, the probes in our galaxy. But the satellite the work, I mean just simple things like global positioning, which makes airplanes safer and then people convenience in cars, all the studies of nature and its behaviours, predicting hurricanes, there’s enormous amount of information. So a space program I think is a great and beneficial both on the scientific, but much more on the practical side. I think space program’s a wonderful thing. |
| Q14 | **Do you think that one day with the kind of experiment that goes on and the ideas that we are formulating here on earth, that we will know whether there’s life after universe?** |
|  | Arno Penzias: I doubt it. I really doubt it. I doubt it, but I could be wrong because I can’t think of an experiment that would do this. It’s probably the only question about where I care much more about the question than the answer. If we say that in our galaxy there are probably 200,000 million suns in our galaxy alone. And there are more than 100,000 million other galaxies. So think how many stars that is. And if among all those stars we are alone, we’re the only ones. I mean it just makes me shiver. It’s such a, it’s like you know you’re standing up, all of a sudden realise that there’s a cliff over here. It just makes me shiver. On the other hand if we’re not alone also makes me shiver. I mean either case we come back to the same question that was asked in the book of Ecclesiastes: *What is man, that thou art mindful of him? And the son of man that taketh notice of him.* And they just, you know what is all this all about? Is it really as meaningless as scientist force themselves to believe? |

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| **Physics\_1999-** | |
| **ID** | 0118 |
| **Biographical** | My grandparents moved to Texas from the South after the U.S. Civil War and settled on small farms in the Dallas-Ft. Worth area. Both families emphasized education as the way to improve their children’s lives and both my parents managed to graduate from college. After receiving an M.A. in chemistry from Rice University, my father worked for an oil well service company in Houston. I was born on January, 10, 1936. Two sisters followed, three and seven years later.  I attended public school in Houston. I took piano lessons for several years, and in high school, I played trombone in the marching band. I remember especially enjoying two seasonal activities: ice skating with the Houston Figure Skating Club in the winter and visiting an aunt and uncle’s farm in west Texas in the summer.  During my pre-college years I went on many trips with my father into the oil fields to visit their operations. On Saturday mornings I often went with him to visit the company shop. I puttered around the machine, electronics, and automobile shops while he carried on his business. Both of my parents are inveterate do-it-yourselfers, almost no task being beneath their dignity or beyond their ingenuity. Having picked up a keen interest in electronics from my father, I used to fix radios and later television sets for fun and spending money. I built my own hi-fi set and enjoyed helping friends with their amateur radio transmitters, but lost interest as soon as they worked.  My high school career was undistinguished except for math and science. However, having barely been admitted to Rice University, I found that I enjoyed the courses and the elation of success and graduated with honors in physics. I did a senior thesis with C.F. Squire building a regulator for a magnet for use in low-temperature physics. Following that I had a summer job with Exxon and obtained my first patent. It covered the high-voltage pulse generator for a pulsed neutron source in a down-hole well-logging tool.  Following Rice, I went to Caltech for a Ph.D in physics, without any strong idea of what I wanted to do for a thesis topic. For the first year I lived in the Athenaeum (faculty club) where I became acquainted with a small group of graduate students and visiting faculty members, with whom I often dined and went on weekend outings. When the end of my second quarter approached, I needed a trial research project. David Dewhirst, a Cambridge astronomer and one of the Athenaeum group, suggested that I see John Bolton and Gordon Stanley about radio astronomy. The situation seemed perfect for me. John had come to Caltech to build the Owens Valley Radio Observatory, and the heavy construction was finished. Radio astronomy offered a nice mixture of electronics and physics.  My introduction to radio astronomy was, however, delayed for a summer. I returned to Houston to court and marry Elizabeth Rhoads Sawin, whose spirit and varied interests have added much to my happiness during our twenty-year marriage.  The following year I took my first astronomy courses and went to the observatory during school breaks. That summer John Bolton asked me to join him in observing some of the bright regions on a radio map of the Milky Way which had been made by Westerhaut. By the end of the summer, this project had expanded to making a complete map of that part of the Milky Way which was visible to us. When it was time to measure our chart records and start drawing contour maps from the data, John set up a drawing board in his office, and worked with me on the project. This was typical of John. Whatever the project, whether digging a hole, surveying, laying cables, observing, or reducing data, John would work along with the others. His interest in our map-making and the location of the drawing board kept me at the map-making task instead of designing the next piece of equipment, which would have been my natural inclination.  Our first son, Philip, was born during my fourth year at Caltech. He had many trips to the Owens Valley Radio Observatory, the first at the age of two weeks. He and Betsy were readily accepted at the observatory.  My thesis project was to have been hydrogen-line interferometry, but when the first plans for a local oscillator system didn’t work out, I used the galactic survey as the basis for my thesis. John Bolton returned to Australia before I completed my Ph.D. Maarten Schmidt, who had previously done galactic research and was currently working on quasars, saw me through the last months of thesis work. I remained at Caltech for an additional year as a postdoctoral fellow to finish several projects in which I was involved.  The project of setting up and running the Owens Valley Radio Observatory was very much a community effort. At one time or another I worked with all of the staff and other students and learned from all of them. My collaborations with V. Radhakrishram and B.G. Clark were especially fruitful. I also had the opportunity to meet many of the world’s astronomers who visited Caltech.  In 1961, H.E.D. Scovil at Bell Labs offered to help us make a pair of traveling-wave maser amplifiers for the interferometer. V. Radhakrishran got the job of going to Bell Labs to make our masers. I had wanted to go, but had not yet completed my degree work. I worked with Rad on that project, though, and developed a good feeling toward Bell Labs which was later a strong influence on my decision to take a job there.  I joined Bell Laboratories at Crawford Hill in 1963 as part of A.B. Crawford’s Radio Research department in R. Kompfner’s laboratory. I started working with the only other radio astronomer, Arno Penzias, who had been there about two years. Our early radio astronomy projects are described in my Nobel lecture.  With the creation of Comsat by U.S. Congress, Bell System satellite efforts and related space research were reduced. In 1965 Arno and I were told that the radio astronomy effort could only be supported at the level of one full-time staff member, even though Art Crawford and Rudi Kompfner strongly supported our astronomical research. Arno and I agreed that having two half-time radio astronomers was a better solution to our problem than having one full-time one, so we started taking on other projects. The first one was a joint project – a propagation experiment on a terrestrial path using a 10.6µ carbon dioxide laser as a source. Following that, I did two applied radio astronomy projects. For the first, I designed a device we called the Sun Tracker. It automatically pointed to the sun while it was up every day and measured the attenuation of the sun’s cm-wave radiation in the earth’s atmosphere. Since, as we expected, the attenuation was large for too much of the time for a practical satellite system, I next set up three fixed-pointed radiometers at spaced locations to check on the feasibility of working around heavy rains.  In 1969 Arno suggested that we start doing millimeter wave astronomy. We could take the low noise millimeter-wave receivers which had been developed at Crawford Hill by C.A. Burrus and W.M. Sharpless for a waveguide communication system and make an astronomical receiver with them. We planned to use it at the National Radio Astronomy Observatory’s new 36-foot radio telescope at Kitt Peak in Arizona. Our observations began in 1969 with a continuum receiver. The next year, K.B. Jefferts joined us, and with much help from C.A. Burrus at Crawford Hill and S. Weinreb at NRAO we made a spectral line receiver at 100 – 120 GHz. We were excited to discover unexpectedly large amounts of carbon monoxide in a molecular cloud behind the Orion Nebula. We quickly found that CO is widely distributed in our galaxy and so abundant that the rare isotopic species 13C16O and 12C18O were readily measurable. We soon observed a number of other simple molecules. Our major efforts were directed toward isotope ratios as a probe of nucleogenesis and understanding the structure of molecular clouds.  In 1972, S.J. Buchsbaum, who was our new executive director, revived an earlier proposal and suggested that we build a millimeter-wave facility at Crawford Hill. It was to be used partly for radio astronomy, and partly to monitor the beacons on the Comstar satellites which AT&T was planning to put up. I was project director for the design and construction of the antenna and was responsible for the equipment and programming necessary to make it a leading millimeter-radio telescope. The winter of 1977 – 78 was our first good observing season with the 7-meter antenna and I am looking forward to several more years of millimeter wave astronomy with it.  We still live in the house in Holmdel which we bought when I first came to Bell Laboratories. Our two younger children were born here, Suzanne in 1963, and Randal in 1967. We have come to enjoy the eastern woodlands and I now look forward to skiing and outdoor ice skating with my family and associates in the winter. I spend many evenings reading or continuing the day’s work, but I also enjoy playing the piano, jogging, and traveling with the family. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0119 |
| **Biographical** | My father, Harry Warren Anderson, was a professor of plant pathology at the University of Illinois in Urbana, where I was brought up from 1923 to 1940. Although raised on the farm – my grandfather was an unsuccessful fundamentalist preacher turned farmer – my father and his brother both became professors. My mother’s father was a professor of mathematics at my father’s college, Wabash, in Crawfordsville, Indiana, and her brother was a Rhodes Scholar, later a professor of English, also at Wabash College; on both sides my family were secure but impecunious Midwestern academics. At Illinois my parents belonged to a group of warm, settled friends, whose life centered on the outdoors and in particular on the “Saturday Hikers”, and my happiest hours as a child and adolescent were spent hiking, canoeing, vacationing, picnicking, and singing around the campfire with this group. They were unusually politically conscious for that place and time, and we lived with a strong sense of frustration and foreboding at the events in Europe and Asia. My political interests were later strengthened by the excesses in the name of “security” and “loyalty” of the “McCarthy” years, to the extent that I have never accepted work on classified matters and have from time to time worked for liberal causes and against the Vietnam war.  Among my parents’ friends were a number of physicists (such as Wheeler Loomis and Gerald Almy) who encouraged what interest in physics I showed. An important impression was my father’s one Sabbatical year, spent in England and Europe in 1937. I read voraciously, but among the few intellectual challenges I remember at school was a first-rate mathematics teacher at the University High School, Miles Hartley, and I went to college intending to major in mathematics. I was one of several students sent to Harvard from Uni High in those years on the new full-support National Scholarships. The first months at Harvard were more than challenging, as I came to the realization that the humanities could be genuinely interesting, and, in fact, given the weaknesses of my background, very difficult. Nonetheless in time I relaxed and enjoyed the experience of Harvard, and was in the end pleasantly surprised to come out with a good record.  In those wartime years (1940-43) we were urged to concentrate in the immediately applicable subject of “Electronic Physics” and I was then bundled off to the Naval Research Laboratory to build antennas (1943-45). (It may be remembered that such war work was advisable for those of us who wore glasses, the “services” at that time being convinced that otherwise we would be best utilized as infantry.) This work left me with a lasting admiration for Western Electric equipment and Bell engineers, and for the competence of my former physics (not electronics) professors at Harvard; after the war, I went back to learn what the latter could teach me.  Graduate school (1945-49) consisted of excellent courses; a delightful group of friends, including for instance Dave Robinson and Tom Lehrer, centered around bridge, puzzles, and singing; a happy decision that [Schwinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html) and Q.E.D. would lead only to standing in the long line outside Schwinger’s office, whereas Van Vleck, whom I already knew from undergraduate school and a wartime incident, seemed to have time to think about what I might do; meeting and marrying one summer the niece of old family friends, Joyce Gothwaite, and therefore settling down to work on my problem. Further motivation was provided by the birth of a daughter, Susan. When I did settle down, I rather suddenly came to realize that the sophisticated mathematical techniques of modern quantum field theory which I was learning in advanced courses from Schwinger and Furry were really genuinely useful in the experimental problem of spectral line broadening in the new radio-frequency spectra, just then being exploited because of wartime electronics advances. Although I didn’t know it, across the world – in England with Fröhlich and Peierls, in Princeton with Bohm and later Pines, and in Russia with Bogoliubov and especially Landau – the new subject of many-body physics was being born from similar marriages of maturing mathematical techniques with new experimental problems.  In spite of a number of contretemps, with the help of Van and of an understanding recruiter, Deming Lewis, who seemed to be the only person who believed me when I said I *had* solved my problem and wanted to do something else, I got to Bell Laboratories to work with the constellation of theorists who were then there: [Bill Shockley, John Bardeen](https://www.nobelprize.org/nobel_prizes/physics/laureates/1956/index.html), Charles Kittel, Conyers Herring, Gregory Wannier, Larry Walker, John Richardson, and later others. Kittel in particular fostered my interest in linebroadening problems and introduced Wannier and me to antiferromagnetism, while Wannier taught me many fundamental techniques, and Herring put me in touch with the ideas of Landau and Mott and kept us all abreast of the literature in general. I learned crystallography and solid state physics from Bill Shockley, Alan Holden, and Betty Wood. And I learned most of all the Bell mode of close experiment theory teamwork – at first with Jack Galt, Bill Yager, Bernd Matthias, and Walter Merz.  Much of the rest is a matter of record. One important experience was Ryogo Kubo’s convincing the Japanese in 1952 that they should invite as their first Fulbright scholar in physics an unknown 28-year-old. This Sabbatical was postponed to 1953, the year of the Kyoto International Theoretical Physics Conference, which was dominated by Mott as the president of IUPAP, and was my first meeting with many other friends of later years. Lecturing has never come easily to me, but I gave, as best I could, lectures on magnetism and a seminar on linebroadening which included Kubo, Toru Moriya, Kei Yosida, Jun Kanamori, among other wellknown Japanese solid staters. I acquired an admiration for Japanese culture, art, and architecture, and learned of the existence of the game of GO, which I still play.  Another milestone for me was a year at the Cavendish Laboratory and Churchill College (1961-62), which was not at Oxford because Brian Pippard promised me that I could lecture and that the lectures would be attended. Mott kept asking me what my 1958 paper meant, and there were a lot of discussions centered around broken symmetry and some ideas of Brian Josephson, who attended my lectures.  When he left Princeton for Illinois in 1959, David Pines bequeathed me a French student named Pierre Morel; Morel and I worked in 1959-61 on some unconventional ideas on anisotropic superfluidity I had, which became related to He3 by discussions with Keith Brueckner; later we worked on solving the Eliashberg equations for superconductivity. Some of these ideas came to fruition working with a young experimentalist, John Rowell, on my return to Bell: we discovered the Josephson effect and worked on “phonon bumps”.  In 1967 Nevill Mott managed what must have been a most difficult arrangement to steer through the Cambridge system: a permanent “Visiting Professorship” for two terms out of three at the Cavendish. This arrangement would have been totally impossible without the self-effacing and unsparing cooperation of Volker Heine who joined with me in leading the “TCM Group” (Theory of Condensed Matter) for eight productive and exciting years, spiced with warm encounters with students, visitors and associates from literally the four corners of the earth. One of our brainchildren is a still viable Science and Society course. Through the good offices of John Adkins, Jesus College gave me a Fellowship for this period. A souvenir of those years is a small cottage on the cliffs of Cornwall, where Joyce and I spend a spring month every year, hiking and seeing friends. After eight years the sense of being tourists in each of two cultures, with no really satisfactory role in either, led us reluctantly to return to the United States, and in 1975 the job at Cambridge was replaced with a half-time appointment at Princeton.  The years since the Nobel Prize have been productive ones for me. For instance, in 1978, shortly after receiving the prize in part for localization theory, I was one of the “Gang of Four” (with Elihu Abrahams, T.V. Ramakrishnan, and Don Licciardello) who revitalized that theory by developing a scaling theory which made it into a quantitative experimental science with precise predictions as a function of magnetic field, interactions, dimensionality, etc.; a major branch of science continues to flow from the consequences of this work. (Most recently, “photon localization” has been in th news.)  In 1975 S.F. (now Sir Sam) Edwards and I wrote down the “replica” theory of the phenomenon I had earlier named “spin glass”, followed up in ’77 by a paper of [D.J. Thouless](https://www.nobelprize.org/nobel_prizes/physics/laureates/2016/thouless-facts.html), my student Richard Palmer, and myself. A brilliant further breakthrough by G. Toulouse and G. Parisi led to a full solution of the problem, which turned out to entail a new form of statistical mechanics of wide applicability in fields as far apart as computer science, protein folding, neural networks, and evolutionary modelling, to all of which directions my students and/or I contributed. The field of quantum valence fluctuations was another older interest which became much more active during this period, partly as a consequence of my own efforts.  Finally, in early 1987 the news of the new “high-Tc” cuprate superconductors galvanized the world of many-body quantum physics, and led many of us to reexamine older ideas and dig for new ones. Putting together a cocktail of older ideas of my own (the “RVB” singlet pair fluid state) and of many others, mixed with brand new insights, I have been able to arrive at an account of most of the wide variety of unexpected anomalies observed in these materials. The theory involves a new state of matter (the two-dimensional “Luttinger liquid”) and a quite new mechanism for electron pairing (“deconfinement”). Experimental confirmations of the predictions of this theory are appearing regularly.  The prize seemed to change my professional life very little. Management chores at AT&T Bell Labs continued and culminated in an informal arrangement as consultant for the new Vice President of Research, Arno Penzias, during the first two years of his tenure, which coincided with the first difficult years of “divestiture” for the AT&T company. I thereupon gratefully retired in 1984 from Bell and am now full-time Joseph Henry Professor of Physics at Princeton. I served a 5-year stint as Chairman of the Board of the Aspen Center for Physics, retiring 3 years ago, and for 4 years was on the Council and Executive Committee of the American Physical Society. Since 1986 or so I have been deeply involved (though officially I am merely a co-vice-chairman) with a new, interdisciplinary institution, the Sante Fe Institute, dedicated to emerging scientific syntheses, especially those involving the sciences of complexity. Two other Nobelists are involved: [Murray Gell-Mann](https://www.nobelprize.org/nobel_prizes/physics/laureates/1969/index.html), who is our science board chairman and an eloquent spokesperson for our ideas and ideals; and Ken Arrow, with whom I cochaired the workshops founding our interdisciplinary study of the bases of economic theory. My own work in spin glass and its consequences has formed some of the intellectual basis for these interests.  The Nobel Prize gives one the opportunity to take public stands. I happened to be in a position to be caught up in the campaign against “Star Wars” very early (summer ’83) and wrote, spoke and testified repeatedly, with my finest moment a debate with Secretary George Schultz in the Princeton Alumni Weekly, reprinted in *Le Monde* in 1987. I have also testified repeatedly and published some articles in favor of Small Science.  Some further honors after the Nobel Prize of which I am particularly conscious were the National Medal of Science; an ScD from my father’s, mother’s, sister’s and wife’s Alma Mater, the University of Illinois; foreign membership in the Royal Society, the Accademia Lincei, and the Japan Academy; and honorary fellowship of Jesus College, Cambridge.  We have kept our cottage on the cliffs of Cornwall, and our custom of seeing English and other friends in April there. We abandoned our much loved house, designed by Joyce, in New Vernon near Bell Labs for another of her good designs on some brushy acres with a view across the Hopewell Valley near Princeton. Susan is established as a painter in Boston of, at the moment, primarily scenes of Martha’s Vineyard, and teaching some drawing classes at MIT. A prize of which I am, vicariously, enormously proud is the designation as Northeast U.S. Tree Farmers of the Year earned by my sister and her husband of New Milford, Pa in 1990.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Physics 1971-1980*, Editor Stig Lundqvist, World Scientific Publishing Co., Singapore, 1992  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  Copyright © The Nobel Foundation 1977  **Addendum, April 2005**  I retired to emeritus status in 1996, after spending a sabbatical year as Eastman Professor in Balliol College Oxford in 1993-4. In 2000 I gave up contract funding but am still active in research and writing, mainly book reviews, many of which appear in the Higher Education Supplement of the *Times* of London. I retired from the Steering Committee of SFI in 2001. We sold the house in Cornwall in 2003.  My main interest scientifically continues to be high Tc superconductivity. The theory I was so enthusiastic about in 1990 was shown experimentally to be incorrect, and I had to revert to an earlier version (actually first promulgated by several younger associates in 1988) which has been revived and seems to pass the crucial tests. (Though it is not consensual, the field being in a state which I call “epistemological trainwreck”.)  Among further honors I have received are the Centennial Medal of the GSAS at Harvard, and honorary degrees from the Ecole Normale Superieure in Paris (historically #1 from that institution, thanks to having an “A” name) and the University of Tokyo (actually, their #2); also, the John Bardeen prize at the “M2S” conference, the major international conference on superconductivity. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0120 |
| **Biographical** | Nevill Francis Mott was born in Leeds, U.K., on September 30th, 1905. His parents, Charles Francis Mott and Lilian Mary (née) Reynolds, met when working under [J.J. Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1906/index.html) in the Cavendish Laboratory; his great grandfather was Sir John Richardson, the arctic explorer. He was educated at Clifton College, Bristol and St. John’s College, Cambridge, where he studied mathematics and theoretical physics. He started research in Cambridge under R.H. Fowler, in Copenhagen under [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html) and in Göttingen under [Max Born](https://www.nobelprize.org/nobel_prizes/physics/laureates/1954/index.html), and spent a year as a lecturer at Manchester with W.L. Bragg before accepting a lectureship at Cambridge. Here he worked on collision theory and nuclear problems in Rutherford’s laboratory. In 1933 he went to the chair of theoretical physics at Bristol, and under the influence of H. W. Skinner and H. Jones turned to the properties of metals and semiconductors. Work during his Bristol period before the war included a theory of transition metals, of rectification, hardness of alloys (with Nabarro) and of the photographic latent image (with Gurney). After a period of military research in London during the war, he became head of the Bristol physics department, publishing papers on low-temperature oxidation (with Cabrera) and the metal-insulator transition.  In 1954 he was appointed Cavendish Professor of Physics, a post which he held till 1971, serving on numerous government and university committees. The research for which he was awarded the Nobel Prize began about 1965. Some of his main books are “The Theory of Atomic Collisions” (with H.S.W. Massey), “Electronic Processes in Ionic Crystals” (with R.W. Gurney) and “Electronic Processes in Non-Crystalline Materials” (with E.A. Davis).  Outside research in physics he has taken a leading part in the reform of science education in the United Kingdom and is still active on committees about educational problems. He was chairman of a Pugwash meeting in Cambridge in 1965. He was chairman of the board and is now president of Taylor & Francis Ltd., scientific publishers since 1798. He was Master of his Cambridge college (Gonville and Caius) from 1959-66. He was President of the International Union of Physics from 1951 to 1957, and holds more than twenty honorary degrees, including Doctor of Technology at Linkoping.  In 1930 he married Ruth Eleanor Horder. They have two daughters and three grandchildren, Emma, Edmund and Cecily Crampin.  For the last ten years he has lived in a village, Aspley Guise, next door to his son-in-law and family. During this period he has written an autobiography “A Life in Science” (Taylor and Francis) and edited a book with several authors on a religion-science interface “Can Scientists Believe?” (James and James, London), together with many scientific papers, mainly in the last 3 years on high-temperature superconductors. |
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| **Physics\_1999-** | |
| **ID** | 0121 |
| **Biographical** | I was born in Middletown, Connecticut, March 13, 1899 where my father and grandfather were respectively professors of mathematics and of astronomy at Wesleyan University. However, when I was seven years old father accepted a professorship at the University of Wisconsin, so I grew up in Madison, Wisconsin, where I attended the public schools, and graduated from the University of Wisconsin in 1920. As a sort of revolt against having two generations of academic forbears, I vowed as a child that I would not be a college professor, but after a semester of graduate work at Harvard, I outgrew my childish prejudices, and realized that the life work for which I was best qualified was that of a physicist, not of the experimental variety, but in an academic environment.  I have been lucky in a number of respects. Coming from an academic family, I had invaluable parental guidance or advice at various times. At Harvard I took most of my courses under Professor Bridgman or Professor Kemble. The latter’s course on quantum theory fascinated me, so I decided to write my doctor’s thesis under Kemble’s supervision. He was the one person in America at that time qualified to direct purely theoretical research in quantum atomic physics. My doctor’s thesis was the calculation of the binding energy of a certain model of the helium atom, which Kemble and [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html) suggested independently and practically simultaneously, with Kramers making the corresponding calculation in Copenhagen. The results did not agree with experiment for the “old quantum theory” was not the real thing. However, when the true quantum mechanics was discovered by Heisenberg and others in 1926, my background in the old quantum theory and its correspondence principle was a great help in learning the new mechanics, particularly the matrix form which is especially useful in the theory of magnetism.  I was fortunate in being offered an assistant professorship at the University of Minnesota in 1923, a year after my Ph. D. at Harvard, with purely graduate courses to teach. This was an unusual move by that institution, as at that time, posts with this type of teaching were generally reserved for older men, and recent Ph. D.’s were traditionally handicapped by heavy loads of undergraduate teaching which left little time to think about research. Also it was at Minnesota that I met Abigail Pearson, a student there, whom I married June 10, 1927, and on Nobel Day, December 10, 1977 we had been married exactly 50 1/2 years!  I was also lucky in choosing the theory of magnetism as my principal research interest, as this is a field which has continued to be of interest over the years, with new ramfications continuing to make their appearance (magnetic resonance, relaxation, microwave devices, etc.). So often a particular field loses general interest after a span of time. My last paper dealing with magnetism was published fifty years after my first one.  Besides my work on magnetism, and the closely related subjects of ligand fields and of dielectrics, one of my interests has been molecular spectra. The theoretical problems associated with the fine structures therein appeared rather academic at the time, but recently have burgeoned in interest in connection with radioastronomical investigations, including notably those of the observatory at Gothenburg. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0122 |
| **Biographical** | I was born on 22 March 1931 in New York, the elder child of Abraham and Fanny Richter. In 1948 I entered the Massachusetts Institute of Technology, undecided between studies of chemistry and physics, but my first year convinced me that physics was more interesting to me. The most influential teachers in my undergraduate years were Professors Francis Friedman, who opened my eyes to the beauty of physics, and Francis Bitter, who gave me my first opportunity to do serious experimental physics.  In the summer following my junior year, I began work with Bitter in MIT’s magnet laboratory. During that summer I had my introduction to the electron-positron system, working part-time with Professor Martin Deutsch, who was conducting his classical positronium experiments using a large magnet in Bitter’s laboratory. Under Bitter’s direction, I completed my senior thesis on the quadratic [Zeeman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1902/index.html) effect in hydrogen.  I entered graduate school at MIT in 1952, continuing to work with Bitter and his group. During my first year as a graduate student, we worked on a measurement of the isotope shift and hyperfine structure of mercury isotopes. My job was to make the relatively short-lived mercury-197 isotope by using the MIT cyclotron to bombard gold with a deuteron beam, a kind of reverse alchemy. By the end of the year I found myself more interested in the nuclear and particle physics problems to which I had been exposed and in the accelerator I had used, than in the main theme of the experiment. I arranged to spend six months at the Brookhaven National Laboratory’s 3-GeV proton accelerator to see if particle physics was really what I wanted to do. It was, and I returned to the MIT synchrotron laboratory. This small machine was a magnificent training ground for students, for not only did we have to design and build the apparatus required for our experiments, but we also had to help maintain and operate the accelerator. My Ph.D. thesis was completed in 1956 on the photoproduction of pi-mesons from hydrogen, under the direction of Prof. L.S. Osborne.  During my years at the synchrotron laboratory, I had become interested in the theory of quantum electrodynamics and had decided that what I would most like to do after completing my dissertation work was to probe the short-distance behavior of the electromagnetic interaction. At that time renormalization was not yet part of the theory bag of tricks, and many talked of the possibility of a high energy cutoff to the electromagnetic force. I wanted to see if the cutoff was real, and, if so, at how small a distance it came into force. So I sought a job at Stanford’s High-Energy Physics Laboratory where there was a 700-MeV electron linear accelerator. My first experiment there, the study of electron-positron pairs by gamma-rays, established that quantum electrodynamics was correct to distances as small as about 10-13 cm which was a new limit on its range of validity.  In 1957, G.K O’Neill of Princeton proposed building a colliding beam machine that would use the HEPL linac as an injector, and allow electron-electron scattering to be studied at a center-of-mass energy ten times larger (or a distance ten times smaller) than my pair experiment. I joined O’Neill, and with W.C. Barber and B. Gittelman, we began to build the first colliding-beam device. It took us about six years to make the beams behave properly. This device was the ancestor of all of the colliding-beam storage rings to follow. The technique has been so productive that all high-energy physics accelerators now being developed are colliding-beam devices.  In 1960, I married Laurose Becker. We have two children, Elizabeth, born in 1961, and Matthew, born in 1963.  In 1965, after we had finally made a very complicated accelerator work and had built the needed experimental apparatus, the experiment was carried out, with the result that the validity of quantum electrodynamics was extended down to less than 10-14 cm.  Even before the ring at HEPL was operating, I had begun to think about a high-energy electron-positron colliding-beam machine and what one could do with it. In particular, I wanted to study the structure of the strongly-interacting particles. [Robert Hofstadter](https://www.nobelprize.org/nobel_prizes/physics/laureates/1961/index.html) had studied the structure of the proton with electron-proton scattering (for which he was awarded the Nobel Prize in 1961), and, in principle, it was possible to get a related picture of the structure of unstable particles such as mesons with electron-positron colliding beams.  I had been thinking about how to do it when in 1963 Professor W.K.H. Panofsky invited me to come to the Stanford Linear Accelerator Center (SLAC). I accepted, and, with his encouragement, I set up a group to make a final design of a high-energy electron-positron machine. We completed a preliminary design in 1963 and in 1964 submitted a formal request for funds to the Atomic Energy Commission. That was the beginning of a long struggle to obtain funding for the device, during which I made some excursions into other experiments. My group designed and built part of the large magnetic spectrometer complex at SLAC and used it to do a series of pi- and K-meson photoproduction experiments. Throughout this time, however, I kept pushing for the storage ring and kept the design group alive. Finally, in 1970, we received funds to begin building the storage ring (now called SPEAR) as well as a large magnetic detector that we had designed for the first set of experiments. In 1973 the experiments finally began, and the results were all that I had hoped for. The discovery for which I was honored with the Nobel Prize and the experiments that elucidated exactly what that discovery implied are described in the accompanying lecture. Much more has been done with the SPEAR storage ring, but that is another story.  I spent the academic year, 1975-76, on sabbatical leave at CERN, Geneva. During that year I began an experiment on the ISR, the (CERN 30 by 30 GeV proton storage rings), and worked out the general energy scaling laws for high-energy electron-positron colliding-beam storage rings. My motive for this last work was two-fold: to solve the general problems and to look specifically at the parameters of a collider in the 100-200 GeV center-of-mass (c.m.) energy range that would, I thought, be required to better understand the weak interaction and its relation to the electromagnetic interaction. That study turned into the first-order design of the 27-km circumference LEP project at CERN that was so brilliantly brought into being by the CERN staff in the l980s.  An interesting sidelight to the LEP story is the attempt by Professor Guy von Dardel (then at Lund), Chairman of the European Committee for Future Accelerators, and I to turn LEP into an inter-regional project. We failed because we couldn’t interest either the American or European high-energy physics communities in collaboration even on as large a scale as LEP. The time was not right.  The general scaling laws for storage rings showed that the size and cost of such machines increased as the square of the energy. LEP, though very large, was financially feasible, but a machine of ten times the energy of LEP would not be. I began to think about alternative approaches with more favorable scaling laws and soon focused on the idea of the linear collider where electron and positron beams from separate linear accelerators were fired at each other to produce the high-energy interactions. The key to achieving sufficient reaction rate to allow interesting physics studies at high energies was to make the beam extremely small at the interaction point, many orders of magnitude less in area than the colliding beams in the storage rings.  In 1978 I met A.N. Skrinsky of Novosibirsk and Maury Tigner of Cornell at a workshop we were attending on future possibilities for high-energy machines. We discovered that we had all been thinking along the same general lines and at that workshop we derived, with the help of others present, the critical equations for the design of linear colliders. On returning from the workshop I got a group of people together at the Stanford Linear Accelerator Center and we began to investigate the possibility of turning the two-mile-long SLAC linac into a linear collider. It would be a hybrid kind of machine, with both electrons and positrons accelerated in the same linear accelerator, and with an array of magnets at the end to separate the two beams and then bring them back into head-on collisions. The beams had to have a radius of no more than two microns at the collision point to get enough events to be interesting as a physics research tool, roughly a factor of 1000 less in area than the colliding beams in a storage-ring collider like LEP.  Construction of SLAC Linear Collider began in 1983, and was finished in late 1987. The first physics experiments began in 1990 after a difficult start up. This was a new kind of accelerator complex, and, though we had anticipated some of the new problems we would face, we had not anticipated all. The staff of SLAC did overcome them all and by the time the machine was turned off to make way for the next lab facility, the machine was colliding polarized electrons with positrons at an interaction point where the beam area was a factor of 20 smaller than the original design.  While some important experiments were carried out, probably the most lasting contribution that this facility made to particle physics was the work on accelerator physics and beam dynamics that was been done with the machine and which forms the basis of a very active world wide R&D programs aimed at TeV-scale linear colliders for the future. This R&D program is being pursued in the U.S., Europe, and Japan. This will be the inter-regional machine that von Dardel and I tried to make of LEP in the later 1970s.  Along the way I succumbed to temptation and became a scientific administrator, first as Technical Director of the Stanford Linear Accelerator Center from 1982 to 1984, and then Director from 1984 to 1999. The job of a laboratory director is much different from the job of a physicist, particularly in a time of tight budgets. It is much easier to do physics when someone else gets the funds than it is to get the funds for others to do the research.  While a lab Director can get done the things that he regards as important, he has the more important job of bringing out the best ideas of the broader scientific community. I learned this early in my career while I was leading the construction of the SPEAR facility. During the construction process I was approached by two Stanford faculty, Sebastian Doniac and William Spicer, who wanted to use the intense x-rays produced in the colliding beam facility to do work in condensed matter physics. Their point was that it was possible to make x-ray beams a million times more intense than could be achieved with x-ray tubes, and with such intensity it would be possible to do revolutionary experiments. They were convincing and, since the world was simpler then than now, I just did it, spending the extra funds required to make a port in the machine to let the beams out. This was the first of the modern synchrotron radiation facilities and it did what they promised. It is still in operation with many other such facilities for which it served as the prototype.  Modern science is fast-moving and no laboratory can exist for long with a program based on old facilities. Innovation and renewal are required to keep a laboratory on the frontiers of science and only if it remains on the frontiers will it have a long-term future. Developing the idea of the community to the point where the best of them are ready for implementation requires significant resources. It is important that these resources be made available even in times of tight budgets. Starving the future to feed the present is a mistake – it leads to obsolescence and stagnation. Sometimes it is hard to make this understood.  During my time as Director the synchrotron radiation program was integrated into the laboratory and the development of an x-ray free-electron laser, based on the linear-collider technology, was begun. It is today (2005) under construction.  SLAC also began to move into astro-particle physics with the international collaboration know as GLAST (gamma-ray, large area, space telescope). The astro-particle program has continued to grow under the new Director.  I also began an interregional collaboration with my counterparts in Europe (Bjorn Wiik at the DESY lab in Germany) and Asia (Hirotaka Sugawara at the KEK laboratory in Japan) aimed at bringing into existence a high-energy linear collider as a world collaboration. There is now a consensus that this is the right thing to do and a global organization is being created to bring it to reality. Perhaps von Dardel and my dream will now become real.  Since stepping down as laboratory director in 1999 I have devoted an increasing fraction of my time to international issues. I am involved with energy, environment, and sustainability issues, particularly as they involve new energy sources free of greenhouse gases. Getting enough energy to satisfy the needs of the developing world without bringing on an eco-disaster is not going to be easy. It will require a marriage of science and technology with good international policy, something that is always hard to bring off. We need to get it right this time. |
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| **Physics\_1999-** | |
| **ID** | 0123 |
| **Biographical** | I was born on 27 January 1936 in Ann Arbor, Michigan, the first of three children of Kuan Hai Ting, a professor of engineering, and Tsun-Ying Wang, a professor of psychology. My parents had hoped that I would be born in China, but as I was born prematurely while they were visiting the United States, by accident of birth I became an American citizen. Two months after my birth we returned to China. Owing to wartime conditions I did not have a traditional education until I was twelve. Nevertheless, my parents were always associated with universities, and I thus had the opportunity of meeting the many accomplished scholars who often visited us. Perhaps because of this early influence I have always had the desire to be associated with university life.  Since both my parents were working, I was brought up by my maternal grandmother. My maternal grandfather lost his life during the first Chinese Revolution. After that, at the age of thirty-three, my grandmother decided to go to school, became a teacher, and brought my mother up alone. When I was young I often heard stories from my mother and grandmother recalling the difficult lives they had during that turbulent period and the efforts they made to provide my mother with a good education. Both of them were daring, original, and determined people, and they have left an indelible impression on me.  When I was twenty years old I decided to return to the United States for a better education. My parents’ friend, G.G. Brown, Dean of the School of Engineering, University of Michigan, told my parents I would be welcome to stay with him and his family. At that time I knew very little English and had no idea of the cost of living in the United States. In China, I had read that many American students go through college on their own resources. I informed my parents that I would do likewise. I arrived at the Detroit airport on 6 September 1956 with $100, which at the time seemed more than adequate. I was somewhat frightened, did not know anyone, and communication was difficult.  Since I depended on scholarships for my education, I had to work very hard to keep them. Somehow, I managed to obtain degrees in both mathematics and physics from the University of Michigan in three years, and completed my Ph.D. degree in physics under Drs. L.W. Jones and M.L. Perl in 1962.  I went to the European Organization for Nuclear Research (CERN) as a Ford Foundation Fellow. There I had the good fortune to work with Giuseppe Cocconi at the Proton Synchrotron, and I learned a lot of physics from him. He always had a simple way of viewing a complicated problem, did experiments with great care, and impressed me deeply.  In the spring of 1965 I returned to the United States to teach at Columbia University. In those years the Columbia Physics Department was a very stimulating place, and I had the opportunity of watching people such as [L. Lederman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1988/index.html), [T.D. Lee](https://www.nobelprize.org/nobel_prizes/physics/laureates/1957/index.html), [I.I. Rabi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/index.html), [M. Schwarts](https://www.nobelprize.org/nobel_prizes/physics/laureates/1988/index.html), [J. Steinberger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1988/index.html), C.S. Wu, and others. They all had their own individual style and extremely good taste in physics. I benefitted greatly from my short stay at Columbia.  In my second year at Columbia there was an experiment done at the Cambridge Electron Accelerator on electron-positron pair production by photon collision with a nuclear target. It seemed to show a violation of quantum electrodynamics. I studied this experiment in detail and decided to duplicate it. I contacted G. Weber and W. Jentschke of the Deutsches Elektronen Synchrotron (DESY) about the possibility of doing a pair production experiment at Hamburg. They were very enthusiastic and encouraged me to begin right away. In March 1966 I took leave from Columbia University to perform this experiment in Hamburg. Since that time I have devoted all my efforts to the physics of electron or muon pairs, investigating quantum electrodynamics, production and decay of photon-like particles, and searching for new particles which decay to electron or muon pairs. These types of experiments are characterized by the need for a high-intensity incident flux, for high rejection against a large number of unwanted background events, and at the same time the need for a detector with good mass resolution.  In order to search for new particles at a higher mass, I brought my group back to the United States in 1971 and started an experiment at Brookhaven National Laboratory. In the fall of 1974 we found evidence of a new, totally unpredicted, heavy particle – the J particle. Since then a whole family of new particles has been found.  In 1969 I joined the Physics Department of the Massachusetts Institute of Technology (MIT). In 1977, I was appointed as the first Thomas Dudley Cabot Institute Professor of Physics at MIT. In recent years it has been my privilege to be associated with M. Deutsch, A.G. Hill, H. Feshbach, W. Jentschke, H. Schopper and G. Weber. All have strongly supported me. In addition, I have enjoyed working with many very outstanding young physicists such as U. Becker, J. Burger, M. Chen, R. Marshall and A.J.S. Smith.  I married Dr. Susan Marks in 1985. We have one son, Christopher, born in 1986 and I have two daughters, Jeanne and Amy, from an earlier marriage.  I have been awarded the Ernest Orlando Lawrence Award from the US government in 1976 and the DeGasperi Award in Science from the Italian government in 1988. I have also received the Eringen Medal awarded by the Society of Engineering Science in 1977, the Golden Leopard Award for Excellence from the town of Taormina, Italy in 1988 and the Gold Medal for Science and Peace from the city of Brescia, Italy in 1988. I am a member of the National Academy of Sciences (US) and the American Physical Society, the Italian Physical Society and the European Physical Society. I have also been elected as a foreign member in Academia Sinica, the Pakistan Academy of Science and the Academy of Science of the USSR (now Russian Academy of Science). I also hold Doctor Honoris Causa degrees from the University of Michigan, The Chinese University of Hong Kong, Columbia University, the University of Bologna, Moscow State University and the University of Science and Technology in China and am an honorary professor at Jiatong University in Shanghai, China. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0124 |
| **Biographical** | I was born in Copenhagen on June 19, 1922, as the fourth son of [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html) and Margrethe Bohr (née Nørlund). During my early childhood, my parents lived at the Institute for Theoretical Physics (now the Niels Bohr Institute), and the remarkable generation of scientists who came to join my father in his work became for us children Uncle Kramers, Uncle Klein, Uncle Nishina, Uncle Heisenberg, Uncle Pauli, etc. When I was about ten years old, my parents moved to the mansion at Carlsberg, where they were hosts for widening circles of scholars, artists, and persons in public life.  I went to school for twelve years at Sortedam Gymnasium (H. Adler’s fæellesskole) and am indebted to many of my teachers, both in the humanities and in the sciences, for inspiration and encouragement.  I began studying physics at the University of Copenhagen in 1940 (a few months after the German occupation of Denmark). By that time, I had already begun to assist my father with correspondence, with his writing of articles of a general epistemological character, and gradually also in connection with his work in physics. In those years, he was concerned partly with problems of nuclear physics and partly with problems relating to the penetration of atomic particles through matter.  In October 1943, my father had to flee Denmark to avoid arrest by the Nazis, and the whole family managed to escape to Sweden, where we were warmly received. Shortly afterwards, my father proceeded to England, and I followed after him. He became associated with the atomic energy project and, during the two years until we returned to Denmark, in August 1945, we travelled together spending extensive periods in London, Washington, and Los Alamos. I was acting as his assistant and secretary and had the opportunity daily to share in his work and thoughts. We were members of the British team, and my official position was that of a junior scientific officer employed by the Department of Scientific and Industrial Research in London. In another context, I have attempted to describe some of the events of those years and my father’s efforts relating to the prospects raised by the atomic weapons[1](https://www.nobelprize.org/prizes/physics/1975/bohr/biographical/#footnote).  On my return to Denmark, I resumed my studies at the University and obtained a master’s degree in 1946. My thesis was concerned with some aspects of atomic stopping problems.  For the spring term of 1948, I was a member of the Institute for Advanced Study in Princeton. On a visit during that period to Columbia University and through discussions with professor [I.I. Rabi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/index.html), I became interested in a newly discovered effect in the hyperfine structure in deuterium. This led on to my association with Columbia University from January 1949 to August 1950. As described in my lecture, this was for me a very fruitful association.  Soon after my return to Copenhagen, I began the close cooperation with Ben Mottelson which has continued ever since. The main direction of our work is described in the lectures included in the present volume. During the last fifteen years, a major part of our efforts has been connected with the attempt to present the status of our understanding of nuclear structure in a monograph, of which Volume I (Single-Particle Motion) appeared in 1969, and Volume II (Nuclear Deformations) in 1975. We feel that in our cooperation, we have been able to exploit possibilities that lie in a dialogue between kindred spirits that have been attuned through a long period of common experience and jointly developed understanding. It has been our good fortune to work closely together with colleagues at the Niels Bohr Institute and Nordita, including the many outstanding scientists who have come from all parts of the world and have so greatly enriched the scientific atmosphere and personal contacts.  I have been connected with the Niels Bohr Institute since the completion of my university studies, first as a research fellow and from 1956 as a professor of physics at the University of Copenhagen. After the death of my father in 1962, I followed him as director of the Institute until 1970.  For our whole circle, it has been a challenge to exploit the opportunities provided by the traditions of the Institute, of which I would like especially to mention two aspects. One concerns the fruitful interplay between experimental and theoretical investigations. The other concerns the promotion of international cooperation as a vital factor in the development of science itself and also as a means to strengthen the mutual knowledge and understanding between nations.  In 1957, Nordita (Nordisk Institut for Teoretisk Atomfysik) was founded on the premises of the Niels Bohr Institute, and the two institutes operate in close association. I have been a member of the Board of Nordita from 1957 until 1975, and since then director of this institute.  In March 1950, in New York City, I was married to Marietta Soffer. We have three children, Vilhelm, Tomas, and Margrethe. Both for my wife and myself, the personal friendships that have grown out of scientific contacts with colleagues from many different countries have been an important part of our lives, and the travels we have made together in connection with the world-wide scientific co-operation have given us rich treasures of experiences. |
| **Autobiographical** |  |
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| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0125 |
| **Biographical** | I was born in Chicago, Illinois, on July 9, 1926, the second of three children of Goodman Mottelson and Georgia Mottelson (*née* Blum). My father held a university degree in engineering. My childhood home was a place where scientific, political and moral issues were freely and vigorously discussed. I attended primary school and high school in the village of La Grange, Illinois.  Graduating from high school during the second world war, I was sent by the U.S. Navy to Purdue University for officers training (V12 program) and remained there to receive a Bachelor of Science degree in 1947. My graduate studies were at Harvard University and my PhD work on a problem in nuclar physics was directed by Professor [Julian Schwinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html) and completed in 1950.  Receiving a Sheldon Traveling Fellowship from Harvard University I chose to spend the year (1950-51) at the Institute for Theoretical Physics in Copenhagen (later the Niels Bohr Institute) where so much of modern physics had been created and where there were such special traditions for international cooperation. A fellowship from the U.S. Atomic Energy Commission permitted me to continue my work in Copenhagen for two more years after which I held a research position in the CERN (European Organization for Nuclear Research) theoretical study group that was formed in Copenhagen. With the founding of the Nordic Institute for Theoretical Atomic Physics in Copenhagen (1957) I received a position as professor which I have held since. The spring term of 1959 was spent as visiting professor in the University of California at Berkeley.  The close scientific collaboration with Aage Bohr was begun in 1951 and has continued ever since. We feel that in this cooperation we have been able to exploit possibilities that lie in a dialogue between kindred spirits that have been attuned through a long period of common experience and jointly developed understanding. The lectures that are published in this volume attempt a discussion of the main influences that we have built on and the viewpoints that have been developed in this collaboration. It has been our good fortune to work closely together with colleagues at the Niels Bohr Institute and Nordita, including the many outstanding scientists who have come from all parts of the world and have so enriched the scientific atmosphere and personal contacts.  Married Nancy Jane Reno, 1948 (dec. 1975); 3 children, Malcolm Graham (1950), Daniel John (1953), Martha (1954). Married Britta Marger Siegumfeldt, 1983.\* |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0126 |
| **Biographical** | I was born December 9, 1917 in a small town in Idaho (Council) where my parents had moved to from California to operate a general store. My father, who had previously been a civil engineer, died in the great influenza epidemic of 1918. My mother then moved with me and her mother to Hanford, Calif. in the San Joaquin Valley of California, where she was re-married to George Fowler a few years later. In my schooling through high school, I excelled mainly in chemistry, physics and mathematics. Due mainly to my record on an open chemistry competition given by Cal Tech, I was admitted, graduating in 1939 as a physics major. [Carl David Anderson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1936/index.html) was my physics group recitation instructor when he received his Nobel Prize and Milliken was the President of the Institute. I had a short biology course taught by Thomas Hunt Morgan. In 1939 I began graduate study in physics as a teaching assistant at Columbia University where I have remained. During the first two years, I had courses under [I.I. Rabi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/index.html), [Enrico Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html), Edward Teller and J.R. Dunning. Fermi was working on neutron moderator assemblies which led to the first working nuclear “pile” after his group was moved to Chicago. Dunning, Booth, Slack, and Von Grosse held the basic patent on the gaseous diffusion process for 235U enrichment and were working on its development. This evolved into the Oak Ridge enrichment plants and the present U.S. technology for 235U enrichment.  In March 1942, I married Emma Louise Smith. We have three sons, James, Robert and William who are all now adults. We also had a daughter, Elizabeth Ann, who died while young.  During W.W. II, I worked with W.W. Havens, Jr. and C.S. Wu under Dr. Dunning (Manhattan Project) mainly doing pulsed neutron spectroscopy using the small Columbia cyclotron. I received my Ph.D after my thesis was de-classified in 1946. I continued at Columbia, first as an instructor, reaching the rank of full professor in 1952. About 1946 funding was obtained from the Office of Naval Research to build a synchrocyclotron which became operational in early 1950. I was involved with the facility development from the beginning and my research has used that facility ever since. The research included neutron resonance spectroscopy, the angular distribution of pion elastic and inelastic scattering on nuclei with optical model fitting. Best known are the muanic-atom-x-ray studies starting with the pioneering 1953 paper with [Val Fitch](https://www.nobelprize.org/nobel_prizes/physics/laureates/1980/index.html) which first established the smaller proton charge radii of nuclei.  Starting in 1948, I taught an advanced nuclear physics graduate course. The Maria Mayer shell model suggestion in 1949 was a great triumph and fitted my belief that a nuclear shell model should represent a proper approach to understanding nuclear structure. Combined with developments of Weizsaker’s semi-empirical explanation of nuclear binding, and the Bohr-Wheeler 1939 paper on nuclear fission, emphasizing distorted nuclear shapes, I was prepared to see an explanation of large nuclear quadrupole moments. The full concept came to me in late 1949 when attending a colloquium by Prof. C.H. Townes who described the experimental situation for nuclear quadrupole moments. It was a fortuitous situation made even more so by the fact that I was sharing an office with Aage Bohr that year. We had many discussions of the implications, subsequently very successfully exploited by Bohr, Mottelson, and others of the Copenhagen Institute.  Since I joined the Columbia Physics Dept., in 1939, it has been my privilege to have as teachers and/or colleagues many previous Nobel Laureates in Physics: E. Fermi, I.I. Rabi, [H. Bethe](https://www.nobelprize.org/nobel_prizes/physics/laureates/1967/index.html) (Visiting Prof.), [P. Kusch, W. Lamb](https://www.nobelprize.org/nobel_prizes/physics/laureates/1955/index.html), [C.H. Townes](https://www.nobelprize.org/nobel_prizes/physics/laureates/1964/index.html), [T.D. Lee](https://www.nobelprize.org/nobel_prizes/physics/laureates/1957/index.html) and [L. Cooper](https://www.nobelprize.org/nobel_prizes/physics/laureates/1972/index.html) in addition to [R.A. Milliken](https://www.nobelprize.org/nobel_prizes/physics/laureates/1923/index.html), [C.D. Anderson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1936/index.html), and [T.H. Morgan](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1933/index.html) (Biology) while I was an undergraduate at Cal Tech. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0127 |
| **Biographical** | I was born on September 27, 1918, the second of five children. My father John A. Ryle was a doctor who, after the war, was appointed to the first Chair of Social Medicine at Oxford University.  I was educated at Bradfield College and Oxford, where I graduated in 1939. During the war years I worked on the development of radar and other radio systems for the R.A.F. and, though gaining much in engineering experience and in understanding people, rapidly forgot most of the physics I had learned.  In 1945 J.A. Ratcliffe, who had been leading the ionospheric work in the Cavendish Laboratory, Cambridge before the war, suggested that I apply for a fellowship to join his group to start an investigation of the radio emission from the Sun, which had recently been discovered accidentally with radar equipment.  During these early months, and for many years afterwards both Ratcliffe and Sir Lawrence Bragg, then Cavendish Professor, gave enormous support and encouragement to me. Bragg’s own work on X-ray crystallography involved techniques very similar to those we were developing for “aperture synthesis”, and he always showed a delighted interest in the way our work progressed.  In 1948 I was appointed to a Lectureship in Physics and in 1949 elected to a Fellowship at Trinity College. At this time Tony Hewish joined me, and in fact four other members of our present team started their research during the period 1948-52.  In 1959 the University recognized our work by appointing me to a new Chair of Radio Astronomy.  During 1964-7 I was president of Commission 40 of the International Astronomical Union, and in 1972 was appointed Astronomer Royal.  In 1947 I married Rowena Palmer, and we have two daughters, Alison and Claire, and a son, John. We enjoy sailing small boats, two of which I have designed and built myself. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0128 |
| **Biographical** | I was born in Fowey, Cornwall, on 11 May 1924, the youngest of three sons and my father was a banker. I grew up in Newquay, on the Atlantic coast and there developed a love of the sea and boats. I was educated at King’s College, Taunton and went to the University of Cambridge in 1942. From 1943-46 I was engaged in war service at the Royal Aircraft Establishment, Farnborough and also at the Telecommunications Research Establishment, Malvern. I was involved with airborne radar-counter-measure devices and during this period I also worked with Martin Ryle.  Returning to Cambridge in 1946 I graduated in 1948 and immediately joined Ryle’s research team at the Cavendish Laboratory. I obtained my Ph.D. in 1952, became a Research Fellow at Gonville and Caius College where I had been an undergraduate, and in 1961 transferred to Churchill College as Director of Studies in Physics. I was University Lecturer during 1961-69, Reader during 1969-71 and Professor of Radio Astronomy from 1971 until my retirement in 1989. Following Ryle’s illness in 1977 I assumed leadership of the Cambridge radio astronomy group and was head of the Mullard Radio Astronomy Observatory from 1982-88.  My decision to begin research in radio astronomy was influenced both by my wartime experience with electronics and antennas and by one of my teachers, Jack Ratcliffe, who had given an excellent course on electromagnetic theory during my final undergraduate year and whom I had also encountered at Malvern. He was head of radiophysics at the Cavendish Laboratory at that time.  My first research was concerned with propagation of radiation through inhomogeneous transparent media and this has remained a lifelong interest. The first two radio “stars” had just been discovered and I realised that their scintillation, or “twinkling”, could be used to probe conditions in the ionosphere. I developed the theory of diffraction by phase-modulating screens and set up radio interferometers to exploit my ideas. Thus I was able to make pioneering measurements of the height and physical scale of plasma clouds in the ionosphere and also to estimate wind speeds in this region. Following our Cambridge discovery of interplanetary scintillation in 1964 I developed similar methods to make the first ground-based measurements of the solar wind and these were later adopted in the USA, Japan and India for long term observations. I also showed how interplanetary scintillation could be used to obtain very high angular resolution in radio astronomy, equivalent to an interferometer with a baseline of 1000 km – something which had not then been achieved in this field. It was to exploit this technique on a large sample of radio galaxies that I conceived the idea of a giant phased-array antenna for a major sky survey. This required instrumental capabilities quite different from those of any existing radio telescope, namely very high sensitivity at long wavelengths, and a multi-beam capability for repeated whole-sky surveys on a day to day basis.  I obtained funds to construct the antenna in 1965 and it was completed in 1967. The sky survey to detect all scintillating sources down to the sensitivity threshold began in July. By a stroke of good fortune the observational requirements were precisely those needed to detect pulsars. Jocelyn Bell joined the project as a graduate student in 1965, helping as a member of the construction team and then analysing the paper charts of the sky survey. She was quick to spot the week to week variability of one scintillating source which I thought might be a radio flare star, but our more detailed observations subsequently revealed the pulsed nature of the signal.  Surprisingly, the phased array is still a useful research instrument. It has been doubled in area and considerably improved over the years and one of my present interests is the way our daily observations of scintillation over the whole sky can be used to map large-scale disturbances in the solar wind. At present this is the only means of seeing the shape of interplanetary weather patterns so our observations make an useful addition to *in-situ* measurements from spacecraft such as Ulysses, now (1992) on its way to Jupiter.  Looking back over my forty years in radio astronomy I feel extremely privileged to have been in at the beginning as a member of Martin Ryle’s group at the Cavendish. We were a closely-knit team and besides my own research programmes I was also involved in the design and construction of Ryle’s first antennas employing the novel principle of aperture synthesis.  Teaching physics at the University, and more general lecturing to wider audiences has been a major concern. I developed an association with the Royal Institution in London when it was directed by Sir Lawrence Bragg, giving one of the well known Christmas Lectures and subsequently several Friday Evening Discourses. I believe scientists have a duty to share the excitement and pleasure of their work with the general public, and I enjoy the challenge of presenting difficult ideas in an understandable way.  I have been happily married since 1950. My son is a physicist and obtained his Ph.D. for neutron scattering in liquids, while my daughter is a language teacher. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0129 |
| **Biographical** | Leo Esaki was born in Osaka, Japan in 1925. Esaki completed work for a B.S. in Physics in 1947 and received his Ph.D in 1959, both from the University of Tokyo. Esaki is an IBM Fellow and has been engaged in semiconductor research at the IBM Thomas J. Watson Research Center, Yorktown Heights, New York, since 1960. Prior to joining IBM, he worked at the Sony Corp. where his research on heavily-doped Ge and Si resulted in the discovery of the Esaki tunnel diode; this device constitutes the first quantum electron device. Since 1969, Esaki has, with his colleagues, pioneered “designed semiconductor quantum structures” such as man-made superlattices, exploring a new quantum regime in the frontier of semiconductor physics.  The Nobel Prize in Physics (1973) was awarded in recognition of his pioneering work on electron tunneling in solids. Other awards include the Nishina Memorial Award (1959), the Asahi Press Award (1960), the Toyo Rayon Foundation Award for the Promotion of Science and Technology (1960), the Morris N. Liebmann Memorial Prize from IRE (1961), the Stuart Ballantine Medal from the Franklin Institute (1961), the Japan Academy Award (1965), the Order of Culture from the Japanese Government (1974), the American Physical Society 1985 International Prize for New Materials for his pioneering work in artificial semiconductor superlattices, the IEEE Medal of Honor in 1991 for contributions to and leadership in tunneling, semiconductor superlattices, and quantum wells. Dr. Esaki holds honorary degrees from Doshisha School, Japan, the Universidad Politecnica de Madrid, Spain, the University of Montpellier, France, Kwansei Gakuin University, Japan and the University of Athens, Greece. Dr. Esaki is a Director of IBM-Japan, Ltd., on the Governing Board of the IBM-Tokyo Research Laboratory, a Director of the Yamada Science Foundation and the Science and Technology Foundation of Japan. He serves on numerous international scientific advisory boards and committees, and is an Adjunct Professor of Waseda University, Japan. Currently he is a Guest Editorial writer for the Yomiuri Press. Dr. Esaki was elected a Fellow of the American Academy of Arts and Sciences in May 1974, a member of the Japan Academy on November 12, 1975, a Foreign Associate of the National Academy of Engineering (USA) on April 1, 1977, a member of the Max-Planck-Gesellschaft on March 17, 1989, and a foreign member of the American Philosophical Society in April of 1991. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q3 | **I would like to start off by asking you what made you interested in physics at the very beginning. Why did you choose that subject?** |
|  | Leo Esaki: When I was at high school I had the opportunity of what kind of field I should go into. This is during the war. Distraction, Japanese suffer of that. I was at high school and university. At that time I liked to get fundamental knowledge in that circumstance and I wanted more basic knowledge in the natural science, physics certainly most fundamental. If you know physics I think you can do many other things. During the psychology of war you like to know – before you die – to understand the most fundamental things. |
| Q18 | **Did you want to be part of rebuilding Japan?** |
|  | Leo Esaki: This is after the war. I entered university in 1944 and I graduated university -47. The war ended in -45. I entered Tokyo Imperial University and after the war when I graduated just Tokyo University. After I graduated Japan was in a bad situation. Everything was destructed and Japanese industry business was in poor condition. I could have stayed at the university to study, but I decided to go into industry. |
| Q10 | **Was it better for you to go into industry rather than stay on at university?** |
|  | Leo Esaki: I thought I could contribute to the Japanese industry because the Japanese economy and industry were so bad so this was my prime motivation to go into industry. |
| Q9 | **You made your major discovery in 1957 which then you were awarded the Nobel Prize for, that was at Sony. After that you moved to the United States. Was that a difficult transition for you, to go from Japan to the United States in 1957 or -60?** |
|  | Leo Esaki: What do you mean difficult? It was easy because I got many offers from the United States, in that aspect very easy, I just accepted one. Of course culture is very different between Japan and the United States so that made it slightly difficult. Way of thinking and how you do many things different. Sort of a cultural shock for me to move from Japan to the United States. But science is a more international enterprise, it’s a global enterprise. As a scientist I don’t think it was much of a problem. It was more problems of daily life, a different lifestyle. Even the language English – Japanese to English is a big difference – so that created some problems of course.  After two years I decided to stay in the United States, but I had a call from Japan, so I had another culture shock – from United States to go back to Japan. In the United States I worked in industry. As I mentioned, after graduating my university I entered Japanese industry, I changed from American industry, but I got an offer from Japan, I was called from the President of the National University. I went from scientist in a private industry to educator at this national university. The culture shock once again, United States to Japan, industrial researcher to government educator. |
| Q10 | **I would like to come back to your move going back from United States to Japan just in a moment, but to stay in the United States for a little bit longer: you were working at IBM, what was it there that you found useful working the industry over those years? You said as a scientist it wasn’t difficult, but daily life was a shock. As a scientist at IBM, what culture did you find there that was creative for a scientist of your status?** |
|  | Leo Esaki: I joined IBM in 1960 and then after a few years I was elected IBM Fellow. Being a Fellow is I was given freedom, suddenly. More obligation to the company but I could do anything I wanted, so I got the idea to make a man-made quantum structure. Superlattices is one of the man-made quantum structures. I’m a material scientist and usually you study many materials, but I got the idea that I’d like to create material, I like to design, I like to engineer man-made materials. This is a new avenue of material science. We like to make man-made material that doesn’t exist in nature. These synthesized new materials had totally new properties. The materials – usually God gave these materials to us – gold and silver. God gives us creativity so I can make and create materials which God never gives us. The new materials had very many interesting properties, we started those programmes. In material science somebody has to prepare the materials. Somebody has to make the measurements – this is not one man’s effort. I organised a group, not a very big group, about ten people, to work with me. This is the basic research, not very committed. |
| Q10 | **Can I come back to when you moved back from the United States to Japan, you said it was a culture shock. What were the biggest difficulties at that time? Was there something you wanted to bring with you from America that was difficult to implement in Japan?** |
|  | Leo Esaki: That the culture is very different between Japan and the United States is one thing, but the other is that in the United States I was a researcher, but in Japan I’m President of that university which is more an educator. For scientists, you need more creative minds. For an educator, you need a judicious mind because it’s a national university. You’re a bureaucrat, a government officer. |
| Q10 | **Do you miss that part of the creativeness as a researcher when you had to go back and be more of an educator?** |
|  | Leo Esaki: I think I missed the research, I missed that direct contact of that activities. |
| Q10 | **Is there also a difference in the hierarchical structure within the research departments if you compare between the United States and Japan? Has the Japanese a more hierarchical structure and in which way could that either enhance or diminish creativity?** |
|  | Leo Esaki: United States is more bottom-up kind of activity for research but Japan is generally top down, so that’s slightly different. But certainly in Japan that is changing very fast now to give more opportunities to young scientists and young people, to be more encouraged and creative. History tells us to progress science you need two things. One is individual creativity, the other is the dynamic interaction among scientists. That dynamic interaction is very active in the United States, so that’s one of the progresses for the United States’ scientists, very easy dynamic interactions – more open with things. Of course individual creativity is more fundamental, necessarily. |
| Q14 | **My last question, Professor, would be: what is out there to discover? What is your biggest question mark? If you had a wish list, what would you ask somebody or what is the big discovery to be done in physics?** |
|  | Leo Esaki: We still need a breakthrough in many ideas. Communication, computer fields. One breakthrough we can expect is quantum computers and quantum telecommunications, using the principle of quantum mechanics. Quantum mechanics – already in the 20th century that was formulated, quantum mechanics implemented many things, but the 21st century we will use principle of quantum mechanics, there is one principle of the super position, the super position principle. If you use those principles, you can make quantum computers which is a vast improvement of the present computers. If you use that quantum communication you can use the cryptography. You cannot decode your code, you could make very safe communication. I think that’s one of the fields which is a big project of the Japanese government in quantum telecommunications, and I’m organising these activities. We need individual creativity to make these kinds of things, I think this is one of the important things. The communication is very important to everybody. Everybody has a portable phone and a PC, and we still need improvement in those fields. |

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| **Physics\_1999-** | |
| **ID** | 0130 |
| **Biographical** | Ivar Giaever was born in Bergen, Norway, April 5, 1929, the second of three children. He grew up in Toten where his father, John A. Giaever, was a pharmacist. He attended elementary school in Toten but received his secondary education in the city of Hamar. Next he worked one year at the Raufoss Munition Factories before entering the Norwegian Institute of Technology in 1948. He graduated in 1952 with a degree in mechanical engineering.  In 1953, Giaever completed his military duty as a corporal in the Norwegian Army, and thereafter he was employed for a year as a patent examiner for the Norwegian Government.  Giaever emigrated to Canada in 1954 and after a short period as an architect’s aide he joined Canadian General Electric’s Advanced Engineering Program. In 1956, he emigrated to the USA where he completed the General Electric Company’s A, B and C engineering courses. In these he worked in various assignments as an applied mathematician. He joined the General Electric Research and Development Center in 1958 and concurrently started to study physics at Rensselaer Polytechnical Institute where he obtained a Ph.D. degree in 1964.  From 1958 to 1969 Dr. Giaever worked in the fields of thin films, tunneling and superconductivity. In 1965 he was awarded the Oliver E. Buckley Prize for some pioneering work combining tunneling and superconductivity. In 1969 he received a Guggenheim Fellowship and thereupon spent one year in Cambridge, England studying biophysics. Since returning to the Research and Development Center in 1970, Dr. Giaever has spent most of his effort studying the behavior of protein molecules at solid surfaces. In recognition of his work he was elected a Coolidge fellow at General Electric in May, 1973.  Dr Giaever is a member of the Institute of Electrical and Electronic Engineers, and the Biophysical Society, and he is a Fellow of the American Physical Society. Dr. Giaever has served on committees for several international conferences and presently he is a member of the Executive Committee of the Solid State division in the American Physical Society.  Ivar Giaever married Inger Skramstad in 1952 and they have four children. He became a naturalized US citizen in 1964.  **Notes added** Linus Pauling is reported to have said that the Nobel Prize did not change his life – he was already famous! That was not true for me. The Nobel Prize opened a lot of doors, but also provided me with many distractions. I have, however, continued to work in biophysics, attempting to use physical methods and thoughts to solve biological problems. At the present time, I am studying the motion of mammalian cells in tissue culture by growing both normal and cancerous cells on small electrodes.  I left General Electric in 1988 to become an Institute Professor at Rensselaer (RPI) in Troy, New York 12180-3590, and concurrently I am also a Professor at the University of Oslo, Norway, sponsored by STATOIL.  On a personal note my wife and I are now the proud grandparents of almost four grandchildren. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q10 | **You said you were working at General Electric. You left Norway at quite an early age to move to Canada and eventually you came to the United States. What made you leave Norway and with what kind of spirit did you go?** |
|  | Ivar Giaever: I was not a very good student and I had mediocre grades from Norway, but I had no difficulty at getting a job, but I could not get a place to live. This was in 1952-53 and it was impossible to get a place to live. I was married with a child, and we tried to do that and we couldn’t do it. Then what precipitated the move was that I roomed with a friend of mine in Oslo. My wife stayed up in the country with her parents with the child. He said: You have to go and register to a waiting line, and I said: No, I’m not going to do that because a waiting line is eight years. I’m not going to wait for eight years, no way. He said: Register anyway. He took me down to the office and I registered and then the guy said: Where does your wife live? and I said: Up in the country with her parents. And he said: You can’t register now because your wife doesn’t live in Oslo. I recognised this is what we call “catch 22” in the United States and so I left. I got so … I didn’t get mad, but I recognised I wanted to leave, so I went to Canada because it was very easy to get a visa to Canada at that time. United States took about a year, but I left actually three weeks later to Canada. |
| Q17 | **In what spirit did you leave? You seem to be a person who goes out, looks for opportunities, a very entrepreneurial spirit. Did you have that with you from the beginning?** |
|  | Ivar Giaever: No, I don’t think so. I think that’s something I’ve learned in America, in Canada and in the United States. When I got to Canada it was very, very difficult to get a job because Canadian economy even today is seasonal. I came there before Christmas and they said: Merry Christmas, why don’t you come back in the Spring when we hire people? We came to Canada with $200, that’s because that’s all the money you’re allowed to bring out of Norway; $100 per person, so I know exactly what kind of money I’ve had. |
| Q3 | **Were you into science already then? Did you know you wanted to do research?** |
|  | Ivar Giaever: No. Absolutely not. As I said I was a mediocre student and when I worked in Canada, General Electric had a course, they called it A course and a B course and a C course for over three years to try to teach engineers more science. At that time it wasn’t common to have a PhD degree in engineering. I took the A course in Canada and I liked that a lot and I worked very hard. Sooner or later you’ve got to work hard to catch up for what you haven’t done. Then they didn’t have the B course, but in the United States they had a B course. I also found out that they made 30 percent more money in the United States than in Canada, so the choice was easy. I left and went to the United States.  To tell the true story really, as I said, in Norway I had very bad grades and in Norway the grades were such that one zero is the best you can get, six zero is the worst you can get and four zero you’re just about failing, like a D in the United States. I got four zero in mathematics and four zero in physics. When I got down to General Electric, I was interviewed and the guy says: Oh, I see you have good grades, four zero in mathematics and four zero in physics, so you must be pretty good, because of course four zero is the best you can get in the United States. I’m normally an honest person, but I have to admit that I just mumbled something at that particular point. I didn’t think that was the right time to explain to him what the difference in grade meant. |
| Q10 | **You stayed on at General Electric didn’t you? I mean what kind of an environment was it?** |
|  | Ivar Giaever: It was wonderful when I came because General Electric had this what they call the test program and they hired engineers and you could work with different people for maybe an assignment for three to six months at the time. I worked, I came down to Connecticut in New York. I looked at all the people who were there and I picked up the most famous people. And the people said: Oh no, you shouldn’t do that because these people are very difficult to work for. But I stuck to that, and I wanted to work for these famous people and that paid off very generously for me. |
| Q3 | **In which way were they inspiring you, these people?** |
|  | Ivar Giaever: Because they were people who were exceedingly well thought of, were very good. I worked for a German mathematician for example named Buechner. He was a fantastic person, and it worked this way. Then I thought I was going to be an applied mathematician, so I worked with him, then at that time, right before the computers really, so we worked in parallel. We have a problem and then you have to write down the equations, square root of two and plus and minus, and you make mistakes. Every week we worked in parallel and then every week we compared the results. And every week when we got in there, if we did not agree, he calmly took my paper and said: Let me see where *you* have gone wrong. I was always wrong and he was always right, so I recognised I couldn’t compete with him so I had to do something simpler like physics. |
| Q2 | **It seems that during your time at General Electric … You both had to feed a family and you were working very hard and you learnt a lot. Was there a change? You said you went into physics, did you then realise that science was your topic?** |
|  | Ivar Giaever: As I said I was on this General Electric program where you have different assignments and I had an assignment at General Electric research lab. Then I made what I call my biggest discovery. I discovered that people could get paid for doing research. I was completely flabbergasted. I never even heard of that. And these people ran around, sat on the windowsill discussed things, wrote on the blackboard, looked like they had a good time. I said that’s what I wanted to do, so after I was finished I went and I asked if I could get a job there. And I was fortunate, they hired me on a trial period for a year, and they never said I was hired really. Nobody remembered I was hired on a trial, or something. I was very lucky because basically they don’t hire people who don’t have a PhD degree, so I was just lucky. There was a man named John Fisher who took care of me. He was a wonderful person. |
| Q6 | **Did any of this spirit that you have carried with you filtered down to your children, or to people around you? Have they learnt from you?** |
|  | Ivar Giaever: I don’t know. I can’t call on that. I’m very happy with my kids and my wife, and my kids have been very successful. I have one child who’s in biophysics and has a PhD degree and working for Stanford, so they’ve all done very well in their own field. |
| Q3 | **In one of your lectures that I recently heard, you were talking about how to start the business and specifically for United States, but there were some general tips and ideas that you came up with. For you, what was the reason to become an entrepreneur and start your own business a few years back?** |
|  | Ivar Giaever: The reason for that was that in 1988 I left General Electric. The reason I left General Electric is that we got a new director and his motto or whatever you want to call it, was that from now on, he said, all research has to be directly connected with present General Electric processes and products. I was working with biology and I wasn’t connected at all. It wasn’t that he didn’t like my work, but I was sort of an embarrassment because I was not directly connected with General Electric, so we decided in a sort of mutual decision that I should leave, and I’m very happy that I left. I should have left before as a matter of fact because I like it at the university, so that was very good. But I worked with a friend at General Electric named Charles Keese and when I left to RPI, he wasn’t willing to go. Then I went and after a year he said: I’d like to come too, but then I lost my negotiation powers. I didn’t have any powers because I already was there, so he came on soft money. That means in the United States that you have to get grants to support yourself, and we found out that to be very difficult and then we decided that it was maybe easier to get grants for doing business. You can get that in the United States, called a SBIR program, small business innovating research program. This is a wonderful program where they then give grants to small businesses to try out things and we were successful in doing that. But when we started the business, I was not very happy about that because I didn’t really regard myself as a business person, but now I’m very happy about it because it’s a different thing. |
| Q18 | **You said in your lecture that scientists are not good business people, is that true?** |
|  | Ivar Giaever: That is probably true because you think, I mean what you think is that here you do this wonderful science, this wonderful thing, and now people will come and buy these things from you, but they absolutely don’t because what you do is very difficult, to get it known among the people who need these things. And scientists also tend to be careful, so you do science, you want to have things exactly right, you want to do this and that. When you’re in business you can’t afford that. If you do something and it works, you’ve got to go with that because otherwise you have nothing. But if you’re a scientist, oh no I want to work on this for another couple of years, see if I can make it better. In that sense, I don’t think scientists are good businessmen including myself. |
| Q17 | **What did you do in your business to make it shoot up?** |
|  | Ivar Giaever: We started this business, my friend and I, Charlie Keese and I, and basically we started the business to get money to do research. We weren’t really thinking so much of making a real business, but a few years ago we got, as a third person joining us, a very wonderful guy named Chris Dennett who knows how to sell things, he knows how to market things. He came and said: You know ‘I’d like to join you guys, and that was a good decision on us and now we work together all three of us and it looks like it’s working out very well. By the time this is actually going on to the internet, we’ll probably be all broke, but that’s ok. |
| Q1 | **Advice to young people who want to go into science today, or who are already studying maybe, and see that it’s difficult to find a possibility to do the basic research and has to be more worried about the future of having to maybe go into the industry and applied research, which is more common today. What is your advice? How could one think?** |
|  | Ivar Giaever: Let me tell you what John Fisher told me when I came to General Electric. When I came I was then 30 years old and I said to him that I’d really like to learn some physics, but actually I know I’m too old to make any discovery in physics because most physicists do that when they’re in their twenties or something. John said to me: No, no you’re not too old, he said. You make discoveries when you’re learning. If you start learning at 30, you can still make discoveries. And, he said, I give you the advice that when you’ve worked in physics for a while maybe you should change your field again because then you learn again. The fun thing in life is just learning different things. I don’t understand this business about fundamental science and basic research and applied research and things because I don’t think people recognise that we know almost everything today. The science is not an infinite field, it’s a finite field and a certain number of rules and we practically know all the rules in science today. People don’t agree with me with that, but that’s my particular feeling. So therefore, I think that if I give people advice today I think the action is going to be making inventions in the future. I think that’s what people should try to do. See how they can make invention, how they make things better, how they combine different things. I think that’s where the action is going to be. Like the laser for example is really an invention. Take the knowledge of what they had and invent the laser. Magnetic imaging is an invention and people should just think about how can they do things better and invent things better. There are no new laws that are going to come out, maybe one or two, but basically we know most of the things. |
| Q14 | **What is the big challenge then if you look at the medical side for example?** |
|  | Ivar Giaever: I work in biology and there is no law in biology. All the laws in biology comes from chemistry and physics, but the exciting thing is to work in biology because it’s easy to state problems and a lot of unknown things. For example, I sit here and talk to you and tomorrow you’ll remember hopefully some of that. That means that I have changed your brain. Maybe I have damaged your brain, I don’t know. |

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| **Physics\_1999-** | |
| **ID** | 0131 |
| **Biographical** | Date of birth: 4 January 1940    Place of birth: Cardiff, Wales, U.K.      **Education**Cardiff High School    University of Cambridge, B.A.  1960  University of Cambridge, M.A., Ph.D  1964      **Academic Career**    Fellow of Trinity College, Cambridge  1962  Research Assistant Professor, University of Illinois  1964-65  Assistant Director of Research, University of Cambridge  1967-72  NSF Senior Foreign Scientist Fellow, Cornell University  1971  Reader in Physics, University of Cambridge  1972-74  Professor of Physics, University of Cambridge  1974-  Visiting Professor – Computer Science Department, Wayne State University, Detroit  1983  Visiting Professor, Indian Institute of Science, Bangalore  1984  Visiting Professor, University of Missouri-Rolla  1987      **Awards**    New Scientist  1969  Research Corporation  1969  Fritz London  1970      **Medals**    Guthrie (Institute of Physics) 1972  1972  van der Pol 1972  1972  Elliott Cresson (Franklin Institute) 1972  1972  Hughes (Royal Society) 1972  1972  Holweck (Institute of Physics and French Institute of Physics) 1972  1972  Faraday (Institution of Electrical Engineers) 1982  1982  Sir George Thomson (Institute of Measurement and Control) 1984  1984      **Other Information**    Fellow of the Institute of Physics    Honorary D.Sc., University of Wales 1974  1974  Honorary Member, American Academy of Arts and Sciences 1974  1974  Honorary Member, Institute of Electrical and Electronic Engineers 1982  1982  Honorary D.Sc., University of Exeter 1983  1983  Invited presentation on subject of ‘Higher States of Consciousness’, to US Congressional Committee 1983  1983 |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q9 | **Professor Josephson, welcome to this interview. We’re very happy to have you here today. You were very young when you got the prize, only 33, and when you made your major discovery you were 22. In which way has this had an impact on your life, to start off with?** |
|  | Brian Josephson: It’s made it very busy. I get lots of invitations, most of which I have to turn down. It’s given me a bit more freedom in that I can work on my interests which are on the unconventional side, without people feeling that they can say: You can’t work on that. Some things are a bit of a nuisance, the amount of correspondence one gets, with a corresponding increase in secretarial help. So it has its good and bad sides and it does not seem to help one get grants I’m afraid. |
| Q20 | **Is it important to be young? I mean you were very young. Is that the height of your creativity or what is the major quality that you need?** |
|  | Brian Josephson: I suppose being young helped. I was a bit of a child prodigy so maybe I was more likely to see deep ideas than most people. But I feel that I’m still just as creative and in fact the work I’ve doing on the brain I think that’s in a way more significant and more difficult than the superconductivity work. |
| Q2 | **You say your creativity is just as great now as it was, that’s your feeling. It has nothing to do with age but it has to do with curiosity?** |
|  | Brian Josephson: Yes, I think curiosity has a lot to do with it. |
| Q4 | **Next year is the year that UNESCO have announced that that’s the year of**[**Einstein**](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/)**. Do you think it’s important to highlight these great scientists to make the public in general understand the need for science?** |
|  | Brian Josephson: Yes, I suppose. I think scientists themselves would want to celebrate Einstein since he had such an important role. I guess it may be a good thing to help in presenting science to the public since everybody’s heard of Einstein, so it’s really more an excuse than anything else, but there it is. |
| Q4 | **Is it difficult for scientists to at times explain or make the public at large understand the need for science?** |
|  | Brian Josephson: Perhaps the problem is the range of abilities of the public in that some people,e they can tune easily into anything, whereas other people you have to talk in a very low brow way and grossly oversimplify. |
| Q3 | **Do you feel that there is a great interest in science among young people today? What do you encounter?** |
|  | Brian Josephson: To some extent. Certainly the students here seem very interested in the Lindau meeting. But I think there’s been some turning away from science perhaps because people find it – I’m talking about the UK – that’s because people feel it’s a difficult subject and perhaps it’s not being presented as interesting. It’s a presentation problem I suppose. Or also perhaps the difficulty in getting good teachers in school. If you have a teacher who doesn’t really understand science, he or she is not going to inspire the pupils. |
| Q18 | **If you compare basic to applied research and science, what is your feeling around that? It has been maybe more a drive towards to applied research over the last 10, 15 years particularly. Industry is so focused on it.** |
|  | Brian Josephson: I feel it’s politicians as well. Some politicians don’t quite seem to understand what science is and they only think of it as something which produces practical spin offs. But I suppose the climate has changed anyway, certainly at the Cavendish where I work there’s a great emphasis on practical applications. Of course that is important if they can produce discoveries that are of practical importance. But when I was younger it seemed to be quite acceptable to do pure research and now it’s seemingly not so important, not so accepted. |
| Q10 | **Is there a difference between America and Europe do you think? It seems like many scientists did at least move to America and for different reasons, but often to where they will … Particularly after they had an award or have made major discoveries, they were asked to come over to America. Do you see a change? Do you think it’s changing in Europe?** |
|  | Brian Josephson: That move to America is probably because there are more resources available there. I don’t know if it’s so applicable these days. One thing about public understanding of science is that TV programmes are probably quite important in getting people’s interest and there seems to have been a feeling that the programmers must entertain, which to some extent is at the cost of the science. |
| Q4 | **Can one do science more entertaining do you think – for example the research that you are involved with? Could that be made in such a way that people could find it entertaining?** |
|  | Brian Josephson: They make it entertaining with add-ons like music or jazzy effects. It’s very distracting from the science and very annoying to real scientists, so I think it’s not good. Again it’s a phenomenon of our times. |
| Q1 | **Is there any specific advice that you could give to young scientists that might listen to this interview?** |
|  | Brian Josephson: I think I might duck on that one, because I don’t know any real answers. I don’t know whether we might talk a bit about my campaign for, let’s see, when I talked about pathological disbelief … |
| Q1 | **Is it so that you have to have patience as well. We talked about advice to young students, there have been examples of scientific discoveries that were almost seen as nonsense and then almost as an embarrassment, and then of course turned out to be major discoveries. Could that be an advice to young people that they need to have the courage of standing up for your fight?** |
|  | Brian Josephson: It’s not so practical for graduate students because they only have a limited length of time and they may just not get anything accepted at all, so it’s more for people who are already established that they have to be patient. |
| Q4 | **What would you like to see as an alternative if you feel that there is a difficulty to get certain issues discussed or papers published? What could be an alternative if it at the moment is not as open minded as you would like to see?** |
|  | Brian Josephson: There are some journals that are more open minded but the problem is that they’re ignored by the scientists, so something must be done forcefully to bring scientists into contact with reality, that’s how I see it. A lecture such as the one I’ve just given does make the point rather forcibly and perhaps that will have an effect. Once people start to understand that errors are still being made like the continental drift area which people ignored extremely strong evidence for continental drift and if people can start to understand that the same kind of errors are happening today they must try as well as possible not to condemn something unless the arguments are clear. What actually happens is that the people who try to put down these subjects are arguing unscientifically but somehow they are authorities and people take what they say rather uncritically. |
| Q9 | **Just to finish off this interview, if I may ask on a more personal note, are there any memories from Stockholm that you are particularly fond of when you were there as a young person, 33 years old, one of the youngest Nobel laureates of any time?** |
|  | Brian Josephson: An ice rink readily available near the hotel. |

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| **Physics\_1999-** | |
| **ID** | 0132 |
| **Biographical** | John Bardeen was born in Madison, Wisconsin, May 23, 1908.  He attended the University High School in Madison for several years, and graduated from Madison Central High School in 1923. This was followed by a course in electrical engineering at the University of Wisconsin, where he took extra work in mathematics and physics. After being out for a term while working in the engineering department of the Western Electric Company at Chicago, he graduated with a B.S. in electrical engineering in 1928. He continued on at Wisconsin as a graduate research assistant in electrical engineering for two years, working on mathematical problems in applied geophysics and on radiation from antennas. It was during this period that he was first introduced to quantum theory by [Professor J.H. Van Vleck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/index.html).  Professor Leo J. Peters, under whom his research in geophysics was done, took a position at the Gulf Research Laboratories in Pittsburgh, Pennsylvania. Dr. Bardeen followed him there and worked during the next three years (1930-33) on the development of methods for the interpretation of magnetic and gravitational surveys. This was a stimulating period in which geophysical methods were first being applied to prospecting for oil.  Because he felt his interests were in theoretical science, Dr. Bardeen resigned his position at Gulf in 1933 to take graduate work in mathematical physics at Princeton University. It was here, under the leadership of Professor E.P. Wigner, that he first became interested in solid state physics. Before completing his thesis (on the theory of the work function of metals) he was offered a position as Junior Fellow of the Society of Fellows at Harvard University. He spent the next three years there working with Professors Van Vleck and Bridgman on problems in cohesion and electrical conduction in metals and also did some work on the level density of nuclei. The Ph.D. degree at Princeton was awarded in 1936.  From 1938-41 Dr. Bardeen was an assistant professor of physics at the University of Minnesota and from 1941-45 a civilian physicist at the Naval Ordnance Laboratory in Washington, D.C. His war years were spent working on the influence fields of ships for application to underwater ordnance and minesweeping. After the war, he joined the solid-state research group at the Bell Telephone Laboratories, and remained there until 1951, when he was appointed Professor of Electrical Engineering and of Physics at the University of Illinois. Since 1959 he has also been a member of the Center for Advanced Study of the University.  Dr. Bardeen’s main fields of research since 1945 have been electrical conduction in semiconductors and metals, surface properties of semiconductors, theory of superconductivity, and diffusion of atoms in solids. [The Nobel Prize in Physics](https://www.nobelprize.org/nobel_prizes/physics/laureates/1956/index.html) was awarded in 1956 to John Bardeen, Walter H. Brattain, and William Shockley for “investigations on semiconductors and the discovery of the transistor effect,” carried on at the Bell Telephone Laboratories. In 1957, Bardeen and two colleagues, L.N. Cooper and J.R. Schrieffer, proposed the first successful explanation of superconductivity, which has been a puzzle since its discovery in 1908. Much of his research effort since that time has been devoted to further extensions and applications of the theory. Dr. Bardeen died in 1991. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0133 |
| **Biographical** | Leon Cooper was born in 1930 in New York where he attended Columbia University (A.B. 1951; A.M. 1953; Ph.D. 1954). He became a member of the Institute for Advanced Study (1954-55) after which he was a research associate of Illinois (1955-57) and later an assistant professor at the Ohio State University (1957-58). Professor Cooper joined Brown University in 1958 where he became Henry Ledyard Goddard University Professor (1966-74) and where he is presently the Thomas J. Watson, Sr. Professor of Science (1974-).  Professor Cooper is Director of Brown University’s Center for Neural Science. This Center was founded in 1973 to study animal nervous systems and the human brain. Professor Cooper served as the first director with an interdisciplinary staff drawn from the Departments of Applied Mathematics, Biomedical Sciences, Linguistics and Physics. Today, Cooper, with members of the Brown Faculty, postdoctoral fellows and graduate students with interests in the neural and cognitive sciences, is working towards an understanding of memory and other brain functions, and thus formulating a scientific model of how the human mind works.  Professor Cooper has received many forms of recognition for his work in 1972, he received the Nobel Prize in Physics (with J. Bardeen and J.R. Schrieffer) for his studies on the theory of superconductivity completed while still in his 20s. In 1968, he was awarded the Comstock Prize (with J.R. Schrieffer) of the National Academy of Sciences. The Award of Excellence, Graduate Faculties Alumni of Columbia University and Descartes Medal, Academie de Paris, Université Rene Descartes were conferred on Professor Cooper in the mid 1970s. In 1985, Professor Cooper received the John Jay Award of Columbia College. He holds seven honorary doctorates.  Professor Cooper has been an NSF Postdoctoral Fellow, 1954-55, Alfred P. Sloan Foundation Research Fellow, 1959-66 and John Simon Guggenheim Memorial Foundation Fellow, 1965-66. He is a fellow of the American Physical Society and American Academy of Arts and Sciences; Sponsor, Federation of American Scientists; member of American Philosophical Society, National Academy of Sciences, Society of Neuroscience, American Association for the Advancement of Science, Phi Beta Kappa, and Sigma Xi. Professor Cooper is also on the Governing Board and Executive Committee of the International Neural Network Society and a member of the Defense Science Board.  Professor Cooper is Co-founder and Co-chairman of Nestor, Inc., an industry leader in applying neural-network systems to commercial and military applications. Nestor’s adaptive pattern-recognition and risk-assessment systems simulated in small conventional computers *learn by example* to accurately classify complex patterns such as targets in sonar, radar or imaging systems, to emulate human decisions in such applications as mortgage origination and to assess risks. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q3 | **I know. I believe it’s the fifth time you are visiting Lindau, and this very particular meeting where scientists and young scientific students meet. What is it that makes you come back for the fifth time?** |
|  | Leon N. Cooper: Actually I didn’t remember that it was the fifth time. Each time is different. This time in particular I’m interested because it’s a meeting of medicine – doctors and physiologists. I’m a theoretical physicist that has been working in the areas of neuroscience, and basically I’m on a mission to convince the medical and the physiologists that a theoretical physicist can make a contribution to their field. Basically, to be more direct, theory has played an enormously important role in physics, and physicists understand the interaction between theory and experiment. But this is relatively new in neuroscience, and what I would like to do is to present an example of how theory can be useful in neuroscience. And we have lots, I think we have a very convincing case, but we’ll wait to see what the experts say. |
| Q2 | **If we just turn the clock back a little bit and look backwards to what you have achieved, when you were about 28, 27 years old. Could you have imagined what you then found out, what use it would have at that time?** |
|  | Leon N. Cooper: That’s a good question, because we were interviewed over and over toward the end of the 1950s, and the first question was: What are the practical applications of this great theoretical breakthrough? We listed all kinds of things like superconducting power lines, and things of that kind, and some of them are actually coming to pass. Yes, fifty years later, but the most important by far is what’s called the superconducting quantum interference device, based on what is known as the Josephson effect, which we didn’t even know existed at the time. That came just a few years later, and that’s now, it’s a way of measuring magnetic fields that’s far more sensitive, and it’s just used everywhere. It’s used in all kinds of devices, all kinds of electronic devices. And you see, that’s the amusing thing. It was a consequence of our theory, but it wasn’t one that we … You know, I wish I had foreseen it, but then I sometimes kick myself for not having foreseen it, but we didn’t. At least, I didn’t. It’s typical of what happens in scientific discovery, that you don’t necessarily foresee all of the consequences of what you yourself have done. |
| Q20 | **Is it important to be humble as a scientist?** |
|  | Leon N. Cooper: I wouldn’t say that, we’re all pretty arrogant. |
| Q14 | **The work that you’re doing at the moment is to see the way the mind works, to simplify it. What is the … you want to have a talk tomorrow, you’re on a mission, you say, but what can you see in the future, what uses can this have?** |
|  | Leon N. Cooper: First of all there are an enormous number of mental diseases of all kinds. Everything from mood diseases to schizophrenia, to all kinds of things, and hopefully, if you understand how the system works, you would be able to do something about repairing some of those problems. And I have a personal opinion that many of the worst problems might be a little easier than people expect, because it’s so easy to alter moods. We have various conceptual reasons for believing that things like memory consolidation and moods are influenced by overall factors and so you could make therapeutic interventions. We don’t know, of course, how effective they will be or whether they would work. You would never know before you actually do it, but there’s every reason to believe that you could have substantial help in various mental diseases, and then of course there’s the intellectual challenge of seeing how a biological system, how neurons put themselves together to process information and eventually how we achieve our mental states and so on. It’s an enormous intellectual challenge, and you don’t really know what the consequences are, but we can say from a past history that there will be consequences, we can’t foresee them all, and the chances are that some of them will be of immense importance. |
| Q21 | **It obviously still gives you a lot of satisfaction to do this work. What are the biggest challenges on a daily basis?** |
|  | Leon N. Cooper: Getting out of bed in the morning. Getting enough caffeine into use so your mind starts functioning. |
| Q9 | **Do you think that the prize gave you a better opportunity to do your work, the Nobel Prize, back in 1972?** |
|  | Leon N. Cooper: Oh sure, it gives you additional opportunities to get financing, to get people to make foolish statements, people are always asking you to express your opinions about things you don’t know anything about. You have to be a little careful. But sure, of course it gives you opportunities, and I think used properly, carefully, it’s a tremendous advantage, a tremendous advantage. Also, it’s somewhat of a burden, because in the sense that people look at what you do differently, they judge it differently. There’s always the question of what are you going to do next. If you worry about that you just can’t do anything, and basically you just do next what is next, and you don’t worry about it too much. I mean, not every problem you work on is of a Nobel Prize calibre. It would be ridiculous to even think that way. Sometimes it evolves, sometimes it doesn’t. |
| Q18 | **I believe that you have also, for example, spoken out on political issues. I have seen letters that you have signed, for example, in the problems back in the 1980s in Poland and so on. Do the Nobel Prize winners have a special responsibility, do you think, as well, in certain fields?** |
|  | Leon N. Cooper: I think I have a responsibility, just as every other citizen, to express myself on issues, if I feel I have any competence, or have an opinion, and I don’t do it too often but I sometimes do, and if having the Nobel Prize helps then that’s fine. But, sure, but although you do, again, you have to be careful because you can easily abuse the privilege, and find yourself … I mean, I’m asked to sign things that I know nothing about whatsoever. And I try to avoid that. |
| Q10 | **Do you think the climate for scientific studies and for scientists in general is better in the United States than in Europe? There were some talks about that today, briefly mentioned …** |
|  | Leon N. Cooper: I think probably it has its ups and downs, but for many years it was very good in the United States. I think Europe is coming up in certain fields. It’s a question of support, the openness of universities, the freedom to do research, the absence of stifling influences from above, and I think the United States has been freer in that respect. But we have our bad periods, too. There are always politicians, people are the same, there are always politicians, there are always people who will stifle research by saying you should do one thing and not do another. But it’s just harder to do in the United States, because in the United States there are so many centres of power that it’s a little more difficult, but still the agencies in the United States that provide the money for research have enormous influence and they can be stifled by the Congress. The Congress is often trying to do that. The most recent example is stem cell research. But there are always the po … you know, talk about arrogance. You have politicians literally telling scientists what the best way to do their research is, which is something even more than ludicrous, wouldn’t you say? |
| Q1 | **What would your advice be to a young scientific student who would listen in to this interview eventually, on the internet?** |
|  | Leon N. Cooper: I think that any person who is interested in science and is willing to work and to have the discipline should do it. Advice in what respect? Should they go into science, or should they … what country should they go to? |
| Q10 | **If you have these kind of from above and lack of money situation, control, it’s not only that you maybe have to do your scientific studies, you also have to take another fight?** |
|  | Leon N. Cooper: It’s not that bad. The situation … What would be more accurate is to say that the situation has its ups and downs, like most of history, and some periods are a little better, some periods are a little worse. It’s just like the general economic environment. There are some areas in science that become hot, and if you get your PhD there you have a hundred jobs waiting for you, and in other areas you can’t find a job, and five years later it’s just the reverse. It’s somewhat aggravating, but it’s not that different from the economics of the entire community, but I think the main thing is that there are enormous opportunities in science, and for people who can do it and are willing to do the work there is almost always something. But again, there may be some fields that suddenly become over-populated, and some areas that suddenly become less fashionable. It’s unfortunate because it takes, it’s a long-time commitment to become a scientist, and you may go into an area that is not fashionable by the time you get out of it. But I don’t know what to advise about that. If I were, as far as national policy is concerned, my own feeling is it should be kept on a steady course, and not go back and forth with fashions. But politicians don’t listen to that, either. |
| Q6 | **We’re coming to the end of the interview. What is maybe your greatest memory from your time as a scientist? Is there one particular memory that you would like to share with us?** |
|  | Leon N. Cooper: I suppose my greatest memories are of working together on some of the major problems that I’ve solved with other people and realising that I had the solution. It’s simply enormously exciting, but you know, I would almost say that some of the greatest pleasures I’ve had are in smaller problems that no-one has ever heard of, that were just so exciting. The thrill of having just mastered them, but I suppose the quick answer is that I can’t come up with something that sounds like the major major memory to me. Do people, when you ask that question, do people immediately have a memory? |

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| **Physics\_1999-** | |
| **ID** | 0134 |
| **Biographical** | John Robert Schrieffer was born in Oak Park, Illinois on May 31, 1931, son of John H. Schrieffer and his wife Louis (née Anderson). In 1940, the family moved to Manhasset, New York and in 1947 to Eustis, Florida where they became active in the citrus industry.  Following his graduation from Eustis High School in 1949, Schrieffer was admitted to Massachusetts Institute of Technology, where for two years he majored in electrical engineering, then changed to physics in his junior year. He completed a bachelor’s thesis on the multiple structure in heavy atoms under the direction of Professor John C. Slater. Following up on an interest in solid state physics developed while at MIT, he began graduate studies at the University of Illinois, where he immediately began research with Professor John Bardeen. After working out a problem dealing with electrical conduction on semiconductor surfaces, Schrieffer spent a year in the laboratory, applying the theory to several surface problems. In the third year of graduate studies, he joined Bardeen and Cooper in developing the theory of superconductivity, which constituted his doctoral dissertation.  He spent the academic year 1957-58 as a National Science Foundation fellow at the University of Birmingham and the Niels Bohr Institute in Copenhagen, where he continued research in superconductivity. Following a year as assistant professor at the University of Chicago, he returned to the University of Illinois in 1959 as a faculty member. In 1960 he returned to the Bohr Institute for a summer visit, during which he became engaged to Anne Grete Thomsen whom he married at Christmas of that year.  In 1962 Schrieffer joined the faculty of the University of Pennsylvania in Philadelphia, where in 1964 he was appointed Mary Amanda Wood Professor in Physics. In 1980 he was appointed Professor at the University of California, Santa Barbara and to the position of Chancellor Professor in 1984. He served as Director of the Institute for Theoretical Physics in Santa Barbara from 1984-89. In 1992 he was appointed University Professor at Florida State University and Chief Scientist of the National High Magnetic Field Laboratory.  He holds honorary degrees from the Technische Hochschule, Munich and the Universities of Geneva, Pennsylvania, Illinois, Cincinnati, Tel-Aviv, Alabama. In 1969 he was appointed by Cornell to a six-year term as a Andrew D. White Professor-at-Large.  He is a member of the American Academy of Arts and Sciences, the National Academy of Sciences of which he is a member of their council, the American Philosophical Society, the Royal Danish Academy of Sciences and Letters and the Academy of Sciences of the USSR.  His awards include the Guggenheim Fellowship, Oliver E. Buckley Solid State Physics Prize, Comstock Prize, National Academy of Science, the Nobel Prize in Physics shared with John Bardeen and Leon N. Cooper in 1972, John Ericsson Medal, American Society of Swedish Engineers, University of Illinois Alumni Achievement Award, and in 1984 the National Medal of Science. The main thrust of his recent work has been in the area of high-temperature superconductivity, strongly correlated electrons, and the dynamics of electrons in strong magnetic fields.  The Schrieffers have three children, Bolette, Paul, and Regina. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0135 |
| **Biographical** | I was born in Budapest, Hungary, on June 5, 1900, the oldest son of Bertalan Gabor, director of a mining company, and his wife Adrienne. My life-long love of physics started suddenly at the age of 15. I could not wait until I got to the university, I learned the calculus and worked through the textbook of Chwolson, the largest at that time, in the next two years. I remember how fascinated I was by Abbe’s theory of the microscope and by Gabriel Lippmann’s method of colour photography, which played such a great part in my work, 30 years later. Also, with my late brother George, we built up a little laboratory in our home, where we could repeat most experiments which were modern at that time, such as wireless X-rays and radioactivity. Yet, when I reached university age, I opted for engineering instead of physics. Physics was not yet a profession in Hungary, with a total of half-a-dozen university chairs – and who could have been presumptuous enough to aspire to one of these?  So I acquired my degrees, (Diploma at the Technische Hochschule Berlin, 1924, Dr-Ing. in 1927), in electrical engineering, though I sneaked over from the TH as often as possible to the University of Berlin, where physics at that time was at its apogee, with Einstein, Planck, Nernst and v. Laue. Though electrical engineering remained my profession, my work was almost always in applied physics. My doctorate work was the development of one of the first high speed cathode ray oscillographs and in the course of this I made the first iron-shrouded magnetic electron lens. In 1927 I joined the Siemens & Halske AG where I made my first of my successful inventions; the high pressure quartz mercury lamp with superheated vapour and the molybdenum tape seal, since used in millions of street lamps. This was also my first exercise in serendipity, (the art of looking for something and finding something else), because I was not after a mercury lamp but after a cadmium lamp, and that was not a success.  In 1933, when Hitler came to power, I left Germany and after a short period in Hungary went to England. At that time, in 1934, England was still in the depths of the depression, and jobs for foreigners were very difficult. I obtained employment with the British Thomson-Houston Co., Rugby, on an inventor’s agreement. The invention was a gas discharge tube with a positive characteristic, which could be operated on the mains. Unfortunately, most of its light emission was in the short ultraviolet, so that it failed to give good efficiency with the available fluorescent powders, but at least it gave me a foothold in the BTH Research Laboratory, where I remained until the end of 1948. The years after the war were the most fruitful. I wrote, among many others, my first papers on communication theory, I developed a system of stereoscopic cinematography, and in the last year, 1948 I carried out the basic experiments in holography, at that time called “wavefront reconstruction”. This again was an exercise in serendipity. The original objective was an improved electron microscope, capable of resolving atomic lattices and seeing single atoms. Three year’s work, 1950-53, carried out in collaboration with the AEI Research Laboratory in Aldermaston, led to some respectable results, but still far from the goal. We had started 20 years too early. Only in recent years have certain auxiliary techniques developed to the point when electron holography could become a success. On the other hand, optical holography has become a world success after the invention and introduction of the laser, and acoustical holography has now also made a promising start.  On January 1, 1949 I joined the Imperial College of Science & Technology in London, first as a Reader in Electronics, later as Professor of Applied Electron Physics, until my retirement in 1967. This was a happy time. With my young doctorands as collaborators I attacked many problems, almost always difficult ones. The first was the elucidation of Langmuir’s Paradox, the inexplicably intense apparent electron interaction, in low pressure mercury arcs. The explanation was that the electrons exchanged energy not with one another, by collisions, but by interaction with an oscillating boundary layer at the wall of the discharge vessel. We made also a Wilson cloud chamber, in which the velocity of particles became measurable by impressing on them a high frequency, critical field, which produced time marks on the paths, at the points of maximum ionisation. Other developments were: a holographic microscope, a new electron-velocity spectroscope an analogue computer which was a universal, non-linear “learning” predictor, recognizer and simulator of time series, a flat thin colour television tube, and a new type of thermionic converter. Theoretical work included communication theory, plasma theory, magnetron theory and I spent several years on a scheme of fusion, in which a critical high temperature plasma would have been established by a 1000 ampere space charge-compensated ion beam, fast enough to run over the many unstable modes which arise during its formation. Fortunately the theory showed that at least one unstable mode always remained, so that no money had to be spent on its development.  After my retirement in 1967 I remained connected with the Imperial College as a Senior Research Fellow and I became Staff Scientist of CBS Laboratories, Stamford, Conn. where I have collaborated with the President, my life-long friend, Dr. Peter C. Goldmark in many new schemes of communication and display. This kept me happily occupied as an inventor, but meanwhile, ever since 1958, I have spent much time on a new interest; the future of our industrial civilisation. I became more and more convinced that a serious mismatch has developed between technology and our social institutions, and that inventive minds ought to consider social inventions as their first priority. This conviction has found expression in three books, *Inventing the Future*, 1963, *Innovations*, 1970, and *The Mature Society*, 1972. Though I still have much unfinished technological work on my hands, I consider this as my first priority in my remaining years. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0136 |
| **Biographical** | Hannes Olof Gösta Alfvén was born in Norrköping, Sweden, in 1908. His parents Johannes Alfvén and Anna-Clara Romanus were both practising physicians. Hannes Alfvén studied at Uppsala University from 1926, he obtained the degree of doctor of philosophy in 1934, in this same year he was appointed lecturer in physics at Uppsala University. In 1937 he became research physicist at the Nobel Institute for Physics in Stockholm, in 1940 he was appointed Professor in the Theory of Electricity at the Royal Institute of Technology in Stockholm, Professor of Electronics in 1945, and Professor of Plasma Physics in 1963. Since 1967 he is visiting professor of Physics at the University of California at San Diego.  In 1935 Hannes Alfvén married Kerstin Maria Erikson, they have five children: Cecilia, Inger, Gösta, Reidun and Berenike.  Professor Alfvén published a number of papers in physics and astrophysics, and the following monographs: Cosmical Electrodynamics, 1948; Origin of the Solar System, 1956; and together with C.-G. Fälthammar, Cosmical Electrodynamics, Fundamental Principles, 1963.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Physics 1963-1970*, Elsevier Publishing Company, Amsterdam, 1972  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  *Hannes Alfvén died on April 2, 1995.* |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0137 |
| **Biographical** | Louis Néel was born in Lyons on 22 November 1904. In 1931 he married Hélène Hourticq; they have three children, Marie Françoise, Attachée d’Administration at the Conseil d’Etat, Marguerite, married to Guély, Professeur agrégée d’histoire, and Pierre, who is a television producer. Louis Néel studied at the Ecole Normal Supérieure in Paris from 1924-1928, where he was appointed lecturer in 1928. In 1932 he obtained the degree of Doctor of Science at the University of Strasbourg, where he was appointed Professor at the Faculty of Science (1937-1945). He was Professor in Grenoble since 1945. In 1946 he became Director of the laboratory for electrostatics and metal physics (Centre National de la Recherche Scientifique). From 1954 until 1970 he was Director of the Institut Polytechnique de Grenoble and of the Ecole Française de Papeterie; in 1970 he was appointed President of the Institut National Polytechnique in Grenoble. He served as director of the Centre d’Etudes nucléaires de Grenoble from 1956 to 1970. From 1949 to 1969 he was a member of the Board of Directors of the C.N.R.S.; scientific adviser to the French Navy since 1952; French representative at the Scientific Committee of the North Atlantic Treaty Organization.  Louis Néel began his first research work on magnetism between 1928 and 1939 in Professor Weiss’ laboratory in Strasbourg. Called up for war service in 1939, he worked on the defence of ships of the French fleet against German magnetic mines and invented an effective new method of protection (neutralization). After the Armistice of 1940, he went to Grenoble and established the Laboratoire d’Electrostatique et de Physique du Métal, which in 1946 became one of the external laboratories of the Centre National de la Recherche Scientifique. This laboratory extended rapidly and gave rise to new laboratories; even so, it still has a staff of more than 250 at the present time.  In 1956 Louis Néel created and subsequently developed, as part of the French Atomic Energy Commission, the Centre d’Etudes Nucléaires de Grenoble. He also contributed to the decision to install the Franco-German high-flux reactor in Grenoble (1967).  Although he continued with research, sometimes critical and difficult, on the specific heat of nickel, Louis Néel has mainly concentrated on theoretical problems, which have formed the subject of more than 150 publications. Besides his discovery of the concepts of antiferromagnetism and ferrimagnetism and its consequences, for which he was awarded the Nobel Prize, Louis Néel tackled and solved a number of other problems and extended our knowledge of many aspects of magnetism. The most important of these are as follows: theory of Rayleigh’s Laws; magnetic properties of fine grains; magnetic viscosity; internal dispersion fields; superantiferromagnetism; and hysteresis.  The following distinctions and honours have been awarded to Professor Néel: Chevalier de la Légion d’Honneur (military) in 1940, Officier in 1951, Commandeur in 1958, and Grand Officier in 1966; Croix de Guerre with palms (1940); Commandeur de l’Ordre des Palmes Académiques (I957); Chevalier du Mérite Social (1963); Holweck Prize (1952); old Medal of the Centre National de la Recherche Scientifique (1965). He is a member of the French Academy of Science (Paris, 1953); a foreign member of the Soviet Academy of Science (1959), the Royal Dutch Academy of Science (1959), the Deutsche Akademie der Naturforscher Leopoldina (1964), the Rumanian Academy (1965), the Royal Society (London) (1966), and the American Academy of Arts and Sciences (1966).  Prof. Néel is honorary doctor of the Universities of Graz (1948), Nottingham (1951), Oxford (1958), Louvain (1965), Newcastle (1965), Coimbra (1966), Sherbrooke (1967), and Iassy (1971). He holds an honorary degree from the Polytechnic Institute of Turin (1960). He is an honorary member and former president (1957) of the Société Française de Physique. From 1963 to 1966 he was President of the International Union of Pure and Applied Physics. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0138 |
| **Biographical** | Murray Gell-Mann was born on 15th September 1929, in New York City. He obtained his B.Sc. at Yale University in 1948, and his Ph.D. in 1951 at the Massachusetts Institute of Technology. In 1952 he became a member of the Institute for Advanced Study, during 1952-1953 he was instructor at the University of Chicago, from 1953 to 1954 he was Assistant Professor, in 1954 he was appointed Associate Professor for research on dispersion relations. In this period he developed the strangeness theory and the eightfold way theory. In 1956 he was appointed Professor, his research then turned more to the theory of weak interactions.  In 1959 Professor Gell-Mann was awarded the Dannie Heineman Prize of the American Physical Society. He is a Fellow of this society and a member of the National Academy of Sciences.  Murray Gell-Mann was in 1955 married to J. Margaret Dow; they have a daughter, Elizabeth, and a son, Nicholas.  **Addendum, May 2007**  Murray Gell-Mann is one of today’s most prominent scientists. He is currently Distinguished Fellow at the Santa Fe Institute as well as the Robert Andrews Millikan Professor Emeritus at the California Institute of Technology, where he joined the faculty in 1955. In 1969 he received the Nobel Prize in Physics for his work on the theory of elementary particles. He is the author of *The Quark and the Jaguar*, published in 1994, in which his ideas on simplicity and complexity are presented to a general readership.  Among his contributions to Physics was the “eightfold way” scheme that brought order out of the chaos created by the discovery of some 100 kinds of particles in collisions involving atomic nuclei. Gell-Mann subsequently found that all of those particles, including the neutron and proton, are composed of fundamental building blocks that he named “quarks,” with very unusual properties. That idea has since been fully confirmed by experiment. The quarks are permanently confined by forces coming from the exchange of “gluons.” He and others later constructed the quantum field theory of quarks and gluons, called “quantum chromodynamics,” which seems to account for all the nuclear particles and their strong interactions.  Professor Gell-Mann was a director of the J.D. and C.T. MacArthur Foundation from 1979–2002 and is a board member of the Wildlife Conservation Society. From 1974 to 1988, he was a Citizen Regent of the Smithsonian Institution. He belongs to the National Academy of Sciences, the American Academy of Arts and Sciences, the American Philosophical Society, and the Council on Foreign Relations; he is also a Foreign Member of the Royal Society of London. He was on the U.S. President’s Science Advisory Committee from 1969 to 1972 and the President’s Committee of Advisors on Science and Technology from 1994 to 2001.  In 1988 Professor Gell-Mann was listed on the United Nations Environmental Program’s Roll of Honor for Environmental Achievement (The Global 500). He also shared the 1989 Ettore Majorana “Science for Peace” prize. Earlier, he was given the Ernest O. Lawrence Memorial Award of the Atomic Energy Commission, the Franklin Medal of the Franklin Institute, the Research Corporation Award, and the John J. Carty Medal of the National Academy of Sciences. In 2005 Gell-Mann was awarded the Albert Einstein Medal. He has received honorary degrees from many universities, including Yale, Columbia, the University of Chicago, Cambridge, and Oxford. In 1994 the University of Florida awarded him an honorary degree in Environmental Studies.  Gell-Mann’s interests extend to historical linguistics, archeology, natural history, the psychology of creative thinking, and other subjects connected with biological and cultural evolution and with learning. Much of his recent research at the Santa Fe Institute has focused on the theory of complex adaptive systems, which brings many of those topics together. Currently Professor Gell-Mann is spearheading the Evolution of Human Languages Program at the Santa Fe Institute. Another focus of his work relates to simplicity, complexity, regularity, and randomness. He is also concerned with how knowledge and understanding are to be extracted from the welter of “information” that can now be transmitted and stored as a result of the digital revolution. Professor Gell-Mann lives in Santa Fe, New Mexico and he teaches from time to time at the University of New Mexico in Albuquerque.  *Murray Gell-Mann died on 24 May 2019.* |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q17 | **But you speak lots of languages. How many languages can you speak?** |
|  | Murray Gell-Mann: Well, that’s not actually true. That’s another story they make up about me. I’m very interested in languages and I know something about the relations among languages, about the entomologies, about sound changes from one language to another and so on and so forth. Very interested in that, and even in helping a little bit to work on it. But as to speaking languages, any European waiter could do much better than I. |
| Q2 | **You have found that one word that has got the fame, I would say, and also was named in another prize, and that is quark and quarks. What does that come from?** |
|  | Murray Gell-Mann: Well, let’s see. In the citation for the award, it was barely mentioned. But I did propose that the neutron and proton and the related barions were composed, roughly speaking, of three quarks each, and that the quarks were the fundamental entities, they are analogous to the electron, and that the neutron, proton and so on were not elementary. The word I had first as a sound, quark. It might have been spelled K W O R K, for example. But then I thought it was the right sound for the fundamental constituents of nuclei. Sounds, sounds good. |
| Q21 | **So how can you make models or series of these histories that are based on accidents?** |
|  | Murray Gell-Mann: Well, we, the fundamental laws give you the probabilities. But you have to adjoin to that information a lot of information about the accidents that have already occurred, and especially these important ones which we can call frozen accidents which create a great deal of regularity in the future. And by complexity, then, what I call effective complexity, we mean the length of a very concise description of the regularities of something. |
| Q18 | **If I change the subject for a while, you have travelled to the tropics, and you were also engaged in environmental issues?** |
|  | Murray Gell-Mann: Yes, very much, in trying to preserve the heritage of biological diversity. I’m also somewhat interested in the preservation of cultural diversity, although that involves a great many more paradoxes and contradictions. But I’ve worked hard on trying to help with the preservation of biological diversity around the world, and of course on land the greatest diversity is found in the tropics, and also the tropics are full of poor and, in many cases, overpopulated countries. |
| Q14 | **I have one last question about the birds. What is your dream now that you would like to encounter?** |
|  | Murray Gell-Mann: Oh, there are many species. I’ve seen less than 4,000 out of 9,600 or something like that. Almost 10,000 species that are recognised in the world. But there are a few special ones that it would be wonderful to see. The Congo peacock, for example. |

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| **Physics\_1999-** | |
| **ID** | 0139 |
| **Biographical** | Luis W. Alvarez was born in San Francisco, Calif., on June 13, 1911. He received his B.Sc. from the University of Chicago in 1932, a M.Sc. in 1934, and his Ph.D. in 1936. Dr. Alvarez joined the Radiation Laboratory of the University of California, where he is now a professor, as a research fellow in 1936. He was on leave at the Radiation Laboratory of the Massachusetts Institute of Technology from 1940 to 1943, at the Metallurgical Laboratory of the University of Chicago in 1943-1944, and at the Los Alamos Laboratory of the Manhattan District from 1944 to 1945.  Early in his scientific career, Dr. Alvarez worked concurrently in the fields of optics and cosmic rays. He is co-discoverer of the “East-West effect” in cosmic rays. For several years he concentrated his work in the field of nuclear physics. In 1937 he gave the first experimental demonstration of the existence of the phenomenon of *K*-electron capture by nuclei. Another early development was a method for producing beams of very slow neutrons. This method subsequently led to a fundamental investigation of neutron scattering in ortho- and para-hydrogen, with Pitzer, and to the first measurement, with Bloch, of the magnetic moment of the neutron. With Wiens, he was responsible for the production of the first 198Hg lamp; this device was developed by the Bureau of Standards into its present form as the universal standard of length. Just before the war, Alvarez and Cornog discovered the radioactivity of 3H (tritium) and showed that 3He was a stable constituent of ordinary helium. (Tritium is best known as a source of thermonuclear energy, and 3He has become of importance in low temperature research.)  During the war (at M.I.T.) he was responsible for three important radar systems – the microwave early warning system, the Eagle high altitude bombing system, and a blind landing system of civilian as well as military value (GCA, or Ground-Controlled Approach). While at the Los Alamos Laboratory, Professor Alvarez developed the detonators for setting off the plutonium bomb. He flew as a scientific observer at both the Almagordo and Hiroshima explosions.  Dr. Alvarez is responsible for the design and construction of the Berkeley 40-foot proton linear accelerator, which was completed in 1947. In 1951 he published the first suggestion for charge exchange acceleration that quickly led to the development of the “Tandem Van de Graaf accelerator”. Since that time, he has engaged in high-energy physics, using the 6 billion electron volt Bevatron at the University of California Radiation Laboratory. His main efforts have been concentrated on the development and use of large liquid hydrogen bubble chambers, and on the development of high-speed devices to measure and analyze the millions of photographs produced each year by the bubble-chamber complex. The net result of this work has been the discovery by Dr. Alvarez’ research group, of a large number of previously unknown efundamental particle resonances.. Since 1967 Dr. Alvarez has devoted most of this time to the study of cosmic rays, using balloons and superconducting magnets.  Professor Alvarez is a member of the following societies: National Academy of Sciences, American Philosophical Society, American Physical Society (President 1969), American Academy of Arts and Sciences, and National Academy of Engineering. In 1946 he was awarded the Collier Trophy by the National Aeronautical Association for the development of Ground – Controlled Approach. In 1953 he was awarded the John Scott Medaland Prize, by the city of Philadelphia, for the same work. In 1947 he was awarded the Medal for Merit. In 1960 he was named “California Scientist of the Year” for his research work on high-energy physics. In 1961 he was awarded the Einstein Medal for his contribution to the physical sciences. In 1963 he was awarded the Pioneer Award of the AIEEE; in 1964 he was awarded the National Medal of Science for contributions to high-energy physics, and in 1965 he received the Michelson Award. He has received the following honorary de grees: Sc.D., University of Chicago, 1967; Sc.D., Carnegie-Mellon University, 1968; Sc.D., Kenyon College, 1969. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0140 |
| **Biographical** | Hans Albrecht Bethe was born in Strasbourg, Alsace-Lorraine, on July 2 1906. He attended the Gymnasium in Frankfurt from 1915 to 1924. He then studied at the University of Frankfurt for two years, and at Munich for two and one half years, taking his Ph. D. in theoretical physics with Professor Arnold Sommerfeld in July 1928.  He then was an Instructor in physics at Frankfurt and at Stuttgart for one semester each. From fall 1929 to fall 1933 his headquarters were the University of Munich where he became Privatdozent in May 1930. During this time he had a travel fellowship of the International Education Board to go to Cambridge, England, in the fall of 1930, and to Rome in the spring terms of 1931 and 1932. In the winter semester of 1932-1933,he held a position as Acting Assistant Professor at the University of Tubingen which he lost due to the advent of the Nazi regime in Germany.  Bethe emigrated to England in October 1933 where he held a temporary position as Lecturer at the University of Manchester for the year 1933-1934, and a fellowship at the University of Bristol in the fall of 1934. In February 1935 he was appointed Assistant Professor at Cornell University, Ithaca, N. Y. U.S.A., then promoted to Professor in the summer of 1937. He has stayed there ever since, except for sabbatical leaves and for an absence during World War II. His war work took him first to the Radiation Laboratory at the Massachusetts Institute of Technology, working on microwave radar, and then to the Los Alamos Scientific Laboratory which was engaged in assembling the first atomic bomb. He returned to Los Alamos for half a year in 1952. Two of his sabbatical leaves were spent at Columbia University, one at the University of Cambridge, and one at CERN and Copenhagen.  Bethe’s main work is concerned with the theory of atomic nuclei. Together with Peierls, he developed a theory of the deuteron in 1934 which he extended in 1949. He resolved some contradictions in the nuclear mass scale in 1935. He studied the theory of nuclear reactions in 1935-1938, predicting many reaction cross sections. In connection with this work, he developed Bohr’s theory of the compound nucleus in a more quantitative fashion. This work and also the existing knowledge on nuclear theory and experimental results, was summarized in three articles in the *Reviews of Modern Physics* which for many years served as a textbook for nuclear physicists.  His work on nuclear reactions led Bethe to the discovery of the reactions which supply the energy in the stars. The most important nuclear reaction in the brilliant stars is the carbon-nitrogen cycle, while the sun and fainter stars use mostly the proton-proton reaction. Bethe’s main achievement in this connection was the exclusion of other possible nuclear reactions. The Nobel Prize was given for this work, as well as his work on nuclear reactions in general.  In 1955 Bethe returned to the theory of nuclei, emphasizing a different phase. He has worked since then on the theory of nuclear matter whose aim it is to explain the properties of atomic nuclei in terms of the forces acting between nucleons.  Before his work on nuclear physics, Bethe’s main attention was given to atomic physics and collision theory. On the former subject, he wrote a review article in *Handbuch der Physik* in which he filled in the gaps of the existing knowledge, and which is still up-to-date. In collision theory, he developed a simple and powerful theory of inelastic collisions between fast particles and atoms which he has used to determine the stopping power of matter for fast charged particles, thus providing a tool to nuclear physicists. Turning to more energetic collisions, he calculated with Heitler the bremsstrahlung emitted by relativistic electrons, and the production of electron pairs by high energy gamma rays.  Bethe also did some work on solid-state theory. He discussed the splitting of atomic energy levels when an atom is inserted into a crystal, he did some work on the theory of metals, and especially he developed a theory of the order and disorder in alloys.  In 1947, Bethe was the first to explain the Lamb-shift in the hydrogen spectrum, and he thus laid the foundation for the modern development of quantum electrodynamics. Later on, he worked with a large number of collaborators on the scattering of pi mesons and on their production by electromagnetic radiation.  Bethe is married to the daughter of P.P. Ewald, the well-known X-ray physicist. They have two children, Henry and Monica. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0141 |
| **Biographical** | Alfred Kastler was born in Guebwiller in Alsace on May 3, 1902. He followed his early studies at the school in his native town, and continued at the Oberrealschule of Colmar, which became the Lycee Bartholdi in 1918, when Alsace was returned to France.  He entered the École Normale Superieure in 1921, and left in 1926 to teach in a lycée. He taught for 5 years, first in the Mulhouse lycée, then in those of Colmar and Bordeaux. The next stage of his career was in higher education: assistant at the Bordeaux Faculty of Science from 1931 to 1936, lecturer at Clermont-Ferrand from 1936 to 1938, professor at Bordeaux from 1938 to 1941. In 1941, in the midst of the German occupation, Georges Bruhat asked him to come to Paris to help him in establishing physics teaching at the Ecole Normale Superieure. The post was provisional, but was confirmed by the allocation of a chair in a personal capacity at the Paris Faculty of Sciences in 1952.  His mathematics teachers at the Colmar Lycée, Fröhlich from Bavaria and Edouard Greiner from Alsace, were the first to awaken his interest in science. This predilection became consolidated in the special mathematics class held by Mahuet and Brunold, who helped Kastler to gain entry to the École Normale Superieure by the side entrance, so to speak. In the stimulating and friendly atmosphere of this college, the teacher Eugène Bloch (who came from the upper Rhine and who subsequently disappeared without trace in Auschwitz) initiated his students into the concepts of Bohr’s atom and quantum physics, and drew Kastler’s attention to Sommerfeld’s book on atomic structure and spectral lines. This book introduced him to the principle of the conservation of momentum applied by A. Rubinowicz to the exchange of energy between atoms and radiation. This principle was to guide the whole of Kastler’s research, beginning with his thesis up to the most recent investigations of the Parisian team.  Alfred Kastler was in 1931 appointed assistant to Pierre Daure, professor at the Bordeaux Faculty of Science. His teaching duties were then less onerous, and Kastler was able to devote all his free time to research, aided by Professor Daure who initiated him into experimental spectroscopy. For many years, he worked in the field of optical spectroscopy, particularly on atomic fluorescence and Raman spectroscopy. [In I937 he became interested in the luminescence of sodium atoms in the upper atmosphere; after establishing that the D line of the twilight sky could be absorbed by sodium vapour, and after some studies at Abisko where twilight is prolonged, he was able to demonstrate in cooperation with his colleague Jean Bricard, that this line is polarized, as it must be if the emission mechanism is one of optical resonance produced by solar radiation.]  During the years of the occupation, French scientists were virtually isolated from the outside world. In 1945, it was possible to send pupils to other western countries, so that they could bring their knowledge of the most recent devel opments in scientific progress up to date. Among them was Jean Brossel, who returned in 1951 in possession of a mass of information gained under Francis Bitter at M.I.T.  Under the influence of Gorter, [Rabi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/index.html) had very successfully applied certain methods to the investigation of atoms in their fundamental state. In 1949, Bitter suggested extending these same methods to the excited states of atoms. Brossel and Kastler together then proposed the ” double resonance method “, which combines optical resonance with magnetic resonance.  While Brossel was at M.I.T., between 1949 and 1951, he carried out pioneer work along these lines on the excited state of the mercury atom. At the same time, Kastler was supplementing the method by the technique of “optical pumping”, which makes it possible to apply “optical methods for studying the microwave resonances” to the fundamental states of atoms.  After 1951, Kastler worked in collaboration with Jean Brossel in Paris to perfect all these methods. Among the young men and women at the École Normale, which nurtures the intellectual elite, they found their research workers. Their theses represent the various stages in their collective work which has been awarded the Nobel Prize, and of which some account is given in Kastler’s Nobel lecture.  Kastler taught as Francqui Professor at the University of Louvain during the year 1953-1954, he hold honorary doctorates from the University of Louvain (1955), Pisa (1960), and Oxford (1966), and he was decorated by the University of Liége.The French and Polish Societies of Physics and the American Society of Optics have elected Kastler to honorary memberships. In 1962, the latter society awarded him the first Mees medal bearing the inscrip tion “Optics transcends all boundaries”. In 1954, the British Physical Society awarded him the prize commemorating Fernand Holweck, who disappeared tragically in 1941. Kastler was made a member of the Royal Flemish Academy of Belgium in 1954, and of the Paris Academy of Sciences in 1964; in 1965, the National Centre for Scientific Research awarded him their gold medal, at the same time as his friend and colleague [Louis Néel](https://www.nobelprize.org/nobel_prizes/physics/laureates/1970/index.html).  In Decermber 1924 Kastler married Elise Cosset, a former pupil of the École Normale Supérieure. By working as a history teacher in secondary schools she made it possible for her husband to devote to research all the leisure time left to him by his own teaching duties. They have three children: Daniel, born in 1926, Mireille born in 1928, and Claude-Yves born in 1936. They have all married, there are now six grandchildren, whose ages range from 14 years to 10 months. Daniel is a Professor of Physics at the Faculty of Science in Marseilles, he is working on theoretical physics problems; Mireille is an ophthalmologist in Paris, and Claude-Yves teaches Russian at the Arts Faculty in Grenoble. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0142 |
| **Biographical** | Sin-Itiro Tomonaga was born in Tokyo, Japan, on March 31, 1906, the eldest son of Sanjuro Tomonaga and Hide Tomonaga. In 1913 his family moved to Kyoto when his father was appointed a professor of philosophy at Kyoto Imperial University. From that time he was brought up in Kyoto. He is a graduate of the Third Higher School, Kyoto, a renowned senior high school which has educated a number of leading personalities in prewarJapan.  Tomonaga completed work for Rigakushi (bachelor’s degree) in physics at Kyoto Imperial University in 1929, with one of his intimate friends. [Dr. Hideki Yukawa](https://www.nobelprize.org/nobel_prizes/physics/laureates/1949/index.html), Nobel laureate. He was engaged in graduate work for three years at the same university and was then appointed a research associate by Dr. Yoshio Nishina at the Institute of Physical and Chemical Research, Tokyo, where he started to work in a newly developed frontier of theoretical physics quantum electrodynamics – under the guidance of Dr. Nishina. His paper on the photoelectric pair creation is well-known.  Tomonaga stayed in Leipzig, Germany, from 1937 to 1939, to study nuclear physics and the quantum field theory in collaboration with the theoretical group of Dr. W. Heisenberg, where he published a paper “Innere Reibung und Wärmeleitfähigkeit der Kernmaterie”, which was chosen as the thesis for Rigakuhakushi (Doctor of Science) at Tokyo Imperial University in December,1939.  In 1940, Dr. Tomonaga directed his attention to the meson theory and developed the intermediate coupling theory in order to clarify the structure of the meson cloud around the nucleon. He joined the faculty of Tokyo Bunrika University (which was absorbed into the Tokyo University of Education in 1949[\*](https://www.nobelprize.org/prizes/physics/1965/tomonaga/biographical/#not)) as Professor of Physics in 1941. It was in 1942 when he first proposed the covariant formulation of the quantum field theory in which the concept of the quantum state was generalized so as to be relativistically covariant.  During the Second World War, Dr. Tomonaga was interested in developing a theory of microwave systems. He solved the motion of electrons in the magnetron and also developed a unified theory of the systems consisting of wave guides and cavity resonators.  As soon as the War was over, Tomonaga came back to academic research again with a programme in which he was first to summarize and extend the intermediate coupling theory and secondly to apply the covariant field theory to actual physical systems. His aim was to investigate the nature of field reaction in the meson theory as well as in quantum electrodynamics. He was confident, prior to the Lamb-Retherford experiment, by means of a model calculation that divergence difficulty in quantumelectrodynamics could be overcome simply by handling the infinite mass and charge due to field reactions in some way or another. It was only a step further for him to develop the renormalization theory with covariant formalism in his right hand and experimental support in his left.  Dr. Tomonaga was invited to the Institute for Advanced Study, Princeton, in 1949 where he was engaged in the investigation of a one-dimensional fermion system. Thus he first succeeded in clarifying the nature of collective oscillations of a quantum-mechanical many-body system and opened a new frontier of theoretical physics, modern many-body problem. In 1955, he published an elementary theory of quantum mechanical collective motions.  Dr. Tomonaga took the leadership in establishing the Institute for Nuclear Study, University of Tokyo, in 1955. From 1956 to 1962 he was appointed President of the Tokyo University of Education and since 1963 he has been President of the Science Council of Japan and Director of the Institute for Optical Research, Tokyo University of Education. He occupies an important position in various governmental committees for scientific research and policymaking.  Tomonaga’s honours and awards include the Japan Academy Prize (1948); the Order of Culture (1952); the Lomonosov Medal, U.S.S.R. (1964).  Dr. Tomonaga is a member of the Japan Academy, the Deutsche Akademie der Naturforscher “Leopoldina” and a foreign member of the [Royal Swedish Academy of Science](http://www.kva.se/). He is a corresponding member of the Bayerische Akademie der Wissenschaften and a foreign associate of the National Academy of Science of U.S.A.  Tomonaga has published widely in scientific journals on such subjects as quantum electrodynamics, the meson theory, nuclear physics, cosmic rays, and the many-body problem. His book, “Quantum Mechanics”, was published in 1949 and translated into English in 1963.  Tomonaga was married in 1940 to Ryoko Sekiguchi, daughter of Dr. K. Sekiguchi, the former Director of the Tokyo Metropolitan Observatory. They have two sons, Atsushi and Makoto and one daughter. Their daughter was married in 1965 to Dr. Y. Nagashima, research associate of the Physics Department, University of Rochester. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0143 |
| **Biographical** | Julian Schwinger was born on 12th February 1918 in New York City. The principal direction of his life was fixed at an early age by an intense awareness of physics, and its study became an all-engrossing activity. To judge by a first publication, he debuted as a professional physicist at the age of sixteen. He was allowed to progress rapidly through the public school system of New York City. Through the kind interest of some friends, and especially [I.I. Rabi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/index.html) of Columbia University, he transferred to that institution, where he completed his college education. Although his thesis had been written some two or three years earlier, it was in 1939 that he received the Ph.D. degree.  For the next two years he was at the University of California, Berkeley, first as a National Research Fellow and then as assistant to J.R. Oppenheimer. The outbreak of the Pacific war found Schwinger as an Instructor, teaching elementary physics to engineering students at Purdue University.  War activities were largely confined to the Radiation Laboratory at the Massachusetts Institute of Technology in Cambridge. Being a confirmed solitary worker, he became the night research staff. More scientific influences were also at work. He first approached electromagnetic radar problems as a nuclear physicist, but soon began to think of nuclear physics in the language of electrical engineering. That would eventually emerge as the effective range formulation of nuclear scattering. Then, being conscious of the large microwave powers available, Schwinger began to think about electron accelerators, which led to the question of radiation by electrons in magnetic fields. In studying the latter problem he was reminded, at the classical level, that the reaction of the electron’s field alters the properties of the particle, including its mass. This would be significant in the intensive developments of quantum electrodynamics, which were soon to follow.  With the termination of the war Dr. Schwinger accepted an appointment as Associate Professor at Harvard University. Two years later he became full Professor. That was also the year of his marriage to Clarice Carrol of Boston.  In subsequent years, he worked in a number of directions, but there was a pattern of concentration on general theoretical questions rather than specific problems of immediate experimental concern, which were nearer to the center ot hls earlier work. A speculative approach to physics has its dangers, but it can have its rewards. Schwinger was particularly pleased by an anticipation, early in 1957, of the existence of two different neutrinos associated, respectively, with the electron and the muon. This has been confirmed experimentally only rather recently. A related and somewhat earlier speculation, that all weak interactions are transmitted by heavy, charged, unit-spin particles still awaits a decisive experimental test. Schwinger’s policy of finding theoretical virtues in experimentally unknown particles has culminated recently in a revived concern with magnetically charged particles, which may also be involved in the understanding of strong interactions.  In later years, Schwinger has followed his own advice about the practical importance of a phenomenological theory of particles. He has invented and systematically developed source theory, which deals uniformly with strongly interacting particles, photons, and gravitons, thus providing a general approach to all physical phenomena. This work has been described in two volumes published under the title “*Particles, Sources, and Fields*“.  Awards and other honors include the first Einstein Prize (1951), the U.S. National Medal of Science (1964), honorary D.Sc. degrees from Purdue University (1961) and Harvard University (1962), and the Nature of Light Award of the U.S. National Academy of Sciences (1949). Prof. Schwinger is a member of the latter body, and a sponsor of the *Bulletin of the Atomic Scientists.* |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0144 |
| **Biographical** | Richard P. Feynman was born in New York City on the 11th May 1918. He studied at the Massachusetts Institute of Technology where he obtained his B.Sc. in 1939 and at Princeton University where he obtained his Ph.D. in 1942. He was Research Assistant at Princeton (1940-1941), Professor of Theoretical Physics at Cornell University (1945-1950), Visiting Professor and thereafter appointed Professor of Theoretical Physics at the California Institute of Technology (1950-1959). At present he is Richard Chace Tolman Professor of Theoretical Physics at the California Institute of Technology.  Professor Feynman is a member of the American Physical Society, the American Association for the Advancement of Science; the National Academy of Science; in 1965 he was elected a foreign member of the Royal Society, London (Great Britain).  He holds the following awards: Albert Einstein Award (1954, Princeton); Einstein Award (Albert Einstein Award College of Medicine); Lawrence Award (1962).  Richard Feynman is married to Gweneth Howarth, they have a son, Carl Richard (born 22nd April 1961), and a daughter Michelle Catherine (born 13th August 1968). |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0145 |
| **Biographical** | Charles Hard Townes was born in Greenville, South Carolina, on July 28, 1915, the son of Henry Keith Townes, an attorney, and Ellen (Hard) Townes. He attended the Greenville public schools and then Furman University in Greenville, where he completed the requirements for the Bachelor of Science degree in Physics and the Bachelor of Arts degree in Modern Languages, graduating *summa cum laude* in 1935, at the age of 19. Physics had fascinated him since his first course in the subject during his sophomore year in college because of its “beautifully logical structure”. He was also interested in natural history while at Furman, serving as curator of the museum, and working during the summers as collector for Furman’s biology camp. In addition, he was busy with other activities, including the swimming team, the college newspaper and the football band.  Townes completed work for the Master of Arts degree in Physics at Duke University in 1936, and then entered graduate school at the California Institute of Technology, where he received the Ph.D. degree in 1939 with a thesis on isotope separation and nuclear spins.  A member of the technical staff of Bell Telephone Laboratories from 1933 to 1947, Dr. Townes worked extensively during World War II in designing radar bombing systems and has a number of patents in related technology. From this he turned his attention to applying the microwave technique of wartime radar research to spectroscopy, which he foresaw as providing a powerful new tool for the study of the structure of atoms and molecules and as a potential new basis for controlling electromagnetic waves.  At Columbia University, where he was appointed to the faculty in 1948, he continued research in microwave physics, particularly studying the interactions between microwaves and molecules, and using microwave spectra for the study of the structure of molecules, atoms, and nuclei. In 1951, Dr. Townes conceived the idea of the maser, and a few months later he and his associates began working on a device using ammonia gas as the active medium. In early 1954, the first amplification and generation of electromagnetic waves by stimulated emission were obtained. Dr. Townes and his students coined the word “maser” for this device, which is an acronym for microwave amplification by stimulated emission of radiation. In 1958, Dr. Townes and his brother-in-law, Dr. [A.L. Schawlow](https://www.nobelprize.org/nobel_prizes/physics/laureates/1981/index.html), for some time a professor at Stanford University but now deceased, showed theoretically that masers could be made to operate in the optical and infrared region and proposed how this could be accomplished in particular systems. This work resulted in their joint paper on optical and infrared masers, or lasers (light amplification by stimulated emission of radiation). Other research has been in the fields of nonlinear optics, radio astronomy, and infrared astronomy. He and his assistants detected the first complex molecules in interstellar space and first measured the mass of the black hole in the center of our galaxy.  Having joined the faculty at Columbia University as Associate Professor of Physics in 1948, Townes was appointed Professor in 1950. He served as Executive Director of the Columbia Radiation Laboratory from 1950 to 1952 and was Chairman of the Physics Department from 1952 to 1955.  From 1959 to 1961, he was on leave of absence from Columbia University to serve as Vice President and Director of Research of the Institute for Defense Analyses in Washington, D.C., a nonprofit organization which advised the U.S. government and was operated by eleven universities.  In 1961, Dr. Townes was appointed Provost and Professor of Physics at the Massachusetts Institute of Technology. As Provost he shared with the President responsibility for general supervision of the educational and research programs of the Institute. In 1966, he became Institute Professor at M.I.T., and later in the same year resigned from the position of Provost in order to return to more intensive research, particularly in the fields of quantum electronics and astronomy. He was appointed University Professor at the University of California in 1967. In this position Dr. Townes is participating in teaching, research, and other activities on several campuses of the University, although he is located at the Berkeley campus.  During 1955 and 1956, Townes was a Guggenheim Fellow and a Fulbright Lecturer, first at the University of Paris and then at the University of Tokyo. He was National Lecturer for Sigma Xi and also taught during summer sessions at the University of Michigan and at the Enrico Fermi International School of Physics in Italy, serving as Director for a session in 1963 on coherent light. In the fall of 1963, he was Scott Lecture at the University of Toronto. More recently (2002-2003) he has been the Karl Schwarzschild Lecturer in Germany and the Birla Lecturer and Schroedinger Lecturer in India.  In addition to the Nobel Prize, Townes has received the Templeton Prize, for contributions to the understanding of religion, and a number of other prizes as well as 27 honorary degrees from various universities.  Dr. Townes has served on a number of scientific committees advising governmental agencies and has been active in professional societies. This includes being a member, and vice chairman, of the Science Advisory Committee to the President of the U.S., Chairman of the Advisory Committee for the first human landing on the moon, and chairman of the Defense Department’s Committee on the MX missile. He also served on the boards of General Motors and of the Perkins Elmer Corporations.  Dr. Townes and his wife (the former Frances H. Brown; they married in 1941) live at 1988 San Antonio Avenue, Berkeley, California. They have four daughters, Linda Rosenwein, Ellen Anderson, Carla Kessler, and Holly Townes. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
| **Interview** |  |
| Q2 | **I would like to ask you about lasers that you are the inventor of and also that you got your Nobel Prize in, masers and lasers, you got your Nobel Prize for, I would say that the whole generation of people today were not even born when you did this invention and discovery, but how did you start thinking about masers?** |
|  | Charles H. Townes: I was especially interested in trying to generate some short wavelengths to do spectroscopy, in other words the study of molecules and atoms. It was basic science I was interested in, I wasn’t worried about applications and so on, and so I tried very hard to find new ways of producing radiation, particularly radiation as small as a fraction of a millimetre, I wanted to get to shorter wavelengths. We could already get to wavelengths down to about three millimetres, centimetres and millimetres and so on, but I wanted to get to a fraction of a millimetre, so I worked at it very hard, I tried a lot of different … I tried several different ways and they didn’t work terribly well until I had the right idea finally. |
| Q2 | **Why did you need those shorter wavelengths? What was the need for the shorter wavelengths?** |
|  | Charles H. Townes: I wanted those shorter wavelengths in order to do science, because we didn’t have those shorter wavelengths and I wanted to study molecules in particular, the structure of molecules and the structure of atoms and the characteristic of nuclei and so on within the molecules, that’s what I wanted. |
| Q10 | **So you were allowed to do basic science in the war?** |
|  | Charles H. Townes: I was lucky in a way! It was a failure you see, a failure to get the right job, I had to take this job, but then I learned a lot from it and then that frequently happens, success grew out of failure. But Bell Lab was a good place, it was really a very good place. |
| Q6 | **Do you remember the moment of this discovery of maser?** |
|  | Charles H. Townes: I certainly do.  Charles H. Townes: I certainly do, it was quite a moment. I had been working very hard and thinking of different ways, some ways of producing short waves, and I’d even been made a chairman of the department for the … chairman of a committee rather … for the country to try to examine how to produce short waves. Our committee travelled around and looked at what people were trying to do and so on, and we just didn’t come up with any good ideas. We would have our last meeting in Washington DC and just before that last meeting I woke up early in the morning because I was worried about that we hadn’t been able to do anything.  I went out, I sat in the park, a beautiful day, and the flowers were blooming and it was sunny. I sat on the bench and now why haven’t we been able to do this, and I went through all the possibilities we had thought of, you see, and I had thought of, things I had tried and this didn’t work and this didn’t work very well, why can’t we do it now. I thought if we could get radiation from molecules – molecules can produce very short wavelengths – but how can we do that … Ah, wait a minute, wait a minute … I thought, yes, this is the way to do it. Oh yes, we can do it from molecules. We get molecules in the right kind of state and that can happen. Ah, and it was exciting.  It was exciting and I went back to my hotel room and I was in the same hotel room with [Arthur [Schawlow]](https://www.nobelprize.org/prizes/physics/1981/schawlow/facts/) who was a post doc with me at the time, and I told him about it, he said Oh well, yeah, that sounds alright. Then I went back to Colombia and after a little while I got a student to work on it and we eventually made it work. |
| Q2 | **What did your colleagues say about your idea?** |
|  | Charles H. Townes: It’s interesting, you see, many new ideas people won’t believe! |
| Q21 | **So how did you manage they would say to be angry instead to be discouraged?** |
|  | Charles H. Townes: Well, that’s part of science. If you’re going to do something original, you have to be prepared to differ with people. It’s very important to be able to differ with people. You listen to people, listen to other people’s ideas carefully, and examine your own ideas, to be sure you’re not wrong, you see, and let them criticise and thing about it, be sure you’re not wrong, but you’ve got to make your own decisions. If you’re going to do anything original then you carefully make your own decisions. |
| Q14 | **Could you ever imagine what will become of both masers and then lasers especially?** |
|  | Charles H. Townes: I knew that masers and lasers would be important and I could see some applications for them, but I couldn’t possibly foresee all the applications, many of the things in fields that I weren’t familiar with for example. I wrote a paper with a doctor, a doctor wanted to see what lasers might do for medicine, and so we wrote a paper about it in the early days shortly after laser was invented, but I had never heard of a detached retina in the eye, and he didn’t mention it to me, so that was not one of the things we put down in this paper as a possibility you see.  But that was one of the first important applications in medicine to reattach detached retina, and I’m so pleased when people tell me their eyes have been cured that way. I realised it would be good in communications, but I didn’t think it would go on fibre optics, I didn’t think about fibre optics, I knew it would be very god for communication but fibre optics … |
| Q4 | **And you are still doing science and actually using lasers in astronomy?** |
|  | Charles H. Townes: Yes, that’s right. I’m doing astronomy now. I frequently, I’ve changed my fields from time to time whenever a field becomes very popular and a lot of people doing it I think well, they don’t need me any more, I’d rather do something that’s being missed. I like to do things that I think are being missed and, so I’ve gone into astronomy particularly in infrared astronomy, which was not a well developed field, and now I’m trying to get a microscope on the sky in infrared wavelengths using two telescopes that are separated, now I have three telescopes, and for detection we use lasers for detection and then we also use lasers to control the telescopes very accurately. To do this for telescopes have to be very, very stationary, very accurately controlled, and lasers can do that, so lasers have made that work practical. |
| Q4 | **And you are looking at the black holes in the centre of the galaxy for example?** |
|  | Charles H. Townes: Yes, that’s one thing I did, I looked for black holes in the centre of the galaxy and we discovered a black hole, it’s about three million times as heavy as the sun, a black hole in the centre of our own galaxy. But right now I’m looking at stars and watching their behaviour. We can see them in so much detail, we can see a lot of things that the stars are doing and all along see them change with time. |
| Q4 | **You read Russian scientific journals in Russian?** |
|  | Charles H. Townes: I studied Russian and I knew some Russian, but the Russian journals were frequently translated into English. So, we could read them in English too. But I met these two Russians at a meeting in England, an international meeting, on spectroscopy and microwave spectroscopy and so on, and they were there, and they gave a talk about the possibility of a maser. They hadn’t yet made one, but we’d already made one and apparently they didn’t realise it! And we had a good discussion, it was very … and particularly just walking on the streets we could talk, they couldn’t talk when other people were around very much, they had to be careful because it was a restriction by the Russian government I guess, but as we walked along the streets we could talk very freely and I’d had a very good time discussing the situation with them. That was the first time I met them. |
| Q17 | **So that’s why you studied French, German, Italian, Spanish and Russian?** |
|  | Charles H. Townes: Yes, I studied languages. I like languages and so I’ve studied languages and I thought it was important to know Russian because very few people in the United States did know Russian and we were in contact with them. But I enjoy languages. |
| Q8 | **I see, and you’re also enjoying music?** |
|  | Charles H. Townes: Yes, that’s right, I’ve studied some voice and studied some music and I like to try many things. |
| Q8 | **Are you still active in music?** |
|  | Charles H. Townes: No, I’m not singing any more now. I think my voice has gotten a little rusty. |
| Q17 | **Okay. I have one final question. I have read in your biography that religion was a part of your upbringing, and I’m curious to hear about your thoughts about on the ways in which scientific and religious thinking can converge?** |
|  | Charles H. Townes: Yes, well I am religiously oriented and I think it’s important, and furthermore I think science and religion will converge in the long run because both are trying to understand our universe. Science is trying to understand how our universe works, religion is basically trying to understand the purpose of the universe, the purpose and how it works must be related, and furthermore we really use all of our human talents to understand both of them. We have to make assumptions in science, they make assumptions or faith in religion and so on, and we use our religion, intuition and so on. So in the long run if we understand both well enough they are bound to converge. They’ll come together I think. |

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| **Physics\_1999-** | |
| **ID** | 0146 |
| **Biographical** | Nikolay Gennadiyevich Basov was born on December, 14, 1922 in the small town of Usman near Voronezh, the son of Gennady Fedorovich Basov and Zinaida Andreevna Molchanova. His father was a professor of the Voronezh Forest Institute and devoted his life to investigation of the influence of forest belts on underground waters and on surface drainage.  After finishing secondary school in 1941 in Voronezh Basov was called up for military service and directed to the Kuibyshev Military Medical Academy. In 1943 he left the Academy with the qualification of a military doctor’s assistant. He served in the Soviet Army and took part in the Second World War in the area of the First Ukrainian Front. In December 1945, he was demobilized and entered the Moscow Institute of Physical Engineers where he studied theoretical and experimental physics.  From 1950 to 1953 he was a postgraduate student of the Moscow Institute of Physical Engineers. At that time, Basov was working on his thesis at the P.N. Lebedev Physical Institute of the Academy of Sciences, U.S.S.R., under the guidance of Professor M.A. Leontovich and Professor A.M. Prochorov.  In 1950 Basov joined the P.N. Lebedev Physical Institute, where at present he is vicedirector and head of the laboratory of quantum radiophysics. He is also a professor of the department of solid-state physics at the Moscow Institute of Physical Engineers.  In 1952 Dr. Basov began to work in the field of quantum radiophysics. He made various attempts (firstly, theoretical and then experimental) to design and build oscillators (together with A.M. Prochorov). In 1956 he defended his doctoral thesis on the theme “A Molecular Oscillator”, which summed up the theoretical and experimental works on creation of a molecular oscillator utilizing an ammonia beam.  In 1955 Basov organized a group for the investigation of the frequency stability of molecular oscillators. Together with his pupils and collaborators A.N. Oraevsky, V.V. Nikitin, G.M. Strakhovsky, V.S. Zuev and others, Dr Basov studied the dependence of the oscillator frequency on different parameters for a series of ammonia spectral lines, proposed methods of increasing the frequency stability by means of slowing down molecules, proposed methods of producing slow molecules, investigated the operation of oscillators with resonators in series, realized phase stabilization of klystron frequency by means of molecular oscillators, studied transition processes in molecular oscillators, and designed an oscillator utilizing a beam of deuterium ammonia. In the result of these investigations the oscillators with a frequency stability of 10-11 have been realized in 1962.  In 1957 Basov started to work on the design and construction of quantum oscillators in the optical range. A group of theorists and research workers began to study the possibilities for realization of quantum oscillators by means of semiconductors, and after A. Javan’s proposal, the possibility of their realization in the gas media was also investigated. In 1958 together with B.M. Vul and Yu.M. Popov he investigated the conditions for production of states with a negative temperature in semiconductors, and suggested utilization of a pulse breakdown for that purpose. In 1961 together with O.N. Krokhin and Yu.M. Popov, Basov proposed three different methods for the obtaining of a negative temperature state in semiconductors in the presence of direct and indirect transitions (optical excitation, utilization of a beam of fast electrons and injection of carriers through a degenerated *p-n* junction).  As a result of a cooperative effort with B.M. Vul and collaborators the injection semiconductor lasers utilizing crystals of gallium arsenide were made at the beginning of 1963.  In 1964 semiconductor lasers with electronic excitation have been made (together with O.V. Bogdankevich and A.N. Devyatkov); and somewhat later, lasers with optical excitation were constructed (together with A.Z. Grasiuk and V.A. Katulin). For these achievements a group of scientists of Lebedev Physical Institute was awarded the Lenin Prize for 1964.  Beginning from 1961 Dr. Basov (together with V.S. Zuev, P.G. Krinkov, V.S. Lctokhov *et al.*) carried out theoretical and experimental research in the field of powerful lasers. There have been found the ways of obtaining powerful short laser pulses. The nature of appearance of such pulses in quantum oscillators and their propagation in quantum amplifiers have been investigated. This work resulted in the development of high-power single-pulse Nd-glass lasers with 30 J energy and 2\*10-11 sec pulse duration (in 1968 together with P.G. Krinkov, Yu.V. Senatsky *et al.*) and multichannel lasers with energy 103 J within 10-9 sec (in 1971 in collaboration with G.V. Sklizkov *et al.*).  In 1962 N. Basov and O.N. Krokhin investigated the possibility of laser radiation usage for the obtaining of thermonuclear plasmas. In 1968 Basov and his associates (P.G. Kriukov, Yu.V. Senatsky, S.D. Zakharov) have succeeded in observing for the first time neutron emission in the laser-produced deuterium plasmas. The spectra of multicharged ions CaXVI, FeXXIII, K XIX and others have also been observed (together with O.N. Krokhin, S.L. Mandelshtam, G.V. Sklizkov). There has been developed a theory of picosecond pulse formation (together with V.S. Letokhov). In the same year Basov and his associate A.N. Oraevsky proposed a method of the thermal laser excitation. Further theoretical considerations of this method by Basov, A.N. Oraevsky and V.A. Sheglov encouraged the development of the socalled gasdynamic lasers.  In 1963 Dr. Basov and his colleagues (V.V. Nikitin, Yu.M. Popov, V.N. Morozov) began to work in the field of optoelectronics. They developed in 1967 a number of fast-operating logic elements on the basis of diode lasers. At present a logic structure of the multichannel optoelectronic systems producing 1010 operations per second for the optical data processing is under the development.  The studies of the radiation of the condensed rare gases under the action of a powerful electron beam have been initiated in 1966 by Basov and his collaborators (V.A. Danilychev, Yu.M. Popov), and they were the first to obtain in 1970 the laser emission in the vacuum ultraviolet range.  In 1968 Basov ( in cooperation with O.V. Bogdankevich and A.S. Nasibov) made a proposal for a laser projection TV. About the same time Dr. Basov (together with V.V. Nikitin) began the studies of the frequency standard in the optical range (on the basis of gas lasers). In 1970 they succeeded in realizing a gas laser stabilized in the methane absorption line with frequency stability 10-11.  In 1969 Basov (together with E.M. Belenov and V.V. Nikitin) hypothe sized that to obtain the frequency standard with the stability 10-12-10-13 a ring laser can be used with a nonlinear absorption cell.  A large contribution has been made by Dr. Basov to the field of chemical lasers. In 1970 under his guidance an original chemical laser was achieved which operates on a mixture of deuterium, F and CO2 at the atmospheric pressure. In the same year Basov (in cooperation with E.M. Belenov, V.A. Danilychev and A.F. Suchkov) proposed and developed experimentally an elion (electrical pumping of ionized compressed gases) method of gaslaser excitation. Using this method for a CO2 and N2 mixture compressed to 25 atm., they achieved a great increase of power of the gas laser volume unit compared to the typical low pressure CO2 lasers.  In the end of 1970 Basov (together with E.P. Markin, A.N. Oraevsky, A.V. Pankratov) presented experimental evidence for the stimulation of chemical reactions by the infrared laser radiation.  In 1959 Dr. Basov was awarded the Lenin Prize together with A. M. Prochorov for the investigation leading to the creation of molecular oscillators and paramagnetic amplifiers. In 1962 Dr. Basov was elected a corresponding member of the Academy of Sciences of the U.S.S.R.; in 1966, a member of the Academy; in 1967, a member of the Presidium of the Academy of Sciences of the U.S.S.R., and a foreign member of the German Academy of Sciences in Berlin; and in 1971, a foreign member of the German Academy “Leopoldina”.  Dr. Basov is Editor-in-chief of the Soviet scientific journals Priroda. (Nature) and “*Kvantovaya Elektornika*” (Quantum Electronics); he is also a member of the Editorial Board of “Il Nuovo Cimento”.  In 1970 Dr. Basov was awarded the rank of Hero of Socialist Labour. Dr. Basov is a member of the Soviet Committee of the Defence of Peace and a member of the World Peace Council.  Nikolai Basov married in 1950. His wife, Ksenia Tikhonovna Basova, is also a physicist and is with the Department of General Physics of the Moscow Institute of Physical Engineers. They have two sons: Gennady (born 1954) and Dmitry (born 1963). |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0147 |
| **Biographical** | Aleksandr Mikhailovich Prokhorov was born on July 11th, 1916, in Australia. After the Great October Revolution he went in 1923 with his parents to the Soviet Union.  In 1934 Alexander Prochorov entered the Physics Department of the Leningrad State University. He attended lectures of Prof. V.A. Fock (quantum mechanics, theory of relativity), Prof. S.E. Frish (general physics, spectroscopy), and Prof. E.K.Gross (molecular physics). After graduating in 1939 he became a postgraduate student of the P.N. Lebedev Physical Institute in Moscow, in the laboratory of oscillations headed by Academician N.D. Papaleksi. There he started to study the problems of propagation of radio waves. In June 1941, he was mobilized in the Red Army. He took part in the Second World War and was wounded twice. After his second injury in 1944, he was demobilized and went back to the laboratory of oscillations of the P.N. Lebedev Physical Institute. There he began to investigate nonlinear oscillations under the guidance of Prof. S.M. Rytov.  In 1946 he defended his thesis on the theme Theory of Stabilization of Frequency of a Tube Oscillator in the Theory of a Small Parameter..  Starting in 1947, upon the suggestion of Academician V.I. Veksler, Prochorov carried out a study of the coherent radiation of electrons in the synchotron in the region of centimetre waves. As a result of these investigations he wrote and defended in 1951 his Ph.D. thesis a “Coherent Radiation of Electrons in the Synchotron Accelerator”.  After the death of Academician I.D. Papaleksi in 1946, the laboratory of oscillations was headed by Academician M.A. Leontovich. Starting from 1950 being assistant chief of the laboratory, Prochorov began to investigate on a wide scale the question of radiospectroscopy and, somewhat later, of quantum electronics. He organized a group of young scientists interested in the subjects.  In 1954, when Academician M.A. Leontovich started to work in the Institute of Atomic Energy, Prochorov became head of the laboratory of oscillations, which position he still holds. In 1959 the laboratory of radio astronomy headed by Prof. V.V. Vitkevitch) was organized from one of the departments of the laboratory of oscillations, and in 1962 another department was separated as the laboratory of quantum radiophysics (headed by Prof. N.G. Basov).  Academician D.V. Skobeltzyn, director of the Institute, and Academician M.A. Leontovich as well, rendered great assistance in the development of the research on radiospectroscopy and quantum electronics. The investigations carried out by Basov and Prochorov in the field of microwave spectroscopy resulted in the idea of a molecular oscillator. They developed theoretical grounds for creation of a molecular oscillator and also constructed a molecular oscillator operating on ammonia. In 1955, Basov and Prochorov proposed a method for the production of a negative absorption which was called the pumping method.  From 1950 to 1955, Prochorov and his collaborators carried out research on molecular structures by the methods of microwave spectroscopy.  In 1955 Professor Prochorov began to develop the research on electronic paramagnetic resonance (EPR). A cycle of investigations of EPR spectra and relaxation times in various crystals was carried out, in particular investigations on ions of the iron group elements in the lattice of Al2O3.  In 1955, Prochorov studied with A.A. Manenkov the EPR spectra of ruby that made it possible to suggest it as a material for lasers in 1957. They designed and constructed masers using various materials and studied characteristics of the masers as well. This research was done in cooperation with the laboratory of radiospectroscopy of the Institute of Nuclear Physics of the Moscow University; this laboratory was organized by Prochorov in 1957. One of the masers constructed for a wavelength of 21 cm is used in the investigations of the radioastronomical station of the Physical Institute in Pushino.  The EPR methods were also utilized for the study of free radicals. In particular, the transition of a free radical of DPPH from a paramagnetic state into an antiferromagnetic state at 0.3K was observed.  In 1958 Prochorov suggested a laser for generation of far-infrared waves. As a resonator it was proposed to use a new type of cavity which was later called “the cavity of an open type”. Practically speaking, it is Fabri-Pero’s interferometer. Similar cavities are widely used in lasers.  At present Prochorov’s principal scientific interests lie in the field of solid lasers and their utilization for physical purposes, in particular for studies of multiquantum processes. In 1963, he suggested together with A.S. Selivanenko, a laser using two-quantum transitions.  Alexander Prochorov is Professor at the Moscow State University and Vice-President of URSI.  He married in I941; his wife, G.A. Shelepina, is a geographer. They have one son. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0148 |
| **Biographical** | Eugene Paul Wigner, born in Budapest, Hungary, on November 17, 1902, naturalized a citizen of the United States on January 8, 1937, has been since 1938 Thomas D. Jones Professor of Mathematical Physics at Princeton University – he retired in 1971. His formal education was acquired in Europe; he obtained the Dr. Ing. degree at the Technische Hochschule Berlin. Married in 1941 to Mary Annette Wheeler, he is the father of two children, David and Martha. His son, David, is teaching mathematics at the University of California in Berkeley. His daughter, Martha, is with the Chicago area transportation system, an organization endeavoring to improve the internal transportation system of that city. Dr.Wigner worked on the Manhattan Project at the University of Chicago during World War II, from 1942 to 1945, and in 1946-1947 became Director of Research and Development at Clinton Laboratories. Official recognition of his work in nuclear research includes the U. S. Medal for Merit, presented in 1946; the Enrico Fermi Prize (U.S.A.E.C.) awarded in 1958; and the Atoms for Peace Award, in 1960. Dr. Wigner holds the Medal of the Franklin Society, the Max Planck Medal of the German Physical Society, the George Washington Award of the American-Hungarian Studies Foundation (1964), the Semmelweis Medal of the American-Hungarian Medical Association (1965), and the National Medal of Science (1969). He has received honorary degrees from the University of Wisconsin, Washington University, Case Institute, University of Alberta ( Canada ), University of Chicago, Colby College, University of Pennsylvania, Yeshiva University, Thiel College, Notre Dame University, Technische Universität Berlin, Swarthmore College, Université de Louvain, Université de Liège, University of Illinois, Seton Hall, Catholic University and The Rockefeller University. He is a past vice- president and president of the American Physical Society, of which he remains a member. He is a past member of the board of directors of the American Nuclear Society and still a member; he holds memberships in the American Philosophical Society, the American Mathematical Society, the American Association of Physics Teachers, the National Academy of Science, the American Academy of Arts and Sciences, the Royal Netherlands Academy of Sciences and Letters, the American Association for the Advancement of Science, the Austrian Academy of Sciences, he is corresponding member of the Gesellschaft der Wissenschaften, Gottingen, and foreign member of the Royal Society of Great Britain. He was a member of the General Advisory Committee to the U.S. Atomic Energy Commission from 1952-1957, was reappointed to this committee in 1959 and served on it until 1964. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0149 |
| **Biographical** | Maria Goeppert Mayer was born on June 28, 1906, in Kattowitz, Upper Silesia, then Germany, the only child of Friedrich Goeppert and his wife Maria, nee Wolff. On her father’s side, she is the seventh straight generation of university professors.  In 1910 her father went as Professor of Pediatrics to Göttingen where she spent most of her life until marriage. She went to private and public schools in Göttingen and had the great fortune to have very good teachers. It somehow was never discussed, but taken for granted by her parents as well as by herself that she would go to the University. Yet, at that time it was not trivially easy for a woman to do so. In Göttingen there was only a privately endowed school which prepared girls for the “abitur”, the entrance examination for the university. This school closed its doors during the inflation, but the teachers continued to give instructions to the pupils. Maria Goeppert finally took the abitur examination in Hannover, in 1924, being examined by teachers she had never seen in her life.  In the spring of 1924 she enrolled at the University at Göttingen, with the intention of becoming a mathematician. But soon she found herself more attracted to physics. This was the time when quantum mechanics was young and exciting.  Except for one term which she spent in Cambridge, England, where her greatest profit was to learn English, her entire university career took place in Göttingen. She is deeply indebted to [Max Born](https://www.nobelprize.org/nobel_prizes/physics/laureates/1954/index.html), for his kind guidance of her scientific education. She took her doctorate in 1930 in theoretical physics. There were three Nobel Prize winners on the doctoral committee, Born, [Franck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1925/index.html) and [Windaus](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1928/index.html).  Shortly before she had met Joseph Edward Mayer, an American Rockefeller fellow working with James Franck. In 1930 she went with him to the Johns Hopkins University in Baltimore. This was the time of the depression, and no university would think of employing the wife of a professor. But she kept working, just for the fun of doing physics.  Karl F. Herzfeld took an interest in her work, and under his influence and that of her husband, she slowly developed into a chemical physicist. She wrote various papers with Herzfeld and with her husband, and she started to work on the color of organic molecules.  In 1939 they went to Columbia. Dr. Goeppert Mayer taught at Sarah Lawrence College between 1941 and 1945, but she worked mainly at the S. A. M. Laboratory, on the separation of isotopes of uranium, with [Harold Urey](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1934/index.html) as director. Urey usually assigned her not to the main line of research of the laboratory, but to side issues, for instance, to the investigation of the possibility of separating isotopes by photochemical reactions. This was nice, clean physics although it did not help in the separation of isotopes.  In 1946 they went to Chicago. This was the first place where she was not considered a nuisance, but greeted with open arms. She was suddenly a Professor in the Physics Department and in the Institute for Nuclear Studies. She was also employed by the Argonne National Laboratory with very little knowledge of Nuclear Physics! It took her some time to find her way in this, for her, new field. But in the atmosphere of Chicago, it was rather easy to learn nuclear physics. She owes a great deal to very many discussions with Edward Teller, and in particular with [Enrico Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html), who was always patient and helpful.  In 1948 she started to work on the magic numbers, but it took her another year to find their explanation, and several years to work out most of the consequences. The fact that Haxel, [Jensen](https://www.nobelprize.org/nobel_prizes/physics/laureates/1963/index.html) and Suess, whom she had never met, gave the same explanation at the same time helped to convince her that it was right. She met Jensen in 1950. A few years later the competitors from both sides of the Atlantic decided to write a book together.  In 1960 they came to La Jolla where Maria Goeppert Mayer is a professor of physics. She is a member of the National Academy of Sciences and a corresponding member of the Akademie der Wissenschaften in Heidelberg. She has received honorary degrees of Doctor of Science from Russel Sage College, Mount Holyoke College and Smith College.  They have two children, both born in Baltimore, Maria Ann Wentzel, now in Ann Arbor, and a son, Peter Conrad, a graduate student of economics in Berkeley. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0150 |
| **Biographical** | J. Hans D. Jensen was born in Hamburg on 25th June 1907, the son of a gardener Karl Jensen. From 1926 he studied physics, mathematics, physical chemistry and philosophy at the Universities of Hamburg and Freiburg i. Br. He obtained his Ph.D in 1932 in Hamburg (physics, Dr.rer.nat.). He became scientific assistant at the Institute for Theoretical Physics of the University of Hamburg. In 1936 he obtained D. Sc. in Hamburg (Dr. habil.), and he became docent in 1937, and Professor of Theoretical Physics at the Technische Hochschule in Hannover in 1941. In 1949 he was appointed Professor at the University of Heidelberg; since 1969 he is emeritus praecox. In 1947 he was honored with a professorship *h. c.* at the University of Hamburg, and in 1964 with a doctorate *h.c.* at the Technische Universitat Hannover. In 1969 he was appointed honorary citizen of Fort Lauderdale (Florida).  He has been a member of the Heidelberg Academy of Sciences since 1947, a corresponding member of the Max Planck Gesellschaft since 1960, and a member of the Sacri Romani Imperii Academia Naturae Coriosorum (Leopoldina, Halle) since 1964.  He was visiting professor at the University of Wisconsin (1951), the Institute of Advanced Study, Princeton (1952), the University of California at Berkeley (1952), the California Institute of Technology (1953), the Indiana University (1953), the University of Minnesota (1956), and the University of California at La Jolla (1961).  Since 1955 he has been, with O. Haxel, co-editor of the *Zeitschrift für Physik*. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0151 |
| **Biographical** | Lev Davidovic Landau was born in Baku on January 22, 1908, as the son of an engineer and a physician.  After graduating from the Physical Department of Leningrad University at the age of 19, he began his scientific career at the Leningrad Physico-Technical Institute. The years 1929 – 1931 he spent abroad, partly as a Rockefeller Foundation Fellow, working in Germany, Switzerland, England and, especially, in Copenhagen under [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html).  During 1932 – 1937 he was head of the Theoretical Department of the Ukrainian Physico-Technical Institute at Kharkov, and since 1937 he has been the head of the Theoretical Department of the Institute for Physical Problems of the Academy of Sciences of the U.S.S.R. in Moscow. Simultaneously he taught constantly as a professor of theoretical physics in the Kharkov and Moscow State Universities.  Landau’s work covers all branches of theoretical physics, ranging from fluid mechanics to quantum field theory. A large portion of his papers refers to the theory of the condensed state. They started in 1936 with a formulation of a general thermodynamical theory of the phase transitions of the second order. After P.L. Kapitsa’s discovery, in 1938, of the superfluidity of liquid helium, Landau began extensive research which led him to the construction of the complete theory of the “quantum liquids” at very low temperatures. His papers of 1941 – 1947 are devoted to the theory of the quantum liquids of the “Bose type”, to which the superfluid liquid helium (the usual isotope 4He) refers. During 1956-1958 he formulated the theory of the quantum liquids of the “Fermi type”, to which liquid helium of isotope 3He refers.  In 1946 he was elected to the membership of the Academy of Sciences of the U.S.S.R. The U.S.S.R. State Prize was awarded to him several times, and in 1962 he received, jointly with E.M. Lifshitz, the Lenin Science Prize for their *Course of Theoretical Physics*.  Landau is a Foreign Member of the Royal Society (London), of the Danish Royal Academy of Sciences, of the Netherlands Royal Academy of Sciences, Foreign Associate of the National Academy of Sciences of the U.S.A., Honorary Member of the American Academy of Arts and Sciences, of the Physical Society (London), and of the Physical Society of France. In 1961, he received the Max Planck Medal and the Fritz London Prize. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0152 |
| **Biographical** | Robert Hofstadter, Professor of Physics at Stanford University, was born in New York, N.Y., of parents Louis Hofstadter and Henrietta Koenigsberg, on February 5, 1915.  Hofstadter attended elementary and high schools in New York City, and was graduated in 1935 from the College of the City of New York with the B.S. degree, *magna cum laude*.  On graduation from college Hofstadter received the Kenyon Prize in Mathematics and Physics, and a little later the Coffin Fellowship, awarded by the General Electric Company. He went to graduate school at Princeton University where he studied physics from 1935 – 1938, and received both the M.A. and Ph.D. degrees in 1938 from that institution. His Ph.D. work was concerned with infrared spectra of simple organic molecules, and in particular, with the partial elucidation of the structure of the now well-known “hydrogen bond”. In 1938 – 1939 he was awarded a Procter Fellowship at Princeton University for postdoctoral work, at which time he began a study of photoconductivity in willemite crystals. This work led to the discovery, with R. Herman, of the warm-up dark currents which demonstrated the existence of trapping states in crystals. In 1939 Hofstadter received the Harrison Fellowship at the University of Pennsylvania where he helped to construct a large Van de Graaff machine for nuclear research. At Pennsylvania he first met L. I. Schiff, who has been a friend and colleague for many years.  During the war years Hofstadter worked first at the National Bureau of Standards and later at the Norden Laboratory Corporation. He left industry at the end of the war to become Assistant Professor of Physics at Princeton University. At Princeton he carried out research on crystal conduction counters, on the Compton effect, and on scintillation counters. In 1948 he discovered that sodium iodide, activated by thallium, made an excellent scintillation counter. In 1950, with J. A. McIntyre, he found that well-formed crystals of this material provided remarkable energy-measuring devices for gamma rays and energetic particles and thus could be used as spectrometers in addition to gamma-ray and particle counters of high efficiency.  In 1950 Hofstadter left Princeton to become Associate Professor of Physics at Stanford University where he initiated a program on the scattering of energetic electrons from the linear accelerator, invented by W. W. Hansen, which was then under construction. While building equipment for the electron-scattering experiments, he continued working on scintillation counters and developed new detectors for neutrons and X-rays. High-speed inorganic (CsF) and useful Cerenkov (TlCl) counters were discovered at Stanford. Other studies carried out in the early years at Stanford were concerned with cosmic rays and with cascade showers generated by high-speed electrons.  After 1953 electron-scattering measurements became Hofstadter’s principal interest. With students and colleagues he investigated the charge distribution in atomic nuclei and afterwards the charge and magnetic moment distributions in the proton and neutron. The electron-scattering method was used to find the size and surface thickness parameters of nuclei. Many of the principal results on the proton and neutron were obtained in the years 1954-1957. Since 1957 emphasis in the research program has been placed on making more precise studies of the nucleon form factors. This work is still in progress.  Hofstadter was elected to the National Academy of Sciences (U.S.A.) in 1958 and was named California Scientist of the Year in 1959. He has also been a Guggenheim Fellow (1958 – 1959) and spent one year at CERN in Geneva, Switzerland, on sabbatical leave.  In 1942 he married Nancy Givan of Baltimore, Maryland, and they have a son, Douglas, and two daughters, Laura and Mary. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0153 |
| **Biographical** | Rudolf Ludwig Mössbauer was born in Munich on the 31st of January 1929, the son of Ludwig Mössbauer and his wife Erna, *née* Ernst. He was educated at the “Oberschule” (non-classical secondary school) in Munich-Pasing and left after matriculating in I948. After working for one year in industrial laboratories, he started reading physics at the Technical University (Technische Hochschule) in Munich in 1949 and passed his intermediate degree examinations in 1952. During the years 1953 and 1954 he completed his thesis at the Laboratory for Applied Physics at the Technical University in Munich, at the same time acting as assistant lecturer at its Institute of Mathematics. From 1955 to 1957 he worked on his thesis for the doctorate and carried out a series of investigations at the Institute for Physics of the Max Planck Institute for Medical Research in Heidelberg, in the course of which he carried out the first experimental observation of the phenomenon of Recoilless Nuclear Resonance Absorption. In January 1958 he received his degree under Professor Maier-Leibnitz at the Technical University in Munich. In 1958, again at the Max Planck Institute in Heidelberg, he provided the direct experimental evidence of the existence of Recoilless Nuclear Resonance Absorption. For the year 1959 he was appointed scientific assistant at the Technical University in Munich.[1](https://www.nobelprize.org/prizes/physics/1961/mossbauer/biographical/#not1) He accepted an invitation by the California Institute of Technology in Pasadena, U.S.A., in 1960 and there continued his investigations of gamma absorption, at first as Research Fellow and later as Senior Research Fellow. He was appointed Professor of Physics at the California Institute of Technology in 1961.  From the year 1953 onwards his main work was directed towards the study of absorption of gamma rays in matter, in particular the study of nuclear resonance absorption. This led to the discovery of Recoilless Nuclear Resonance Absorption and its theoretical interpretation. During the last few years he has been investigating problems of nuclear physics and of solid state physics by applying already previously established methods.  His work in the field of Recoilless Nuclear Resonance Absorption has been rewarded by the following prizes: Prize of the Research Corporation New York (1960); Röntgen Prize of the University of Giessen (1961); Elliot Cresson Medal, Franklin Institute (1961).  He is married to Elisabeth, *née* Pritz, and has a daughter, Susi.[2](https://www.nobelprize.org/prizes/physics/1961/mossbauer/biographical/#not2)  1. Meanwhile, Richard Feynman had become aware of Mössbauer’s work on nuclear resonance absorption and made him accept a position as research fellow at the California Institute of Technology (Caltech) in California, USA, where he quickly became senior research fellow, and full professor in early 1962. It was there and then, when in the small hours of the day he received the phone call from Stockholm that he had been awarded the 1961 Nobel Prize for his work on Recoilless Nuclear Resonance Absorption of Gamma Radiation (1961 co-winner was R. Hofstadter), which proved to be the crucial basis for the discovery of nuclear resonance fluorescence, becoming known as Mössbauer Effect. This ME has since played an important role in applications in science far beyond physics. (Updated by the Laureate, May 2005.)  2. He had two more children with Elisabeth, a son, Peter, and a daughter, Regine. Later, he married Christel, *née* Braun. (Updated by the Laureate, May 2005.)  **Addendum, May 2005**  In 1965 Rudolf Mössbauer accepted a call from the Bavarian Ministry of Culture, Education & Research to become full professor at Technische Universität München, where his scientific interests shifted from nuclear research towards neutrino physics. It was in 1972 that he went to Grenoble (France) to be Director of the Institute Max von Laue-Paul Langevin (ILL), and the German-French-British High-Flux Reactor. After the five years’ directorship period he returned to Munich in 1977, only to find his modernisation of the faculty – a prerequisite for his accepting to waive a US career for Munich – had meanwhile been reversed. Nevertheless, Mössbauer turned down several calls from other universities and Max Planck Institutes, and continued with his research on the “neutrino puzzle” (in particular neutrino oscillation experiments at Goesgen/Switzerland and solar neutrino experiments (gallex) at the Gran Sasso Underground Laboratories in Italy) until his retirement in 1997 but also beyond. In fact, it is still keeping him rather busy today, leaving him less time than anticipated for his hobbies hiking, classical piano, and photography, which stand for many anecdotes.  He holds numerous awards, medals, and prizes from universities and institutions the world over, as well as 13 honorary professorships at the most renowned universities in Europe, and abroad.  *Rudolf Mössbauer died on 14 September 2011.* |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q6 | **We sit here in lovely Lindau in the south of Bavaria and this is the celebration of the 50th anniversary of the Nobelpreisträgertagungen in Lindau. And I have Professor Rudolf Mössbauer, who received the Nobel Prize in Physics in 1961. And I’m going to ask you as the first question. Could you tell us some parts of the story which led to you becoming a physicist?** |
|  | Rudolf Mössbauer: It’s a difficult story. First of all, I grew up right after the war, which was already some handicap. The story really was I had never seen a physicist in my life before I went to studies. That’s the first thing, and therefore I came from the mathematical side. And I had once a teacher for four months in my school in which mathematics was taught, and I suddenly got the idea this was useful. So I went to the university. Physics was the worst field in my school, simply because the teachers were no good. But I had the feeling there is more behind that than actually there was presented in school.  Therefore I went to the university. I enrolled in all four fields which were similar to each other, which were mathematics, which was engineering physics, which was pure physics and which was high school teaching. So it was not too much difference between the four fields. It was only at the pre-diploma, which is roughly equivalent of the bachelor’s degree, in which I then clearly had the feeling I’m a physicist and nothing else. And I then decided to study pure physics and nothing else. |
| Q20 | **So it means that if you start very young and you make some important discovery or invention you actually can have a new career?** |
|  | Rudolf Mössbauer: Yes. I had a completely new career. I mean the neutrinos have nothing whatsoever to do with the old field of research on which I was engaged. |
| Q4 | **But could we go back? I realise that it is asking you quite a lot to go back and to try to remember when you made this discovery, or the effect that has been given your name, the Mössbauer effect. Was there some special moments you remember?  Could you also say a little bit of what it is?** |
|  | Rudolf Mössbauer: What it is, is essentially … I learnt how to get rid of the so-called width of the gamma lines which I was using, which were at that time some ten orders of magnitude and now up to 15 orders of magnitude. You got rid in one step by incorporating the nuclei on which I was working, in solids rather than in gaseous form as most people had done before. So I got rid of the width of the lines immediately in one big step, and this of course opened a new area in which you could make many specialised experiments which were not possible before. That was it in essence. It’s like throwing a stone in a boat. If you throw the stone during the summer period, the majority of the energy is going into the stone but a small amount is going into the recoil. And it was this small amount which prevented the overlap of emission and absorption lines, which made it quite difficult to observe the effect in the old times.  They searched for nearly 40 years on this and they didn’t understand why it didn’t happen. The reason was simply this recoil phenomena. If you do the same experiment on the boat frozen into the lake during wintertime, then the whole lake takes up the energy, you have no recoil energy and the emission and the absorption lines are exactly on the same spot. And this is part of the story, but it’s an essential story, which tells you the emission and absorption line are exactly on the same energy and therefore you are in business. |
| Q21 | **So then of course I have to ask you was this a trial and error discovery, or was this something that you sat down with a paper and pencil and thought about?** |
|  | Rudolf Mössbauer: No, it was really a trial and error discovery. First I should have gone to high temperatures. If I would have done that I would never have discovered the effect. But I went to low energy simply because I felt higher temperatures and lower temperatures were about the same phenomena; it was about the same size of the effect. So I felt it was easier to build a cryostat, which we called a cryostat, you wouldn’t call it today a cryostat, but it was at that time a cryostat. So I went to low temperatures and therefore I discovered the effect, because I always found the wrong sign. It was a very tiny cross-section measurement at that time.  Nowadays you do it by Doppler shift experiments, which I discovered also later on but after my thesis only, which is a factor of 100 higher so that’s easy to do. But at that time I did a cross-section measurement and it was an effect of ten to the minus four plus or minus three times ten to the minus five, which is nearly impossible today to make. Even today I did it with tube electronics rather than transistorised electronics. But you cannot do it today, because we make lots of correlations, and every correlation wipes out the effect and therefore the effects are, even to measure intensity change of one percent or point one percent today, is difficult. Point ten to the minus four with an error of three times and ten to the minus five is nearly impossible today. |
| Q8 | **When you had understood that this had high potential for applications, did you yourself go into those applications?** |
|  | Rudolf Mössbauer: Yes. I went into the applications. I spent about, well, some years in chemistry, I spent some year in biology. These were the most important applications. You could really … Every field, whether it’s medicine or anything you can do today. But I realised, after having been then for five years Director of the neutron research reactor in Grenoble, that it was time to leave. Even nowadays, some 2,000 publications per year annually are being done, so it was hopeless. There were hundreds of laboratories which were involved in the kind of research, and largely working along hyperfine interactions. Isomer shifts, which is zero, M1s are magnetic dipole interactions and E2s are electric quadruple interactions. Most experiments are along these lines.  I even asked the Max Planck Institute at Heidelberg, where I have been doing my work at the beginning, to provide me with iron-57, because the majority of experiments, even nowadays, is done with iron-57 because there you can do it at room temperature already. By lowering the energy sufficiently well you can do it already at room temperature and ask them to provide me this isotope. But unfortunately they declined me. They declined us. |
| Q9 | **It’s very interesting because I know several cases of people who have you know found something out and then have spent the rest of their life just applying you know what they found out. So my next question would be when you decided that it was time to lead the application of the Mössbauer effect, was the Nobel Prize in any way involved in that decision?** |
|  | Rudolf Mössbauer: Actually not. I got the knowledge from receiving the Nobel Prize in 1961, through United Press, because I was in the West Coast in the States in California, and the East Coast was rather close, Stockholm was much further. So, but I had a very bad flu at that time. And we always had a flu each year for about one week. And it was really you felt all your liver and your intestinals you felt and you completely lost your voice.  You had to write to communicate by writing with your wife or with whoever took care of you, because after three days for three days it was completely lost, the voice. But then it came back and I already knew by then it was nothing dangerous. And I didn’t have any fever but I was in bed when I received the knowledge of the Nobel Prize and I didn’t care, I couldn’t care less then. |
| Q9 | **And that change was the Nobel Prize … Did it have nothing to do with that?** |
|  | Rudolf Mössbauer: No, it had nothing to do with that. It was simply it was a field in which I wanted to change to go to something else. But the Nobel Prize had nothing to do with that. |
| Q2 | **No. That’s very interesting. Now, could I ask you also a question which we did not discuss before, but I mean you can say no if you want to? I mean you have worked in many places, I mean many laboratories, and also very different laboratories; some of the smaller, some of the bigger ones and so on. And you know there is a discussion about creative environments. Could you say something from your experience about …?** |
|  | Rudolf Mössbauer: I can only say my own experience. I was first working at Caltech, which was in solid state physics and there work very few people are together. Actually I was essentially alone. I had some students with me, but basically I was alone. If you are depending on machines, on big reactors, or any at Grenoble, it was a big installation. But the bigger installations where I really did experiments of my own was … first of all we started in the neutrino work at Grenoble. We could get very close to the fuel element. We got to within five metres. But I could move, I could move the experiment, therefore we left Grenoble. And then we went to Switzerland and went to Goesgen. It was a pure electrical station but it was a power reactor. I stayed outside. Fortunately, I measured the distance between. The closest distance was 38 metres, the furthest distance was I think 64 metres if I remember, and we had in-between another position. So we did experiments there.  We had a group of maybe ten people. It was a lot of fights. I then went to the Gran Sasso Laboratory in Italy. And we had about 50 people there and I though the fights are continuing and they are getting worse. It was not the case. It was very quiet there and we had no fights whatsoever between ourselves. Simply because we had young people, very young people, at Goesgen in Switzerland and they were very ambitious. We had older people in the Gran Sasso Laboratory and they were not so ambitious. You could just say you didn’t understand that and they didn’t bother about this, while the young ones would bother about this. And therefore we had about 50 people all together in the Gran Sasso Laboratory and it was peaceful. |
| Q10 | **That was very interesting. I mean you say that if you have more grown-up scientists then sort of the general feeling is more peaceful. But a question about creativity. I mean would you say that the peaceful environment would give more new ideas or would that be the other environment where we have the young?** |
|  | Rudolf Mössbauer: It didn’t matter so much. Of course with the younger people you get more fights and they have also strange ideas. I make maybe a remark myself: I think it’s the young people who make the progress in physics. If you consider all the major discoveries, most major discoveries, not all of them but most have been made by people under 30. There’s no question about that. And the older you get the more knowledge you acquire, but the least your temptation is to get into new fields. You just know too much. You know the reason for this and for that and for that, why it doesn’t work.  For instance, when I was a young fellow I remember I had a paper in my hands, which I mentioned before, in which Steinwedel and [Jensen](https://www.nobelprize.org/nobel_prizes/physics/laureates/1963/jensen-facts.html) were the ones. Jensen got later on the Nobel Prize also himself for another area. And he had proven mathematically that it was impossible to find what I had found later in Heidelberg some 40 years back. But he had proven it mathematically, but I was too stupid to understand it. And that’s why I think young people, who are more likely to try something which is unconventional which older ones wouldn’t try because they know too much, are making the progress in physics. |

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| **Physics\_1999-** | |
| **ID** | 0154 |
| **Biographical** | Donald Arthur Glaser was born in Cleveland, Ohio, on September 21, 1926, the son of William J. Glaser, a businessman, and his wife Lena. He received his early education in the public schools of Cleveland Heights, Ohio, and took his B.Sc. degree in physics and mathematics at the Case Institute of Technology in 1946. His first original research is described in his bachelor’s thesis and consists of an electron diffraction study of the properties of thin metallic films evaporated onto crystalline metal substrates.  After serving as a teacher of mathematics at the Case Institute of Technology during the spring of 1946, he began his graduate study at the California Institute of Technology in the autumn of the same year, finishing his Ph.D. work in the autumn of 1949, and receiving his degree in physics and mathematics officially in 1950. His doctoral thesis research was an experimental study of the momentum spectrum of high energy cosmic ray and mesons at sea level.  Glaser began his career of full-time teaching and research in the Physics Department of the University of Michigan in the autumn of 1949, being promoted to the rank of Professor in 1957. In 1959 he became Professor of Physics at the University of California, at Berkeley. His main research interest during this period was the elementary particles of physics, particularly the strange particles. He examined various experimental techniques that seemed useful in this research and constructed a number of diffusion cloud chambers and parallel-plate spark counters before finally beginning to develop the ideas that led to the invention of the bubble chamber in 1952. Since then he has worked on the development of various types of bubble chambers for experiments in high energy nuclear physics, besides carrying out experiments on elementary particles at the Cosmotron of the Brookhaven National Laboratory in New York and the Bevatron of the Lawrence Radiation Laboratory in California. These experiments gave information on the lifetimes, decay modes, and spins of the L° hyperon, K° meson and S° hyperon as well as differential cross-sections for the production of those particles by pions.  Other experiments yielded information on pion-proton scattering, parity violation in non-leptonic hyperon decay, and the branching ratios in positive K meson decay.  All these experiments and technical developments of the past six years were carried out in collaboration with a number of his thesis students and colleagues at the University of Michigan and the University of California at Berkeley, where he worked from 1959. Among his associates in research were J. Brown, H. Bryant, R. Burnstein, [J. Cronin](https://www.nobelprize.org/nobel_prizes/physics/laureates/1980/index.html), C. Graves, R. Hartung, J. Kadyk, D. Meyer, M. Perl, D. Rahm, B. Roe, L. Roellig, D. Sinclair, G. Trilling, J. van der Velde, J. van Putten and T. Zipf.  These researches were supported originally by the University of Michigan and later by the National Science Foundation of the United States and the United States Atomic Energy Commission.  Glaser has received many honours for his work, among which can be mentioned the Henry Russell Award of the University of Michigan, 1953, for distinction and promise in teaching and research; the Charles Vernon Boys Prize of the Physical Society, London, in 1958, for distinction in experimental physics; the American Physical Society Prize (sponsored by the Hughes Aircraft Company) for his contributions to experimental physics in 1959; and the award, in the same year, of the honorary degree of Doctor of Science by the Case Institute of Technology.  1960, the year in which he was awarded the Nobel Prize for Physics, also saw Professor Glaser’s marriage to Miss Ruth Bonnie Thompson.  He has two children by his first marriage, Louise Ferris Glaser, a pediatrician, and William Thompson Glaser, CEO of a computer-related company. The family now includes four granddaughters. In 1975 he married Lynn Bercovitz, a painter and musician. They reside in Berkeley, California.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Physics 1942-1962*, Elsevier Publishing Company, Amsterdam, 1964  This autobiography/biography was written at the time of the award and first published in the book series [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html). It was later edited and republished in [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html). To cite this document, always state the source as shown above.  Copyright © The Nobel Foundation 1960  **Addendum, March 2005**  Glaser turned away from physics in 1962 to explore the new field of molecular biology, which had fascinated him from his time in graduate school at Caltech. In those days, Professor [Max Delbrück](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1969/index.html), another physicist, led a very exciting seminar on the work he and others were doing on the genetics of microorganisms, which previously had been thought to have no genetics. It turned out that the genetic molecules, DNA and RNA, in those organisms were the same as those in human cells, thereby establishing the scientific basis for the biotechnology industry. In addition to studying the control of DNA synthesis in bacteria, Glaser and his students showed that certain mutations in cultivated Chinese hamster ovary cells caused abnormal sensitivity to ultraviolet light which could convert these mutated cells into cancer cells. The seven genes involved in this process are also found in humans, where the same defects lead to a human cancer called *xeroderma pigmentosum* in which patients can lead normal cancer-free lives only if they avoid exposure to daylight.  In about 1970, the new field of molecular biology was producing remarkably detailed knowledge which had not been very extensively applied to medical and other applications. Motivated by this observation, Glaser and two friends co-founded the first biotechnology company, thus starting an industry that is having great success in bringing the fruits of molecular biology to applications in medicine and agriculture.  As molecular biology became industrialized and came to depend on very sophisticated biochemical and molecular technologies, Glaser began to work in neurobiology, another long term interest of his. The human visual system is the best known part of the brain, accounting for about one third of all the neurons in the cerebral cortex. Since its “wiring diagram” is known in considerable detail, computational models of human vision can be used to make predictions about human and monkey visual abilities which are testable by psychophysical and electrophysiological methods. These models have yielded descriptions of the perception of motion and depth and have made predictions concerning two surprising illusory motion effects that are being tested now by Glaser and his research group by psychophysical methods in humans and by collaboration with other groups by electrophysiological methods in monkeys.  In the years since 1960, Glaser has been a consultant and advisor to many governmental organizations, industrial boards of directors, non-profit groups, and a member of the editorial boards of several scientific publications.  *Donald A. Glaser died on 28 February 2013.* |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q6 | **We are now in Lindau, a small town in southern Germany, and this is the celebration, the 50th anniversary of the Nobelprizeträgertagungen and I am here with Professor Donald Glaser. Professor Glaser could you tell us a little bit why did you become physicist?** |
|  | Donald Glaser: As a child I was always interested in mechanical things and I built model airplanes and radios and circuits and so on. At the age of eight I tried to read about relativity because everybody said you had to be very smart to know relativity, but I could not understand a word. But actually, when I came to high school and was getting ready to go to college I did not know the difference between science and engineering and my parents did not either. Nor did my teachers in high school, so I began in college as an engineering student and it took me about six weeks to figure out that that is not what I was interested in and that physics was really the key to fundamental understanding of the physical world. I found I was really fascinated by mathematics and physics and I could do it very well, so it was easy to make the decision to go in that direction.  There was a problem, which is that at first I had no idea of the difference between the level expected in high school and in college, so I got zero on the first physics exam. And then when I went to my professor and said I wanted to be a physicist he looked in the book, he said, ‘No you can’t be.’ He said that if you get a zero in an exam you can’t get an A in physics and you can’t be a physicist unless you get an A in physics. ‘But’ he said, ‘I’ll make you a deal. If you get 100 on every exam, even though it is illegal, I will give you an A.’ So that is what I did, I worked very hard. |
| Q2 | **Eventually, if we go very quickly through your career, eventually you received the Nobel Prize in Physics. I know that this was for the invention of the bubble chamber. Could you say a little bit more about that? Could you say a little bit more about how one detected elementary particles before and how you came to the idea of the bubble chamber and how that was developed?** |
|  | Donald Glaser: As a result of my courses in graduate school at Caltech I became very much interested in the question of the fundamental particles; that is what is the universe made of and how do they interact to explain the properties of things? That is what interested me. I did a thesis in cosmic ray research which was the really only way to study particles produced by very high energy collisions in that time. That was in 1947-48. Using cloud chambers we could get one interesting picture a day. An interesting picture was one which showed a thing called a V particle and we had no idea what that was, but it was clearly something mysterious. Now we call them strange particles for complicated theoretical reasons, and with a slide rule I could compute the relativistic parameters of each picture before I had the next picture, so everything was well matched. But the number of facts that we could gather was much too small to get any theoretical insight and there was an enormous variety with huge errors.  There are a number of very funny things that happened, people would claim they discovered a new particle, and then [Hans Bethe](https://www.nobelprize.org/prizes/physics/1967/bethe/facts/), professor at Cornell, also a Nobel Prize winner, calculated that when a particle goes through a cloud chamber it gets scattered by hitting gas molecules so that you can get almost any mass at all. Therefore, none of these particles meant anything. In fact there was a group in Armenia that published a paper saying there was a thing called a veritron, and a veritron was a particle that could have any mass. It was really nonsense, so things were in bad shape and so I set myself the task of trying to figure out a way to increase the rate of collecting data. The main method at the time was the so called high pressure cloud chamber which was a box with gas at about 20 atmospheres and every time you expanded the chamber there was an enormous bang and then you could not do it again for about a half hour. Because the thermodynamics required a long time for stability in order to get straight tracks, so that did not seem like a very good solution. At that time already people were using various kinds of counters with the accelerators, but the cloud chamber really was not very useful with the accelerator, because the accelerator could produce a beam of high energy particles every 5 seconds typically, and the cloud chamber just could not keep up with that.  Anyway, I did not want to work with accelerators. In those days at least I had the ideal that a scientist was a lone individual who thought very hard and focussed on a particular … It was not the idea of a big group of people. In those days the groups were not very big, but I decided what I wanted to do was make a detector that I could put on top of a mountain in cosmic rays in splendid isolation and discover new particles. That was the dream. I asked myself, ‘Well how can you do that?’ The main idea obviously is what you want is a transparent medium in which you can see tracks and which has high density. The ideal thing would have been a transparent lead brick … You can’t have a transparent lead brick, so I began to think what can you do with glass? What can you do with liquids? What can you do with crystals? I had the idea that … This is a cotton shirt but many shirts in those days were made of Dacron. And the monomer of Dacron I think is acrylonitrile which is soluble in. I do not remember, alcohol, water, forgotten. When it is polymerised to make Dacron it becomes insoluble. The fantasy I had was that a particle would come crashing into a big glass vessel filled with acrylonitrile and it would make whatever it is going to do, a spray of particles, and they would turn into Dacron and then I would fish it out and it would be like a Christmas tree and I would measure it. That was the dream. It turns out that I did not get any Christmas trees, but instead the solution gradually became brown. It was an excellent dosimeter, but it was not a particle detector. |
| Q16 | **… it was special. That was a very special story you told us and I think that we are very grateful for that. For me the really mysterious thing is now you have told us something which clearly shows that you did something, you enjoyed it, it was fascinating to be in the library, with the journal, in the laboratory at night and at the Cosmotron and so on. But I know that since you received the Nobel Prize you have started doing something else. Could you tell us why a little bit?** |
|  | Donald Glaser: It is very easy. As I mentioned I was hoping that I would sit by myself on a mountain and discover particles. But it turned out that this gadget, the bubble chamber, was ideal for the accelerators and so I was trapped, I had to work with accelerators, and so I did and I took car loads of students and equipment and so on. But I did not like that, and finally the last paper that I contributed to, there were 23 authors and we had to go to Geneva to agree on the final draft because we had so many pictures that I sent them to many countries and so then there were Italians and Germans and so on, everybody, so we had to come together to agree, because maybe we used different standards of measurement and so on. And I could see that that was the trend and it used to be until then that if I had an idea that I was excited about, a whim, an impulse, I could go to the lab and collect my students and try to persuade them that this was worth doing and we could start on it a few days later. But in high energy physics you had to submit things to committee after committee after committee and so you become essentially a combination politician-administrator in which the science part is a very small part.  In a way the Nobel Prize set me free because I knew they would not fire me if I did not publish something for two or three years. That allowed me to go into molecular biology, which had always interested me, because when I was a graduate student at Caltech was the time that [Max Delbrück](https://www.nobelprize.org/prizes/medicine/1969/delbruck/facts/) and others were doing their very exciting work, really working out what molecular genetics was. In those days physicists could do that. Because it was mostly a question of being very clever about combinatorics and logic and so on. And it did not require much skill or knowledge of chemistry, either. I wanted to do that and when I got my degree I went to Delbrück who was a tough character, a nice man but very strict. I told him I had been enjoying his seminars and he had seen me around. I said: “Could I be a post doc in your lab?” and he said: “What’s the matter, can’t you get a job in physics?” I was so frightened that I went away and then some years later we became friends, he invited me to teach a course with him on theoretical biology at Caltech. So I went into molecular biology, quite seriously, and worked on, mostly on control and repair of DNA synthesis in bacteria and also in mammalian cells. I suppose the most important thing to come out of that was a study of zero derma pigmentosum which assessed skin cancer, that people are very vulnerable to, who are deficient in repair mechanisms for damage caused by ultraviolet light. Such people can live completely normal lives if they stay out of the sun, so mostly they sleep during the day and they go out at night. But it is known now that there are seven enzymes, and we need not talk about, but the mechanism is now well known and I was not the only one, many people working on it. But we worked on it at some length.  Then at a certain moment some friends approached me and said: “You know, a lot is known about DNA but it has not done humanity any good.” We formed the first biotech company, I did it with a couple of friends, and also I had students in molecular biology that could not get interesting jobs and that was another reason for starting it. I had two kids and I was concerned about graduate school for them, so there were many motives, financial and otherwise. That was a very exciting time. One of our consultants was Ham Smith who was here at this meeting, so we became good friends. That was a very exciting time, but it had the disadvantage that it industrialised molecular biology and also molecular biology became to be more and more biochemistry. I am not good at biochemistry. I do not enjoy it probably because I am not good at it, so I quit. I continued with the things that our little company … I was chief, chairman of the Science Advisory Committee, but I did not have any real business responsibility. That is when I went into neurobiology, which is what I work on now. |
| Q1 | **It was very special I think, so I think we should thank you very much. Is there something more you would like to say to young people around the world?** |
|  | Donald Glaser: Sure, I mean the usual advice which I believe very seriously, is that in picking a profession or having picked a profession, to pick a particular subject, you should try very hard to find one that you really are interested in. Because whatever you do it is going to be very difficult and it is going to take a lot of persistence and if you have to force yourself to do it, you will not be as good at it. It is not good to say: “I am going to suffer for 20 years and then I will finally have the answer.” But you have to enjoy the process within reason and be very very interested in it. That is the idealistic advice. The realistic advice is you also have to ask can you earn an honest living if you do this? That is tougher and it is much harder now than it was after the Second World War where there were many opportunities and even academic jobs were very easy to get. When I got my degree I had five offers from different universities and I had done a respectable thesis, but not heroic, and it was pretty easy.  The problem is, and now it is unfortunate, but the result of the difficulty in finding a good job makes the students very conservative. They want to end up, quite reasonably, as a certified expert in some field which is in demand. If you say: “Hey, I have this idea and it is really exciting if we can do this” they are not willing to gamble, and you can’t blame them. The same thing happens with assistant professors. Until you have tenure you have to be conservative in order to survive, so only the most courageous risk takers can be lucky enough to do something which is far out. By the time you are an associate professor you can’t stay up till midnight every night. Everybody here has been talking about your people are most creative before they are 30. Well, part of the reason is that before you are 30 you have a lot more energy and endurance and it takes hard work and concentration and a very good memory for the mistakes you made. But I tend to forget some of the mistakes I made, which I did not used to do, so I think there are good biological reasons why you do better … What I think is needed is something they have been doing at Harvard for some time, which is called I think a junior fellowship. Anyway, what it amounts to is roughly a five-year appointment with a guarantee of no evaluation, no reports, nothing, until the end of the five years. I think that one of the political speakers on the first day spoke of a thing like that. I think that is really a very very important thing. Because it still will be risk taking. A student will still have to try something, say, “Well I have five years in which to do this wild idea and if it works it is wonderful.” But as whoever supposes, if it does not work, you end up teaching school in a small provincial town, which is a bit severe, but anyway my advice to students is to make some kind of a trade-off. But they know that. They do not need us to give them advice. |

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| **Physics\_1999-** | |
| **ID** | 0155 |
| **Biographical** | Emilio Segrè was born in Tivoli, Rome, on February 1st, 1905, as the son of Giuseppe Segrè, industrialist, and Amelia Treves. He went to school in Tivoli and Rome, and entered the University of Rome as a student of engineering in 1922. In 1927 he changed over to physics and took his doctor’s degree in 1928 under Professor [Enrico Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html), the first one under the latter’s sponsorship.  He served in the Italian Army in 1928 and 1929, and entered the University of Rome as assistant to Professor Corbino in 1929. In 1930 he had a Rockefeller Foundation Fellowship and worked with Professor Otto Stern at Hamburg, Germany, and Professor Pieter Zeeman at Amsterdam, Holland. In 1932 he returned to Italy and was appointed Assistant Professor at the University of Rome, working continuously with Professor Fermi and others. In 1936 he was appointed Director of the Physics Laboratory at the University of Palermo, where he remained until I938.  In 1938 Professor Segrè came to Berkeley, California, first as a research associate in the Radiation Laboratory and later as a lecturer in the Physics Department. From 1943 to 1946 he was a group leader in the Los Alamos Laboratory of the Manhattan Project. In 1946 he returned to the University of California at Berkeley as a Professor of Physics, and still occupies that position.  The work of Professor Segrè has been mainly in atomic and nuclear physics. In the first field he worked in atomic spectroscopy, making contributions to the spectroscopy of forbidden lines and the study of the Zeeman effect. Except for a short interlude on molecular beams, all his work until 1934 was in atomic spectroscopy. In 1934 he started the work in nuclear physics by collaborating with Professor Fermi on neutron research. He participated in the discovery of slow neutrons and in the pioneer neutron work carried on in Rome 1934-1935. Later he was interested in radiochemistry and discovered together with Professor Perrier the element technetium, together with Corson and Mackenzie the element astatine, and together with Kennedy, Seaborg, and Wahl, plutonium-239 and its fission properties.  His other investigations in nuclear physics cover many subjects, e.g., isomerism, spontaneous fission, and lately high-energy physics. Here he, his associates and students have made contributions to the study of the interaction between nucleons and on the related polarization phenomena. In 1955 together with Chamberlain, Wiegand, and Ypsilantis he discovered the antiproton. The study of antinucleons is now his major subject of research.  Professor Segrè has taught in temporary appointments at Columbia University, New York, at the University of Illinois, at the University of Rio de Janeiro and in several other institutions. He is a member of the National Academy of Sciences (U.S.A), of the Academy of Sciences at Heidelberg (Germany), of the Accademia Nazionale dei Lincei of Italy, and of other learned societies. He has received the Hofmann Medal of the German Chemical Society and the Cannizzaro Medal of the Italian Accademia dei Lincei. He is an Honorary Professor of San Marcos University in Peru and has an honorary doctor’s degree of the University of Palermo, Italy.  Professor Segrè is married to Elfriede Spiro; they have a son, Claudio, and two daughters, Amelia and Fausta. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0156 |
| **Biographical** | Owen Chamberlain was born in San Francisco on July 10, 1920. His father was W. Edward Chamberlain, a prominent radiologist with an interest in physics. His mother’s maiden name was Genevieve Lucinda Owen.  He obtained his bachelor’s degree at Dartmouth College in 1941. He entered graduate school in physics at the University of California, but his studies were interrupted by the involvement of the United States in World War II. In early 1942 he joined the Manhattan Project, the U.S. Government organization for the construction of the atomic bomb. Within the Manhattan Project he worked under Professor Emilio Segrè, both in Berkeley, California, and in Los Alamos, New Mexico, investigating nuclear cross sections for intermediate-energy neutrons and the spontaneous fission of heavy elements. In 1946 he resumed graduate work at the University of Chicago where, under the inspired guidance of the late [Professor Enrico Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html), he worked toward his doctorate. He completed experimental work on the diffraction of slow neutrons in liquids in 1948 and his doctor’s degree was awarded in 1949 by the University of Chicago.  In 1948 he accepted a teaching position at the University of California in Berkeley. His research work includes extensive studies of proton-proton scattering, undertaken with Professor Segrè and Dr. Clyde Wiegand, and an important series of experiments on polarization effects in proton scattering, culminating in the triple-scattering experiments with Professor Segrè, Dr. Wiegand, Dr. Thomas Ypsilantis, and Dr. Robert D. Tripp. In 1955 he participated with Dr. Wiegand, Professor Segrè, and Dr. Ypsilantis in the discovery of the antiproton.  For the next few years he and his colleagues studied the interactions of antiprotons with hydrogen, deuterium and other elements, and used antiprotons to produce antineutrons. In 1960 he, together with Professors Carson Jeffries and Gilbert Shapiro, pioneered the development and use of polarized proton targets to study the spin dependence of a wide variety of high energy processes, including the scattering of pi-mesons and protons on polarized protons, the determination of the parity of hyperons, and a test of time reversal symmetry in electron-proton scattering. These and other similar experiments were his main activity for the next 20 years. In the late ’70s and early ’80s he briefly participated in the study of the interactions of energetic light nuclei with nuclear targets at the Berkeley Bevalac accelerator. In the final years before retiring from active service he worked with Dr. David Nygren on the development and construction of the Time-Projection-Chamber that was subsequently used with great success to study high-energy positron-electron interactions at the Stanford Linear Accelerator Center.  He is a Fellow of the American Physical Society and a member of the National Academy of Sciences. He was awarded a Guggenheim Fellowship in 1957 for the purpose of doing studies in the physics of antinucleons at the University of Rome. He was appointed Professor of Physics at the University of California, Berkeley, in 1958, and served as Loeb Lecturer at Harvard University in 1959.  In 1943 he married Beatrice Babette Copper (dec. 1988). They had three daughters and one son. Subsequent marriages to June Steingart Greenfield (dec. 1991) and currently to Senta Pugh Gaiser. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0157 |
| **Biographical** | Pavel Alekseyevich Cherenkov was born in Voronezh Region on July 28, 1904. His parents, Aleksei and Mariya Cerenkov, were peasants. He graduated from the Physico-Mathematical Faculty of Voronezh State University in 1928, and in 1930 he took a post as senior scientific officer in the P.N. Lebedev Institute of Physics in the U.S.S.R. Academy of Sciences. He was promoted to section leader, and in 1940 he was awarded the degree of Doctor in Physico-Mathematical Sciences. In 1953 he was confirmed in the academic rank of Professor of Experimental Physics, and since 1959 he has controlled the photo-meson processes laboratory. He has taught in institutes for higher learning for fourteen years.  It was in 1934, whilst he was working under S.I. Vavilov, that Cerenkov observed the emission of blue light from a bottle of water subjected to radioactive bombardment. This “Cerenkov effect”, associated with charged atomic particles moving at velocities higher than the speed of light, proved to be of great importance in subsequent experimental work in nuclear physics and for the study of cosmic rays. The Cerenkov detector has become a standard piece of equipment in atomic research for observing the existence and velocity of high-speed particles, and the device was installed in Sputnik III. He has shared in the work of development and construction of electron accelerators and in investigations of photo-nuclear and photo-meson reactions.  Cerenkov was awarded State Prizes in 1946 (with Vavilov, Frank, and Tamm) and in 1951.  In 1930 he married Marya Putintseva, daughter of A.M. Putintsev, Professor of Russian Literature. They have a son, Aleksei, and a daughter, Elena. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0158 |
| **Biographical** | Il’ja Mikhailovich Frank was born in Leningrad on October 23, 1908, the younger son of Mikhail Lyudvigovic Frank, a Professor of Mathematics, and his wife, Dr. Yelizaveta Mikhailovna Gratsianova. He attended the Moscow State University as a pupil of Vavilov, and graduated in 1930. In 1931 he became a senior scientific officer in Professor A.N. Terenin’s laboratory in the State Optical Institute in Leningrad, and in 1934 he joined the P.N. Lebedev Institute of Physics of the U.S.S.R. Academy of Sciences as a scientific officer. He was promoted firstly to senior scientific officer and, in 1941, to his present position as officer in charge of the Atomic Nucleus Laboratory. Since 1957 he has simultaneously occupied the post of Director of the Neutron Laboratory of the Joint Institute of Nuclear Investigations.  The first investigations of I.M. Frank were in the field of photoluminescence and in photochemistry. From 1934 he began his work on nuclear physics in the Laboratory of Professor D.V. Skobeltzyn. The experimental investigations of pair creation by g-rays and other problems connected with the measurements and application of g-rays were carried out by him. His further works were devoted to neutron physics, the investigation of reactions on light nuclei and nuclear fission by mesons.  The subject of his theoretical investigations is the Vavilov-Cerenkov effect and related problems.  Frank was awarded the degree of Doctor of Physico-Mathematical Sciences in 1935; in 1944 he was confirmed in the academic rank of Professor, and was elected a Corresponding Member of the U.S.S.R. Academy of Sciences in 1946.  He married Ella Abramovna Beilikhis, a noted historian, in 1937. They have one son, Alexander. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0159 |
| **Biographical** | Igor Yevgenyevich Tamm was born in Vladivostok on July 8, 1895, as the son of Evgenij Tamm, an engineer, and Olga Davydova. He graduated from Moscow State University in 1918, specializing in physics, and immediately commenced an academic career in institutes of higher learning. He was progressively assistant, instructor, lecturer, and professor in charge of chairs, and he has taught in the Crimean and Moscow State Universities, in Polytechnical and Engineering-Physical Institutes, and in the J.M. Sverdlov Communist University. Tamm was awarded the degree of Doctor of Physico-Mathematical Sciences, and he has attained the academic rank of Professor. Since 1934, he has been in charge of the theoretical division of the P.N. Lebedev Institute of Physics of the U.S.S.R. Academy of Sciences.  A decisive influence on his scientific activity was exercised by Prof. L. Mandelstam, under whose guidance he worked a number of years and with whom he was closely associated since 1920, when they met for the first time, and up to the death of Prof. Mandelstam in 1944.  Tamm is an outstanding theoretical physicist, and his early researches were devoted to crystallo-optics and the quantum theory of diffused light in solid bodies. He turned his attention to the theory of relativity and quantum mechanics and he evolved a method for interpreting the interaction of nuclear particles. Together with I.M. Frank, he developed the theoretical interpretation of the radiation of electrons moving through matter faster than the speed of light (the Cerenkov effect), and the theory of showers in cosmic rays. He has also contributed towards methods for the control of thermonuclear reactions. Resulting from his original researches, Tamm has written two important books, *Relativistic Interaction of Elementary Particles* (1935) and *On the Magnetic Moment of the Neutron* (1938).  I. Tamm was elected Corresponding Member of the U.S.S.R. Academy of Sciences in 1933, and in 1953 he became an Academician. He shared the 1946 State Prize with Vavilov, Cerenkov, and Frank, and is a Hero of Socialist Labour. He is also a member of the Polish Academy of Sciences, the American Academy of Arts and Sciences and the Swedish Physical Society. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0160 |
| **Biographical** | Chen Ning Yang was born on September 22, 1922, in Hofei, Anwhei, China, the first of five children of Ke Chuan Yang and Meng Hwa Loh Yang. He is also known as Frank or Franklin.  Yang was brought up in the peaceful and academically inclined atmosphere of the campus of Tsinghua University, just outside of Peiping, China, where his father was a Professor of Mathematics. He received his college education at the National Southwest Associated University in Kunming, China, and completed his B.Sc. degree there in 1942. His M.Sc. degree was received in 1944 from Tsinghua University, which had moved to Kunming during the Sino-Japanese War (1937-1945). He went to the U.S.A. at the end of the war on a Tsinghua University Fellowship, and entered the University of Chicago in January 1946. At Chicago he came under the strong influence of [Professor E. Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html). After receiving his Ph.D. degree in 1948, Yang served for a year at the University of Chicago as an Instructor. He has been associated with the Institute for Advanced Study, Princeton, New Jersey, U.S.A., since 1949, where he became a Professor in 1955.  Yang has worked on various subjects in physics, but has his chief interest in two fields: statistical mechanics and symmetry principles. His B.Sc. thesis: “Group Theory and Molecular Spectra”, written under the guidance of Professor Ta-You Wu, his M.Sc. thesis: “Contributions to the Statistical Theory of Order-Disorder Transformations”, written under the guidance of Professor J.S. Wang, and his Ph.D. thesis: “On the Angular Distribution in Nuclear Reactions and Coincidence Measurements”, written under the guidance of Professor E. Teller, were instrumental in introducing him to these fields.  Dr. Yang is a prolific author, his numerous articles appearing in the *Bulletin of the American Mathematical Society*, *The Physical Review*, *Reviews of Modern Physics*, and the *Chinese Journal of Physics*.  Professor Yang has been elected Fellow of the American Physical Society and the Academia Sinica, and honoured with the Albert Einstein Commemorative Award (1957). The U.S. Junior Chamber of Commerce named him one of the outstanding young men of 1957. He was also awarded an honorary doctorate of the Princeton University, N.J. (1958).  In 1950 Yang married Chih Li Tu and is now the father of three children: Franklin, born 1951; Gilbert, born 1958; and Eulee, born 1961.  Dr. Yang is a quiet, modest, and affable physicist; he met his wife Chih Li Tu while teaching mathematics at her high school in China. He is a hard worker allowing himself very little leisure time. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0161 |
| **Biographical** | Tsung-Dao (T.D.) Lee was born in Shanghai, China, on November 24, 1926, the third of six children of Tsing-Kong Lee and Ming-Chang Chang.  He received most of his high school education in Shanghai. During 1943-1944, he attended the National Chekiang University in Kweichow Province. In 1945, he attended the National Southwest Associated University in Kunming, Yunnan Province. Lee’s early aptitude for physics was recognized and encouraged by Professor Ta-You Wu. After completing only his sophomore year at Southwest Associated University, Lee received a Chinese government fellowship for graduate study in the United States. From 1946-50, Lee studied at the University of Chicago where [Enrico Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/) selected Lee to be his doctoral student. In 1950, Lee received his Ph.D. degree on his thesis *Hydrogen Content of White Dwarf Stars.*  During the years 1950-53, Lee worked as a research associate and lecturer at Yerkes Astronomical Observatory, Wisconsin; at the University of California at Berkeley, and at the Institute for Advanced Study at Princeton, N.J.  Lee was then fast becoming a widely known scientist, especially for his work in elementary particles, statistical mechanics, field theory, astrophysics, condensed matter physics and turbulence, having solved several problems of long standing and great complexity. Dr. J. Robert Oppenheimer praised him as one of the most brilliant theoretical physicists then known, whose work was characterized by “a remarkable freshness, versatility, and style”.  In 1953, Lee joined Columbia University as an Assistant Professor. His first work was on the renormalizable field theory model, better known as the *Lee Model*. He was successively promoted to Associate Professor in 1955 and Professor in 1956. At age 29, Lee was then the youngest-ever full professor in Columbia University’s faculty history. In 1957, when awarded the Nobel Prize at barely 31 years of age, Lee became the second youngest scientist ever to receive this distinction. (The youngest was [Sir Lawrence Bragg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1915/), who shared the Physics Prize with his father in 1915, at the age of twenty-five).  Lee has published over 300 scientific papers and several books.  Among Lee’s many prizes and awards are the Albert Einstein Award in Science, Galileo Galilei Medal, G. Bude Medal, Science for Peace Prize, China National-International Cooperation Award, New York City Science Award, New York Academy of Science Award, Order of Merit Grande Ufficiale from Italy; and the Order of the Rising Sun, Gold and Silver Star from Japan. He received honorary doctorates, professorships, lectureships and trusteeships from over thirty universities worldwide. Lee is a member of the National Academy of Sciences, the American Academy of Arts and Sciences, the American Philosophical Society, Academia Sinica, Academia Nazionale del Lincei, the Chinese Academy of Sciences, the Third World Academy of Sciences, and the Pontifical Academy of Sciences.  Lee married Jeannette Hui-Chun Chin in 1950. They have two sons, James and Stephen. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q18 | **You were awarded the prize for overturning what was thought to be one of the fundamental laws of physics. You showed that there was not parity in weakened directions of elementary particles, which as I understand it basically means that you showed that elementary particles possess a handedness, that they possess the property of being either right or left handed. Is it still the case that people can do work that changes one’s view of the fundamental laws of physics or has that time passed?** |
|  | Tsung-Dao Lee: I would hope it’s still true. We are constantly, as we struggle against nature the challenge is never diminished of our understanding and lead us to a further puzzle. |
| Q4 | **So from the experimental front you set the parameters for the experiments that needed to be done in your theoretical work and then there was no technological barrier?** |
|  | Tsung-Dao Lee: Wu’s experiment was not the easiest experiment, but it was a sure bet that they would test the idea. And that requires low temperature so took a few months. And it’s a 100% effect. So there is no doubt about the accuracy. But once it’s known then within a few days there are more than a dozen experiments. And people did that. And then within a month there are nearly 100 experiments. So the accuracy of the theoretical idea to nature can be verified without any doubt within a short time and that also made it easier for the Nobel committee to make a decision. So that perhaps explains. |
| Q17 | **Again it begs the question what is it that causes a couple of people in their early 30s to challenge such an accepted tenant? I meant your age. What was it that caused you to challenge this?** |
|  | Tsung-Dao Lee: I think that one was the behaviour of what one called the strange particles. This was from cosmic radiation. It’s called the Theta-Tau Puzzle. It may be too long to explain. Anyway there was a puzzle and it was two particles. Obviously they are different because they have different parity. They have the same lifetime. And they have the same mass. So why should there be a doubler. |
| Q3 | **Let’s talk about those early years now. So you were born in China and what sparked your interest in physics? Where did that come from?** |
|  | Tsung-Dao Lee: I grew up in a family of learning. But then in 1941, it’s really after Pearl Harbour, then I left my home, my father’s home. And then I did not know the physics. Zero idea. So therefore from there, before I went to the US, my education was totally interrupted from second in the high school … six year of middle school and then after that you go to college. So I had four years and then I left home so that was interrupted. Then during the war I had two years of college later. But in that period of course I tried to learn things in an unorganised way. So therefore I tend to think in my own way more so. |
| Q10 | **But without formal instruction in how to read physics how did you teach yourself to work with the maths involved?** |
|  | Tsung-Dao Lee: Maths is easy because that follows … you take the beginning and you get the end. That is easy. You see. physics is much more … I can give my own reaction because I remember it vividly. Accidentally I saw one book was by Tuff. American. I think it was college physics. And the other was a Chinese book. This was accidental. And then I learned there were Newton’s three laws. I thought that’s very interesting. There are laws of nature. Then there are three laws. The first you know everyone knows. And I looked at that and say this is very good. There must be something in it. First law and third law. I said that seems to be very reasonable. It was the second law that I had … But in the book that’s Newton’s greatest contribution. /- – -/  Now my own reaction is still very vivid in my mind. On the left hand side you have f which you don’t know what it is. The right hand side you have acceleration which you want to find out. So what kind of law is that? The left hand side you don’t know. The right hand you want to find out. So I thought and thought and I looked at the book and I realised that there are two cases. Newton realised the force is a function of space. And he knew the function. One was elasticity. It’s linear in the distance. And the other one is gravitation. So once the left hand side is a non-function of space and right hand side is acceleration. Then you can solve it to be right. I thought that’s interesting. But that was not the thing that was stated in the book and so this is my approach. |
| Q2 | **How old were you when you were having these thoughts?** |
|  | Tsung-Dao Lee: I was 16. In China is where we’re fortunate, if you had no formal education during the war years, you could take what they call equal ability which is much harder, but if you perform well then you can enter college. So I had two years of college and then I got a fellowship for graduate studies in Chicago and then I studied under [Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/fermi-facts.html) and then once you get that you are alright. |
| Q10 | **Were you satisfied with the physics education that you received at university given that you’d approached it in this novel way and you hadn’t come out of some formal training system? You were training yourself and then you stepped into a formal environment of university, did you find that pleasurably, did you like what you found at university?** |
|  | Tsung-Dao Lee: I was very fortunate because I was allowed to, even though this was during the war years, I actually entered two different universities. First year, the second year was different because the war. The first year I didn’t finish and everybody had to move. The second one was in Kunming. But in both places I was treated very nice. The second year I was a second year student but I could go to any classroom I wanted to provided I took exams. So I actually tried to cross the whole college so that’s why I got the fellowship for graduate student school in the US. |
| Q4 | **How was Fermi as a supervisor? What was he like to work with?** |
|  | Tsung-Dao Lee: He took very few students. Because I was his theory student. And he would have each week we spent one afternoon with us just talking, the two of us. |
| Q5 | **You say you were his theory student. Did he have one theoretical student at a time?** |
|  | Tsung-Dao Lee: At that time when I was his student he only had one theory student. He had other experimental students. You see that is very time consuming. He spent an afternoon each week. He was at the zenus of his career. |
| Q17 | **Did you feel a great burden that you had to prepare for each of these afternoon sessions?** |
|  | Tsung-Dao Lee: He had what I would call and later I realised a tremendous technique. You see he said there are things I would like to know. He called me Lee because Tsung-Dao was much too difficult. Lee, why don’t you look up and give me a lecture next week. He was preparing something. I was very happy to teach Fermi. Of course this is an excellent way of building the student confidence and then he would ask questions and I would have to answer. Everything has to be proved just like that and why the reason. Later I realised that this was a fantastic effort of Fermi’s part. Personally guiding. To transfer his knowledge to build up the young man’s confidence. I mean this was a phenomenal thing. this is why Fermi produced so many good students. |
| Q5 | **And he died in -54 so you did know him for some time after you have finished your PhD. Did you keep an association during that time or did you go your own way?** |
|  | Tsung-Dao Lee: I passed through Chicago and I visited him. And he was also invited to other places. Looking back, I felt much more. Being young it’s like the children where you have parents who are that good to you but the realisation of the depth usually come even stronger years after that. |
| Q5 | **Yes he died very young. The things you learnt from him about the way you should treat students is that something you’ve carried on in your own teaching career?** |
|  | Tsung-Dao Lee: I try. Also I maintain with my PhD student … I always spend similar, whole afternoon, talking. And different people you do slightly different things. |
| Q1 | **What do you hope you can give your students?** |
|  | Tsung-Dao Lee: You try to give part of your love for physics and the way that you do things to a younger generation. And this of course is on top of that we teach. And the teaching is to a much bigger class. |
| Q8 | **Do you manage to have small interactions?** |
|  | Tsung-Dao Lee: I am now not at university because of my age so teaching I’m not doing. But I have colleagues which work together. |

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| **Physics\_1999-** | |
| **ID** | 0162 |
| **Biographical** | William Shockley was born in London, England, on 13th February, 1910, the son of William Hillman Shockley, a mining engineer born in Massachusetts and his wife, Mary (*née* Bradford) who had also been engaged in mining, being a deputy mineral surveyor in Nevada.  The family returned to the United States in 1913 and William Jr. was educated in California, taking his B.Sc. degree at the California Institute of Technology in 1932. He studied at Massachusetts Institute of Technology under Professor J.C. Slater and obtained his Ph.D. in 1936, submitting a thesis on the energy band structure of sodium chloride. The same year he joined Bell Telephone Laboratories, working in the group headed by Dr. C.J. Davisson and remained there (with brief absences for war service, etc.) until 1955. He resigned his post of Director of the Transistor Physics Department to become Director of the Shockley Semi-conductor Laboratory of Beckman Instruments, Inc., at Mountain View, California, for research development and production of new transistor and other semiconductor devices. In 1963 he was named first Alexander M. Poniatoff Professor of Engineering Science at Stanford University, where he will act as professor-at-large in engineering and applied sciences.  During World War II he was Research Director of the Anti-submarine Warfare Operations Research Group and he afterwards served as Expert Consultant in the offce of the Secretary for War.  He held two visiting lectureships: in 1946 at Princeton University, and in 1954 at the California Institute of Technology. For one year (1954-1955) he was Deputy Director and Research Director of the Weapons System Evaluation Group in the Defence Department.  Shockley’s research has been centred on energy bands in solids; order and disorder in alloys; theory of vacuum tubes; self-diffusion of copper; theories of dislocations and grain boundaries; experiment and theory on ferromagnetic domains; experiments on photoelectrons in silver chloride; various topics in transistor physics and operations research on the statistics of salary and individual productivity in research laboratories.  His work has been rewarded with many honours. He received the Medal for Merit in 1946, for his work with the War Department; the Morris Leibmann Memorial Prize of the Institute of Radio Engineers in 1952; the following year, the Oliver E. Buckley Solid State Physics Prize of the American Physical Society, and a year later the Cyrus B. Comstock Award of the National Academy of Sciences. The crowning honour – the Nobel Prize for Physics – was bestowed on him in 1956, jointly with his two former colleagues at the Bell Telephone Laboratories, John Bardeen and Walter H. Brattain.  In 1963 he was selected as recipient of the Holley Medal of the American Society of Mechanical Engineers.  Dr. Shockley has been a member of the Scientific Advisory Panel of the U.S. Army since 1951 and he has served on the Air Force Scientific Advisory Board since 1958. In 1962 he was appointed to the President’s Scientific Advisory Committee. He has received honorary science doctorates from the University of Pennsylvania, Rutgers University and Gustavus Adolphus Colleges (Minn.).  In addition to numerous articles in scientific and technical journals, Shockley has written *Electrons and Holes in Semiconductors* (1950) and has edited *Imperfections of Nearly Perfect Crystals* (1952). He has taken out more than 50 U.S. patents for his inventions.  Dr. Shockley has been married twice, and has three children by his first marriage to Jean (*née* Bailey). This union ended in divorce; his second wife is Emmy Lanning. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0163 |
| **Biographical** | John Bardeen was born in Madison, Wisconsin, on May 23, 1908, son of Dr. Charles R. Bardeen, and Althea Harmer. Dr. Bardeen was Professor of Anatomy, and Dean of the Medical School of the University of Wisconsin at Madison. After the death of Althea, when John was about twelve years old, Dr. Bardeen married Ruth Hames, now Mrs. Kenelm McCauley, of Milwaukee, Wisconsin.  John Bardeen attended the University High School at Madison for several years, but graduated from Madison Central High School in 1923. This was followed by a course in electrical engineering at the University of Wisconsin, in which much extra work was taken in mathematics and physics. After being out for a term while working in the engineering department of the Western Electric Company at Chicago, he graduated with a B.S. in Electrical Engineering in 1928. He continued on at Wisconsin as a graduate research assistant in electrical engineering for two years, working on mathematical problems in applied geophysics and on radiation from antennas. It was during this period that he got his first introduction to quantum theory from Professor J.H. Van Vleck.  Professor Leo J. Peters, under whom the research in geophysics was done, took a position at the Gulf Research Laboratories in Pittsburgh, Pennsylvania, and Bardeen followed him there and worked during the next three years (1930-1933) on the development of methods for the interpretation of magnetic and gravitational surveys. This was a stimulating period in which geophysical methods were first being applied to prospecting for oil.  Because he felt his interests were more in pure than in applied science, Bardeen resigned his position at Gulf in 1933 to take graduate work in mathematical physics at Princeton University. It was here under the leadership of Professor E.P. Wigner, that he first became interested in solid state physics. Before completing his thesis (on the theory of the work function of metals) he was offered a position as Junior Fellow of the Society of Fellows at Harvard University. He spent there the next three years, 1935-1938, working with [Professors Van Vleck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/index.html) and Bridgman on problems in cohesion and electrical conduction in metals, and also did some work on level density of nuclei. The Ph.D. degree at Princeton was awarded in 1936.  From 1938-1941, Bardeen was an Assistant Professor of Physics at the University of Minnesota and from 1941-1945 a civilian physicist at the Naval Ordnance Laboratory in Washington, D.C. Work done during the war was on influence fields of ships for application to underwater ordnance and mine-sweeping. After the war, in late 1945, he joined the solid state research group at the Bell Telephone Laboratories, and remained there until 1951, when he was appointed Professor of Electrical Engineering and of Physics at the University of Illinois. Since 1959 he has also been a member of the Center for Advanced Study of the University.  Main fields of research since 1945 have been electrical conduction in semiconductors and metals, surface properties of semiconductors, theory of superconductivity, and diffusion of atoms in solids. In 1957, Bardeen and two colleagues, [L.N. Cooper and J.R. Schrieffer](https://www.nobelprize.org/nobel_prizes/physics/laureates/1972/index.html), proposed the first successful explanation of superconductivity. Much of his research effort since that time has been devoted to further extensions and applications of the theory.  He is a Fellow of the American Physical Society, has been (1954-1957) a member of its Council, and on the Editorial Board of *The Physical Review* and *Reviews of Modern Physics*. From 1959-1962, he served as a member of the United States President’s Science Advisory Committee.  Bardeen was elected to the National Academy of Sciences in 1954. Honours include the Stuart Ballentine Medal of the Franklin Institute, Philadelphia (1952) and the John Scott Medal of the City of Philadelphia (1955), both awarded jointly with Dr. W.H. Brattain, the Buckley Prize of the American Physical Society (1955) and D.Sc. (Hon.) from Union College and from the University of Wisconsin. He received the Fritz London Award for work in low temperature physics in 1962.  Bardeen married Jane Maxwell in 1938. They have three children, James Maxwell, William Allen and Elizabeth Ann. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0164 |
| **Biographical** | Walter H. Brattain was born in Amoy, China, on February 10, 1902, the son of Ross R. Brattain and Ottilie Houser. He spent his childhood and youth in the State of Washington and received a B.S. degree from Whitman College in 1924. He was awarded the M.A. degree by the University of Oregon in 1926 and the Ph.D. degree by the University of Minnesota in 1929.  Dr. Brattain has been a member of the Bell Laboratories technical staff since 1929. The chief field of his research has been the surface properties of solids. His early work was concerned with thermionic emission and adsorbed layers on tungsten. He continued on into the field of rectification and photo-effects at semiconductor surfaces, beginning with a study of rectification at the surface of cuprous oxide. This work was followed by similar studies of silicon. Since World War II he has continued in the same line of research with both silicon and germanium.  Dr. Brattain’s chief contributions to solid state physics have been the discovery of the photo-effect at the free surface of a semiconductor; the invention of the point-contact transistor jointly with Dr. John Bardeen, and work leading to a better understanding of the surface properties of semiconductors, undertaken first with Dr. Bardeen, later with Dr. C.G.B. Garrett, and currently with Dr. P.J. Boddy.  Dr. Brattain received the honorary Doctor of Science degree from Portland University in 1952, from Whitman College and Union College in 1955, and from the University of Minnesota in 1957. In 1952 he was awarded the Stuart Ballantine Medal of the Franklin Institute, and in 1955 the John Scott Medal. The degree at Union College and the two medals were received jointly with Dr. John Bardeen, in recognition of their work on the transistor.  Dr. Brattain is a member of the National Academy of Sciences and of the Franklin Institute; a Fellow of the American Physical Society, the American Academy of Arts and Sciences, and the American Association for the Advancement of Science. He is also a member of the commission on semiconductors of the International Union of Pure and Applied Physics, and of the Naval Research Advisory Committee.  In 1935 he married the late Dr. Keren (Gilmore) Brattain; they had one son, William Gilmore Brattain. In 1958 he married Mrs. Emma Jane (Kirsch) Miller. Dr. Brattain lives in Summit, New Jersey, near the Murray Hill (N.J.) laboratory of Bell Telephone Laboratories. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0165 |
| **Biographical** | Willis Eugene Lamb, Jr. was born on July 12, 1913 in Los Angeles, California. His father Willis Eugene Lamb, born in Minnesota, was by profession a telephone engineer and his mother Marie Helen Metcalf came from Nebraska.  Except for three years schooling in Oakland, Calif., he was educated in the public schools of Los Angeles, Calif. In 1930 he entered the University of California at Berkeley and received a B.S. (Chemistry) in 1934. His graduate work in theoretical physics at the same university led to the Ph.D. degree in 1938. His thesis research on the electromagnetic properties of nuclear systems was directed by Professor J.R. Oppenheimer.  He went to Columbia University as Instructor in Physics in 1938, became an Associate (1943), Assistant Professor (1945), Associate Professor (1947) and Professor in 1948. From 1943 to 1951, he was associated also with the Columbia Radiation Laboratory where the research described in the Nobel Lecture was done. In 1951 he went to Stanford University in California as Professor of Physics. During 1953-1954 he was Morris Loeb Lecturer at Harvard University. From 1956 to 1962 he was a Fellow of New College and Wykeham Professor of Physics at the University of Oxford, England. In 1962 he became Henry Ford II Professor of Physics at Yale University, New Haven, Conn.  His research has been on the following subjects: theory of the interactions of neutrons and matter, field theories of nuclear structure, theories of beta decay, range of fission fragments, fluctuations in cosmic ray showers, pair production, order-disorder problems, ejection of electrons by metastable atoms, quadrupole interactions in molecules, diamagnetic corrections for nuclear resonance experiments; theory and design of magneton oscillators, theory of a microwave spectroscope, study of the fine structure of hydrogen, deuterium and helium; theory of electrodynamic energy level displacements.  In 1953 he received the Rumford Premium of the American Academy of Arts and Sciences. The University of Pennsylvania conferred an honorary degree of D.Sc. upon him in 1954. He received the Research Corporation Award in 1955. He is a member of the National Academy of Sciences, and a Fellow of the American Physical Society.  In 1939 he married Ursula Schaefer, a student from Germany. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0166 |
| **Biographical** | Polykarp Kusch was born in Blankenburg, Germany, on the 26th January, 1911, the son of a clergyman. He has lived in the United States since 1912 and is a citizen of that country. He received his early education in the midwest of the United States. His original professional goal was in the field of chemistry, but soon after beginning his course of studies at the Case Institute of Technology, Cleveland, Ohio, his interest rapidly shifted to physics. In 1931 he received the B.S. degree in physics. He carried on his graduate study at the University of Illinois which awarded him the M.S. degree in 1933 and the Ph.D. degree in 1936. At Illinois he worked on problems in the field of optical molecular spectroscopy under the guidance of Professor F. Wheeler Loomis. He worked with Professor John T. Tate at the University of Minnesota in the field of mass spectroscopy during 1936-1937.  Since 1937 Kusch has been associated with the Department of Physics of Columbia University, New York City, except for interruptions engendered by World War II. These years were spent in research and development on microwave generators at the Westinghouse Electric Corporation, the Bell Telephone Laboratories and Columbia University. The experience was important not only in that it gave him knowledge of microwave methods, but also in that it suggested application of the special techniques of vacuum tube technology to a large range of problems in experimental physics.  Kusch has been a Professor of Physics at Columbia University since 1949. From his first days at Columbia, he has been intimately associated with Professor [I.I. Rabi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/index.html) in his programme of research on atomic, molecular and nuclear properties and phenomena by the method of molecular beams. The direction in which his own research has been directed has been greatly influenced by this long association. His research has dealt principally with the small details of the interactions of the constituent particles of atoms and of molecules with each other and with externally applied fields. The establishment of the reality of the anomalous magnetic moment of the electron and the precision determination of its magnitude was part of an intensive programme of postwar research with atomic and molecular beams. Later, he has also become interested in problems in chemical physics to whose experimental study he has applied the molecular beams technique.  Professor Kusch has been awarded honorary Sc.D. degrees of the Case Institute of Technology, the Ohio State University, the University of Illinois and Colby College. He was elected to the membership in the National Academy of Sciences (USA) in 1956.  In recent years he is increasingly concerned with problems of education, especially that of educating the young to understand a civilization strongly affected by the knowledge of science and by the techniques that result from this knowledge.  Kusch married Edith Starr McRoberts; they had three daughters. His wife died in 1959 and he was married to Betty Pezzoni in 1960. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0167 |
| **Biographical** | Max Born was born in Breslau on the 11th December, 1882, to Professor Gustav Born, anatomist and embryologist, and his wife Margarete, *née* Kauffmann, who was a member of a Silesian family of industrialists.  Max attended the König Wilhelm’s Gymnasium in Breslau and continued his studies at the Universities of Breslau (where the well-known mathematician Rosanes introduced him to matrix calculus), Heidelberg, Zurich (here he was deeply impressed by Hurwitz’s lectures on higher analysis), and Göttingen. In the latter seat of learning he read mathematics chiefly, sitting under Klein, Hilbert, Minkowski, and Runge, but also studied astronomy under Schwarzschild, and physics under Voigt. He was awarded the Prize of the Philosophical Faculty of the University of Göttingen for his work on the stability of elastic wires and tapes in 1906, and graduated at this university a year later on the basis of this work.  Born next went to Cambridge for a short time, to study under Larmor and [J.J. Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1906/index.html). Back in Breslau during the years 1908-1909, he worked with the physicists Lummer and Pringsheim, and also studied the theory of relativity. On the strength of one of his papers, Minkowski invited his collaboration at Göttingen but soon after his return there, in the winter of 1909, Minkowski died. He had then the task of sifting Minkowski’s literary works in the field of physics and of publishing some uncompleted papers. Soon he became an academic lecturer at Göttingen in recognition of his work on the relativistic electron. He accepted Michelson’s invitation to lecture on relativity in Chicago (1912) and while there he did some experiments with the Michelson grating spectrograph.  An appointment as professor (extraordinarius) to assist [Max Planck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1918/index.html) at Berlin University came to Born in 1915 but he had to join the German Armed Forces. In a scientific office of the army he worked on the theory of sound ranging. He found time also to study the theory of crystals, and published his first book, *Dynamik der Kristallgitter* (Dynamics of Crystal Lattices), which summarized a series of investigations he had started at Göttingen.  At the conclusion of the First World War, in 1919, Born was appointed Professor at the University of Frankfurt-on-Main, where a laboratory was put at his disposal. His assistant was [Otto Stern](https://www.nobelprize.org/nobel_prizes/physics/laureates/1943/index.html), and the first of the latter’s well-known experiments, which later were rewarded with a Nobel Prize, originated there.  Max Born went to Göttingen as Professor in 1921, at the same time as [James Franck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1925/index.html), and he remained there for twelve years, interrupted only by a trip to America in 1925. During these years the Professor’s most important works were created; first a modernized version of his book on crystals, and numerous investigations by him and his pupils on crystal lattices, followed by a series of studies on the quantum theory. Among his collaborators at this time were many physicists, later to become well-known, such as [Pauli](https://www.nobelprize.org/nobel_prizes/physics/laureates/1945/index.html), [Heisenberg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1932/index.html), Jordan, [Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html), [Dirac](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/index.html), Hund, Hylleraas, Weisskopf, Oppenheimer, Joseph Mayer and [Maria Goeppert-Mayer](https://www.nobelprize.org/nobel_prizes/physics/laureates/1963/index.html). During the years 1925 and 1926 he published, with Heisenberg and Jordan, investigations on the principles of quantum mechanics (matrix mechanics) and soon after this, his own studies on the statistical interpretation of quantum mechanics.  As were so many other German scientists, he was forced to emigrate in 1933 and was invited to Cambridge, where he taught for three years as Stokes Lecturer. His main sphere of work during this period was in the field of nonlinear electrodynamics, which he developed in collaboration with Infeld.  During the winter of 1935-1936 Born spent six months in Bangalore at the Indian Institute of Science, where he worked with [Sir C.V. Raman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1930/index.html) and his pupils. In 1936 he was appointed Tait Professor of Natural Philosophy in Edinburgh, where he worked until his retirement in 1953. He is now living at the small spa town, Bad Pyrmont.  Max Born has been awarded fellowships of many academies – Göttingen, Moscow, Berlin, Bangalore, Bucharest, Edinburgh, London, Lima, Dublin, Copenhagen, Stockholm, Washington, and Boston, and he has received honorary doctorates from Bristol, Bordeaux, Oxford, Freiburg/Breisgau, Edinburgh, Oslo, Brussels Universities, Humboldt University Berlin, and Technical University Stuttgart. He holds the Stokes Medal of Cambridge, the Max Planck Medaille der Deutschen Physikalischen Gesellschaft (i.e. of the German Physical Society); the Hughes Medal of the Royal Society, London, the Hugo Grotius Medal for International Law, and was also awarded the MacDougall-Brisbane Prize and the Gunning-Victoria Jubilee Prize of the Royal Society, Edinburgh. In 1953 he was made honorary citizen of the town of Göttingen and a year later was granted the Nobel Prize for Physics. He was awarded the Grand Cross of Merit with Star of the Order of Merit of the German Federal Republic in 1959.  The year 1913 saw his marriage to Hedwig, *née* Ehrenberg, and there are three children of the marriage. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0168 |
| **Biographical** | Walther Bothe was born on January 8, 1891, at Oranienburg, near Berlin.  From 1908 until 1912 he studied physics at the University of Berlin, where he was a pupil of [Max Planck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1918/index.html), obtaining his doctorate just before the outbreak of the 1914-1918 war. From 1913 until 1930 he worked at the Physikalisch-Technische Reichsanstalt in the same city, becoming a Professor Extraordinary in the University there. In 1930 he was appointed Professor of Physics, and Director of the Institute of Physics at the University of Giessen.  In 1932 he was appointed Director of the Institute of Physics at the University of Heidelberg, in succession to Philipp Lenard, becoming in 1934 Director of the Institute of Physics at the Kaiser Wilhelm Institute for Medical Research (re-established in 1948 as Institute of Physics at the Max Planck Institute for Medical Research) in that city. At the end of the Second World War, when this Institute was taken over for other purposes, Bothe returned to the Department of Physics in the University, where he taught until the illness which had handicapped him for several years compelled him to restrict the scope of his work. He was able, however, to supervise the work of the Institute of Physics in the Max Planck Institute and he continued to do this until his death in Heidelberg on February 8, 1957.  Bothe’s scientific work coincided with the opening up of the vast field of nuclear physics and the results he obtained led to new outlooks and methods.  He was, during the First World War, taken prisoner by the Russians and spent a year in captivity in Siberia. This year he devoted to mathematical studies and to learning the Russian language; in 1920 he was sent back to Germany.  He then collaborated with H. Geiger at the Physikalisch-Technische Reichsanstalt in Berlin. Together with Geiger, whose influence determined much of his scientific work, he published, in 1924, his method of coincidence, by which important discoveries were subsequently made. It is based on the fact that, when a single particle passes through two or more Geiger counters, the pulses from each counter are practically coincident in time. The pulse from each counter is then sent to a coincidence circuit which indicates pulses that are coincident in time. Arrays of Geiger counters in coincidence select particles moving in a given direction and the method can be used, for example, to measure the angular distribution of cosmic rays. Bothe applied this method to the study of the Compton effect and to other problems of physics. Together he and Geiger clarified ideas about the small angle scattering of light rays and Bothe summarized their work on this problem in his *Handbuch* article published in 1926 and 1933, establishing the foundations of modern methods for the analysis of scatter processes. From 1923 until 1926 Bothe concentrated, especially on experimental and theoretical work on the corpuscular theory of light. He had, some months before the discovery of the Compton effect, observed, in a Wilson chamber filled with hydrogen, the short track of the recoil electrons of X-rays and he did further work on the direction of the emission of photo electrons. Together he and Geiger related the Compton effect to the theory of [Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html), Kramers, and Slater, and the results of their work provided strong support for the corpuscular theory of light.  In 1927 Bothe further clarified, by means of his coincidence method, ideas about light quanta in a paper on light quanta and interference.  In the same year he began to study the transformation of light elements by bombardment with alpha rays. The resulting fission products had, until then, been seen by the eye only as scintillations, but Bothe, in collaboration with Fränz, made it possible to count them by means of their needle counter.  In 1929, in collaboration with W. Kolhörster, Bothe introduced a new method for the study of cosmic and ultraviolet rays by passing them through suitably arranged Geiger counters, and by this method demonstrated the presence of penetrating charged particles in the rays, and defined the paths of individual rays.  For his discovery of the method of coincidence and the discoveries subsequently made by it, which laid the foundations of nuclear spectroscopy, Bothe was awarded, jointly with Max Born, the [Nobel Prize for Physics for 1954](https://www.nobelprize.org/nobel_prizes/physics/laureates/1954/index.html).  In 1930 Bothe, in collaboration with H. Becker, bombarded beryllium of mass 9 (and also boron and lithium) with alpha rays derived from polonium, and obtained a new form of radiation that was even more penetrating than the hardest gamma rays derived from radium, and this led to the discovery of the neutron, made by [Sir James Chadwick](https://www.nobelprize.org/nobel_prizes/physics/laureates/1935/index.html) in 1932.  At Heidelberg, Bothe was able, after much difficulty, to obtain the money necessary for building a cyclotron. He worked, during the 1939-1945 war, on the diffusion theory of neutrons and on measurements related to these.  In June 1940 he published his *Atlas of Cloud-Chamber Figures*.  He was a member of the Academies of Sciences of Heidelberg and Göttingen, and a Corresponding Member of the Saxon Academy of Sciences, Leipzig. He was awarded the Max Planck Medal and the Grand Cross of the Order for Federal Services. In 1952, he was made a Knight of the Order of Merit for Science and the Arts.  Bothe’s remarkable gifts were not restricted to physics. He had an astonishing gift of concentration and his habit of carefully making the best use of his time enabled him to work at great speed. In the laboratory he was often a difficult and strict master, at his best in discussions in small classes there, but in the evenings at home he was, with his Russian wife, very hospitable and all the difficulties of the day were then forgotten.  To his hobbies and recreations he gave the same concentration and intensity of effort that he gave to his scientific work. Chief among them were music and painting. He went to many musical concerts and himself played the piano, being especially fond of Bach and Beethoven. During his holidays he visited the mountains and did many paintings in oil and water colour. In these his style was his own. He admired the French impressionists and was eager and vigorous in his discussions of the merits and demerits of various artists.  Bothe married Barbara Below of Moscow. Her death preceded his by some years. They had two children. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0169 |
| **Biographical** | Frits Zernike was born in Amsterdam, 16th July 1888, as the second son in a family of six children. His father, Carl Frederick August Zernike, was teacher in mathematics and head of a primary school in Amsterdam, and was a highly gifted man having interests in many branches of science; he compiled numerous elementary books in a series of subjects, and had also articles on pedagogy to his credit. His mother, Antje Dieperink, was also a teacher of mathematics. One of his brothers also became a professor of physics, one of his sisters, married to the well-known painter Jan Mankes, was the first woman ordained in the Dutch Protestant Church, another sister is one of The Netherlands’ foremost literary figures.  Frits inherited his passion for physics from his father; as a boy he already possessed an arsenal of pots, crucibles, tubes, which he scraped together with his own pocket money, or received as gifts from understanding manufacturers. At the secondary school he excelled in the scientific subjects, and neglected topics such as history and languages, including Greek and Latin, for which later on he was obliged to pass a State matriculation test in order to be fully admitted to the University.  During these school years he devoted all his spare time to his endless experiments, entering also the realms of colour photography. His limited financial means forced him to synthesize his own ether which he required for his photographic experiments. Other results of his ingenuity were a photographic camera and a miniature astronomical observatory equipped with the clockwork of an old record player, which enabled him to take pictures of a comet. Together with his father and mother he also indulged in solving arduous mathematical problems.  He entered the University of Amsterdam in 1905, studying chemistry, with physics and mathematics as minor subjects. His early interest in mathematics appears from a prize essay on probabilities for which he obtained a gold medal of the University of Groningen in 1908. A more elaborate work on critical opalescence was similarly rewarded in 1912 by the Dutch Society of Sciences at Haarlem, which had as jury distinguished scientists of those days: Lorentz, Van der Waals, and Haga. When asked to choose between a gold medal and an amount of money, he wrote back that he preferred the money, since he had already enjoyed the privilege of receiving a gold medal. The prize essay later formed the basis of his doctor’s thesis (1915). In its theoretical part he applied Gibbs’ statistical mechanics and this formed the starting-point of years of fruitful collaboration with L.S. Ornstein, who worked in the same field.  In 1913 Kapteyn, the famous Professor of Astronomy at Groningen University, invited him to be his assistant. In 1915 he got his first university teaching post, not in chemistry, not in astronomy, but as successor of Ornstein as lecturer in mathematical physics at Groningen, where he was made a full professor in 1920. His papers on statistics include a paper with J.A. Prins, introducing the *g*-function for the correlation of the position of two molecules in a liquid, an extensive article in the Geiger and Scheel handbook, and an approximation method in the order-disorder problem (1940). Of his experimental work, the sensitive galvanometer, manufactured since 1923 by Kipp and Sons, Delft, is well known. From 1930 on he turned to optics, developed phase contrast, wrote on imaging errors of the concave grating and on partial coherence. With the collaboration of his pupils he solved the problem of the influence of lens aberrations on the diffraction pattern at a focus (1938-1948)  It is interesting to know that his great discovery of the phase-contrast phenomenon, which he discovered one evening in 1930 in his totally blackpainted optical laboratory, did not immediately receive the attention it deserved. The world-famous Zeiss factories at Jena completely underestimated the value of his phase-contrast microscope. It was not until the German *Wehrmacht* took stock of all inventions which might serve in the war that at last (in 1941) the first phase-contrast microscopes were manufactured. The grotesque situation thus arose that the German war machinery helped to develop on an industrial scale Professor Zernike’s long-neglected invention while its inventor, like his fellow-countrymen, suffered under the oppression by the same German powers during the occupation of the Netherlands. After the war, other firms also took up the production of many thousands of phase-contrast microscopes, thereby providing the service to science, and in particular to medicine, which should have been effectuated some twenty years earlier.  Zernike’s achievements were recognized by the Royal Microscopical Society; he was also awarded the Rumford Medal of the Royal Society (London) and an honorary doctorate in Medicine from the University of Amsterdam.  Zernike married twice. His first wife, Dora van Bommel van Vloten, died in 1945; they had one son. In 1954 he married Mrs. L. Koperberg-Baanders. After his retirement from Groningen University they moved to Naarden, a town in the countryside near Amsterdam. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0170 |
| **Biographical** | Felix Bloch was born in Zurich, Switzerland, on October 23, 1905, as the son of Gustav Bloch and Agnes Bloch (*née* Mayer). From 1912 to 1918 he attended the public primary school and subsequently the “Gymnasium” of the Canton of Zurich, which he left in the fall of 1924 after having passed the “Matura”, i.e. the final examination which entitled him to attend an institution of higher learning.  Planning originally to become an engineer, he entered directly the Federal Institute of Technology (Eidgenössische Technische Hochschule) in Zurich. After one year’s study of engineering he decided instead to study physics, and changed therefore over to the Division of Mathematics and Physics at the same institution. During the following two years he attended, among others, courses given by [Debye](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1936/index.html), Scherrer, Weyl, as well as [Schrödinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/index.html), who taught at the same time at the University of Zurich and through whom he became acquainted, toward the end of this period, with the new wave mechanics. Bloch’s interests had by that time turned toward theoretical physics. After Schrödinger left Zurich in the fall of 1927 he continued his studies with Heisenberg at the University of Leipzig, where he received his degree of Doctor of Philosophy in the summer of 1928 with a dissertation dealing with the quantum mechanics of electrons in crystals and developing the theory of metallic conduction. Various assistantships and fellowships, held in the following years, gave him the opportunity to work with [Pauli](https://www.nobelprize.org/nobel_prizes/physics/laureates/1945/index.html), Kramers, [Heisenberg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1932/index.html), [Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html), and [Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html), and to further theoretical studies of the solid state as well as of the stopping power of charged particles.  Upon Hitler’s ascent to power, Bloch left Germany in the spring of 1933, and a year later he accepted a position which was offered to him at Stanford University. The new environment in which he found himself in the United States helped toward the maturing of the wish he had had for some time to undertake also experimental research. Working with a very simple neutron source, it occurred to him that a direct proof for the magnetic moment of the free neutrons could be obtained through the observation of scattering in iron. In 1936, he published a paper in which the details of the phenomenon were worked out and in which it was pointed out that it would lead to the production and observation of polarized neutron beams. The further development of these ideas led him in 1939 to an experiment, carried out in collaboration with [L.W. Alvarez](https://www.nobelprize.org/nobel_prizes/physics/laureates/1968/index.html) at the Berkeley cyclotron, in which the magnetic moment of the neutron was determined with an accuracy of about one percent.  During the war years Dr. Bloch was also engaged in the early stages of the work on atomic energy at Stanford University and Los Alamos and later in counter-measures against radar at Harvard University. Through this latter work he became acquainted with the modern developments of electronics which, toward the end of the war, suggested to him, in conjunction with his earlier work on the magnetic moment of the neutron, a new approach toward the investigation of nuclear moments.  These investigations were begun immediately after his return to Stanford in the fall of 1945 and resulted shortly afterward in collaboration with W.W. Hansen and M.E. Packard in the new method of nuclear induction, a purely electromagnetic procedure for the study of nuclear moments in solids, liquids, or gases. A few weeks after the first successful experiments he received the news of the same discovery having been made independently and simultaneously by E.M. Purcell and his collaborators at Harvard.  Most of Bloch’s work in the subsequent years has been devoted to investigations with the use of this new method. In particular, he was able, by combining it with the essential elements of his earlier work on the magnetic moment of the neutron, to remeasure this important quantity with great accuracy in collaboration with D. Nicodemus and H.H. Staub (1948). His more recent theoretical work has dealt primarily with problems which have arisen in conjunction with experiments carried out in his laboratory.  In 1954, Bloch took a leave of absence to serve for one year as the first Director General of CERN in Geneva. After his return to Stanford University he continued his investigations on nuclear magnetism, particularly in regard to the theory of relaxation. In view of new developments, a major part of his recent work deals with the theory of superconductivity and of other phenomena at low temperatures.  In 1961, he received an endowed Chair by his appointment as Max Stein Professor of Physics at Stanford University.  Prof. Bloch married in 1940 Dr. Lore Misch, a refugee from Germany and herself a physicist. |
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| **Physics\_1999-** | |
| **ID** | 0171 |
| **Biographical** | Edward Mills Purcell was born in Taylorville, Illinois, U.S.A., on August 30, 1912. His parents, Edward A. Purcell and Mary Elizabeth Mills, were both natives of Illinois. He was educated in the public schools in Taylorville and in Mattoon, Illinois, and in 1929 entered Purdue University in Indiana. He graduated from Purdue in electrical engineering in 1933.  His interest had already turned to physics, and through the kindness of Professor K. Lark-Horovitz he was enabled, while an undergraduate, to take part in experimental research in electron diffraction. As an Exchange Student of the Institute of International Education, he spent one year at the Technische Hochschule, Karlsruhe, Germany, where he studied under Professor W. Weizel. He returned to the United States in 1934 to enter Harvard University, where he received the Ph.D. degree in 1938. After serving two years as instructor in physics at Harvard, he joined the Radiation Laboratory, Massachusetts Institute of Technology, which was organized in 1940 for military research and development of microwave radar. He became Head of the Fundamental Developments Group in the Radiation Laboratory, which was concerned with the exploration of new frequency bands and the development of new microwave techniques. This experience turned out to be very valuable. Perhaps equally influential in his subsequent scientific work was the association at this time with a number of physicists, among them [I.I. Rabi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/index.html), with a continuing interest in the study of molecular and nuclear properties by radio methods.  The discovery of nuclear magnetic resonance absorption was made just after the end of the War, and at about that time Purcell returned to Harvard as Associate Professor of Physics. He became Professor of Physics in 1949; his present title is Gerhard Gade University Professor. He has continued to work in the field of nuclear magnetism, with particular interest in relaxation phenomena, related problems of molecular structure, measurement of atomic constants, and nuclear magnetic behaviour at low temperatures. He has made some contribution to the subject of radioastronomy.  He is a Fellow of the American Physical Society, a member of the National Academy of Sciences, of the American Academy of Arts and Sciences, and of the President’s Science Advisory Committee under President Eisenhower from 1957-1960 and under President Kennedy as from 1960.  In 1937, Purcell married Beth C. Busser. They have two sons, Dennis and Frank. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0172 |
| **Biographical** | John Douglas Cockcroft was born at Todmorden, England, on May 27th, 1897. His family had for several generations been cotton manufacturers.  He was educated at Todmorden Secondary School and studied mathematics at Manchester University under Horace Lamb in 1914-1915. After serving in the First World War in the Royal Field Artillery he returned to Manchester to study electrical engineering at the College of Technology under Miles Walker. After two years apprenticeship with Metropolitan Vickers Electrical Company he went to St. John’s College, Cambridge, and took the Mathematical Tripos in 1924. He then worked under Lord Rutherford in the Cavendish Laboratory.  He first collaborated with [P. Kapitsa](https://www.nobelprize.org/nobel_prizes/physics/laureates/1978/index.html) in the production of intense magnetic fields and low temperatures. In 1928 he turned to work on the acceleration of protons by high voltages and was soon joined in this work by E.T.S. Walton. In 1932 they succeeded in transmuting lithium and boron by high energy protons. In 1933 artificial radioactivity was produced by protons and a wide variety of transmutations produced by protons and deuterons was studied. In 1934 he took charge of the Royal Society Mond Laboratory in Cambridge.  In 1929 he was elected to a Fellowship in St. John’s College and became successively University demonstrator, lecturer and in 1939 Jacksonian Professor of Natural Philosophy.  In September 1939 he took up a war-time appointment as Assistant Director of Scientific Research in the Ministry of Supply and started to work on the application of radar to coast and air defence problems. He was a member of the Tizard Mission to the United States in the autumn of 1940. After this he was appointed Head of the Air Defence Research and Development Establishment. In 1944 he went to Canada to take charge of the Canadian Atomic Energy project and became Director of the Montreal and Chalk River Laboratories until 1946 when he returned to England as Director of the Atomic Energy Research Establishment, Harwell.  For the period 1954-1959 he was scientific research member of the U.K. Atomic Energy Authority and has since continued this function on a part time basis. Election to Master, Churchill College, Cambridge, followed in October 1959. In addition he is Chancellor of the Australian National University, Canberra, and a past President of the Institute of Physics, the Physical Society (1960 to 1962) and the British Association for the Advancement of Science (1961 to 1963).  He has received honorary doctorates from some 19 universities and is a fellow or honorary member of many of the principal scientific societies. In addition, numerous honours and awards have also been bestowed upon him.  He married Eunice Elizabeth Crabtree in 1925 and has four daughters and a son. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0173 |
| **Biographical** | Ernest Thomas Sinton Walton was born at Dungarvan, County Waterford on the south coast of Ireland on October 6th, 1903, the son of a Methodist Minister from County Tipperary. The ministry demanded that his father move from place to place every few years, and he attended day schools in Banbridge (County Down) and Cookstown (County Tyrone). In 1915 he was sent as a boarder to the Methodist College, Belfast, where he excelled in mathematics and science, and in 1922 he entered Trinity College, Dublin , on a scholarship. He read the honours courses in both mathematics and experimental science, specializing in physics, and graduated in 1926 with firstclass honours in both subjects; he received his M.Sc. degree in 1927.  In 1927, he was awarded a Research Scholarship by the Royal Commissioners for the Exhibition of 1851 and he went to Cambridge University to work in the Cavendish Laboratory under [Lord Rutherford](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1908/index.html). He continued at Cambridge after receiving a senior research award of the Department of Scientific and Industrial Research in 1930, and received his Ph.D. in 1931. Walton was Clerk Maxwell Scholar from 1932 to 1934 when he returned to Trinity College, Dublin, as Fellow: he was appointed Erasmus Smith’s Professor of Natural and Experimental Philosophy in 1946, and in 1960 he was elected Senior Fellow of Trinity College.  Prof. Walton’s first researches involved theoretical and experimental studies in hydrodynamics and, at the Cavendish Laboratory, he worked on indirect methods for producing fast particles, working on the linear accelerator and on what was later to become known as the betatron. He followed this with work on the direct method of producing fast particles by the use of high voltages this work being done jointly with J.D. Cokcroft. A suitable apparatus was built which made it possible to show that various light elements could be disintegrated by bombardment with fast protons. They were directly responsible for disintegrating the nucleus of the lithium atom by bombardment with accelerated protons, and for identifying the products as helium nuclei.  Prof. Walton has taken part in many activities outside his academic work, and he has served on committees connected with the Dublin Institute for Advanced Studies, the Institute for Industrial Research and Standards, the Royal City of Dublin Hospital, the Royal Irish Academy, the Royal Dublin Society, Wesley College, Dublin, and many government and church committees. He has had numerous scientific papers published in the journals of learned societies, particularly on the subjects of hydrodynamics, nuclear physics, and microwaves.  He was awarded the Hughes Medal, jointly with Sir John Cockcroft, by the Royal Society of London in 1938, and in 1959 he received an honorary Doctor of Science degree from Queen’s University, Belfast.  E.T.S. Walton married Freda Wilson, daughter of a Methodist Minister and a former pupil of Methodist College, Belfast, in 1934. They have two sons and two daughters, Alan, Marian, Philip, and Jean. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0174 |
| **Biographical** | Cecil Frank Powell was born on December 5th, 1903, at Tonbridge, Kent, where his father, Frank Powell, was one of a family of gunsmiths who had long practised the trade in the town. His grandfather, George Bisacre, had established a private school in the nearby town of Southborough and his family ties and influences therefore tended to encourage a regard for the value both of learning and the practical arts.  He attended a local elementary school and won a scholarship, at the age of eleven, to Judd School, Tonbridge. From there he won open scholarships to Sidney Sussex College, Cambridge, where he graduated with First Class Honours in the Natural Science Tripos (1924-1925).  As a postgraduate student, Powell worked in the Cavendish Laboratory under [C.T.R. Wilson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1927/index.html) and [Lord Rutherford](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1908/index.html) until 1927 when he gained his Ph.D. and moved to the University of Bristol as Research Assistant to A.M. Tyndall in the H.H. Wills Physical Laboratory. He was eventually appointed lecturer, then reader and, in 1936, he visited the West Indies as seismologist of an expedition investigating volcanic activity. He returned to Bristol in the following year and in 1948 he was established as Melville Wills Professor of Physics.  Powell was Director of a European expedition for making high-altitude balloon flights in Sardinia (1952) and in the Po Valley (1954, 1955, and 1957).  His first researches at the Cavendish Laboratory concerned condensation phenomena and it led indirectly to an explanation of the anomalously high rate of discharge of steam through nozzles. He showed this to be due to the existence of supersaturation in the rapidly expanding steam and his results were found to have a bearing on the design and performance of the steam turbine.  At Bristol he devoted years of patient work to the development of accurate techniques for measuring the mobility of positive ions and to establishing the nature of the ions in most of the common gases. After his sojourn in the Caribbean, he returned to work on the construction of a Cockcroft generator for accelerating fast protons and deuterons – employing them in conjunction with a Wilson chamber, to study neutron-proton scattering. In 1938, he undertook experiments in cosmic radiation and employed methods of directly recording the tracks of the particles in photographic emulsions and, when the Cockcroft machine came into operation, he employed similar methods for determining the energy of neutrons, that is, by observing the tracks of the recoiling protons. The length of the track of a charged particle in the emulsion was found to give an accurate measure of its range and the great advantages of this method for experiments in nuclear physics were soon clearly established.  This development led him to a study of the scattering and disintegration processes produced by a beam of high-energy deuterons and he later returned, with the development of photographic emulsions of increased sensitivity, to experiments on cosmic radiation: in 1947 heavy mesons were discovered and many of their more important properties established.  Powell has contributed numerous papers to learned societies on the discharge of electricity in gases, and on the development of photographic methods in nuclear physics. He is a co-author of *Nuclear Physics in Photographs* (1947) and *The Study of Elementary Particles by the Photographic Method* (1959).  Prof. Powell was elected Fellow of the Royal Society in 1949: he was awarded the Hughes Medal in the same year and the Royal Medal in 1961. He has received honorary Doctor of Science degrees from the Universities of Dublin, Bordeaux and Warsaw, and he is a Foreign Member of the Academy of Sciences of the U.S.S.R. He was Vernon Boys Prizeman and Honorary Fellow of the Physical Society (1947), and he served on the Scientific Policy Committee of the European Organization for Nuclear Research (Geneva, 1961).  Powell married Isobel Therese Artner, who has assisted him in his researches, in 1932; they have two daughters. His chief recreations are squash racquets and tennis. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0175 |
| **Biographical** | Hideki Yukawa was born in Tokyo, Japan, on 23rd January, 1907, the third son of Takuji Ogawa, who later became Professor of Geology at Kyoto University. The future Laureate was brought up in Kyoto and graduated from the local university in 1929. Since that time he has been engaged on investigations in theoretical physics, particularly in the theory of elementary particles.  Between 1932 and 1939 he was a lecturer at the Kyoto University and lecturer and Assistant Professor at the Osaka University. Yukawa gained the D.Sc. degree in 1938 and from the following year he has been, and still is, Professor of Theoretical Physics at Kyoto University. While at Osaka University, in 1935, he published a paper entitled “On the Interaction of Elementary Particles. I.” (*Proc. Phys.-Math. Soc. Japan, 17, p. 48*), in which he proposed a new field theory of nuclear forces and predicted the existence of the meson. Encouraged by the discovery by American physicists of one type of meson in cosmic rays, in 1937, he devoted himself to the development of the meson theory, on the basis of his original idea. Since 1947 he has been working mainly on the general theory of elementary particles in connection with the concept of the “non-local” field.  Yukawa was invited as Visiting Professor to the Institute for Advanced Study at Princeton, U.S.A., in 1948, and since July, 1949 he has been Visiting Professor at Columbia University, New York City.  The learned societies of his native land have recognised his ability and he is a member of the Japan Academy, the Physical Society and the Science Council of Japan, and is Emeritus Professor of Osaka University. As Director of the Research Institute for Fundamental Physics in Kyoto University he has his office in the Yukawa Hall, which is named after him. He is also a Foreign Associate of the American National Academy of Sciences and a Fellow of the American Physical Society.  The Imperial Prize of the Japan Academy was awarded to Yukawa in 1940; he received the Decoration of Cultural Merit in 1943, and the crowning award, the Nobel Prize for Physics, in 1949.  A large number of scientific papers have been published by him and many books, including *Introduction to Quantum Mechanics* (1946) and *Introduction to the Theory of Elementary Particles* (1948), both in Japanese, have come from his pen. He has edited a journal in English, *Progress of Theoretical Physics*, since 1946.  An honorary doctorate of the University of Paris and honorary memberships of the Royal Society of Edinburgh, the Indian Academy of Sciences, the International Academy of Philosophy and Sciences, and the Pontificia Academia Scientiarum have marked the recognition he has earned in world scientific circles.  A civic honour was awarded to him when he was created Honorary Citizen of the City of Kyoto, Japan.  In 1932 he married, and he and his wife Sumiko have two sons, Harumi and Takaaki. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0176 |
| **Biographical** | Patrick Maynard Stuart Blackett was born on 18th November, 1897, the son of Arthur Stuart Blackett. He was originally trained as a regular officer for the Navy (Osborne Naval College, 1917; Dartmouth, 1912), and started his career as a naval cadet (1914), taking part, during the First World War, in the battles of Falkland Islands and Jutland. At the end of the war he resigned with the rank of Lieutenant, and took up studies of physics under [Lord Rutherford](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1908/index.html) at Cambridge.  After having taken his B.A. degree in 1921, he started research with cloud chambers which resulted, in 1924, in the first photographs showing the transmutation of nitrogen into an oxygen isotope. During 1924-1925 he worked at Göttingen with [James Franck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1925/index.html), after which he returned to Cambridge. In 1932, together with a young Italian scientist, G.P.S. Occhialini, he designed the counter-controlled cloud chamber, a brilliant invention by which they managed to make cosmic rays take their own photographs. By this method the cloud chamber is brought into function only when the impulses from two Geiger-Muller tubes, placed one above and one below the vertical Wilson chamber, coincide as the result of the passing of an electrically charged particle through both of them.  In the spring of 1933 they not only confirmed Anderson’s discovery of the positive electron, but also demonstrated the existence of “showers” of positive and negative electrons, both in approximately equal numbers. This fact and the knowledge that positive particles (positrons) do not normally exist as normal constituents of matter on the earth, formed the basis of their conception that gamma rays can transform into two material particles (positrons and electrons), plus a certain amount of kinetic energy – a phenomenon usually called *pair production*. The reverse process – a collision between a positron and an electron in which both are transformed into gamma radiation, so-called *annihilation radiation* – was also verified experimentally. In the interpretation of these experiments Blackett and Occhialini were guided by Dirac’s theory of the electron.  Blackett became Professor at Birkback College, London, in 1933, and there continued cosmic ray research work, hereby collecting a cosmopolitan school of research workers. In 1937 he succeeded [Sir Lawrence Bragg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1915/index.html) at Manchester University, Bragg himself having succeeded Rutherford there; his school of cosmic research work continued to develop, and since the war the Manchester laboratory has extended its field of activity, particularly into that of the radar investigation of meteor trails under Dr. Lovell.  At the start of World War II, Blackett joined the Instrument Section of the Royal Aircraft Establishment. Early in 1940, he became Scientific Advisor to Air Marshall Joubert at Coastal Command, and started the analytical study of the anti U-boat war, building up a strong operational research group. In the same year he became Director of Naval Operational Research at the Admiralty, and continued the study of the anti U-boat war and other naval operations: later in 1940 he was appointed Scientific Advisor to General Pile, C.M.C., Anti-Aircraft Command, and built up an operational research group to study scientifically the various aspects of Staff work. During the blitz he was also concerned with the employment and use of anti-aircraft defence of England.  In 1945, at the end of the Second World War, work was resumed on cosmic ray investigations in the University of Manchester: in particular on the further study of cosmic ray particles by the counter-controlled cloud chamber in a strong magnetic field, built and used before the War. In 1947, Rochester and Butler, working in the laboratory, discovered the first two of what is now known to be a large family of the so-called strange particles. They identified one charged and one uncharged particle which were intrinsically unstable and decayed with a lifetime of some 10-10 of a second into lighter particles. This result was confirmed a few years later by [Carl Anderson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1936/index.html) in Pasadena.  Soon after this discovery, the magnet and cloud chamber were moved to the Pic du Midi Observatory in the Pyrenees in order to take advantage of the greater intensity of cosmic ray particles at a very high altitude. This move was rewarded almost immediately by the discovery by Butler and coworkers, within a few hours of starting work, of a new and still stranger strange particle, which was called the negative cascade hyperon. This was a particle of more than protonic mass which decayed into a (p)-meson and another unstable hyperon, also of more than protonic mass, which itself decayed into a proton and (p)-meson.  In 1948 Blackett followed up speculations about the isotropy of cosmic rays and began speculating on the origin of the interstellar magnetic fields, and in so doing revived interest in some 30-year old speculations of Schuster and H. A. Wilson, and others, on the origin of the magnetic field of the earth and sun. Although these speculations are not now considered as likely to be valid, they led him to interest in the history of the earth’s magnetic field, and so to the newly born subject of the study of rock magnetism.  Professor Blackett was appointed Head of the Physics Department of the Imperial College of Science and Technology, London, in 1953 and retired in July, 1963. He is continuing at the Imperial College as Professor of Physics and Pro-Rector.  Over the last ten years or so a group under his direction have studied many aspects of the properties of rocks with the object of finding out the precise history of the earth’s magnetic field, in magnitude and direction back to the earliest geological times. Such results, together with those of workers in many other countries, seem to indicate that the rock magnetism data supports strongly the conclusions of Wegener and Du Toit that the continents have drifted relative to each other markedly in the course of geological history.  The study is now being continued, directed to explaining the remarkable phenomenon that about 50% of all rocks are reversely magnetized. The experiments are directed towards deciding whether this reversed magnetization is due to reversal of the earth’s magnetic field or to a complicated physical or chemical process occurring in the rocks.  Blackett was awarded the Royal Medal by the Royal Society in 1940 and the American Medal for Merit, for operational research work in connection with the U-boat campaign, in 1946. He is the author of *Military and Political Consequences of Atomic Energy* (1948; revised edition 1949; American edition *Fear, War, and the Bomb*, 1949).  In 1924 he married Constanza Bayon; they have one son and one daughter. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0177 |
| **Biographical** | Edward Victor Appleton was born in Bradford, England, on 6th September, 1892, the son of Peter and Mary Appleton. He received his early education at Hanson Grammar School. Bradford then took his B.A. degree in Natural Science at St. John’s College, Cambridge,, in 1913 and 1914, with physics for Part II. He won the Wiltshire Prize in 1913 and the Hutchinson Research Studentship in 1914, studying under [Sir J.J. Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1906/index.html) and [Lord Rutherford](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1908/index.html). During the First World War he joined the West Riding Regiment, transferring later to the Royal Engineers. At the conclusion of hostilities he returned to Cambridge and took up research on radio waves.  Since 1919 Appleton has devoted himself to scientific problems in atmospheric physics, using mainly radio techniques. In 1920 he was appointed assistant demonstrator in experimental physics at the Cavendish Laboratory. Two years later he became sub-rector at Trinity College.  In 1924 Appleton was appointed Professor of Physics at London University and served there for twelve years, returning to Cambridge in 1936 to take the Chair of Natural Philosophy.  In the latter part of 1924 Appleton began a series of experiments which proved the existence of that layer in the upper atmosphere now called the ionosphere. With the co-operation of the British Broadcasting Corporation the Bournemouth transmitter shot waves up to the layer to see if they were reflected by it and came back. The experiment was entirely successful, for the reflection was proved. Moreover, by a slight change of wavelength it was possible to measure the time taken by the waves to travel to the upper atmosphere and back. The position of the reflecting layer was thus identified and its height (60 miles above ground) determined. The method used was what is now called “frequency-modulation radar”. The ionosphere was thus the first “object” detected by radiolocation, and this led to a great development of radio research and to a military invention of the greatest importance in World War IL  Further experiments which led to the possibility of round-the-world broadcasting were carried out and in 1926 he discovered a further atmospheric layer 150 miles above ground, higher than the Heaviside Layer and electrically stronger. This layer, named the Appleton Layer after him, reflects short waves round the earth. Three years later Appleton made an expedition to Northern Norway for radio research, studying the Aurora Borealis and in 1931 he published the results of further research on determining the height of reflecting layers of the ionosphere, including the use of a transmitter that sent out “spurts” of radio energy, and the photography of the received echo-signals by cathode ray oscillography. In 1932 he was elected Vice-President of the American Institute of Radio Engineers.  When hostilities broke out in 1939 Appleton was appointed Secretary of the Department of Scientific and Industrial Research – the senior British Government post concerned with physical science.  Researches into the atmospheric layers and cathode ray oscillography were developed for aircraft detection when Sir Robert Watson-Watt and his group of scientists, working on Appleton’s findings, brought Britain’s secret weapon to perfection. Commonwealth researchers working with Appleton in Britain all became leaders in the development of radiolocation in their home countries and Sir Robert Watson-Watt has stated that, but for Appleton’s scientific work, radar would have come too late to have been of decisive use in the Battle of Britain. Appleton was knighted in 1941, being created K.C.B., and he was a member of the Scientific Advisory Committee of the War Cabinet which, in 1941, advised the Government that the manufacture of an atomic bomb was feasible. Later, under Sir John Anderson, and as technical head of the Department of Scientific and Industrial Research, he assumed administrative control of all British work on the subject. He paid a visit to the United States and Canada in 1943 to arrange details of collaboration between American and British scientists. He continued research work even during this arduous period and has demonstrated that ionospheric reflecting power varies with sunspot activities. Also, working with Dr. J.S. Hey of the Ministry of Supply, he discovered that sunspots are powerful emitters of short radio waves. An important result of Appleton’s work has been the establishment of a system of ionospheric forecasts, in which more than 40 stations all over the world co-operate, enabling the production of the most suitable wavelengths for communication over any particular radio circuit.  In 1947, the year in which he received the Nobel Prize for Physics, he was also awarded the highest civilian decoration of the United States – the Medal of Merit – and was made an Officer of the French Legion of Honour. He was also awarded the Norwegian Cross of Freedom for his war work. Appleton’s work has been recognized by India, Norway and Denmark, and in 1948 he was appointed by the Pope to the Pontificial Academy of Science. He received the Albert Medal of the Royal Society of Arts, in 1950, for outstanding services to science and industrial research and was elected President of the British Association for the Advancement of Science for the Liverpoo1 meeting in 1953. He has been Chairman of the British National Committee for Radio-Telegraphy and Honorary President of the International Scientific Radio Union. During the International Geophysical Year 1957-1958 he played an active part in the world planning of radio experiments as Chairman of the International Geophysical Year Committee of the Internationa1 Scientific Radio Union, and continues to remain a scientific research worker. He is now engaged on the interpretation of l.G.V. ionospheric measurements on a global basis.  In 1956 Sir Edward gave the Reith Lectures of the B.B.C. on “Science and the Nation”. Recent awards made to him have been the Gunning Victoria Jubilee Prize of the Royal Society, Edinburgh, in 1960, and the Medal of Honour of the Institute of Radio Engineers of America in 1962.  In 1915 Appleton married Jessie, daughter of the Rev. J. Longson, and they have two daughters. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0178 |
| **Biographical** | Percy Williams Bridgman was born in Cambridge, Massachusetts, on April 21st, 1882. He received his early education in public schools in the nearby city of Newton until 1900 when he entered Harvard University. He graduated A.B. in 1904, A.M. in 1905 and was awarded his Ph.D. (Physics) in 1908 when he joined the Faculty of the University. Bridgman was successively appointed Instructor (1910), Assistant Professor (1919), before becoming Hollis Professor of Mathematics and Natural Philosophy in 1926. He was appointed Higgins University Professor in 1950.  His researches concerning the effects of high pressures on materials and their thermodynamic behaviour commenced in 1905 and have continued throughout his career. He has carried out extensive investigations on the properties of matter at pressures up to 100,000 atmosphere including a study of the compressibility, electric and thermal conductivity, tensile strength and viscosity of more than 100 different compounds. He developed a method of packing which eliminated leak, and later introduced various methods of external support to pressure vessels as higher pressures were demanded. Bridgman has also contributed to crystallography, where he devised a method of growing single crystals; to the problems of electrical conduction in metals, where he discovered internal Peltier heat – a new electrical effect; and to the philosophy of modern physics. In the latter field, he is a strong supporter of the operational viewpoint, considering it meaningless to interpret physical concepts except as they are capable of observation.  Prof. Bridgman has contributed many papers to leading scientific journals and he is the author of *Dimensional Analysis* (1922), *The Logic of Modern Physics* (1927), *The Physics of High Pressure* (1931), *The Thermodynamics of Electrical Phenomena in Metals* (1934), *The Nature of Physical Theory* (1936), *The Intelligent Individual and Society* (1938), *The Nature of Thermodynamics* (1941), and, more recently, *Refections of a Physicist*.  He has received honorary Doctor of Science degrees from Stevens Institute (1934), Harvard (1939), Brooklyn Polytechnic (1941), Princeton (1950), Paris (1950), and Yale (1951). He has received the Rumford Medal (American Academy of Arts and Sciences), the Cresson Medal (Franklin Institute), the Roozeboom Medal (Royal Academy of Sciences of the Netherlands), the Comstock Prize (National Academy of Sciences), and the New York Award of the Research Corporation. He was a member of the American Physical Society (President, 1942), the American Association for the Advancement of Science, the American Academy of Arts and Sciences, the American Philosophical Society, and the National Academy of Sciences. He was a Foreign Member of the Royal Society and Honorary Fellow of the Physical Society (London).  Bridgman married Olive Ware in 1912. Their daughter, Jane, was born in I9I4, and their son, Robert Ware, in 1915.  Prof. Bridgman died in 1961. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0179 |
| **Biographical** | Wolfgang Pauli was born on April 25th, 1900 in Vienna. He received his early education in Vienna before studying at the University of Munich under Arnold Sommerfeld. He obtained his doctor’s degree in 1921 and spent a year at the University of Göttingen as assistant to [Max Born](https://www.nobelprize.org/nobel_prizes/physics/laureates/1954/index.html) and a further year with [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html) at Copenhagen. The years 1923-1928 were spent as a lecturer at the University of Hamburg before his appointment as Professor of Theoretical Physics at the Federal Institute of Technology in Zurich. During 1935-1936, he was visiting Professor at the Institute for Advanced Study, Princeton, New Jersey and he had similar appointments at the University of Michigan (1931 and 1941) and Purdue University (1942). He was elected to the Chair of Theoretical Physics at Princeton in 1940 but he returned to Zurich at the end of World War II.  Pauli was outstanding among the brilliant mid-twentieth century school of physicists. He was recognized as one of the leaders when, barely out of his teens and still a student, he published a masterly exposition of the theory of relativity. His exclusion principle, which is often quoted bearing his name, crystallized the existing knowledge of atomic structure at the time it was postulated and it led to the recognition of the two-valued variable required to characterize the state of an electron. Pauli was the first to recognize the existence of the neutrino, an uncharged and massless particle which carries off energy in radioactive ß-disintegration; this came at the beginning of a great decade, prior to World War II, for his centre of research in theoretical physics at Zurich.  Pauli helped to lay the foundations of the quantum theory of fields and he participated actively in the great advances made in this domain around 1945. Earlier, he had further consolidated field theory by giving proof of the relationship between spin and”statistics” of elementary particles. He has written many articles on problems of theoretical physics, mostly quantum mechanics, in scientific journals of many countries; his *Theory of Relativity* appears in the *Enzyklopaedie der Mathematischen Wissenschaften*, Volume 5, Part 2 (1920), his *Quantum Theory* in *Handbuch der Physik*, Vol. 23 (1926), and his *Principles of Wave Mechanics* in *Handbuch der Physik*, Vol. 24 (1933).  Pauli was a Foreign Member of the Royal Society of London and a member of the Swiss Physical Society, the American Physical Society and the American Association for the Advancement of Science. He was awarded the Lorentz Medal in 1930.  Wolfgang Pauli married Franciska Bertram on April 4th, 1934. He died in Zurich on December 15th, 1958. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0180 |
| **Biographical** | Isidor Isaac Rabi was born in Raymanov, Austria, on July 29, 1898, the son of David Rabi and Janet Teig. He was brought to the United States by his family, in 1899, and his early education was in New York City (Manhattan and Brooklyn). In 1919 he graduated Bachelor of Chemistry at Cornell University (New York). After three years in non-scientific occupation, he started postgraduate studies in physics at Cornell in 1921, which he later continued at Columbia University. In 1927 he received his Ph.D. degree for work on the magnetic properties of crystals. Aided by fellowships, he spent two years in Europe, working at different times with Sommerfeld, [Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html), [Pauli](https://www.nobelprize.org/nobel_prizes/physics/laureates/1945/index.html), [Stern](https://www.nobelprize.org/nobel_prizes/physics/laureates/1943/index.html), and [Heisenberg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1932/index.html). On his return in 1929 he was appointed lecturer in Theoretical Physics at Columbia University, and after promotion through the various grades became professor in 1937.  In 1940 he was granted leave from Columbia to work as Associate Director of the Radiation Laboratory at the Massachusetts Institute of Technology on the development of radar and the atomic bomb. In 1945 he returned to Columbia as executive officer of the Physics Department. In this capacity he is also concerned with the Brookhaven National Laboratory for Atomic Research, Long Island, an organization devoted to research into the peaceful uses of atomic energy.  His early work was concerned with the magnetic properties of crystals. In 1930 he began studying the magnetic properties of atomic nuclei, developing Stern’s molecular beam method to great precision, as a tool for measuring these properties. His apparatus was based on the production of ordinary electromagnetic oscillations of the same frequency as that of the Larmor precession of atomic systems in a magnetic field. By an ingenious application of the resonance principle he succeeded in detecting and measuring single states of rotation of atoms and molecules, and in determining the mechanical and magnetic moments of the nuclei.  Prof. Rabi has published his most important papers in *The Physical Review*, of which he was an Associate Editor for two periods. In 1939 he received the Prize of the American Association for the Advancement of Science and, in 1942, the Elliott Cresson Medal of the Franklin Institute. He was awarded the Medal for Merit, the highest civilian award in World War II, in 1948, the King’s Medal for Service in the Cause of Freedom the same year, and is an Officer of the Legion of Honour.  He is an honorary D. Sc. of Princeton, Harvard, and Birmingham Universities. He is a Fellow of the American Physical Society (was its President in 1950) and a member of the National Academy of Sciences, the American Philosophical Society, and of the American Academy of Arts and Sciences.  In 1959 he was appointed a member of the Board of Governors of the Weizmann Institute of Science, Rehovoth, Israel. He holds foreign memberships of the Japanese and Brazilian Academies, and is a member of the General Advisory Committee to the Arms Control and Disarmament Agency, and of the United States National Commission for UNESCO. At the International Conference on Peaceful Uses of Atomic Energy (Geneva, 1955) he was the United States delegate and Vice-President. He is also a member of the Science Advisory Committee of the International Atomic Energy Agency.  Dr. Rabi married Helen Newmark in 1926. They have two daughters. His recreations are travel, walking, and the theatre. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0181 |
| **Biographical** | Otto Stern was born in Sorau, Upper Silesia, Germany, on February 17, 1888. In 1892 he moved with his parents to Breslau, where he attended high school. He began to study physical chemistry in 1906, receiving his Ph.D. degree from the University of Breslau in 1912. In the same year he joined Einstein at the University of Prague and later followed him to the University of Zurich, where he became Privatdocent of Physical Chemistry at the Eidgenössische Technische Hochschule in 1913.  In 1914 he went to the University of Frankfurt am Main as Privatdocent of Theoretical Physics, remaining there until 1921, except for a period of military service. From 1921 to 1922 he was Associate Professor of Theoretical Physics at the University of Rostock, becoming, in 1923, Professor of Physical Chemistry and Director of the laboratory at the University of Hamburg, where he remained until 1933. In that year he moved to the United States, being appointed Research Professor of Physics at the Carnegie Institute of Technology, Pittsburgh where he remained until 1945, then becoming professor emeritus.  His earliest work was in the field of theoretical physics, especially that of statistical thermodynamics and quantum theory, on which he has published important papers. After 1919, his attention was directed more to experimental physics. His development and application of the molecular beam method proved to be a powerful tool for investigating the properties of molecules, atoms and atomic nuclei. One of the early applications of this was the experimental verification of Maxwell’s law of velocity distribution in gases. He collaborated with Gerlach to work on the deflection of atoms by the action of magnetic fields on their magnetic moment, then went on to measure the magnetic moments of sub-atomic particles, including the proton. His work on the production of interference by rays of hydrogen and helium was a striking demonstration of the wave nature of atoms and molecules.  Dr. Stern was awarded an LL.D. by the University of California, Berkeley, 1930. He is a member of the National Academy of Sciences (USA), the American Association for the Advancement of Science, and the Philosophical Society. He holds foreign membership of the Royal Danish Academy of Sciences. He lives at Berkeley, California. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0182 |
| **Biographical** | Ernest Orlando Lawrence was born on 8th August, 1901, at Canton, South Dakota (United States). His parents, Carl Gustavus and Gunda (née Jacobson) Lawrence, were the children of Norwegian immigrants, his father being a Superintendant of Schools. His early education was at Canton High School, then St. Olaf College. In 1919 he went to the University of South Dakota, receiving his B.A. in Chemistry in 1922. The following year he received his M.A. from the University of Minnesota. He spent a year at University of Chicago doing physics and was awarded his Ph.D. from Yale University in 1925. He continued at Yale for a further three years, the first two as a National Research Fellow and the third as Assistant Professor of Physics. In 1928 he was appointed Associate Professor of Physics at the University of California, Berkeley, and two years later he became Professor, being the youngest professor at Berkeley. In 1936 he became Director of the University’s Radiation Laboratory as well, remaining in these posts until his death.  During World War II he made vital contributions to the development of the atomic bomb, holding several official appointments in the project. After the war he played a part in the attempt to obtain international agreement on the suspension of atomic-bomb testing, being a member of the U.S. delegation at the 1958 Geneva Conference on this subject.  Lawrence’s research centred on nuclear physics. His early work was on ionization phenomena and the measurement of ionization potentials of metal vapours. In 1929 he invented the cyclotron, a device for accelerating nuclear particles to very high velocities without the use of high voltages. The swiftly moving particles were used to bombard atoms of various elements, disintegrating the atoms to form, in some cases, completely new elements. Hundreds of radioactive isotopes of the known elements were also discovered. His brother, Dr. John Lawrence, who became Director of the University’s Medical Physics Laboratory, collaborated with him in studying medical and biological applications of the cyclotron and himself became a consultant to the Institute of Cancer Research at Columbia.  Larger and more powerful versions of the cyclotron were built by Lawrence. In 1941 the instrument was used to generate artificially the cosmic particles called mesons, and later the studies were extended to antiparticles.  Lawrence was a most prolific writer: during 1924-1940 his name appeared on 56 papers (an average of 31/2 papers a year), showing his exceptional breadth of interest. He was also the inventor of a method for obtaining time intervals as small as three billionths of a second, to study the discharge phenomena of an electric spark. In addition he devised a very precise method for measuring the *e/m* ratio of the electron, one of the fundamental constants of Nature. Most of his work was published in *The Physical Review* and the *Proceedings of the National Academy of Sciences*.  Among his many awards may be mentioned the Elliott Cresson Medal of the Franklin Institute, the Comstock Prize of the National Academy of Sciences, the Hughes Medal of the Royal Society, the Duddell Medal of the Royal Physical Society, the Faraday Medal, and the Enrico Fermi Award. He was decorated with the Medal for Merit and was an Officer of the Legion of Honour. He held honorary doctorates of thirteen American and one British University (Glasgow). He was a member or fellow of many American and foreign learned societies.  Lawrence married Mary Kimberly Blumer, daughter of the Emeritus Dean at Yale Medical School, in May 1932. They had six children. His recreations were boating, tennis, ice-skating, and music. He died on 27th August, 1958, at Palo Alto, California. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0183 |
| **Biographical** | Enrico Fermi was born in Rome on 29th September, 1901, the son of Alberto Fermi, a Chief Inspector of the Ministry of Communications, and Ida de Gattis. He attended a local grammar school, and his early aptitude for mathematics and physics was recognized and encouraged by his father’s colleagues, among them A. Amidei. In 1918, he won a fellowship of the Scuola Normale Superiore of Pisa. He spent four years at the University of Pisa, gaining his doctor’s degree in physics in 1922, with Professor Puccianti.  Soon afterwards, in 1923, he was awarded a scholarship from the Italian Government and spent some months with [Professor Max Born](https://www.nobelprize.org/nobel_prizes/physics/laureates/1954/index.html) in Göttingen. With a Rockefeller Fellowship, in 1924, he moved to Leyden to work with P. Ehrenfest, and later that same year he returned to Italy to occupy for two years (1924-1926) the post of Lecturer in Mathematical Physics and Mechanics at the University of Florence.  In 1926, Fermi discovered the statistical laws, nowadays known as the «Fermi statistics», governing the particles subject to Pauli’s exclusion principle (now referred to as «fermions», in contrast with «bosons» which obey the Bose-Einstein statistics).  In 1927, Fermi was elected Professor of Theoretical Physics at the University of Rome (a post which he retained until 1938, when he – immediately after the receipt of the Nobel Prize – emigrated to America, primarily to escape Mussolini’s fascist dictatorship).  During the early years of his career in Rome he occupied himself with electrodynamic problems and with theoretical investigations on various spectroscopic phenomena. But a capital turning-point came when he directed his attention from the outer electrons towards the atomic nucleus itself. In 1934, he evolved the ß-decay theory, coalescing previous work on radiation theory with Pauli’s idea of the neutrino. Following the discovery by Curie and Joliot of artificial radioactivity (1934), he demonstrated that nuclear transformation occurs in almost every element subjected to neutron bombardment. This work resulted in the discovery of slow neutrons that same year, leading to the discovery of nuclear fission and the production of elements lying beyond what was until then the Periodic Table.  In 1938, Fermi was without doubt the greatest expert on neutrons, and he continued his work on this topic on his arrival in the United States, where he was soon appointed Professor of Physics at Columbia University, N.Y. (1939-1942).  Upon the discovery of fission, by Hahn and Strassmann early in 1939, he immediately saw the possibility of emission of secondary neutrons and of a chain reaction. He proceeded to work with tremendous enthusiasm, and directed a classical series of experiments which ultimately led to the atomic pile and the first controlled nuclear chain reaction. This took place in Chicago on December 2, 1942 – on a squash court situated beneath Chicago’s stadium. He subsequently played an important part in solving the problems connected with the development of the first atomic bomb (He was one of the leaders of the team of physicists on the Manhattan Project for the development of nuclear energy and the atomic bomb.)  In 1944, Fermi became an American citizen, and at the end of the war (1946) he accepted a professorship at the Institute for Nuclear Studies of the University of Chicago, a position which he held until his untimely death in 1954. There he turned his attention to high-energy physics, and led investigations into the pion-nucleon interaction.  During the last years of his life Fermi occupied himself with the problem of the mysterious origin of cosmic rays, thereby developing a theory, according to which a universal magnetic field – acting as a giant accelerator – would account for the fantastic energies present in the cosmic ray particles.  Professor Fermi was the author of numerous papers both in theoretical and experimental physics. His most important contributions were:  “Sulla quantizzazione del gas perfetto monoatomico”, *Rend. Accad. Naz. Lincei,* 1935 (also in *Z. Phys.,* 1936), concerning the foundations of the statistics of the electronic gas and of the gases made of particles that obey the Pauli Principle.  Several papers published in *Rend. Accad. Naz. Lincei,* 1927-28, deal with the statistical model of the atom (Thomas-Fermi atom model) and give a semiquantitative method for the calculation of atomic properties. A resumé of this work was published by Fermi in the volume: *Quantentheorie und Chemie,* edited by H. Falkenhagen, Leipzig, 1928.  “Uber die magnetischen Momente der AtomKerne”, *Z. Phys.,* 1930, is a quantitative theory of the hyperfine structures of spectrum lines. The magnetic moments of some nuclei are deduced therefrom.  “Tentativo di una teoria dei raggi ß”, *Ricerca Scientifica,* 1933 (also *Z. Phys.,* 1934) proposes a theory of the emission of ß-rays, based on the hypothesis, first proposed by Pauli, of the existence of the neutrino.  The Nobel Prize for Physics was awarded to Fermi for his work on the artificial radioactivity produced by neutrons, and for nuclear reactions brought about by slow neutrons. The first paper on this subject “Radioattività indotta dal bombardamento di neutroni” was published by him in *Ricerca Scientifica,* 1934. All the work is collected in the following papers by himself and various collaborators: “Artificial radioactivity produced by neutron bombardment”, *Proc. Roy. Soc.,* 1934 and 1935; “On the absorption and diffusion of slow neutrons”, *Phys. Rev.,* 1936. The theoretical problems connected with the neutron are discussed by Fermi in the paper “Sul moto dei neutroni lenti”, *Ricerca Scientifica,* 1936.  His *Collected Papers* are being published by a Committee under the Chairmanship of his friend and former pupil, [Professor E. Segrè](https://www.nobelprize.org/nobel_prizes/physics/laureates/1959/index.html) (Nobel Prize winner 1959, with O. Chamberlain, for the discovery of the antiproton).  Fermi was member of several academies and learned societies in Italy and abroad (he was early in his career, in 1929, chosen among the first 30 members of the Royal Academy of Italy).  As lecturer he was always in great demand (he has also given several courses at the University of Michigan, Ann Arbor; and Stanford University, Calif.). He was the first recipient of a special award of $50,000 – which now bears his name – for work on the atom.  Professor Fermi married Laura Capon in 1928. They had one son Giulio and one daughter Nella. His favourite pastimes were walking, mountaineering, and winter sports.  He died in Chicago on 28th November, 1954. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0184 |
| **Biographical** | Clinton Joseph Davisson was born at Bloomington, Illinois, U.S.A., October 22, 1881, son of Joseph Davisson, an artisan, native of Ohio, descendant of early Dutch and French settlers of Virginia, Union veteran of the American Civil War, and Mary Calvert, a school-teacher, native of Pennsylvania, of English and Scotch parentage.  He attended the Bloomington public schools, and on graduation from High School in 1902 was granted a scholarschip by the University of Chicago for proficiency in mathematics and physics. In September of that year he entered the University of Chicago and came at once under the influence of Professor R.A. Millikan. Unable for financial reasons to continue at Chicago the following year he found employment with a telephone company in his home town. In January 1904 he was appointed assistant in physics at Purdue University on recommendation of [Professor Millikan](https://www.nobelprize.org/nobel_prizes/physics/laureates/1923/index.html). He returned to Chicago in June 1904 and remained in residence at the University until August 1905. In September 1905, again on the recommendation of Professor Millikan, he was appointed part-time instructor in physics at Princeton University. This post he held until 1910, studying, as his duties permitted, under Professor Francis Magie, Professor E. P. Adams, Professor ( later Sir ) James Jeans and particularly under [Professor O.W. Richardson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1928/index.html). During a part of this period Davisson returned to the University of Chicago for the summer sessions and in August 1908 received a B.S. degree from that institution.  He was awarded a Fellowship in Physics at Princeton for the year 1910-1911 and during that year completed requirements for the degree of Ph.D. which he received dune 1911. His thesis, under Professor Richardson, was *On The Thermal Emission of Positive Ions From Alkaline Earth Salts*.  From September 1911 until June 1917 he was an instructor in the Department of Physics at the Carnegie Institute of Technology, Pittsburgh, Pa. During the summer of 1913 he worked in the Cavendish Laboratory under [Professor (later Sir) J.J. Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1906/index.html).  In April 1917 he was refused enlistment in the United States Army. In June of the same year he accepted war-time employment in the Engineering Department of the Western Electric Company (later Bell Telephone Laboratories), New York City – at first for summer, then, on leave of absence from Carnegie Tech., for the duration of the World War. At the end of the war he resigned an assistant professorship to which he had been appointed at Carnegie Tech. to continue as a Member of the Technical Staff of the Telephone Laboratories.  The series of investigations which led to the discovery of electron diffraction in 1927 was begun in 1919 and was continued into 1929 with the collaboration first of Dr. C.H. Kunsman, and from 1924 on, of Dr. L.H. Germer. During the same period researches were carried on in thermal radiation with the collaboration of Mr. J.R. Weeks, and in thermionics with Dr. H.A. Pidgeon and Dr. Germer.  From 1930-1937 Dr. Davisson devoted himself to the study of the theory of electron optics and to applications of this theory to engineering problems. He then investigated the scattering and reflection of very slow electrons by metals. During World War II he worked on the theory of electronic devices and on a variety of crystal physics problems.  In 1946 he retired from Bell Telephone Laboratories after 29 years of service. From 1947 to 1949, he was Visiting Professor of Physics at the University of Virginia, Charlottesville, Va.  In 1928 he was awarded the Comstock Prize by the National Academy of Sciences, in 1931 the Elliott Cresson Medal by the Franklin Institute, and in 1935 the Hughes Medal by the Royal Society (London), and in 1941 the Alumni Medal by the University of Chicago. He held honorary doctorates from Purdue University, Princeton University, the University of Lyon and Colby College.  In 1911 he married Charlotte Sara Richardson, a sister of Professor Richardson. He died in Charlottesville on February 1, 1958, at the age of 76, and was survived by his wife, three sons and one daughter. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0185 |
| **Biographical** | George Paget Thomson was born in 1892 at Cambridge, the son of the late [Sir J J. Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1906/index.html) (then Professor of Physics at Cambridge University), a Nobel Prize winner who, more than anyone else, was responsible for the discovery of the electron, and Rose Elisabeth Paget, daughter of the late Sir George Paget, Regius Professor of Medicine at Cambridge.  George Thomson went to school in Cambridge, and then up to the University. As an undergraduate at Trinity College he took mathematics followed by physics, and had done a year’s research under his father when the 1914-1918 war broke out.  He joined the Queen’s Regiment of Infantry as a Subaltern and served for a short time in France, but returned to work on the stability of aeroplanes and other aerodynamical problems at Farnborough, and continued to work on this kind of problem at various establishments throughout the war, apart from eight months in the United States attached to the British War Mission.  After the war he spent three years as Fellow and Lecturer at Corpus Christi College, Cambridge, and continued his research on physics. He was then appointed Professor of Natural Philosophy (as physics is called in Scotland) at the University of Aberdeen, a post he held for eight years. At Aberdeen he carried out experiments on the behaviour of electrons going through very thin films of metals, which showed that electrons behave as waves in spite of being particles. For this work he later shared the Nobel Prize in Physics with C.J. Davisson of the Bell Telephone Laboratories, who had arrived at the same conclusions by a different kind of experiment. The process of electron diffraction which these experiments established to be possible has been widely used in the investigation of the surfaces of solids.  In the winter of 1929-1930 Thomson visited Cornell University, Ithaca, N.Y. as a “non-resident” lecturer. In 1930 he was appointed Professor at Imperial College in the University of London; he held this post until 1952, when he became Master of Corpus Christi College, Cambridge, retiring from the latter in 1962.  During his time at Imperial College he became interested in nuclear physics, and when the fission of uranium by neutron was discovered at the beginning of 1939 he was struck by its military and other possibilities, and persuaded the British Air Ministry to procure a ton of uranium oxide for experiments. These experiments were incomplete at the outbreak of war, when Thomson went back to the Royal Aircraft Establishment to work on a series of war problems, including magnetic mines. A year later he was made Chairman of the British Committee set up to investigate the possibilities of atomic bombs. This committee reported in 1941 that a bomb was possible, and Thomson was authorized to give this report to the American scientists Vannevar Bush and James Conant.  He spent the next year as Scientific Liaison Officer at Ottawa, and for part of this time was in close touch with the American atomic bomb effort. On returning to England he was appointed Vice-Chairman of the Radio Board and later became Scientific Adviser to the Air Ministry.  After the war he returned to work at Imperial College, and early in 1946 became interested in the possibilities of nuclear power from deuterium (heavy hydrogen). Some experiments bearing on this were started at Imperial College under Dr. Ware, but Thomson’s work was theoretical. Later, because of the requirements of secrecy, this work was transferred to the Associated Electrical Industry’s Research Laboratories at Aldermaston, where Thomson continued to act as Consultant.  Sir George T. is a Fellow of the Royal Society, and has received the Royal Medal and the Hughes Medal of that Society. He is a Doctor of Science at Cambridge, Hon. D.Sc. (Lisbon), Hon. LL.D. (Aberdeen), Hon. Sc. D. ( Dublin ), Sheffield, University of Wales and Reading. He has written a book on aerodynamics and other scientific works. His published works also include a popular book on *The Atom and The Foreseeable Future*, published in 1955, and *The Inspiration of Science*, published in 1962. He is a Foreign Member of the American Academy of Arts and Sciences and of the Lisbon Academy, and a Corresponding Member of the Austrian Academy.  In 1924 he married Kathleen Buchanan, daughter of the Very Rev. Sir George Adam Smith. They have two sons and two daughters. Ship models form part of his recreations. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0186 |
| **Biographical** | Victor Franz Hess was born on the 24th of June, 1883, in Waldstein Castle, near Peggau in Steiermark, Austria. His father, Vinzens Hess, was a forester in Prince Öttingen-Wallerstein’s service and his mother was Serafine Edle von Grossbauer-Waldstätt.  He received his entire education in Graz: Gymnasium (1893-1901), and afterwards Graz University (1901-1905), where he took his doctor’s degree in 1910.  He worked, for a short time, at the Physical Institute in Vienna, where Professor von Schweidler initiated him in recent discoveries in the field of radioactivity. During 1910-1920 he was Assistant under Stephan Meyer at the Institute of Radium Research of the Viennese Academy of Sciences. In 1919 he received the Lieben Prize for his discovery of the”ultra-radiation” (cosmic radiation), and the year after became Extraordinary Professor of Experimental Physics at the Graz University.  From 1921 to 1923, Hess was granted leave of absence, and worked in the United States, where he took a post as Director of the Research Laboratory (created by him) of the U.S. Radium Corporation, at Orange (New Jersey), and as Consulting Physicist for the U.S. Department of the Interior (Bureau of Mines), Washington D.C.  In 1923 he returned to Graz University and in 1925 he was appointed Ordinary Professor of Experimental Physics. In 1931 came his appointment as Professor at Innsbruck University and Director of the newly established Institute of Radiology. He founded the station at the Hafelekar mountain (2,300 m) near Innsbruck for observing and studying cosmic rays.  As well as the Nobel Prize for 1936, which he shared with C.D. Anderson, Hess has been awarded the Abbe Memorial Prize and the Abbe Medal of the Carl Zeiss Institute in Jena (1932); he was also Corresponding Member of the Academy of Sciences in Vienna.  Hess’s work which gained him the Nobel Prize, was carried out during the years 1911-1913, and published in the Proceedings of the Viennese Academy of Sciences. In addition he has published some sixty papers and several books, of which the most important were: “Die Wärmeproduktion des Radiums” (The heat production of radium), 1912; “Konvektionserscheinungen in ionisierten Gasen-Ionenwind” (Convection phenomena in ionized gas-ionwinds), 1919-1920; “The measurement of gamma rays”, 1916 (with R.W. Lawson); “The counting of alpha particles emitted from radium”, 1918 (also with R. W. Lawson); *Elektrische Leitfähigkeit der Atmosphäre und ihre Ursachen* (book), 1926 (in English: *The Electrical Conductivity of the Atmosphere and Its Causes*, 1928); *Ionenbilanz der Atmosphäre* (The ionization balance of the atmosphere – book), 1933; *Luftelektrizität* (Electricity of the air – book, with H. Benndorf), 1928; “Lebensdauer der Ionen in der Atmosphäre” (Average life of the ions in the atmosphere), 1927-1928; “Schwankungen der Intensität in den kosmischen Strahlen” (Intensity fluctuations in cosmic rays), 1929-1936.  Hess has been American citizen since 1944, and is living in New York. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0187 |
| **Biographical** | Carl David Anderson, who was born of Swedish parents – his father was Carl David Anderson and his mother Emma Adolfina Ajaxson – in New York City (USA) on 3rd September, 1905, has spent the bulk of his life in the United States. He graduated from the California Institute of Technology in 1927 with a B.Sc. degree in Physics and Engineering, and was awarded his Ph.D. degree by the same Institute, in 1930. For the period 1930-1933 he was Research Fellow there, subsequently (1933) Assistant Professor of Physics, and Professor of Physics (1939) During the war years (1941-1945) he was also active on projects for the National Defence Research Committee and the Office of Scientific Research and Development.  His early researches were in the field of X-rays. For his doctoral thesis he studied the space distribution of photoelectrons ejected from various gases by X-rays. In 1930, with [Professor Millikan](https://www.nobelprize.org/nobel_prizes/physics/laureates/1923/index.html), he began his cosmic-ray studies which led in 1932 to the discovery of the positron. He has studied the energy distribution of cosmic-ray particles and the energy loss of very high speed electrons in traversing matter. In 1933 he and Dr. Neddermeyer obtained the first direct proof that gamma rays from ThC” generate positrons in their passage through material substances. Since 1933 he has continued his work on radiation and fundamental particles. Most of Anderson’s researches and discoveries have been published in *The Physical Review and Science*.  Among the scientific honours bestowed upon him, in addition to the Nobel Prize, may be mentioned the following: Gold Medal of the American Institute of City of New York (1935); Sc.D. of Colgate University (1937); Elliott Cresson Medal of the Franklin Institute (1937); Presidential Certificate of Merit (1945); LL.D. Temple University (1949); John Ericsson Medal of the American Society of Swedish Engineers (1960).  In 1946 Anderson married Lorraine Bergman; they have two sons, Marshall and David. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0188 |
| **Biographical** | James Chadwick was born in Cheshire, England, on 20th October, 1891, the son of John Joseph Chadwick and Anne Mary Knowles. He attended Manchester High School prior to entering Manchester University in 1908; he graduated from the Honours School of Physics in 1911 and spent the next two years under [Professor (later Lord) Rutherford](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1908/index.html) in the Physical Laboratory in Manchester, where he worked on various radioactivity problems, gaining his M.Sc. degree in 1913. That same year he was awarded the 1851 Exhibition Scholarship and proceeded to Berlin to work in the Physikalisch Technische Reichsanstalt at Charlottenburg under Professor H. Geiger.  During World War I, he was interned in the Zivilgefangenenlager, Ruhleben. After the war, in 1919, he returned to England to accept the Wollaston Studentship at Gonville and Caius College, Cambridge, and to resume work under Rutherford, who in the meantime had moved to the Cavendish Laboratory, Cambridge. Rutherford had succeeded that year in disintegrating atoms by bombarding nitrogen with alpha particles, with the emission of a proton. This was the first artificial nuclear transformation. In Cambridge, Chadwick joined Rutherford in accomplishing the transmutation of other light elements by bombardment with alpha particles, and in making studies of the properties and structure of atomic nuclei.  He was elected Fellow of Gonville and Caius College (1921-1935) and became Assistant Director of Research in the Cavendish Laboratory (1923). In 1927 he was elected a Fellow of the Royal Society.  In 1932, Chadwick made a fundamental discovery in the domain of nuclear science: he proved the existence of *neutrons* – elementary particles devoid of any electrical charge. In contrast with the helium nuclei (alpha rays) which are charged, and therefore repelled by the considerable electrical forces present in the nuclei of heavy atoms, this new tool in atomic disintegration need not overcome any electric barrier and is capable of penetrating and splitting the nuclei of even the heaviest elements. Chadwick in this way prepared the way towards the fission of uranium 235 and towards the creation of the atomic bomb. For this epoch-making discovery he was awarded the Hughes Medal of the Royal Society in 1932, and subsequently the Nobel Prize for Physics in 1935.  He remained at Cambridge until 1935 when he was elected to the Lyon Jones Chair of Physics in the University of Liverpool. From 1943 to 1946 he worked in the United States as Head of the British Mission attached to the Manhattan Project for the development of the atomic bomb. He returned to England and, in 1948, retired from active physics and his position at Liverpool on his election as Master of Gonville and Caius College, Cambridge. He retired from this Mastership in 1959. From 1957 to 1962 he was a parttime member of the United Kingdom Atomic Energy Authority.  Chadwick has had many papers published on the topic of radioactivity and connected problems and, with Lord Rutherford and C. D. Ellis, he is co-author of the book *Radiations from Radioactive substances* (1930).  Sir James was knighted in 1945. Apart from the Hughes Medal (Royal Society) mentioned above, he received the Copley Medal (1950) and the Franklin Medal of the Franklin Institute, Philadelphia (1951). He is an Honorary Fellow of the Institute of Physics and, in addition to receiving honorary doctorate degrees from the Universities of Reading, Dublin, Leeds, Oxford, Birmingham, Montreal (McGill), Liverpool, and Edinburgh, he is a member of several foreign academies, being Associé of the Académie Royale de Belgique; Foreign Member of the Kongelige Danske Videnskabernes Selskab and the Koninklijke Nederlandse Akademie van Wetenschappen; Corresponding Member of the Sächsische Akademie der Wissenschaften, Leipzig; Member of the Pontificia Academia Scientiarum and the Franklin Institute; Honorary Member of the American Philosophical Society and the American Physical Society.  In 1925, he married Aileen Stewart-Brown of Liverpool. They have twin daughters, and live at Denbigh, North Wales. His hobbies include gardening and fishing. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0189 |
| **Biographical** | Erwin Schrödinger was born on August 12, 1887, in Vienna, the only child of Rudolf Schrödinger, who was married to a daughter of Alexander Bauer, his Professor of Chemistry at the Technical College of Vienna.  Erwin’s father came from a Bavarian family which generations before had settled in Vienna. He was a highly gifted man with a broad education. After having finished his chemistry studies, he devoted himself for years to Italian painting. After this he took up botany, which resulted in a series of papers on plant phylogeny.  Schrödinger’s wide interests dated from his school years at the Gymnasium, where he not only had a liking for the scientific disciplines, but also appreciated the severe logic of ancient grammar and the beauty of German poetry. (What he abhorred was memorizing of data and learning from books.)  From 1906 to 1910 he was a student at the University of Vienna, during which time he came under the strong influence of Fritz Hasenöhrl, who was Boltzmann’s successor. It was in these years that Schrödinger acquired a mastery of eigenvalue problems in the physics of continuous media, thus laying the foundation for his future great work. Hereafter, as assistant to Franz Exner, he, together with his friend K. W. F. Kohlrausch, conducted practical work for students (without himself, as he said, learning what experimenting was). During the First World War he served as an artillery officer.  In 1920 he took up an academic position as assistant to [Max Wien](https://www.nobelprize.org/nobel_prizes/physics/laureates/1911/index.html), followed by positions at Stuttgart (extraordinary professor), Breslau (ordinary professor), and at the University of Zurich (replacing [von Laue](https://www.nobelprize.org/nobel_prizes/physics/laureates/1914/index.html)) where he settled for six years. In later years Schrödinger looked back to his Zurich period with great pleasure – it was here that he enjoyed so much the contact and friendship of many of his colleagues, among whom were Hermann Weyl and [Peter Debye](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1936/index.html). It was also his most fruitful period, being actively engaged in a variety of subjects of theoretical physics. His papers at that time dealt with specific heats of solids, with problems of thermodynamics (he was greatly interested in Boltzmann’s probability theory) and of atomic spectra; in addition, he indulged in physiological studies of colour (as a result of his contacts with Kohlrausch and Exner, and of Helmholtz’s lectures). His great discovery, Schrödinger’s wave equation, was made at the end of this epoch-during the first half of 1926.  It came as a result of his dissatisfaction with the quantum condition in Bohr’s orbit theory and his belief that atomic spectra should really be determined by some kind of eigenvalue problem. For this work he shared with Dirac the Nobel Prize for 1933.  In 1927 Schrödinger moved to Berlin as Planck’s successor. Germany’s capital was then a centre of great scientific activity and he enthusiastically took part in the weekly colloquies among colleagues, many of whom “exceeding him in age and reputation”. With Hitler’s coming to power (1933), however, Schrödinger decided he could not continue in Germany. He came to England and for a while held a fellowship at Oxford. In 1934 he was invited to lecture at Princeton University and was offered a permanent position there, but did not accept. In 1936 he was offered a position at University of Graz, which he accepted only after much deliberation and because his longing for his native country outweighed his caution. With the annexation of Austria in 1938, he was immediately in difficulty because his leaving Germany in 1933 was taken to be an unfriendly act. Soon afterwards he managed to escape to Italy, from where he proceeded to Oxford and then to University of Ghent. After a short stay he moved to the newly created Institute for Advanced Studies in Dublin, where he became Director of the School for Theoretical Physics. He remained in Dublin until his retirement in 1955.  All this time Schrödinger continued his research and published many papers on a variety of topics, including the problem of unifying gravitation and electromagnetism, which also absorbed [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/index.html) and which is still unsolved; (he was also the author of the well-known little book “What is Life?”, 1944). He remained greatly interested in the foundations of atomic physics. Schrödinger disliked the generally accepted dual description in terms of waves and particles, with a statistical interpretation for the waves, and tried to set up a theory in terms of waves only. This led him into controversy with other leading physicists.  After his retirement he returned to an honoured position in Vienna. He died on the 4th of January, 1961, after a long illness, survived by his faithful companion, Annemarie Bertel, whom he married in 1920. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0190 |
| **Biographical** | Paul Adrien Maurice Dirac was born on 8th August, 1902, at Bristol, England, his father being Swiss and his mother English. He was educated at the Merchant Venturer’s Secondary School, Bristol, then went on to Bristol University. Here, he studied electrical engineering, obtaining the B.Sc. (Engineering) degree in 1921. He then studied mathematics for two years at Bristol University, later going on to St. John’s College, Cambridge, as a research student in mathematics. He received his Ph.D. degree in 1926. The following year he became a Fellow of St.John’s College and, in 1932, Lucasian Professor of Mathematics at Cambridge.  Dirac’s work has been concerned with the mathematical and theoretical aspects of quantum mechanics. He began work on the new quantum mechanics as soon as it was introduced by [Heisenberg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1932/index.html) in 1925 – independently producing a mathematical equivalent which consisted essentially of a noncommutative algebra for calculating atomic properties – and wrote a series of papers on the subject, published mainly in the Proceedings of the Royal Society, leading up to his relativistic *theory of the electron* (1928) and the theory of holes (1930). This latter theory required the existence of a positive particle having the same mass and charge as the known (negative) electron. This, the positron was discovered experimentally at a later date (1932) by [C. D. Anderson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1936/index.html), while its existence was likewise proved by [Blackett](https://www.nobelprize.org/nobel_prizes/physics/laureates/1948/index.html) and Occhialini (1933 ) in the phenomena of “pair production” and “annihilation”.  The importance of Dirac’s work lies essentially in his famous wave equation, which introduced special relativity into Schrödinger’s equation. Taking into account the fact that, mathematically speaking, relativity theory and quantum theory are not only distinct from each other, but also oppose each other, Dirac’s work could be considered a fruitful reconciliation between the two theories.  Dirac’s publications include the books *Quantum Theory of the Electron* (1928) and *The Principles of Quantum Mechanics* (1930; 3rd ed. 1947).  He was elected a Fellow of the Royal Society in 1930, being awarded the Society’s Royal Medal and the Copley Medal. He was elected a member of the Pontifical Academy of Sciences in 1961.  Dirac has travelled extensively and studied at various foreign universities, including Copenhagen, Göttingen, Leyden, Wisconsin, Michigan, and Princeton (in 1934, as Visiting Professor). In 1929,after having spent five months in America, he went round the world, visiting Japan together with Heisenberg, and then returned across Siberia.  In 1937 he married Margit Wigner, of Budapest. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0191 |
| **Biographical** | Werner Heisenberg was born on 5th December, 1901, at Würzburg. He was the son of Dr. August Heisenberg and his wife Annie Wecklein. His father later became Professor of the Middle and Modern Greek languages in the University of Munich. It was probably due to his influence that Heisenberg remarked, when the Japanese physicist Yukawa discovered the particle now known as the meson and the term “mesotron” was proposed for it, that the Greek word “mesos” has no “tr” in it, with the result that the name “mesotron” was changed to “meson”.  Heisenberg went to the Maximilian school at Munich until 1920, when he went to the University of Munich to study physics under Sommerfeld, Wien, Pringsheim, and Rosenthal. During the winter of 1922-1923 he went to Göttingen to study physics under [Max Born](https://www.nobelprize.org/nobel_prizes/physics/laureates/1954/index.html), [Franck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1925/index.html), and Hilbert. In 1923 he took his Ph.D. at the University of Munich and then became Assistant to Max Born at the University of Göttingen, and in 1924 he gained the *venia legendi* at that University.  From 1924 until 1925 he worked, with a Rockefeller Grant, with [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html), at the University of Copenhagen, returning for the summer of 1925 to Göttingen.  In 1926 he was appointed Lecturer in Theoretical Physics at the University of Copenhagen under [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html) and in 1927, when he was only 26, he was appointed Professor of Theoretical Physics at the University of Leipzig.  In 1929 he went on a lecture tour to the United States, Japan, and India.  In 1941 he was appointed Professor of Physics at the University of Berlin and Director of the Kaiser Wilhelm Institute for Physics there.  At the end of the Second World War he, and other German physicists, were taken prisoner by American troops and sent to England, but in 1946 he returned to Germany and reorganized, with his colleagues, the Institute for Physics at Göttingen. This Institute was, in 1948, renamed the Max Planck Institute for Physics.  In 1948 Heisenberg stayed for some months in Cambridge, England, to give lectures, and in 1950 and 1954 he was invited to lecture in the United States. In the winter of 1955-1956 he gave the Gifford Lectures at the University of St. Andrews, Scotland, these lectures being subsequently published as a book.  During 1955 Heisenberg was occupied with preparations for the removal of the Max Planck Institute for Physics to Munich. Still Director of this Institute, he went with it to Munich and in 1958 he was appointed Professor of Physics in the University of Munich. His Institute was then being renamed the Max Planck Institute for Physics and Astrophysics.  Heisenberg’s name will always be associated with his theory of quantum mechanics, published in 1925, when he was only 23 years old. For this theory and the applications of it which resulted especially in the discovery of allotropic forms of hydrogen, Heisenberg was awarded the Nobel Prize for Physics for 1932.  His new theory was based only on what can be observed, that is to say, on the radiation emitted by the atom. We cannot, he said, always assign to an electron a position in space at a given time, nor follow it in its orbit, so that we cannot assume that the planetary orbits postulated by Niels Bohr actually exist. Mechanical quantities, such as position, velocity, etc. should be represented, not by ordinary numbers, but by abstract mathematical structures called “matrices” and he formulated his new theory in terms of matrix equations.  Later Heisenberg stated his famous *principle of uncertainty*, which lays it down that the determination of the position and momentum of a mobile particle necessarily contains errors the product of which cannot be less than the quantum constant *h* and that, although these errors are negligible on the human scale, they cannot be ignored in studies of the atom.  From 1957 onwards Heisenberg was interested in work on problems of plasma physics and thermonuclear processes, and also much work in close collaboration with the International Institute of Atomic Physics at Geneva. He was for several years Chairman of the Scientific Policy Committee of this Institute and subsequently remained a member of this Committee.  When he became, in 1953, President of the Alexander von Humboldt Foundation, he did much to further the policy of this Foundation, which was to invite scientists from other countries to Germany and to help them to work there.  Since 1953 his own theoretical work was concentrated on the unified field theory of elementary particles which seems to him to be the key to an understanding of the physics of elementary particles.  Apart from many medals and prizes, Heisenberg received an honorary doctorate of the University of Bruxelles, of the Technological University Karlsruhe, and recently (1964) of the University of Budapest; he is also recipient of the Order of Merit of Bavaria, and the Grand Cross for Federal Services with Star (Germany). He is a Fellow of the Royal Society of London and a Knight of the Order of Merit (Peace Class). He is a member of the Academies of Sciences of Göttingen, Bavaria, Saxony, Prussia, [Sweden](http://www.kva.se/), Rumania, Norway, Spain, The Netherlands, Rome (Pontificial), the German Akademie der Naturforscher Leopoldina (Halle), the Accademia dei Lincei (Rome), and the American Academy of Sciences. During 1949-1951 he was President of the Deutsche Forschungsrat (German Research Council) and in 1953 he became President of the Alexander von Humboldt Foundation.  One of his hobbies is classical music: he is a distinguished pianist. In 1937 Heisenberg married Elisabeth Schumacher. They have seven children, and live in Munich. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0192 |
| **Biographical** | Chandrasekhara Venkata Raman was born at Tiruchirappalli in Southern India on November 7th, 1888. His father was a lecturer in mathematics and physics so that from the first he was immersed in an academic atmosphere. He entered Presidency College, Madras, in 1902, and in 1904 passed his B.A. examination, winning the first place and the gold medal in physics; in 1907 he gained his M.A. degree, obtaining the highest distinctions.  His earliest researches in optics and acoustics – the two fields of investigation to which he has dedicated his entire career – were carried out while he was a student.  Since at that time a scientific career did not appear to present the best possibilities, Raman joined the Indian Finance Department in 1907; though the duties of his office took most of his time, Raman found opportunities for carrying on experimental research in the laboratory of the Indian Association for the Cultivation of Science at Calcutta (of which he became Honorary Secretary in 1919).  In 1917 he was offered the newly endowed Palit Chair of Physics at Calcutta University, and decided to accept it. After 15 years at Calcutta he became Professor at the Indian Institute of Science at Bangalore (1933-1948), and since 1948 he is Director of the Raman Institute of Research at Bangalore, established and endowed by himself. He also founded the *Indian Journal of Physics* in 1926, of which he is the Editor. Raman sponsored the establishment of the Indian Academy of Sciences and has served as President since its inception. He also initiated the *Proceedings* of that academy, in which much of his work has been published, and is President of the Current Science Association, Bangalore, which publishes *Current Science (India)*.  Some of Raman’s early memoirs appeared as Bulletins of the *Indian Associationfor the Cultivation of Science* (Bull. 6 and 11, dealing with the “Maintenance of Vibrations”; Bull. 15, 1918, dealing with the theory of the musical instruments of the violin family). He contributed an article on the theory of musical instruments to the 8th Volume of the *Handbuch der Physik*, 1928. In 1922 he published his work on the “Molecular Diffraction of Light”, the first of a series of investigations with his collaborators which ultimately led to his discovery, on the 28th of February, 1928, of the radiation effect which bears his name (“A new radiation”, *Indian J. Phys.*, 2 (1928) 387), and which gained him the 1930 Nobel Prize in Physics.  Other investigations carried out by Raman were: his experimental and theoretical studies on the diffraction of light by acoustic waves of ultrasonic and hypersonic frequencies (published 1934-1942), and those on the effects produced by X-rays on infrared vibrations in crystals exposed to ordinary light. In 1948 Raman, through studying the spectroscopic behaviour of crystals, approached in a new manner fundamental problems of crystal dynamics. His laboratory has been dealing with the structure and properties of diamond, the structure and optical behaviour of numerous iridescent substances (labradorite, pearly felspar, agate, opal, and pearls).  Among his other interests have been the optics of colloids, electrical and magnetic anisotropy, and the physiology of human vision.  Raman has been honoured with a large number of honorary doctorates and memberships of scientific societies. He was elected a Fellow of the Royal Society early in his career (1924), and was knighted in 1929. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0193 |
| **Biographical** | Prince Louis-Victor de Broglie of the French Academy, Permanent Secretary of the Academy of Sciences, and Professor at the Faculty of Sciences at Paris University, was born at Dieppe (Seine Inférieure) on 15th August, 1892, the son of Victor, Duc de Broglie and Pauline d’Armaillé. After studying at the Lycée Janson of Sailly, he passed his school-leaving certificate in 1909. He applied himself first to literary studies and took his degree in history in 1910. Then, as his liking for science prevailed, he studied for a science degree, which he gained in 1913. He was then conscripted for military service and posted to the wireless section of the army, where he remained for the whole of the war of 1914-1918. During this period he was stationed at the Eiffel Tower, where he devoted his spare time to the study of technical problems. At the end of the war Louis de Broglie resumed his studies of general physics. While taking an interest in the experimental work carried out by his elder brother, Maurice, and co-workers, he specialized in theoretical physics and, in particular, in the study of problems involving quanta. In 1924 at the Faculty of Sciences at Paris University he delivered a thesis *Recherches sur la Théorie des Quanta* (Researches on the quantum theory), which gained him his doctor’s degree. This thesis contained a series of important findings which he had obtained in the course of about two years. The ideas set out in that work, which first gave rise to astonishment owing to their novelty, were subsequently fully confirmed by the discovery of electron diffraction by crystals in 1927 by [Davisson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1937/index.html) and Germer; they served as the basis for developing the general theory nowadays known by the name of *wave mechanics*, a theory which has utterly transformed our knowledge of physical phenomena on the atomic scale.  After the maintaining of his thesis and while continuing to publish original work on the new mechanics, Louis de Broglie took up teaching duties. On completion of two year’s free lectures at the Sorbonne he was appointed to teach theoretical physics at the Institut Henri Poincaré which had just been built in Paris. The purpose of that Institute is to teach and develop mathematical and theoretical physics. The incumbent of the chair of theoretical physics at the Faculty of Sciences at the University of Paris since 1932, Louis de Broglie runs a course on a different subject each year at the Institut Henri Poincaré, and several of these courses have been published. Many French and foreign students have come to work with him and a great deal of doctorate theses have been prepared under his guidance.  Between 1930 and 1950, Louis de Broglie’s work has been chiefly devoted to the study of the various extensions of wave mechanics: Dirac’s electron theory, the new theory of light, the general theory of spin particles, applications of wave mechanics to nuclear physics, etc. He has published numerous notes and several papers on this subject, and is the author of more than twenty-five books on the fields of his particular interests.  Since 1951, together with young colleagues, Louis de Broglie has resumed the study of an attempt which he made in 1927 under the name of the *theory of the double solution* to give a causal interpretation to wave mechanics in the classical terms of space and time, an attempt which he had then abandoned in the face of the almost universal adherence of physicists to the purely probabilistic interpretation of [Born](https://www.nobelprize.org/nobel_prizes/physics/laureates/1954/index.html), [Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html), and [Heisenberg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1932/index.html). Back again in this his former field of research, he has obtained a certain number of new and encouraging results which he has published in notes to *Comptes Rendus de l’Académie des Sciences* and in various expositions.  After crowning Louis de Broglie’s work on two occasions, the Academie des Sciences awarded him in 1929 the Henri Poincaré medal (awarded for the first time), then in 1932, the Albert I of Monaco prize. In 1929 the [Swedish Academy of Sciences](http://www.kva.se/) conferred on him the Nobel Prize for Physics “for his discovery of the wave nature of electrons”. In 1952 the first Kalinga Prize was awarded to him by UNESCO for his efforts to explain aspects of modern physics to the layman. In 1956 he received the gold medal of the French National Scientific Research Centre. He has made major contributions to the fostering of international scientific co-operation.  Elected a member of the Academy of Sciences of the French Institute in 1933, Louis de Broglie has been its Permanent Secretary for the mathematical sciences since 1942. He has been a member of the Bureau des Longitudes since 1944. He holds the Grand Cross of the Légion d’Honneur and is an Officer of the Order of Leopold of Belgium. He is an honorary doctor of the Universities of Warsaw, Bucharest, Athens, Lausanne, Quebec, and Brussels, and a member of eighteen foreign academies in Europe, India, and the U.S.A.  Professor de Broglie’s most important publications are:  *Recherches sur la théorie des quanta* (Researches on the quantum theory), Thesis Paris, 1924.  *Ondes et mouvements* (Waves and motions), Gauthier-Villars, Paris, 1926.  *Rapport au 5e Conseil de Physique Solvay*, Brussels, 1927.  *La mécanique ondulatoire* (Wave mechanics), Gauthier-Villars, Paris, 1928.  *Une tentative d’interprétation causale et non linéaire de la mécanique ondulatoire: la théorie de la double solution*, Gauthier-Villars, Paris, 1956.  English translation: *Non-linear Wave Mechanics: A Causal Interpretation*, Elsevier, Amsterdam, 1960.  *Introduction à la nouvelle théorie des particules de M. Jean-Pierre Vigier et de ses collaborateurs*, Gauthier-Villars, Paris, 1961.  English translation: *Introduction to the Vigier Theory of elementary particles*, Elsevier, Amsterdam, 1963.  *Étude critique des bases de l’interprétation actuelle de la mécanique ondulatoire*, Gauthier-Villars, Paris, 1963.  English translation: *The Current Interpretation of Wave Mechanics: A Critical Study*, Elsevier, Amsterdam, 1964. |
| **Autobiographical** |  |
| **Podcast** |  |
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| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0194 |
| **Biographical** | Owen Willans Richardson was born on the 26th of April, 1879, at Dewsbury, Yorkshire, England, as the only son of Joshua Henry and Charlotte Maria Richardson.  Educated at Batley Grammar School, he proceeded to Cambridge in 1897, having obtained an Entrance Major Scholarship at Trinity College; he gained First Class Honours in Natural Science at the examinations of the Universities of Cambridge and London, with particular distinctions in Physics and Chemistry. After graduating at Cambridge in 1900, he began to investigate the emission of electricity from hot bodies at the Cavendish Laboratory. In 1902 he was elected a Fellow of Trinity College, Cambridge. The law for the discovery of which the Nobel Prize was specially given, was first announced by him in a paper read before the Cambridge Philosophical Society on the 25th November, 1901, in the following words, as recorded in the published Proceedings: “If then the negative radiation is due to the corpuscles coming out of the metal, the saturation current s should obey the law s = AT1/2e-b/T. This law is fully confirmed by the experiments to be described.” Richardson continued working at this subject at Cambridge until 1906, when he was appointed Professor of Physics at Princeton University in America, where he remained until the end of 1913, working at thermionic emission, photoelectric action, and the gyromagnetic effect. In 1911 he was elected a member of the American Philosophical Society, and in 1913 a Fellow of the Royal Society, whereupon (1914) he returned to England as Wheatstone Professor of Physics at King’s College in the University of London. Among his publications were: *The Electron Theory of Matter*, 1914 (2nd ed., 1916), *The Emission of Electricity from Hot Bodies*, 1916 (2nd ed., 1921), *Molecular Hydrogen and its Spectrum*, 1934.  He was awarded the Hughes Medal by the Royal Society (1920), especially for work on thermionics; elected President, Section A, of the British Association (1921) and President of the Physical Society, London (1926-1928); appointed Yarrow Research Professor of the Royal Society, London (1926-1944), and knighted in 1939. Since 1914 he worked at thermionics, photoelectric effects, magnetism, the emission of electrons by chemical action, the theory of electrons, the quantum theory, the spectrum of molecular hydrogen, soft X-rays, the fine structure of Ha and Da. His last paper, with E. W. Foster, appeared in 1953. He received honorary degrees from the Universities of St. Andrews, Leeds, and London.  In 1906 he married Lilian Maud Wilson, the only sister of the well-known physicist H. A. Wilson, who was a fellow-student with him in Cambridge. There were two sons and one daughter of this marriage. After the death of his wife in 1945, Richardson married the physicist Henriette Rupp in 1948; he himself died in 1959. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0195 |
| **Biographical** | Arthur Holly Compton was born at Wooster, Ohio, on September 10th, 1892, the son of Elias Compton, Professor of Philosophy and Dean of the College of Wooster. He was educated at the College, graduating Bachelor of Science in 1913, and he spent three years in postgraduate study at Princeton University receiving his M.A. degree in 1914 and his Ph.D. in 1916. After spending a year as instructor of physics at the University of Minnesota, he took a position as a research engineer with the Westinghouse Lamp Company at Pittsburgh until 1919 when he studied at Cambridge University as a National Research Council Fellow. In 1920, he was appointed Wayman Crow Professor of Physics, and Head of the Department of Physics at the Washington University, St. Louis; and in 1923 he moved to the University of Chicago as Professor of Physics. Compton returned to St. Louis as Chancellor in 1945 and from 1954 until his retirement in 1961 he was Distinguished Service Professor of Natural Philosophy at the Washington University.  In his early days at Princeton, Compton devised an elegant method for demonstrating the Earth’s rotation, but he was soon to begin his studies in the field of X-rays. He developed a theory of the intensity of X-ray reflection from crystals as a means of studying the arrangement of electrons and atoms, and in 1918 he started a study of X-ray scattering. This led, in 1922, to his discovery of the increase of wavelength of X-rays due to scattering of the incident radiation by free electrons, which implies that the scattered quanta have less energy than the quanta of the original beam. This effect, nowadays known as the *Compton effect*, which clearly illustrates the particle concept of electromagnetic radiation, was afterwards substantiated by C. T. R. Wilson who, in his cloud chamber, could show the presence of the tracks of the recoil electrons. Another proof of the reality of this phenomenon was supplied by the coincidence method (developed by Compton and A.W. Simon, and independently in Germany by W. Bothe and H. Geiger), by which it could be established that individual scattered X-ray photons and recoil electrons appear at the same instant, contradicting the views then being developed by some investigators in an attempt to reconcile quantum views with the continuous waves of electromagnetic theory. For this discovery, Compton was awarded the Nobel Prize in Physics for 1927 (sharing this with C. T. R. Wilson who received the Prize for his discovery of the cloud chamber method).  In addition, Compton discovered (with C. F. Hagenow) the phenomenon of total reflection of X-rays and their complete polarization, which led to a more accurate determination of the number of electrons in an atom. He was also the first (with R. L. Doan) who obtained X-ray spectra from ruled gratings, which offers a direct method of measuring the wavelength of X-rays. By comparing these spectra with those obtained when using a crystal, the absolute value of the grating space of the crystal can be determined. The Avogadro number found by combining above value with the measured crystal density, led to a new value for the electronic charge. This outcome necessitated the revision of the [Millikan](https://www.nobelprize.org/nobel_prizes/physics/laureates/1923/index.html) oil-drop value from 4.774 to 4.803 X 10-10 e.s.u. (revealing that systematic errors had been made in the measurement of the viscosity of air, a quantity entering into the oil-drop method).  During 1930-1940, Compton led a world-wide study of the geographic variations of the intensity of cosmic rays, thereby fully confirming the observations made in 1927 by J. Clay from Amsterdam of the influence of latitude on cosmic ray intensity. He could, however, show that the intensity was correlated with geomagnetic rather than geographic latitude. This gave rise to extensive studies of the interaction of the Earth’s magnetic field with the incoming isotropic stream of primary charged particles.  Compton has numerous papers on scientific record and he is the author of *Secondary Radiations Produced by X-rays* (1922), *X-Rays and Electrons* (1926, second edition 1928), *X-Rays in Theory and Experiment* (with S. K. Allison, 1935, this being the revised edition of *X-rays and Electrons*), *The Freedom of Man* (1935, third edition 1939), *On Going to College* (with others, 1940), and *Human Meaning of Science* (1940).  Dr. Compton was awarded numerous honorary degrees and other distinctions including the Rumford Gold Medal (American Academy of Arts and Sciences), 1927; Gold Medal of Radiological Society of North America, 1928; Hughes Medal (Royal Society) and Franklin Medal (Franklin Institute), 1940.  He served as President of the American Physical Society (1934), of the American Association of Scientific Workers (1939-1940), and of the American Association for the Advancement of Science (1942).  In 1941 Compton was appointed Chairman of the National Academy of Sciences Committee to Evaluate Use of Atomic Energy in War. His investigations, carried out in cooperation with [E. Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html), L. Szilard, [E. P. Wigner](https://www.nobelprize.org/nobel_prizes/physics/laureates/1963/index.html) and others, led to the establishment of the first controlled uranium fission reactors, and, ultimately, to the large plutonium-producing reactors in Hanford, Washington, which produced the plutonium for the Nagasaki bomb, in August 1945. (He also played a role in the Government’s decision to use the bomb; a personal account of these matters may be found in his book, *Atomic Quest – a Personal Narrative*, 1956.)  In 1916, he married Betty Charity McCloskey. The eldest of their two sons, Arthur Allen, is in the American Foreign Service and the youngest, John Joseph, is Professor of Philosophy at the Vanderbilt University (Nashville, Tennessee ). His brother Wilson is a former President of the Washington State University, and his brother Karl Taylor was formerly President of the Massachusetts Institute of Technology.  Compton’s chief recreations were tennis, astronomy, photography and music.  He died on March 15th, 1962, in Berkeley, California. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0196 |
| **Biographical** | Charles Thomson Rees Wilson was born on the 14th of February, 1869, in the parish of Glencorse, near Edinburgh. His father, John Wilson, was a farmer, and his ancestors had been farmers in the South of Scotland for generations. His mother was Annie Clerk Harper.  At the age of four he lost his father, and his mother moved with the family to Manchester, where he was at first educated at a private school, and later at Owen’s College – now the University of Manchester. Here, intending to become a physician, Wilson took up mainly biology. Having been granted an entrance scholarship in 1888 he went on to Cambridge (Sidney Sussex College), where he took his degree in 1892. It was here that he became interested in the physical sciences, especially physics and chemistry. (It was also possible that Wilson’s decision to abandon medicine was influenced by Balfour Stewart, who was professor of physics at Owen’s College at that time – about a dozen years earlier, [J. J. Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1906/index.html), who also went to Cambridge, had passed through the same College.)  When standing on the summit of Ben Nevis, the highest of the Scottish mountains, in the late summer of 1894, Wilson was struck by the beauty of coronas and “glories” (coloured rings surrounding shadows cast on mist and cloud), and he decided to imitate these natural phenomena in the laboratory (early 1895). His sharp observation and keen intellect, however, led him to suspect (after a few months’ work at the Cavendish Laboratory) that the few drops reappearing again and again each time he expanded a volume of moist, dust-free air, might be the result of condensation on nuclei – possibly the ions causing the “residual” conductivity of the atmosphere-produced continuously. Wilson’s hypothesis was supported after exposure (early 1896) of his primitive *cloud chamber* to the newly discovered (end of 1895) X-rays. The immense increase of the “rain-like” condensation fitted excellently with the observation made by Thomson and McClelland immediately after Röntgen’s discovery, that air was made conductive by the passage of X-rays. When, during the summer of that year, it was firmly established by Thomson and [Rutherford](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1908/index.html) that the conductivity was indeed due to ionization of the gas, there was no longer any doubt that ions in gases could be detected and, photographically, recorded and thus studied at leisure. Wilson’s appointment as Clerk Maxwell Student, at the end of that year, enabled him to devote all his time for the next three years to research, and for a year subsequent to this he was employed by the Meteorological Council in research on atmospheric electricity. The greater part of his work on the behaviour of ions as condensation nuclei was thus carried out in the years 1895-1900, whilst after this his other occupations – mainly tutorial – prevented him from dealing sufficiently with the development of the cloud chamber. Early in 1911, however, he was the first person to see and photograph the tracks of individual alpha- and beta-particles and electrons. (The latter were described by him as “little wisps and threads of clouds”.) The event aroused great interest as the paths of the alpha-particles were just as [W. H. Bragg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1915/index.html) had drawn them in a publication some years earlier. But it was not until 1923 that the cloud chamber was brought to perfection and led to his two, beautifully illustrated, classic papers on the tracks of electrons. Wilson’s technique was promptly followed with startling success in all parts of the world – in Cambridge, by [Blackett](https://www.nobelprize.org/nobel_prizes/physics/laureates/1948/index.html) (who in 1948 received the Nobel Prize on account of his further development of the cloud chamber and his discoveries made therewith) and [Kapitsa](https://www.nobelprize.org/nobel_prizes/physics/laureates/1978/index.html); in Paris, by Irène Curie and Auger; in Berlin, by Bothe, Meitner, and Philipp; in Leningrad, by Skobelzyn; in Tokio, by Kikuchi.  Some of the most important achievements using the Wilson chamber were: the demonstration of the existence of Compton recoil electrons, thus establishing beyond any doubt the reality of the Compton effect (Compton shared the Nobel Prize with Wilson in 1927); the discovery of the positron by Anderson (who was awarded the Nobel Prize for 1936 for this feat); the visual demonstration of the processes of “pair creation” and “annihilation” of electrons and positrons by Blackett and Occhialini; and that of the transmutation of atomic nuclei carried out by Cockcroft and Walton. Thus, Rutherford’s remark that the cloud chamber was “the most original and wonderful instrument in scientific history” has been fully justified.  In 1900, Wilson was made Fellow of Sidney Sussex College, and University Lecturer and Demonstrator. From then until 1918 he was in charge of the advanced teaching of practical physics at the Cavendish Laboratory, and also gave lectures on light. As well as his experimental work at the Cavendish Laboratory, he also made observations (1900-1901) on atmospheric electricity (mainly in the surroundings of Peebles in Scotland). In 1913, he was appointed Observer in Meteorological Physics at the Solar Physics Observatory, and most of his research both on the tracks of ionizing particles and on thunderstorm electricity was carried out there. In 1918, he was appointed Reader in Electrical Meteorology, and in 1925, Jacksonian Professor of Natural Philosophy. He was elected a Fellow of the Royal Society in 1900, and this Society also honoured him with the Hughes Medal (1911), a Royal Medal (1922), and the Copley Medal (1935). The Cambridge Philosophical Society awarded him the Hopkins Prize (1920), and the Royal Society of Edinburgh the Gunning Prize (1921), while the Franklin Institute presented him the Howard Potts Medal (1925).  After his retirement Wilson moved to Edinburgh, and later, at the age of 80, to the village of Carlops, close to his birthplace at the farmhouse of Crosshouse, at Glencorse. Life after this, however, was not an empty one: C.T.R. as his friends and colleagues called him, maintained social contacts, making a weekly journey by bus to the city to lunch with them. Scientifically, too, he was active to the end, finishing his long-promised manuscript on the theory of thundercloud electricity (*Proc. Roy. Soc. London*, August (1956)).  Among the few who enjoyed his personal guidance may be mentioned: Wormell (in the general field of atmospheric electricity), C. F. Powell ([Nobel Prize winner 1950](https://www.nobelprize.org/nobel_prizes/physics/laureates/1950/index.html), for his development of the photographic method of studying nuclear processes and the discoveries made therewith on mesons), P. I. Dee and J. G. Wilson.  In 1908, Professor Wilson married Jessie Fraser, daughter of Rev. G. H. Dick of Glasgow; there were two sons and two daughters.  He died on the 15th of November, 1959, in the midst of his family. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0197 |
| **Biographical** | Jean Baptiste Perrin was born in Lille, September 30, 1870, where he was educated at the École Normal Supérieure, becoming an assistant in physics during 1894-1897, when he began his researches on cathode rays and X-rays. He received the degree of “docteur ès sciences” in 1897 for a thesis on cathode and Röntgen rays and was appointed, in the same year, to a readership in physical chemistry at the Sorbonne, University of Paris. He became Professor here in 1910; a post which he held till 1940, when the Germans invaded his country.  His earliest work was on the nature of cathode rays, and their nature was proved by him to be that of negatively charged particles. He also studied the effect of the action of X-rays on the conductivity of gases. In addition, he worked on fluorescence, the disintegration of radium, and the emission and transmission of sound. The work for which he is best known is the study of colloids and, in particular, the so-called Brownian movement. His results in this field were able to confirm Einstein’s theoretical studies in which it was shown that colloidal particles should obey the gas laws, and hence to calculate Avogadro’s number *N*, the number of molecules per grammolecule of a gas. The value thus calculated agreed excellently with other values obtained by entirely different methods in connection with other phenomena, such as that found by him as a result of his study of the sedimentation equilibrium in suspensions containing microscopic gamboge particles of uniform size. In this way the discontinuity of matter was proved by him beyond doubt: an achievement rewarded with the 1926 Nobel Prize.  Perrin was the author of many books and scientific papers. His book *Les Atomes*, published in 1913, sold 30,000 copies up to 1936. His principal papers were: “Rayons cathodiques et rayons X” (Cathode rays and X-rays), *Ann. Phys*., 1897; *Les Principes* (The principles), Gauthier-Villars, 1901; “Electrisation de contact” (Contact electrificaton), *J. Chim. Phys.*, 1904-1905; “Réalité moléculaire” (Molecular reality), *Ann. Phys.*, 1909; “Matière et Lumière” (Matter and light), *Ann. Phys.*, 1919; “Lumière et Reaction chimique” (Light and chemical reaction), *Conseil Solvay de Chimie*, 1925.  Many honours were conferred on him for his scientific work; the Joule Prize of the Royal Society in 1896, the Vallauri Prize of Bologna in 1912 and, in 1914, the La Caze Prize of the Paris Academy of Sciences.  He held honorary doctorates of the Universities of Brussels, Liege, Ghent, Calcutta, New York, Princeton, Manchester, and Oxford. He was twice appointed a member of the Solvay Committee at Brussels in 1911 and in 1921. He held memberships of the Royal Society (London) and of the Academies of Sciences of Belgium, [Sweden](http://www.kva.se/), Turin, Prague, Rumania, and China. In 1923 he was elected to the French Academy of Sciences. He became a Commander of the Legion of Honour in 1926, and was also made Commander of the British Empire and of the Order of Leopold (Belgium).  Perrin was the creator of the Centre National de la Recherche Scientifique, an organization offering to most promising French scientists – whose scientific talents would otherwise be lost – a career outside the University. It was due to this institute that [Frédéric Joliot](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1935/index.html) could carry out his magnificent investigations. In addition to this, he founded the Palais de la Découverte (Palace of discovery); he was also responsible for the establishment of the Institut d’Astrophysique, in Paris, and for the construction of the large Observatoire de Haute Provence; without his prestige and his power of persuasion the Institut de Biologie Physico-Chimique would never have come into being.  Perrin was an officer in the engineer corps during the 1914-1918 War. When the Germans invaded his country in 1940 he escaped to the U.S.A., where he died on the 17th of April, 1942. After the War, in 1948, his remains were transferred to his fatherland by the battleship Jeanne d’Arc, and buried in the Panthéon. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0198 |
| **Biographical** | James Franck was born on August 26, 1882, in Hamburg, Germany. After attending the Wilhelm Gymnasium there, he studied mainly chemistry for a year at the University of Heidelberg, and then studied physics at the University of Berlin, where his principal tutors were Emil Warburg and Paul Drude. He received his Ph.D. at Berlin in 1906 under Warburg, and after a short period as an assistant in Frankfurt-am-Main, he returned to Berlin to become assistant to Heinrich Rubens. In 1911, he obtained the “venia legendi” for physics to lecture at the University of Berlin, and remained there until 1918 (with time out for the war in which he was awarded the Iron Cross, first class) as a member of the physics faculty having achieved the rank of associate professor.  After World War I, he was appointed member and Head of the Physics Division in the Kaiser Wilhelm Institute for Physical Chemistry at Berlin-Dahlem, which was at that time under the chairmanship of Fritz Haber. In 1920, Franck became Professor of Experimental Physics and Director of the Second Institute for Experimental Physics at the University of Göttingen. During the period 1920-1933, when Göttingen became an important center for quantum physics, Franck was closely cooperating with [Max Born](https://www.nobelprize.org/nobel_prizes/physics/laureates/1954/index.html), who then headed the Institute for Theoretical Physics. It was in Göttingen that Franck revealed himself as a highly gifted tutor, gathering around him and inspiring a circle of students and collaborators (among them: [Blackett](https://www.nobelprize.org/nobel_prizes/physics/laureates/1948/index.html), Condon, Kopfermann, Kroebel, Maier-Leibnitz, Oppenheimer, and Rabinovich, to mention some of them), who in later years were to be renowned in their own fields.  After the Nazi regime assumed power in Germany, Franck and his family moved to Baltimore, U.S.A., where he had been invited to lecture as Speyer Professor at Johns Hopkins University. He then went to Copenhagen, Denmark, as a guest professor for a year. In 1935, he returned to the United States as Professor of Physics at Johns Hopkins University, leaving there in 1938 to accept a professorship in physical chemistry at the University of Chicago. During World War II Franck served as Director of the Chemistry Division of The Metallurgical Laboratory at the University of Chicago, which was the center of the Manhattan District’s Project.  In 1947, at the age of 65, Franck was named professor emeritus at the University of Chicago, but he continued to work at the University as Head of the Photosynthesis Research Group until 1956.  While in Berlin Professor Franck’s main field of investigation was the kinetics of electrons, atoms, and molecules. His initial researches dealt with the conduction of electricity through gases (the mobility of ions in gases). Later, together with Hertz, he investigated the behaviour of free electrons in various gases – in particular the inelastic impacts of electrons upon atomswork which ultimately led to the experimental proof of some of the basic concepts of Bohr’s atomic theory, and for which they were awarded the Nobel Prize, for 1925. Franck’s other investigations, many of which were carried out with collaborators and students, were also dedicated to problems of atomic physics – those on the exchange of energy of excited atoms (impacts of the second type, photochemical researches), and optical problems connected with elementary processes during chemical reactions.  During his period at Göttingen most of his studies were dedicated to the fluorescence of gases and vapours. In 1925, he proposed a mechanism to explain his observations of the photochemical dissociation of iodine molecules. Electronic transitions from a normal to a higher vibrational state occur so rapidly, he suggested, that the position and momenta of the nuclei undergo no appreciable change in the process. This proposed mechanism was later expanded by E. U. Condon to a theory permitting the prediction of mostfavoured vibrational transitions in a band system, and the concept has since been known as the Franck-Condon principle.  Mention should be made of Professor Franck’s courage in following what was morally right. He was one of the first who openly demonstrated against the issue of racial laws in Germany, and he resigned from the University of Göttingen in 1933 as a personal protest against the Nazi regime under Adolf Hitler. Later, in his second homeland, his moral courage was again evident when in 1945 (two months before Hiroshima) he joined with a group of atomic scientists in preparing the so-called “Franck Report” to the War Department, urging an open demonstration of the atomic bomb in some uninhabited locality as an alternative to the military decision to use the weapon without warning in the war against Japan. This report, although failing to attain its main objective, still stands as a monument to the rejection by scientists of the use of science in works of destruction.  In addition to the Nobel Prize, Professor Franck received the 1951 Max Planck Medal of the German Physical Society, and he was honoured, in 1953, by the university town of Göttingen, which named him an honorary citizen. In 1955, he received the Rumford Medal of the American Academy of Arts and Sciences for his work on photosynthesis, a subject with which he had become increasingly preoccupied during his years in the United States. In 1964, Professor Franck was elected as a Foreign Member of the Royal Society, London, for his contribution to the understanding of exchanges of energy in electron collisions, to the interpretation of molecular spectra, and to problems of photosynthesis.  Franck was first married (1911) to Ingrid Josefson, of Göteborg, Sweden, and had two daughters, Dagmar and Lisa. Some years after the death of his first wife, he was married (1946) to Hertha Sponer, Professor of Physics at Duke University in Durham, North Carolina (U.S.A.).  Professor Franck died in Germany on May 21, 1964, while visiting in Göttingen. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0199 |
| **Biographical** | Gustav Ludwig Hertz was born in Hamburg on July 22nd, 1887, the son of a lawyer, Dr. Gustav Hertz, and his wife Auguste, *née* Arning. He attended the Johanneum School in Hamburg before commencing his university education at Göttingen in 1906; he subsequently studied at the Universities of Munich and Berlin, graduating in 1911. He was appointed Research Assistant at the Physics Institute of Berlin University in 1913 but, with the onset of World War I, he was mobilized in 1914 and severely wounded in action in 1915. Hertz returned to Berlin as Privatdozent in 1917. From 1920 to 1925 he worked in the physics laboratory of the Philips Incandescent Lamp Factory at Eindhoven.  In 1925, he was elected Resident Professor and Director of the Physics Institute of the University of Halle, and in 1928 he returned to Berlin as Director of the Physics Institute in the Charlottenburg Technological University. Hertz resigned from this post for political reasons in 1935 to return to industry as director of a research laboratory of the Siemens Company. From 1945 tot 1954 he worked as the head of a research laboratory in the Soviet Union, when he was appointed Professor and Director of the Physics Institute at the Karl Marx University in Leipzig. He was made emeritus in 1961, and since then he has lived in retirement, first in Leipzig and later in Berlin.  Hertz’s early researches, for his thesis, involved studies on the infrared absorption of carbon dioxide in relation to pressure and partial pressure. Together with J. Franck he began his studies on electron impact in 1913 and before his mobilization, he spent much patient work on the study and measurement of ionization potentials in various gases. He later demonstrated the quantitative relations between the series of spectral lines and the energy losses of electrons in collision with atoms corresponding to the stationary energy states of the atoms. His results were in perfect agreement with [Bohr’s](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html) theory of atomic structure, which included the application of [Planck’s](https://www.nobelprize.org/nobel_prizes/physics/laureates/1918/index.html) quantum theory.  On his return to Berlin in 1928, it was his first task to rebuild the Physics Institute and re-establish the School, and he worked tirelessly towards this end. There he was responsible for a method of separating the isotopes of neon by means of a diffusion cascade.  Hertz has published many papers, alone, with Franck, and with Kloppers, on the quantitative exchange of energy between electrons and atoms, and on the measurement of ionization potentials. He also is the author of some papers concerning the separation of isotopes.  Gustav Hertz is Member of the German Academy of Sciences in Berlin, and Corresponding Member of the Göttingen Academy of Sciences; he is also Honorary Member of the Hungarian Academy of Sciences, Member of the Czechoslovakian Academy of Sciences, and Foreign Member of the Academy of Sciences U.S.S.R. He is recipient of the Max Planck Medal of the German Physical Society.  Professor Hertz was married in 1919, with Ellen *née* Dihlmann, who died in 1941. They had two sons, both physicists: Dr. Hellmuth Hertz, Professor at the Technical College in Lund, and Dr. Johannes Hertz, working at the Institute for Optics and Spectroscopy of the German Academy of Sciences in Berlin.  Since 1943, Professor Hertz is married with Charlotte, *née* Jollasse. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0200 |
| **Biographical** | Karl Manne Georg Siegbahn was born on the 3rd of December, 1886, at Örebro in Sweden. His father was Nils Reinhold Georg Siegbahn, a stationmaster of the State Railways, and his mother was Emma Sofia Mathilda Zetterberg.  After receiving a high-school education he entered the University of Lund in 1906, where he obtained his doctor’s degree, in 1911, on the thesis “Magnetische Feldmessung”. From 1907 to 1911 he served as Assistant to Professor J. R. Rydberg in the Physics Institute of the University, afterwards he was appointed lecturer and (in 1915) Deputy Professor of Physics. On the death of Rydberg, he was appointed Professor (1920). In 1923 he became Professor of Physics at the University of Uppsala. In 1937 came his appointment as Research Professor of Experimental Physics, at the Royal Swedish Academy of Sciences. When the Physics Department of the Nobel Institute of the Academy came into being, that same year, Siegbahn was made its first Director.  Siegbahn’s early work (1908-1912) was concerned with problems of electricity and magnetism.  From 1912 to 1937 his research work was mainly devoted to X-ray spectroscopy. He developed new methods, and designed instruments for this purpose. His improvements and new constructions of air pumps and X-ray tubes enabled a considerable increase of the radiation intensity, and the numerous spectrographs and crystal or linear gratings which he constructed, have resulted in a highly increased accuracy of his measurements. In this way, a large number of new series within the characteristic X-radiations of elements could be discovered. The new precision technique thus developer by Siegbahn led to a practically complete knowledge of the energy and radiation conditions in the electron shells of the atoms, while at the same I time a solid empirical foundation was created for the quantum-theoretical interpretation of attendant phenomena. Siegbahn’s findings in this field havt been summarized by him in his book *Spektroskopie der Röntgenstrahlen*, 1923 (rev. ed., 1931; ed. in English, 1924), a classic in scientific literature. As a measure of the high precision achieved by Siegbahn’s spectrographs (which are held at a constant temperature and read, in tenths of seconds, by means of two microscopes mounted diametrically opposite one another on a precision goniometer) may be mentioned the fact that his energy-level values, arrived at thirty years ago, still serve for many purposes.  The research activity in the Institute under Siegbahn’s leadership was directed towards problems of nuclear physics. For this purpose a cyclotron was constructed capable of accelerating deuterons of up to 5 to 6 MeV (1939), which was soon to make place for a larger one for deuteron energies of up to 30 MeV. In addition to this, a high-tension generator for 400,000 volts was built, as a provisional measure, during the War (transformed into a plant for 1.5 million volts in 1962). For the purpose of studying the energy and radiation of the different radioactive isotopes an electromagnetic separator has been constructed at the Institute, and several new types of ß-spectrographs for various purposes have been designed and built. With these technical resources, and after suitable methods had been developed, a number of important projects for research were taken up. The radiation processes of unstable atomic nuclei and nuclear reactions of various kinds have been studied and exact measurements made of the magnetic properties of atomic nuclei. Other projects tackled by Siegbahn and his staff include the construction of an electron microscope of a new pattern and an automatically working ruling-engine for scratching well-defined gratings (with up to 1,800 lines per mm), especially for X-rays and the extreme ultraviolet field. A large number of young scientists, including many from foreign countries, have taken part in the progressively developed research work to study the atomic nucleus and its radioactive properties.  Siegbahn travelled a great deal and visited practically all important centres of scientific activity in Europe (1908-1922), Canada and the United States (1924-1925), where he, on invitation of the Rockefeller Foundation, gave lectures at the Universities of Columbia, Yale, Harvard, Cornell, Chicago, Berkeley, Pasadena, Montreal, and several other universities. After World War II, he visited the main nuclear research institutes in the U.S.A. during the years 1946 and 1953 (Berkeley, Pasadena, Los Angeles, St. Louis, Chicago, M.I.T. Boston, Brookhaven, Columbia, etc.).  As member of the Commission Internationale des Poids et Mesures (1937) he took part in annual meetings of this Commission in Paris; he was elected honorary member of this Commission when he left his membership ( 1956). Siegbahn was President of the International Union of Physics, during the period 1938-1947. Other honours, in addition to the Nobel Prize in Physics (1924) awarded to Professor Siegbahn included the Hughes Medal (1934) and the Rumford Medal (1940) from the Royal Society, London; the Duddel Medal from the Physical Society, London (1948). He is honorary doctor in Freiburg (1931), Bukarest (1942), Oslo (1946), Paris (1952) and the Technical Faculty in Stockholm (1957). He is Member of the Royal Society, London and Edinburgh, of the Academie des Sciences, Paris, and of several other academies.  Professor Siegbahn married Karin Högbom in 1914. They have two sons: Bo (b. 1915), at present (1964) Ambassador at Marocco; and [Kai](https://www.nobelprize.org/nobel_prizes/physics/laureates/1981/index.html) (b. 1918), since 1954 Professor of Physics at the University of Uppsala, on the same Chair that his father held during 1923-1937. |
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| **Physics\_1999-** | |
| **ID** | 0201 |
| **Biographical** | Karl Manne Georg Siegbahn was born on the 3rd of December, 1886, at Örebro in Sweden. His father was Nils Reinhold Georg Siegbahn, a stationmaster of the State Railways, and his mother was Emma Sofia Mathilda Zetterberg.  After receiving a high-school education he entered the University of Lund in 1906, where he obtained his doctor’s degree, in 1911, on the thesis “Magnetische Feldmessung”. From 1907 to 1911 he served as Assistant to Professor J. R. Rydberg in the Physics Institute of the University, afterwards he was appointed lecturer and (in 1915) Deputy Professor of Physics. On the death of Rydberg, he was appointed Professor (1920). In 1923 he became Professor of Physics at the University of Uppsala. In 1937 came his appointment as Research Professor of Experimental Physics, at the Royal Swedish Academy of Sciences. When the Physics Department of the Nobel Institute of the Academy came into being, that same year, Siegbahn was made its first Director.  Siegbahn’s early work (1908-1912) was concerned with problems of electricity and magnetism.  From 1912 to 1937 his research work was mainly devoted to X-ray spectroscopy. He developed new methods, and designed instruments for this purpose. His improvements and new constructions of air pumps and X-ray tubes enabled a considerable increase of the radiation intensity, and the numerous spectrographs and crystal or linear gratings which he constructed, have resulted in a highly increased accuracy of his measurements. In this way, a large number of new series within the characteristic X-radiations of elements could be discovered. The new precision technique thus developer by Siegbahn led to a practically complete knowledge of the energy and radiation conditions in the electron shells of the atoms, while at the same I time a solid empirical foundation was created for the quantum-theoretical interpretation of attendant phenomena. Siegbahn’s findings in this field havt been summarized by him in his book *Spektroskopie der Röntgenstrahlen*, 1923 (rev. ed., 1931; ed. in English, 1924), a classic in scientific literature. As a measure of the high precision achieved by Siegbahn’s spectrographs (which are held at a constant temperature and read, in tenths of seconds, by means of two microscopes mounted diametrically opposite one another on a precision goniometer) may be mentioned the fact that his energy-level values, arrived at thirty years ago, still serve for many purposes.  The research activity in the Institute under Siegbahn’s leadership was directed towards problems of nuclear physics. For this purpose a cyclotron was constructed capable of accelerating deuterons of up to 5 to 6 MeV (1939), which was soon to make place for a larger one for deuteron energies of up to 30 MeV. In addition to this, a high-tension generator for 400,000 volts was built, as a provisional measure, during the War (transformed into a plant for 1.5 million volts in 1962). For the purpose of studying the energy and radiation of the different radioactive isotopes an electromagnetic separator has been constructed at the Institute, and several new types of ß-spectrographs for various purposes have been designed and built. With these technical resources, and after suitable methods had been developed, a number of important projects for research were taken up. The radiation processes of unstable atomic nuclei and nuclear reactions of various kinds have been studied and exact measurements made of the magnetic properties of atomic nuclei. Other projects tackled by Siegbahn and his staff include the construction of an electron microscope of a new pattern and an automatically working ruling-engine for scratching well-defined gratings (with up to 1,800 lines per mm), especially for X-rays and the extreme ultraviolet field. A large number of young scientists, including many from foreign countries, have taken part in the progressively developed research work to study the atomic nucleus and its radioactive properties.  Siegbahn travelled a great deal and visited practically all important centres of scientific activity in Europe (1908-1922), Canada and the United States (1924-1925), where he, on invitation of the Rockefeller Foundation, gave lectures at the Universities of Columbia, Yale, Harvard, Cornell, Chicago, Berkeley, Pasadena, Montreal, and several other universities. After World War II, he visited the main nuclear research institutes in the U.S.A. during the years 1946 and 1953 (Berkeley, Pasadena, Los Angeles, St. Louis, Chicago, M.I.T. Boston, Brookhaven, Columbia, etc.).  As member of the Commission Internationale des Poids et Mesures (1937) he took part in annual meetings of this Commission in Paris; he was elected honorary member of this Commission when he left his membership ( 1956). Siegbahn was President of the International Union of Physics, during the period 1938-1947. Other honours, in addition to the Nobel Prize in Physics (1924) awarded to Professor Siegbahn included the Hughes Medal (1934) and the Rumford Medal (1940) from the Royal Society, London; the Duddel Medal from the Physical Society, London (1948). He is honorary doctor in Freiburg (1931), Bukarest (1942), Oslo (1946), Paris (1952) and the Technical Faculty in Stockholm (1957). He is Member of the Royal Society, London and Edinburgh, of the Academie des Sciences, Paris, and of several other academies.  Professor Siegbahn married Karin Högbom in 1914. They have two sons: Bo (b. 1915), at present (1964) Ambassador at Marocco; and [Kai](https://www.nobelprize.org/nobel_prizes/physics/laureates/1981/index.html) (b. 1918), since 1954 Professor of Physics at the University of Uppsala, on the same Chair that his father held during 1923-1937. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0202 |
| **Biographical** | Niels Henrik David Bohr was born in Copenhagen on October 7, 1885, as the son of Christian Bohr, Professor of Physiology at Copenhagen University, and his wife Ellen, *née* Adler. Niels, together with his younger brother Harald (the future Professor in Mathematics), grew up in an atmosphere most favourable to the development of his genius – his father was an eminent physiologist and was largely responsible for awakening his interest in physics while still at school, his mother came from a family distinguished in the field of education.  After matriculation at the Gammelholm Grammar School in 1903, he entered Copenhagen University where he came under the guidance of Professor C. Christiansen, a profoundly original and highly endowed physicist, and took his Master’s degree in Physics in 1909 and his Doctor’s degree in 1911.  While still a student, the announcement by the Academy of Sciences in Copenhagen of a prize to be awarded for the solution of a certain scientific problem, caused him to take up an experimental and theoretical investigation of the surface tension by means of oscillating fluid jets. This work, which he carried out in his father’s laboratory and for which he received the prize offered (a gold medal), was published in the *Transactions of the Royal Society*, 1908.  Bohr’s subsequent studies, however, became more and more theoretical in character, his doctor’s disputation being a purely theoretical piece of work on the explanation of the properties of the metals with the aid of the electron theory, which remains to this day a classic on the subject. It was in this work that Bohr was first confronted with the implications of [Planck](https://www.nobelprize.org/prizes/physics/1918/planck/facts/)‘s quantum theory of radiation.  In the autumn of 1911 he made a stay at Cambridge, where he profited by following the experimental work going on in the Cavendish Laboratory under [Sir J.J. Thomson’s](https://www.nobelprize.org/nobel_prizes/physics/laureates/1906/index.html) guidance, at the same time as he pursued own theoretical studies. In the spring of 1912 he was at work in [Professor Rutherford’s](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1908/index.html) laboratory in Manchester, where just in those years such an intensive scientific life and activity prevailed as a consequence of that investigator’s fundamental inquiries into the radioactive phenomena. Having there carried out a theoretical piece of work on the absorption of alpha rays which was published in the *Philosophical Magazine*, 1913, he passed on to a study of the structure of atoms on the basis of Rutherford’s discovery of the atomic nucleus. By introducing conceptions borrowed from the Quantum Theory as established by Planck, which had gradually come to occupy a prominent position in the science of theoretical physics, he succeeded in working out and presenting a picture of atomic structure that, with later improvements (mainly as a result of [Heisenberg](https://www.nobelprize.org/prizes/physics/1932/heisenberg/facts/)‘s ideas in 1925), still fitly serves as an elucidation of the physical and chemical properties of the elements.  In 1913-1914 Bohr held a Lectureship in Physics at Copenhagen University and in 1914-1916 a similar appointment at the Victoria University in Manchester. In 1916 he was appointed Professor of Theoretical Physics at Copenhagen University, and since 1920 (until his death in 1962) he was at the head of the Institute for Theoretical Physics, established for him at that university.  Recognition of his work on the structure of atoms came with the award of the Nobel Prize for 1922.  Bohr’s activities in his Institute were since 1930 more and more directed to research on the constitution of the atomic nuclei, and of their transmutations and disintegrations. In 1936 he pointed out that in nuclear processes the smallness of the region in which interactions take place, as well as the strength of these interactions, justify the transition processes to be described more in a classical way than in the case of atoms (Cf.  »Neutron capture and nuclear constitution«, *Nature*, 137 (1936) 344).  A liquid drop would, according to this view, give a very good picture of the nucleus. This so-called *liquid droplet theory* permitted the understanding of the mechanism of nuclear fission, when the splitting of uranium was discovered by [Hahn](https://www.nobelprize.org/prizes/chemistry/1944/hahn/facts/) and Strassmann, in 1939, and formed the basis of important theoretical studies in this field (among others, by Frisch and Meitner).  Bohr also contributed to the clarification of the problems encountered in quantum physics, in particular by developing the *concept of complementarity*. Hereby he could show how deeply the changes in the field of physics have affected fundamental features of our scientific outlook and how the consequences of this change of attitude reach far beyond the scope of atomic physics and touch upon all domains of human knowledge. These views are discussed in a number of essays, written during the years 1933-1962. They are available in English, collected in two volumes with the title Atomic Physics and Human Knowledge and Essays 1958-1962 on *Atomic Physics and Human Knowledge*, edited by John Wiley and Sons, New York and London, in 1958 and 1963, respectively.  Among Professor Bohr’s numerous writings (some 115 publications), three appearing as books in the English language may be mentioned here as embodying his principal thoughts: *The Theory of Spectra and Atomic Constitution*, University Press, Cambridge, 1922/2nd. ed., 1924; *Atomic Theory and the Description of Nature*, University Press, Cambridge, 1934/reprint 1961; *The Unity of Knowledge*, Doubleday & Co., New York, 1955.  During the Nazi occupation of Denmark in World War II, Bohr escaped to Sweden and spent the last two years of the war in England and America, where he became associated with the Atomic Energy Project. In his later years, he devoted his work to the peaceful application of atomic physics and to political problems arising from the development of atomic weapons. In particular, he advocated a development towards full openness between nations. His views are especially set forth in his *Open Letter to the United Nations*, June 9, 1950.  Until the end, Bohr’s mind remained alert as ever; during the last few years of his life he had shown keen interest in the new developments of molecular biology. The latest formulation of his thoughts on the problem of Life appeared in his final (unfinished) article, published after his death: “Licht und Leben-noch einmal”, *Naturwiss*., 50 (1963) 72: (in English: “Light and Life revisited”, *ICSU Rev*., 5 ( 1963) 194).  Niels Bohr was President of the Royal Danish Academy of Sciences, of the Danish Cancer Committee, and Chairman of the Danish Atomic Energy Commission. He was a Foreign Member of the Royal Society (London), the Royal Institution, and Academies in Amsterdam, Berlin, Bologna, Boston, Göttingen, Helsingfors, Budapest, München, Oslo, Paris, Rome, [Stockholm](http://www.kva.se/), Uppsala, Vienna, Washington, Harlem, Moscow, Trondhjem, Halle, Dublin, Liege, and Cracow. He was Doctor, *honoris causa*, of the following universities, colleges, and institutes: *(1923-1939)* – Cambridge, Liverpool, Manchester, Oxford, Copenhagen, Edinburgh, Kiel, Providence, California, Oslo, Birmingham, London; *(1945-1962)* – Sorbonne (Paris), Princeton, Mc. Gill (Montreal), Glasgow, Aberdeen, Athens, Lund, New York, Basel, Aarhus, Macalester (St. Paul), Minnesota, Roosevelt (Chicago, Ill.), Zagreb, Technion (Haifa), Bombay, Calcutta, Warsaw, Brussels, Harvard, Cambridge (Mass.), and Rockefeller (New York).  Professor Bohr was married, in 1912, to Margrethe Nørlund, who was for him an ideal companion. They had six sons, of whom they lost two; the other four have made distinguished careers in various professions – Hans Henrik (M.D.), Erik (chemical engineer), [Aage](https://www.nobelprize.org/nobel_prizes/physics/laureates/1975/index.html) (Ph.D., theoretical physicist, following his father as Director of the Institute for Theoretical Physics), Ernest (lawyer).  Niels Bohr died in Copenhagen on November 18, 1962. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0203 |
| **Biographical** | Albert Einstein was born at Ulm, in Württemberg, Germany, on March 14, 1879. Six weeks later the family moved to Munich, where he later on began his schooling at the Luitpold Gymnasium. Later, they moved to Italy and Albert continued his education at Aarau, Switzerland and in 1896 he entered the Swiss Federal Polytechnic School in Zurich to be trained as a teacher in physics and mathematics. In 1901, the year he gained his diploma, he acquired Swiss citizenship and, as he was unable to find a teaching post, he accepted a position as technical assistant in the Swiss Patent Office. In 1905 he obtained his doctor’s degree.  During his stay at the Patent Office, and in his spare time, he produced much of his remarkable work and in 1908 he was appointed Privatdozent in Berne. In 1909 he became Professor Extraordinary at Zurich, in 1911 Professor of Theoretical Physics at Prague, returning to Zurich in the following year to fill a similar post. In 1914 he was appointed Director of the Kaiser Wilhelm Physical Institute and Professor in the University of Berlin. He became a German citizen in 1914 and remained in Berlin until 1933 when he renounced his citizenship for political reasons and emigrated to America to take the position of Professor of Theoretical Physics at Princeton[\*](https://www.nobelprize.org/prizes/physics/1921/einstein/biographical/#footnote). He became a United States citizen in 1940 and retired from his post in 1945.  After World War II, Einstein was a leading figure in the World Government Movement, he was offered the Presidency of the State of Israel, which he declined, and he collaborated with Dr. Chaim Weizmann in establishing the Hebrew University of Jerusalem.  Einstein always appeared to have a clear view of the problems of physics and the determination to solve them. He had a strategy of his own and was able to visualize the main stages on the way to his goal. He regarded his major achievements as mere stepping-stones for the next advance.  At the start of his scientific work, Einstein realized the inadequacies of Newtonian mechanics and his special theory of relativity stemmed from an attempt to reconcile the laws of mechanics with the laws of the electromagnetic field. He dealt with classical problems of statistical mechanics and problems in which they were merged with quantum theory: this led to an explanation of the Brownian movement of molecules. He investigated the thermal properties of light with a low radiation density and his observations laid the foundation of the photon theory of light.  In his early days in Berlin, Einstein postulated that the correct interpretation of the special theory of relativity must also furnish a theory of gravitation and in 1916 he published his paper on the general theory of relativity. During this time he also contributed to the problems of the theory of radiation and statistical mechanics.  In the 1920s, Einstein embarked on the construction of unified field theories, although he continued to work on the probabilistic interpretation of quantum theory, and he persevered with this work in America. He contributed to statistical mechanics by his development of the quantum theory of a monatomic gas and he has also accomplished valuable work in connection with atomic transition probabilities and relativistic cosmology.  After his retirement he continued to work towards the unification of the basic concepts of physics, taking the opposite approach, geometrisation, to the majority of physicists.  Einstein’s researches are, of course, well chronicled and his more important works include *Special Theory of Relativity* (1905), *Relativity* (English translations, 1920 and 1950), *General Theory of Relativity* (1916), *Investigations on Theory of Brownian Movement* (1926), and *The Evolution of Physics* (1938). Among his non-scientific works, *About Zionism* (1930), *Why War?* (1933), *My Philosophy* (1934), and *Out of My Later Years* (1950) are perhaps the most important.  Albert Einstein received honorary doctorate degrees in science, medicine and philosophy from many European and American universities. During the 1920’s he lectured in Europe, America and the Far East, and he was awarded Fellowships or Memberships of all the leading scientific academies throughout the world. He gained numerous awards in recognition of his work, including the Copley Medal of the Royal Society of London in 1925, and the Franklin Medal of the Franklin Institute in 1935.  Einstein’s gifts inevitably resulted in his dwelling much in intellectual solitude and, for relaxation, music played an important part in his life. He married Mileva Maric in 1903 and they had a daughter and two sons; their marriage was dissolved in 1919 and in the same year he married his cousin, Elsa Löwenthal, who died in 1936. He died on April 18, 1955 at Princeton, New Jersey. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0204 |
| **Biographical** | Charles-Edouard Guillaume was born at Fleurier, in the Swiss-Jura, on February 15, 1861. His grandfather had left France for political reasons during the Revolution and established a watchmaking business in London. The business was carried on by his three sons but Charles’ father, Édouard, eventually returned to settle in Fleurier.  Guillaume received his early education in Neuchâtel before going to the Zurich Polytechnic where he obtained his doctor’s degree. He spent a short time as an officer in the artillery before entering the International Bureau of Weights and Measures, as an assistant, in 1883. He became Associate Director in 1902 and from 1915 until his retirement in 1936, he was Director of the Bureau. He remained as Honorary Director from 1936 until his death.  During his brief military career, Guillaume studied mechanics and ballistics but his earliest investigations at the bureau were with thermometry. He carried out important investigations on corrections to mercury-in-glass thermometers and he was responsible for the detailed calibration of thermometers used at the Bureau in the establishment of the thermal expansions of the standards of length. He was concerned in initial work on the International Metre and undertook a determination of the volume of one kilogram of water by the contact method.  A chance observation by Guillaume on the coefficient of expansion of nickel-iron alloys led to a systematic investigation of a whole series of alloys and the discovery of invar, an alloy with a very low coefficient of expansion; elinvar, for which the thermoelastic coefficient is practically zero, i.e. Young’s modulus constant, over a considerable temperature range; together with other useful alloys. The applications of invar were quickly recognized and the material was used in rapid methods for the measurement of geodetic baselines. The alloy is widely used in instruments of precision, such as thermostats and pendulums of astronomic clocks. Guillaume’s total compensating balance for high-grade watches and chronometers, which eliminates the secondary error, was perfected by an elinvar hair spring.  Guillaume’s work is recorded in many papers published by the Bureau and he has written, amongst other works, *Études thermométriques* (Studies on Thermometry, 1886), *Traité de thermométrie* (Treatise on Thermometry, 1889), *Unités et Étalons* (Units and Standards, 1894), *Les rayons X* (X-Rays, 1896), *Recherches sur le nickel et ses alliages* (Investigations on Nickel and its Alloys, 1898), *La vie de la matière* (The Life of Matter, 1899), *La Convention du Mètre et le Bureau international des Poids et Mesures* (Metrical Convention and the International Bureau of Weights and Measures, 1902), *Les applications des aciers au nickel* (Applications of Nickel-Steels, 1904), *Des états de la matière* (States of Matter, 1907), *Les récent progrès du système métrique* (Recent progress in the Metric System, 1907, 1913). His book *Initiation à la Mécanique* (Introduction to Mechanics) has been translated into several languages.  He was appointed Grand Officer of the Legion of Honour and received honorary Doctor of Science degrees from the Universities of Geneva, Neuchatel and Paris. He was a President of the Société Française de Physique and a member, honorary member or corresponding member of more than a dozen of the leading scientific academies of Europe.  Charles-Édouard Guillaume married Mlle. A.M. Taufflieb in 1888. They had three children. He died on May 13, 1938. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0205 |
| **Biographical** | Johannes Stark was born on April 15, 1874 in Schickenhof, Bavaria; his father was a landed proprietor. He was educated at the Gymnasium (grammar school) in Bayreuth and later in Regensburg and proceeded to Munich University in 1894 to read physics, mathematics, chemistry and crystallography. Stark graduated in 1897 on the basis of his doctoral dissertation on Newton’s electrochronic rings in a certain type of dim media. He worked as assistant to von Lommel at the Physics Institute at Munich University from 1897 until 1900 and then became unsalaried university lecturer of physics at the University of Göttingen. In 1906 he was appointed extraordinary professor at the Technische Hochschule in Hannover and in 1909 he followed the invitation of the Technische Hochschule in Aachen to become Professor there. A similar appointment at the University of Greifswald followed in 1917. Three years later he moved to the Physics Institute of the University of Würzburg, where he stayed until 1922.  Stark’s scientific works cover three large fields: the electric currents in gases, spectroscopic analysis, and chemical valency. His spectroscopic work deals with the connection between the alteration in the structure and in the spectrum of chemical atoms. In 1919 Stark was awarded the Nobel Prize for Physics for his “discovery of the Doppler effect in canal rays and the splitting of spectral lines in electric fields”. The prize enabled him to set up his own private laboratory.  In 1933 Stark was elected President of the Physikalisch-Technische Reichsanstalt (Physico-Technical Institute) as successor to von Paschen, where he remained until his retirement in 1939. At the same time he held the post of President of the Deutsche Forschungsgemeinschaft (German Research Association).  Stark was a prolific writer and published more than 300 scientific papers. His book *Die Elektrizität in Gasen* (Electricity in gases) was published in 1902. This was followed by works on elementary radiation and electrical spectroscopic analysis of chemical atoms. In connection with his work on chemical valency he wrote a book *Die Elektrizität im chemischen Atom* (Electricity in the chemical atom). Stark founded the *Jahrbuch der Radioaktivität und Elektronik* (The Year Book of Radioactivity and Electronics) and edited this publication from 1904 until 1913.  Johannes Stark was a corresponding member of the Academies in Göttingen, Rome, Leyden, Vienna and Calcutta, and was awarded the Baumgartner Prize of the Vienna Academy of Sciences in 1910 and the Vahlbruch Prize of the Göttingen Academy of Sciences in 1914, and also the Matteucci Medal of the Rome Academy.  During the last years of his life Stark, in his private laboratory on his country estate Eppenstatt near Traunstein in Upper Bavaria, investigated the effect of light deflection in an unhomogeneous electric field.  He was married to Luise Uepler. They had five children. His recreations were forestry and cultivation of fruit trees.  Stark died on June 21, 1957. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0206 |
| **Biographical** | Max Karl Ernst Ludwig Planck was born in Kiel, Germany, on April 23, 1858, the son of Julius Wilhelm and Emma (*née* Patzig) Planck. His father was Professor of Constitutional Law in the University of Kiel, and later in Göttingen.  Planck studied at the Universities of Munich and Berlin, where his teachers included Kirchhoff and Helmholtz, and received his doctorate of philosophy at Munich in 1879. He was Privatdozent in Munich from 1880 to 1885, then Associate Professor of Theoretical Physics at Kiel until 1889, in which year he succeeded Kirchhoff as Professor at Berlin University, where he remained until his retirement in 1926. Afterwards he became President of the Kaiser Wilhelm Society for the Promotion of Science, a post he held until 1937. The Prussian Academy of Sciences appointed him a member in 1894 and Permanent Secretary in 1912.  Planck’s earliest work was on the subject of thermodynamics, an interest he acquired from his studies under Kirchhoff, whom he greatly admired, and very considerably from reading R. Clausius’ publications. He published papers on entropy, on thermoelectric ity and on the theory of dilute solutions.  At the same time also the problems of radiation processes engaged his attention and he showed that these were to be considered as electromagnetic in nature. From these studies he was led to the problem of the distribution of energy in the spectrum of full radiation. Experimental observations on the wavelength distribution of the energy emitted by a black body as a function of temperature were at variance with the predictions of classical physics. Planck was able to deduce the relationship between the ener gy and the frequency of radiation. In a paper published in 1900, he announced his derivation of the relationship: this was based on the revolutionary idea that the energy emitted by a resonator could only take on discrete values or quanta. The energy for a resonator of frequency *v* is *hv* where *h* is a universal constant, now called Planck’s constant.  This was not only Planck’s most important work but also marked a turning point in the history of physics. The importance of the discovery, with its far-reaching effect on classical physics, was not appreciated at first. However the evidence for its validi ty gradually became overwhelming as its application accounted for many discrepancies between observed phenomena and classical theory. Among these applications and developments may be mentioned Einstein’s explanation of the photoelectric effect.  Planck’s work on the quantum theory, as it came to be known, was published in the *Annalen der Physik*. His work is summarized in two books *Thermodynamik* (Thermodynamics) (1897) and *Theorie der Wärmestrahlung* (Theory of heat radiat ion) (1906).  He was elected to Foreign Membership of the Royal Society in 1926, being awarded the Society’s Copley Medal in 1928.  Planck faced a troubled and tragic period in his life during the period of the Nazi government in Germany, when he felt it his duty to remain in his country but was openly opposed to some of the Government’s policies, particularly as regards the persecuti on of the Jews. In the last weeks of the war he suffered great hardship after his home was destroyed by bombing.  He was revered by his colleagues not only for the importance of his discoveries but for his great personal qualities. He was also a gifted pianist and is said to have at one time considered music as a career.  Planck was twice married. Upon his appointment, in 1885, to Associate Professor in his native town Kiel he married a friend of his childhood, Marie Merck, who died in 1909. He remarried her cousin Marga von Hösslin. Three of his children died young, leaving him with two sons.  He suffered a personal tragedy when one of them was executed for his part in an unsuccessful attempt to assassinate Hitler in 1944.  He died at Göttingen on October 4, 1947. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0207 |
| **Biographical** | Charles Glover Barkla was born on June 7, 1877 at Widnes, Lancashire, England, where his father, J.M. Barkla, was Secretary to the Atlas Chemical Company. He was educated at the Liverpool Institute and entered University College, Liverpool, in 1894 to study mathematics and physics, the latter under Oliver Lodge. He graduated with First Class Honours in Physics in 1898 and in the following year he obtained his master’s degree. Also in 1899, he was awarded a research scholarship by the Royal Commissioners for the Exhibition of 1851 and he proceeded to Trinity College, Cambridge, to work in the Cavendish Laboratory with [J. J. Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1906/index.html). He migrated to King’s College during 1900 and in 1902 returned to Liverpool as Oliver Lodge Fellow. From 1905 to 1909 he was successively demonstrator, assistant lecturer in physics and special lecturer in advanced electricity at the University, and in 1909 he succeeded H. A. Wilson as Wheatstone Professor of Physics in the University of London. In 1913, Barkla accepted the Chair in Natural Philosophy in the University of Edinburgh and he held this position until his death.  Barkla’s first researches concerned the velocity of electric waves along wires but in 1902 he commenced his investigations on Röntgen radiation which were to occupy almost his whole life. His discovery of homogeneous radiations characteristic of the elements showed that these elements had their characteristic line spectra in X-ray and he was the first to show that secondary emission is of two kinds, one consisting of X-rays scattered unchanged, and the other a fluorescent radiation peculiar to the particular substance. He discovered the polarisation of X-rays, an experimental result of considerable importance for it meant that X-radiation could be regarded as similar to ordinary light. Barkla made valuable contributions to present knowledge on the absorption and photographic action of X-rays and his later work demonstrated the relation between the characteristic X-radiation and the corpuscular radiation accompanying it. He has also shown both the applicability and the limitation of the quantum theory in relation to Röntgen radiation. The results of his findings are recorded in various papers which have appeared mainly in the Transactions and Proceedings of the Royal Society. He had a considerable reputation as an examiner in physics.  Barkla, a Fellow of the Royal Society, had several honorary degrees. He was appointed Bakerian Lecturer (Royal Society) in 1916 and he was awarded the Hughes Medal in the following year.  Charles Glover Barkla married Mary Esther, the eldest daughter of John T. Cowell of Douglas, Receiver-General of the Isle of Man, in 1907. They had two sons and one daughter. Their youngest son, Flight Lieutenant Michael Barkla, a brilliant scholar, was killed in action in 1943. Barkla’s chief recreation was singing – he had a powerful baritone voice and he was a member of the King’s College Chapel Choir, 1901-1902. Latterly, he had also become fond of golf.  Barkla died at his home, Braidwood, Edinburgh, on October 23, 1944. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0208 |
| **Biographical** | William Henry Bragg was born at Westward, Cumberland, on July 2, 1862. He was educated at Market Harborough Grammar School and afterwards at King William’s College, Isle of Man. Elected a minor scholar of Trinity College, Cambridge, in 1881, he studied mathematics under the well-known teacher, Dr. E. J. Routh. He was Third Wrangler in the Mathematical Tripos, Part I, in June 1884, and was placed in the first class in Part II in the following January. He studied physics in the Cavendish Laboratory during part of 1885, and at the end of that year was elected to the Professorship of Mathematics and Physics in the University of Adelaide, South Australia. Subsequently he became successively Cavendish Professor of Physics at Leeds (1909-1915), Quain Professor of Physics at University College London (1915-1925), and Fullerian Professor of Chemistry in the Royal Institution.  His research interests embraced a great many topics and he was an adept at picking up a subject, almost casually, making an important contribution, then dropping it again. However, the work of Bragg and his son Lawrence in 1913-1914 founded a new branch of science of the greatest importance and significance, the analysis of crystal structure by means of X-rays. If the fundamental discovery of the wave aspect of X-rays, as evidenced by their diffraction in crystals, was due to [von Laue](https://www.nobelprize.org/nobel_prizes/physics/laureates/1914/index.html) and his collaborators, it is equally true that the use of X-rays as an instrument for the systematic revelation of the way in which crystals are built was entirely due to the Braggs. This was recognized by the award of the [Nobel Prize](https://www.nobelprize.org/nobel_prizes/physics/laureates/1915/index.html) jointly to father and son in 1915.  During the First World War, Bragg was put in charge of research on the detection and measurement of underwater sounds in connection with the location of submarines. It was probably in acknowledgement of his work, as well as of his scientific eminence, that Bragg was made a C.B.E. in 1917 and was knighted in 1920. The Order of Merit followed in 1931. After having been a Fellow since 1907, he was elected President of the Royal Society in 1935.  He was an honorary doctor of some sixteen universities, and a member of the leading foreign societies. Many other medals and awards were bestowed upon him among which may be mentioned the Rumford Medal in 1916 and the Copley Medal (its premier award) in 1930.  He was the author of many books, including *Studies in Radioactivity; X-Rays and Crystal Structure; The World of Sound; Concerning the Nature of Things; Old Trades and New Knowledge; An Introduction to Crystal Analysis,* and *The Universe of Light.* His favorite recreation was golf.  In 1889 he married Gwendoline Todd, daughter of Sir Charles Todd, F.R.S., Postmaster General and Government Astronomer of South Australia. Their son [William Lawrence Bragg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1915/index.html) shared the Nobel Prize with his father.  After a life of astonishing productiveness, Sir William Bragg died on March 10, 1942. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0209 |
| **Biographical** | William Lawrence Bragg, son of William Henry Bragg, was born in Adelaide, South Australia, on March 31, 1890. He received his early education at St. Peter’s College in his birthplace, proceeding to Adelaide University to take his degree in mathematics with first-class honours in 1908. He came to England with his father in 1909 and entered Trinity College, Cambridge, as an Allen Scholar, taking first-class honours in the Natural Science Tripos in 1912. In the autumn of this year he commenced his examination of the [von Laue](https://www.nobelprize.org/nobel_prizes/physics/laureates/1914/index.html) phenomenon and published his first paper on the subject in the *Proceedings of the Cambridge Philosophical Society* in November.  In 1914 he was appointed as Fellow and Lecturer in Natural Sciences at Trinity College and the same year he was awarded the Barnard Medal. From 1912 to 1914 he had been working with his father, and the results of their work were published in an abridged form in *X-rays and Crystal Structure* (1915). It was this work which earned them jointly the [Nobel Prize for Physics](https://www.nobelprize.org/nobel_prizes/physics/laureates/1915/index.html) in 1915, and from this year to 1919, W. L. Bragg served as Technical Advisor on Sound Ranging to the Map Section, G.H.Q., France, receiving the O.B.E. and the M.C. in 1918. He was appointed Langworthy Professor of Physics at Manchester University in 1919, and held this post till 1937.  W. Lawrence Bragg, who had been elected Fellow of the Royal Society in 1921, was Director of the National Physical Laboratory in 1937-1938 and Cavendish Professor of Experimental Physics, Cambridge, from 1938 to 1953. He was Chairman of the Frequency Advisory Committee from 1958 to 1960.  Knighted in 1941, Sir Lawrence holds the degree of M.A. (Cambridge), Honorary D.Sc. (Dublin, Leeds, Manchester, Lisbon, Paris, Brussels, Liege, and Durham), honorary Ph.D. (Cologne), and honorary LL.D. (St.Andrews). He has many honorary fellowships and is an honorary or foreign member of American, French, [Swedish](http://www.kva.se/), Chinese, Dutch, and Belgian Scientific Academies, besides being Membre d’Honneur de la Société Française de Minéralogies et Cristallographie.  He was awarded the Hughes Medal of the Royal Society in 1931; the Royal Medal of the same Society in 1946, and the Roebling Medal of the Mineral Society of America in 1948.  Together with his father, he has published various scientific papers on crystal structure after their joint publication of 1915: *The Crystalline State* (1934), *Electricity* (1936), and *Atomic Structure of Minerals* (1937).  Sir Lawrence’s chief interests at the present time are the application of X-ray analysis to the structure of protein molecules, which are being investigated in the Davy Faraday Laboratory of the Royal Institution, in continuation of similar work at the Cavendish Laboratory, Cambridge. This collaboration has succeeded in determining for the first time the structure of the highly complex molecules of living matter.  Having been awarded the Nobel Prize at the very early age of 25, W. Lawrence Bragg was the youngest-ever laureate. The very rare opportunity of celebrating a golden jubilee as a Nobel Laureate was given special attention during the December ceremonies at Stockholm in 1965, when Sir Lawrence, at the invitation of the Nobel Foundation, delivered a lecture – the first Nobel Guest Lecture – in retrospect, on developments in his field of interest during the last fifty years.  In 1921 he married Alice Grace Jenny (*née* Hopkinson) of Cambridge, and they have two sons (the elder of whom became chief scientist with Rolls Royce, while the younger entered a Cambridge instrument-making firm), and two daughters (the elder of whom married an official of the Foreign Office, while the second married the son of the Master of Corpus Christi College, Cambridge). |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0210 |
| **Biographical** | Max Laue was born on October 9, 1879 at Pfaffendorf, near Koblenz. He was the son of Julius von Laue, an official in the German military administration, who was raised to hereditary nobility in 1913 and who was often sent to various towns, so that von Laue spent his youth in Brandenburg, Altona, Posen, Berlin and Strassburg, going to school in the three last-named cities. At the Protestant school at Strassburg he came under the influence of Professor Goering, who introduced him to the exact sciences.  In 1898 he left school and for a year did his military service. He then went to the University of Strassburg where he studied mathematics, physics and chemistry; but soon he moved to the University of Göttingen, where he worked under Professor W. Voigt and Professor W. Abraham, who greatly influenced him. After a semester at the University of Munich he went, in 1902, to the University of Berlin to work under Professor [Max Planck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1918/index.html). Here he attended lectures by O. Lummer on interference spectroscopy and heat radiation, the influence of which was shown in von Laue’s dissertation on interference phenomena in plane-parallel plates.  After obtaining his doctorate at Berlin in 1903, von Laue went for two years to the University of Göttingen. In 1905 he was offered the post of assistant to Max Planck at the Institute for Theoretical Physics at Berlin. Here he worked on the application of entropy to radiation fields and on the thermodynamic significance of the coherence of light waves.  In 1909 he went as Privatdozent to the University of Munich, where he lectured on optics, thermodynamics and the theory of relativity and in 1912 he became Professor of Physics at the University of Zurich. In 1914 he moved, as Professor of Physics, to Frankfurt on Main and from 1916 he was engaged in war work at the University of Würzburg on high vacuum tubes used for telephony and wireless communication. In 1919 he was appointed Professor of Physics at the University of Berlin, a post which he held until 1943. From 1934 onwards he acted as consultant to the Physikalisch-Technische Reichsanstalt at Berlin-Charlottenburg.  In 1917, when the Institute for Physics was established at Berlin-Dahlem with [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/index.html) as its Director, von Laue had charge, as Second Director, of most of the administrative work of this Institute, which was in close touch with German scientific research. Von Laue exerted, during this period and also later, considerable influence on the development of scientific research in Germany. When Berlin was bombed, this Institute moved to Hechingen, in Württemberg and von Laue accompanied it there. He remained at Hechingen from 1944 until 1945 and here, to distract his thoughts from the war, he wrote a History of Physics, which went into four editions and was translated into seven other languages. Here he welcomed the arrival of the French troops and was taken by an Anglo-American mission, together with nine other German scientists, to England where he remained until 1946 During his confinement in England he wrote a paper on the low absorption of X-rays during diffraction, which he contributed in 1948, to the International Union of Crystallographers at Harvard University. In 1946 he went to Göttingen as Acting Director of the Max Planck Institute and Titular Professor in the University there.  In 1951 he was elected Director of the Fritz Haber Institute for Physical Chemistry at Berlin-Dahlem and here he did much work on X-ray optics in collaboration with Borrmann and others.  In 1958 he retired and in 1959 his 80th birthday was celebrated in Berlin-Dahlem. He lived on, still actively at work, for another six months.  Apart from his earlier work already mentioned, von Laue’s scientific work extended over a wide field. Early in his career he was greatly excited by Einstein’s theory of relativity and between 1907 and 1911 he published eight papers on the application of this theory. In 1911 he published a book on the restricted theory and in 1921 another on the general theory, both books going into several editions  His best known work, however, for which he received the Nobel Prize for Physics for 1914, was his discovery of the diffraction of X-rays on crystals. This discovery originated, as he related in his Nobel Lecture, when he was discussing problems related to the passage of waves of light through a periodic, crystalline arrangement of particles. The idea then came to him that the much shorter electromagnetic rays, which X-rays were supposed to be, would cause in such a medium some kind of diffraction or interference phenomena and that a crystal would provide such a medium. Although his colleagues Sommerfeld, W. Wien and others, with whom he discussed the idea on a skiing expedition, raised objections to the idea, W. Friedrich, one of Sommerfeld’s assistants and P. Knipping tested it out experimentally and, after some failures, succeeded in proving it to be correct. Von Laue worked out the mathematical formulation of it and the discovery was published in 1912. It established the fact that X-rays are electromagnetic in nature and it opened the way to the later work of [Sir William and Sir Lawrence Bragg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1915/index.html). Subsequently von Laue made other contributions to this subject.  Also prominent in von Laue’s work were his contributions to the problems of superconductivity which he made when he was Professor of Theoretical Physics at Berlin University. At this time Walther Meissner was studying at the Physikalisch-Technische Reichsanstalt in Berlin, the remarkable disappearance of ohmic resistance shown by many metals at temperatures of the order of that of liquid helium. An especially valuable contribution then made by von Laue was his explanation, in 1932, of the fact that the threshold of the applied magnetic field which destroys superconductivity varies with the shape of the body because, when the magnetic field is established after the state of superconductivity has been established, the magnetic field is deformed by the supercurrents induced at the surface of the metal being used. This explanation was confirmed and it opened the way to Meissner’s subsequent discovery that a superconductor eliminates the whole magnetic field in its interior and this became the basic idea of F. and H. London’s theory of superconductivity. Von Laue published one paper in collaboration with F. and H. London and between 1937 and 1947 he published a total of 12 papers and a book on this subject.  Among the many honours and distinctions which he was awarded were the Ladenburg Medal, the Max-Planck Medal and the Bimala-Churn-Law Gold Medal of the Indian Association at Calcutta. He held Honorary Doctorates of the Universities of Bonn, Stuttgart, Munich, Berlin, Manchester and Chicago, was a member of the Russian Academy and the Academy of Sciences of Berlin, the German Physical Society and Mathematical Society, the Kant Society, the Academy of Sciences of Vienna, the American Physical Society, the Société Française de Physique and the Société Française de Mineralogie et Crystallographie. He was also Honorary Senator of the MaxPlanck Society and Honorary Member of the German Röntgen Society, and Corresponding Member of the Academies of Sciences of Göttingen, Munich, Turin, Stockholm, Rome (Papal), Madrid, the Academia dei Lincei of Rome, and the Royal Society of London. In 1948 he became Honorary President of the International Union of Crystallographers, in 1952 he was made a Knight of the Order Pour le Mérite, in 1953 he received the Grand Cross with Star for Federal Services, and in 1957 he became an Officer of the Legion of Honour of France.  Much esteemed by his contemporaries for his character and sound judgment, von Laue’s opinions were often sought and during his life he exerted great influence on the direction and development of German scientific work. Among his characteristics were a deep love and admiration of Prussia and a strong sense of justice and fair play. When Hitler and the National Socialist Party were in power, he defended, even at the risk of reprimand or personal injury, scientific views, such as the theory of relativity, which were not approved by the Party or by such strong adherents to it as the physicist Lenard. When Einstein resigned from the Berlin Academy and the Vice-President of this Academy stated that this was no loss, von Laue was the only member of the Academy who protested.  Chief among his recreations were sailing, skiing, mountaineering and motoring. Von Laue was not a rock-climber, but preferred to tour the Alpine glaciers with his scientific friends. As a motorist he was famous in Berlin, first on the motor bicycle on which he went at high speed to his lectures; and later in a car. He loved high speeds, but never, until the fatal collision that ended his life, had any accident.  In his later years he suffered from attacks of depression and a feeling of being persecuted by scientists and by the military authorities, whom he disliked intensely. Usually, however, he successfully overcame these attacks and regained his sense of humour and joy in life. He did not practise any art, but he took an interest in many arts, especially in classical music; and he read widely history and the philosophy of science. He thought of the stars, the mountain peaks and the achievements of the human with awe and humility and was at heart a deeply religious man. He asked that his tombstone should bear the statement that he died trusting firmly in the mercy of God.  In 1910 von Laue married Magdalena Degen.  On April 8, 1960, when he was driving alone to his laboratory, a motor cyclist, who had only received his licence two days previously, collided with von Laue’s car. The motor cyclist was instantly killed and von Laue’s car overturned in the Berlin speedway and he was taken from beneath it by the Fire Brigade. Although he showed at first some signs of recovery from his injuries, he died of them on April 24, at the age of 80. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0211 |
| **Biographical** | Heike Kamerlingh Onnes was born on September 21, 1853, at Groningen, The Netherlands. His father, Harm Kamerlingh Onnes, was the owner of a brickworks near Groningen; his mother was Anna Gerdina Coers of Arnhem, the daughter of an architect.  After spending the allotted time at the “Hoogere Burgerschool” in his native town (secondary school without classical languages), the director of which was the later Professor of Chemistry at Leyden J.M. van Bemmelen, he received supplementary teaching in Greek and Latin. In 1870 he entered the University of Groningen, obtained his “candidaats” degree (approx. B.Sc.) the following year, and then went to Heidelberg as a student of Bunsen and Kirchhoff from October 1871 until April 1873. Thereafter he returned to Groningen, where he passed his “doctoraal” examination (approx. M.Sc.) in 1878 and obtained the doctor’s degree in 1879 with a remarkable thesis *Nieuwe bewijzen voor de aswenteling der aarde* (New proofs of the rotation of the earth).  Meanwhile in 1878 he had become assistant at the Polytechnicum at Delft, working under Bosscha, in whose place he also lectured in 1881 and 1882, the year in which he was appointed Professor of Experimental Physics and Meteorology at Leyden University, in succession to P.L. Rijke.  Kamerlingh Onnes’ talents for solving scientific problems was already apparent in 1871, when at the age of 18 he was awarded a Gold Medal for a competition sponsored by the Natural Sciences Faculty of the University of Utrecht, followed the next year by a Silver Medal for a similar event at the University of Groningen. When working with Kirchhoff he also won the “Seminarpreis”, entitling him to occupy one of the two existing assistantships under Kirchhoff.  In his doctor’s thesis theoretical as well as experimental proof was given that Foucault’s well-known pendulum experiment should be considered as a special case of a large group of phenomena which in a much simpler fashion can be used to prove the rotational movement of the earth. In 1881 he published a paper *Algemeene theorie der vloeistoffen* (General theory of liquids), which dealt with the kinetic theory of the liquid state, approaching Van der Waals’ law of corresponding states from a mechanistic point of view. This work can be considered as the beginning of his life-long investigations into the properties of matter at low temperatures. In his inaugural address *De beteekenis van het quantitatief onderzoek in de natnurkunde* (The importance of quantitative research in physics) he arrived at his well-known motto “Door meten tot weten” (Knowledge through measurement), an appreciation of the value of measurements which concerned him throughout his scientific career.  After his appointment to the Physics Chair at Leyden, Kamerlingh Onnes reorganized the Physical Laboratory (now known as the Kamerlingh Onnes Laboratory) in a way to suit his own programme. His researches were mainly based on the theories of his two great compatriots [J.D. van der Waals](https://www.nobelprize.org/nobel_prizes/physics/laureates/1910/index.html) and [H.A. Lorentz](https://www.nobelprize.org/nobel_prizes/physics/laureates/1902/index.html). In particular he had in mind the establishment of a cryogenic laboratory which would enable him to verify Van der Waals’ law of corresponding states over a large range of temperatures. His efforts to reach extremely low temperatures culminated in the liquefaction of helium in 1908. Bringing the temperature of the helium down to 0,9°K, he reached the nearest approach to absolute zero then achieved, thus justifying the saying that the coldest spot on earth was situated at Leyden. It was on account of these low-temperature studies that he was awarded the Nobel Prize. Later, his pupils W.H. Keesom and W.J. de Haas ( Lorentz’ son-in-law) conducted experiments in the same laboratory which led them still closer to absolute zero.  Other investigations in his laboratory which gradually gained in importance and international fame, included thermodynamics, the radioactivity law, and observations on optical, magnetic and electrical phenomena, such as the study of fluorescence and phosphorescence, the magnetic rotation of the polarization plane, absorption spectra of crystals in the magnetic field; also the Hall effect, dielectric constants, and especially the resistance of metals. A momentous discovery (1911) was that of the *superconductivity* of pure metals such as mercury, tin and lead at very low temperatures, and following from this the observation of *persisting currents.*  The results of Kamerlingh Onnes’ investigations were published in the *Proceedings of the Royal Academy of Sciences of Amsterdam* and also in the *Communications from the Physical Laboratory at Leyden*. Many foreign scientists came to Leyden to work in his laboratory for shorter or longer periods. The laboratory gained additional fame throughout the world through the training school for instrument-makers and glass-blowers housed in it, founded by Kamerlingh Onnes in 1901.  At the early age of 30, Kamerlingh Onnes was appointed a member of the Royal Academy of Sciences of Amsterdam. He was one of the founders of the Association (now Institut) International du Froid. He was a Commander in the Order of the Netherlands Lion, the Order of Orange-Nassau of the Netherlands, the Order of St. Olaf of Norway, and the Order of Polonia Restituta of Poland. He held an honorary doctorate of the University of Berlin, and was awarded the Matteucci Medal, the Rumford Medal, the Baumgarten Preis and the Franklin Medal. He was Member of the Society of Friends of Science in Moscow, and of the Academies of Sciences in Copenhagen, Uppsala, Turin, Vienna, Göttingen and Halle; Foreign Associate of the Académie des Sciences of Paris; Foreign Member of the Accademia dei Lincei of Rome and the Royal Society of London; and Honorary Member of the Physical Society of Stockholm, the Société Helvétique des Sciences Naturelles, the Royal Institution of London, the Sociedad Española da Física y Qumica of Madrid, and the Franklin Institute of Philadelphia.  Outside his scientific work, Kamerlingh Onnes’ favourite recreations were his family life and helpfulness to those who needed it. Although his work was his hobby, he was far from being a pompous scholar. A man of great personal charm and philanthropic humanity, he was very active during and after the First World War in smoothing out political differences between scientists and in succouring starving children in countries suffering from food shortage. In 1887 he married Maria Adriana Wilhelmina Elisabeth Bijleveld, who was a great help to him in these activities and who created a home widely known for its hospitality. They had one son, Albert, who became a high-ranking civil servant at The Hague.  Kamerlingh Onnes’ health had always been somewhat delicate, and, after a short illness, he died at Leyden on February 21, 1926. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0212 |
| **Biographical** | Nils Gustaf Dalén was born at Stenstorp in Skaraborg, Sweden on November 30, 1869, the son of a farmer. After his preliminary education, he entered a School of Agriculture to study dairy farming but he was later advised by Gustaf de Laval, who recognized his natural gift for mechanics, to seek a technical education. He prepared himself for the Chalmers Institute at Gothenburg and gained admission in 1892. He graduated as an engineer in 1896 and spent a year in Switzerland, studying under Professor Stodola at the Eidgenössisches Polytechnikum.  On his return to Sweden, Dalén carried out some research at Gothenburg and set up as a consulting engineer. He became Technical Chief of the Svenska Karbid- och Acetylen A.B. (Swedish Carbide and Acetylene, Ltd.) in 1901 and he later joined the Gas Accumulator Company where he became Chief Engineer in 1906. In 1909, the company was reorganized as Svenska Aktiebolaget Gasaccumulator (AGA) (Swedish Gas Accumulator Ltd.) with Dalén as Managing Director.  Dalén’s inventiveness first showed in his early days on his father’s farm when he built a threshing machine powered by an old spinning wheel. He contrived a device to indicate the butterfat content of milk and thereby made his contact with de Laval. On completion of his advanced education, he worked on the construction of a hot-air turbine and related air compressors and pumps. He also invented a pasteurization apparatus and a milking machine.  In 1901, Dalén’s company purchased the patent rights of the French invention of dissolved acetylene and he began his work on automatic flashing beacons for lighthouses. His subsequent invention of the sun-valve, which causes a beacon to light automatically at dusk and extinguish itself at dawn, enabled lighthouses to function perfectly and unattended for periods of up to a year. His invention of cylinder filled with a porous mass of asbestos and diatomaceous earth for storage of acetylene reduced considerably the hazards in handling this material and its use in welding became safe. He also invented a mixer for providing a constant and correct balance of gas and air for use in the incandescent mantle and a device for removing broken mantles and replacing them by new ones.  In 1912, whilst testing safety devices on cylinders of acetylene in an outdoor location, and when satisfactory safety precautions had been taken, a sudden explosion seriously injured Dalén and caused the loss of his eyesight. He recovered from his other injuries and overcoming his great incapacity, continued his researches. He was awarded the contract for lighting the Panama Canal and later turned to the field of thermal technics to invent a stove, now in universal use, which maintains cooking heat for 24 hours using only eight pounds of coal.  Dalén’s writings were few, but he left his mark in a practical way by the provision of light, and therefore safety, for the benefit of travellers by land, sea and air.  Amongst the many distinctions conferred upon Dalén are membership of the [Swedish Royal Academy of Sciences](http://www.kva.se/), 1913, and the Academy of Science and Engineering, 1919. He was made Honorary Doctor of Lund University in 1918 and received the Morehead Medal of the International Acetylene Association. He took part in debates at the National Society of Economics and served on the Lidingö City Council for almost twenty years.  Dalén married Elma Persson in 1901. They had two sons and two daughters. Their eldest son, Gunnar, qualified as an engineer and followed his father as a Director of AGA; their younger son, Anders, became a Doctor of Medicine; Gustaf’s brother Albin, a famous ophthalmologist, was a Professor at the [Caroline Institute](http://www.ki.se/).  Dalén died on December 9, 1937, in his villa at Lidingö. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0213 |
| **Biographical** | Wilhelm Wien was born on January 13, 1864 at Fischhausen, in East Prussia. He was the son of the landowner Carl Wien, and seemed destined for the life of a gentleman farmer, but an economic crisis and his own secret sense of vocation led him to University studies. When in 1866 his parents moved to Drachstein, in the Rastenburg district of East Prussia, Wien went to school in 1879 first at Rastenburg and later, from 1880 till 1882, at the City School at Heidelberg. After leaving school he went, in 1882, to the University of Göttingen to study mathematics and the natural sciences and in the same year also to the University of Berlin. From 1883 until 1885 he worked in the laboratory of Hermann von Helmholtz and in 1886 he took his doctorate with a thesis on his experiments on the diffraction of light on sections of metals and on the influence of materials on the colour of refracted light.  His studies were then interrupted by the illness of his father and, until 1890, he helped in the management of his father’s land. He was, however, able to spend, during this period, one semester with Helmholtz and in 1887 he did experiments on the permeability of metals to light and heat rays. When his father’s land was sold he returned to the laboratory of Helmholtz, who had been moved to, and had become President of, the Physikalisch-Technische Reichsanstalt, established for the study of industrial problems. Here he remained until 1896 when he was appointed Professor of Physics at Aix-la-Chapelle in succession to Philipp Lenard. In 1899, he was appointed Professor of Physics at the University of Giessen. In 1900 he became Professor of the same subject at Würzburg, in succession to [W.C. Röntgen](https://www.nobelprize.org/nobel_prizes/physics/laureates/1901/index.html), and in this year he published his *Lehrbuch der Hydrodynamik* (Textbook of hydrodynamics).  In 1902 he was invited to succeed Ludwig Boltzmann as Professor of Physics at the University of Leipzig and in 1906 to succeed Drude as Professor of Physics at the University of Berlin; but he refused both these invitations.  In 1920 he was appointed Professor of Physics at Munich, where he remained throughout the rest of his life.  In addition to the early work already mentioned, Wien worked, at the Physikalisch-Technische Reichsanstalt, with Holborn on methods of measuring high temperatures with the Le Chatelier thermoelements and at the same time did theoretical work on thermodynamics, especially on the laws governing the radiation of heat.  In 1893 he announced the law which states that the wavelength changes with the temperature, a law which later became the law of displacement.  In 1894 he published a paper on temperature and the entropy of radiation, in which the terms temperature and entropy were extended to radiation in empty space. In this work he was led to define an ideal body, which he called the black body, which completely absorbs all radiations. In 1896 he published the formula of Wien, which was the result of work undertaken to find a formula for the composition of the radiation of such a black body. Later it was proved that this formula is valid only for the short waves, but Wien’s work enabled Max Planck to resolve the problem of radiation in thermal equilibrium by means of quantum physics. For this work Wien was awarded the Nobel Prize for Physics for 1911. An interesting point about it is that this theoretical work came from an Institute devoted to technical problems and it led to new techniques for illumination and the measurement of high temperatures.  When Wien moved, in 1896, to Aix-la-Chapelle to succeed Lenard, he found there a laboratory equipped for the study of electrical discharges *in vacuo* and in 1897 he began to work on the nature of cathode rays. Using a very high vacuum tube with a Lenard window, he confirmed the discovery that dean Perrin had made two years earlier, that cathode rays are composed of rapidly-moving, negatively-charged particles (electrons). And then, almost at the same time as [Sir J.J. Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1906/index.html) in Cambridge, but by a different method, he measured the relation of the electric charge on these particles to their mass and found, as Thomson did, that they are about two thousand times lighter than the atoms of hydrogen.  In 1898 Wien studied the canal rays discovered by Goldstein and concluded that they were the positive equivalent of the negatively-charged cathode rays. He measured their deviation by magnetic and electric fields and concluded that they are composed of positively-charged particles never heavier than electrons.  The method used by Wien resulted some 20 years later in the spectrography of masses, which has made possible the precise measurement of the masses of various atoms and their isotopes, necessary for the calculation of the energies released by nuclear reactions. In 1900 Wien published a theoretical paper on the possibility of an electromagnetic basis for mechanics. Subsequently he did further work on the canal rays, showing, in 1912, that, if the pressure is not extremely weak, these rays lose and regain, by collision with atoms of residual gas, their electric charge along their course of travel. In 1918 he published further work on these rays on the measurement of the progressive decrease of their luminosity after they leave the cathode and from these experiments he deduced what classical physics calls the decay of the luminous vibrations in the atoms, which corresponds in quantum physics to the limited duration of excited states of atoms.  In this, and other, respects Wien’s work contributed to the transition from Newtonian to quantum physics. As [Max von Laue](https://www.nobelprize.org/nobel_prizes/physics/laureates/1914/index.html) wrote of him, his “immortal glory” was that “he led us to the very gates of quantum physics”.  Wien was a member of the Academies of Sciences of Berlin, Göttingen, Vienna, [Stockholm](http://www.kva.se/), Christiania and Washington, and an Honorary member of the Physical Society of Frankfurt-on-Main.  In 1898 he married Luise Mehler of Aix-la-Chapelle. They had four children. He died in Munich on August 30, 1928. |
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| **Physics\_1999-** | |
| **ID** | 0214 |
| **Biographical** | Johannes Diderik van der Waals was born on November 23, 1837 in Leyden, The Netherlands, the son of Jacobus van der Waals and Elisabeth van den Burg. After having finished elementary education at his birthplace he became a schoolteacher. Although he had no knowledge of classical languages, and thus was not allowed to take academic examinations, he continued studying at Leyden University in his spare time during 1862-65. In this way he also obtained teaching certificates in mathematics and physics.  In 1864 he was appointed teacher at a secondary school at Deventer; in 1866 he moved to The Hague, first as teacher and later as Director of one of the secondary schools in that town.  New legislation whereby university students in science were exempted from the conditions concerning prior classical education enabled Van der Waals to sit for university examinations. In 1873 he obtained his doctor’s degree for a thesis entitled *Over de Continuïteit van den Gas – en Vloeistoftoestand* (On the continuity of the gas and liquid state), which put him at once in the foremost rank of physicists. In this thesis he put forward an “Equation of State” embracing both the gaseous and the liquid state; he could demonstrate that these two states of aggregation not only merge into each other in a continuous manner, but that they are in fact of the same nature. The importance of this conclusion from Van der Waals’ very first paper can be judged from the remarks made by James Clerk Maxwell in Nature, “that there can be no doubt that the name of Van der Waals will soon be among the foremost in molecular science” and “It has certainly directed the attention of more than one inquirer to the study of the Low-Dutch language in which it is written” (Maxwell probably meant to say “Low-German”, which would also be incorrect, since Dutch is a language in its own right). Subsequently, numerous papers on this and related subjects were published in the *Proceedings of the Royal Netherlands Academy of Sciences* and in the *Archives Néerlandaises*, and they were also translated into other languages.  When, in 1876, the new Law on Higher Education was established which promoted the old Athenaeum Illustre of Amsterdam to university status, Van der Waals was appointed the first Professor of Physics. Together with [Van’t Hoff](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1901/index.html) and Hugo de Vries, the geneticist, he contributed to the fame of the University, and remained faithful to it until his retirement, in spite of enticing invitations from elsewhere.  The immediate cause of Van der Waals’ interest in the subject of his thesis was R. Clausius’ treatise considering heat as a phenomenon of motion, which led him to look for an explanation for T. Andrews’ experiments (1869) revealing the existence of “critical temperatures ” in gases. It was Van der Waals’ genius that made him see the necessity of taking into account the volumes of molecules and the intermolecular forces (“Van der Waals forces”, as they are now generally called) in establishing the relationship between the pressure, volume and temperature of gases and liquids.  A second great discovery – arrived at after much arduous work – was published in 1880, when he enunciated the Law of Corresponding States. This showed that if pressure is expressed as a simple function of the critical pressure, volume as one of the critical volume, and temperature as one of the critical temperature, a general form of the equation of state is obtained which is applicable to all substances, since the three constants *a*, *b*, and *R* in the equation, which can be expressed in the critical quantities of a particular substance, will disappear. It was this law which served as a guide during experiments which ultimately led to the liquefaction of hydrogen by J. Dewar in 1898 and of helium by [H. Kamerlingh Onnes](https://www.nobelprize.org/nobel_prizes/physics/laureates/1913/index.html) in 1908. The latter, who in 1913 received the Nobel Prize for his low-temperature studies and his production of liquid helium, wrote in 1910 “that Van der Waals’ studies have always been considered as a magic wand for carrying out experiments and that the Cryogenic Laboratory at Leyden has developed under the influence of his theories “.  Ten years later, in 1890, the first treatise on the “Theory of Binary Solutions” appeared in the *Archives Néerlandaises* – another great achievement of Van der Waals. By relating his equation of state with the Second Law of Thermodynamics, in the form first proposed by W. Gibbs in his treatises on the equilibrium of heterogeneous substances, he was able to arrive at a graphical representation of his mathematical formulations in the form of a surface which he called “Psi-surface” in honour of Gibbs, who had chosen the Greek letter Psi as a symbol for the free energy, which he realised was significant for the equilibrium. The theory of binary mixtures gave rise to numerous series of experiments, one of the first being carried out by J. P. Kuenen, who found characteristics of critical phenomena fully predictable by the theory. Lectures on this subject were subsequently assembled in the *Lehrbuch der Thermodynamik* (Textbook of thermodynamics) by Van der Waals and Ph. Kohnstamm.  Mention should also be made of Van der Waals’ thermodynamic theory of capillarity, which in its basic form first appeared in 1893. In this, he accepted the existence of a gradual, though very rapid, change of density at the boundary layer between liquid and vapour – a view which differed from that of Gibbs, who assumed a sudden transition of the density of the fluid into that of the vapour. In contrast to Laplace, who had earlier formed a theory on these phenomena, Van der Waals also held the view that the molecules are in permanent, rapid motion. Experiments with regard to phenomena in the vicinity of the critical temperature decided in favour of Van der Waals’ concepts.  Van der Waals was the recipient of numerous honours and distinctions, of which the following should be particularly mentioned. He received an honorary doctorate of the University of Cambridge; was made honorary member of the Imperial Society of Naturalists of Moscow, the Royal Irish Academy and the American Philosophical Society; corresponding member of the Institut de France and the Royal Academy of Sciences of Berlin; associate member of the Royal Academy of Sciences of Belgium; and foreign member of the Chemical Society of London, the National Academy of Sciences of the U.S.A., and of the Accademia dei Lincei of Rome.  In 1864, Van der Waals married Anna Magdalena Smit, who died early. He never married again. They had three daughters and one son. The daughters were Anne Madeleine who, after her mother’s early death, ran the house and looked after her father; Jacqueline Elisabeth, who was a teacher of history and a well-known poetess; and Johanna Diderica, who was a teacher of English. The son, Johannes Diderik Jr., was Professor of Physics at Groningen University 1903-08, and subsequently succeeded his father in the Physics Chair of the University of Amsterdam.  Van der Waals’ main recreations were walking, particularly in the country, and reading. He died in Amsterdam on March 8, 1923. |
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| **ID** | 0215 |
| **Biographical** | Guglielmo Marconi was born at Bologna, Italy, on April 25, 1874, the second son of Giuseppe Marconi, an Italian country gentleman, and Annie Jameson, daughter of Andrew Jameson of Daphne Castle in the County Wexford, Ireland. He was educated privately at Bologna, Florence and Leghorn. Even as a boy he took a keen interest in physical and electrical science and studied the works of Maxwell, Hertz, Righi, Lodge and others. In 1895 he began laboratory experiments at his father’s country estate at Pontecchio where he succeeded in sending wireless signals over a distance of one and a half miles.  In 1896 Marconi took his apparatus to England where he was introduced to Mr. (later Sir) William Preece, Engineer-in-Chief of the Post Office, and later that year was granted the world’s first patent for a system of wireless telegraphy. He demonstrated his system successfully in London, on Salisbury Plain and across the Bristol Channel, and in July 1897 formed The Wireless Telegraph & Signal Company Limited (in 1900 re-named Marconi’s Wireless Telegraph Company Limited). In the same year he gave a demonstration to the Italian Government at Spezia where wireless signals were sent over a distance of twelve miles. In 1899 he established wireless communication between France and England across the English Channel. He erected permanent wireless stations at The Needles, Isle of Wight, at Bournemouth and later at the Haven Hotel, Poole, Dorset.  In 1900 he took out his famous patent No. 7777 for “tuned or syntonic telegraphy” and, on an historic day in December 1901, determined to prove that wireless waves were not affected by the curvature of the Earth, he used his system for transmitting the first wireless signals across the Atlantic between Poldhu, Cornwall, and St. John’s, Newfoundland, a distance of 2100 miles.  Between 1902 and 1912 he patented several new inventions. In 1902, during a voyage in the American liner “Philadelphia”, he first demonstrated “daylight effect” relative to wireless communication and in the same year patented his magnetic detector which then became the standard wireless receiver for many years. In December 1902 he transmitted the first complete messages to Poldhu from stations at Glace Bay, Nova Scotia, and later Cape Cod, Massachusetts, these early tests culminating in 1907 in the opening of the first transatlantic commercial service between Glace Bay and Clifden, Ireland, after the first shorter-distance public service of wireless telegraphy had been established between Bari in Italy and Avidari in Montenegro. In 1905 he patented his horizontal directional aerial and in 1912 a “timed spark” system for generating continuous waves.  In 1914 he was commissioned in the Italian Army as a Lieutenant being later promoted to Captain, and in 1916 transferred to the Navy in the rank of Commander. He was a member of the Italian Government mission to the United States in 1917 and in 1919 was appointed Italian plenipotentiary delegate to the Paris Peace Conference. He was awarded the Italian Military Medal in 1919 in recognition of his war service.  During his war service in Italy he returned to his investigation of short waves, which he had used in his first experiments. After further tests by his collaborators in England, an intensive series of trials was conducted in 1923 between experimental installations at the Poldhu Station and in Marconi’s yacht “Elettra” cruising in the Atlantic and Mediterranean, and this led to the establishment of the beam system for long distance communication. Proposals to use this system as a means of Imperial communications were accepted by the British Government and the first beam station, linking England and Canada, was opened in 1926, other stations being added the following year.  In 1931 Marconi began research into the propagation characteristics of still shorter waves, resulting in the opening in 1932 of the world’s first microwave radiotelephone link between the Vatican City and the Pope’s summer residence at Castel Gandolfo. Two years later at Sestri Levante he demonstrated his microwave radio beacon for ship navigation and in 1935, again in Italy, gave a practical demonstration of the principles of radar, the coming of which he had first foretold in a lecture to the American Institute of Radio Engineers in New York in 1922.  He has been the recipient of honorary doctorates of several universities and many other international honours and awards, among them the Nobel Prize for Physics, which in 1909 he shared with Professor Karl Braun, the Albert Medal of the Royal Society of Arts, the John Fritz Medal and the Kelvin Medal. He was decorated by the Tsar of Russia with the Order of St. Anne, the King of Italy created him Commander of the Order of St. Maurice and St. Lazarus, and awarded him the Grand Cross of the Order of the Crown of Italy in 1902. Marconi also received the freedom of the City of Rome (1903), and was created Chevalier of the Civil Order of Savoy in 1905. Many other distinctions of this kind followed. In 1914 he was both created a Senatore in the Italian Senate and app ointed Honorary Knight Grand Cross of the Royal Victorian Order in England. He received the hereditary title of Marchese in 1929.  In 1905 he married the Hon. Beatrice O’Brien, daughter of the 14th Baron Inchiquin, the marriage being annulled in 1927, in which year he married the Countess Bezzi-Scali of Rome. He had one son and two daughters by his first and one daughter by his second wife. His recreations were hunting, cycling and motoring.  Marconi died in Rome on July 20, 1937. |
| **Autobiographical** |  |
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| **ID** | 0216 |
| **Biographical** | Karl Ferdinand Braun was born on June 6, 1850 at Fulda, where he was educated at the local “Gymnasium” (grammar school). He studied at the Universities of Marburg and Berlin and graduated in 1872 with a paper on the oscillations of elastic strings. He worked as assistant to Professor Quincke at Würzburg University and in 1874 accepted a teaching appointment to the St. Thomas Gymnasium in Leipzig. Two years later he was appointed Extraordinary Professor of Theoretical Physics at the University of Marburg, and in 1880 he was invited to fill a similar post at Strasbourg University. Braun was made Professor of Physics at the Technische Hochschule in Karlsruhe in 1883 and was finally invited by the University of Tübingen in 1885; one of his tasks there was to build a new Physics Institute. Ten years later, in 1895, he returned to Strasbourg as Principal of the Physics Institute, where he remained, in spite of an invitation from Leipzig University to succeed G. Wiedemann.  Braun’s first investigations were concerned with oscillations of strings and elastic rods, especially with regard to the influence of the amplitude and environment of rods on their oscillations. Other studies were based on thermodynamic principles, such as those on the influence of pressure on the solubility of solids.  His most important works, however, were in the field of electricity. He published papers on deviations from Ohm’s law and on the calculations of the electromotive force of reversible galvanic elements from thermal sources. His practical experiments led him to invent what is now called Braun’s electrometer, and also a cathode-ray oscillograph, constructed in 1897.  In 1898 he started to occupy himself with wireless telegraphy, by attempting to transmit Morse signals through water by means of high-frequency currents. Subsequently he introduced the closed circuit of oscillation into wireless telegraphy, and was one of the first to send electric waves in definite directions. In 1902 he succeeded in receiving definitely directed messages by means of inclined beam antennae.  Braun’s papers on wireless telegraphy were published in 1901 in the form of a brochure under the title Drahtlose Telegraphie durch Wasser und Luff (Wireless telegraphy through water and air).  After the outbreak of the First World War, Braun was summoned to New York to attend as a witness in a lawsuit regarding a patent claim. Owing to his absence from his laboratory and due to illness he was unable to carry out further scientific work. Braun thus spent the last years of his life peacefully in the United States, where he died on April 20, 1918. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0217 |
| **Biographical** | Gabriel Lippmann was born of French parents at Hollerich, Luxembourg on August 16, 1845. The family moved to Paris and he received his early education at home. In 1858 he entered the Lycée Napoleon and ten years later he was admitted to the École Normale. His school career was not markedly successful, for he concentrated only on the work which interested him and neglected that which did not appeal to his taste, and he failed the examination which would have qualified him as a teacher. In 1873, he was appointed to a Government scientific mission visiting Germany to study methods for teaching science: he worked with Kühne and Kirchhoff in Heidelberg and with Helmholtz in Berlin.  Lippmann joined the Faculty of Science in Paris in 1878 and in 1883 he was appointed Professor of Mathematical Physics. Three years later he became Professor of Experimental Physics, succeeding Jamin, and he was appointed Director of the Research Laboratory which was subsequently transferred to the Sorbonne. He retained this position until his death.  Lippmann, of original and independent mind, made many valuable fundamental contributions to many different branches of physics, especially electricity, thermodynamics, optics and photochemistry. In Heidelberg he studied the relationship between electrical and capillary phenomena: this led to the development, amongst other instruments, of his extraordinarily sensitive capillary electrometer.  Professor Lippmann had evolved the general theory of his process for the photographic reproduction of colour in 1886 but the practical execution presented great difficulties. However, after years of patient and skilful experiment, he was able to communicate the process to the Academy of Sciences in 1891, although the photographs were somewhat defective due to the varying sensitivity of the photographic film. In 1893, he was able to present to the Academy photographs taken by A. and L. Lumière in which the colours were produced with perfect ortho-chromatism. He published the complete theory in 1894.  In 1895, Lippmann evolved a method of eliminating the personal equation in measurements of time, using photographic registration, and he studied the eradication of irregularities of pendulum clocks, devising a method of comparing the times of oscillation of two pendulums of nearly equal period. He contributed to astronomy with his invention of the coelostat, a device which immobilizes the image of a star and its surrounding stars so that a photograph may be taken. He was also responsible for many more ingenious devices and improvements to standard instruments to the benefit of many branches of physics.  His work is mainly recorded in communications to the Paris Academy of Sciences where his papers are noted for their conciseness and originality. His method of reproducing colours in photography, based on the interference phenomenon, gained him the Nobel Prize for Physics for 1908.  Professor Lippmann became a member of the Academy of Sciences in 1886 and served as its President in 1912. He was a member of the Board of the Bureau des Longitudes and a Foreign Member of the Royal Society of London.  In 1888 Lippmann married the daughter of the writer V. Cherbuliez, member of the French Academy.  He died at sea on July 13, 1921, during his return from a journey to North America as a member of a mission headed by Marshal Fayolle. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0218 |
| **Biographical** | Albert Abraham Michelson was born in Strelno, Prussia, on December 19, 1852. Two years later his family emigrated to the United States to settle at Virginia City, Nevada, but they eventually moved to San Francisco where Michelson received his early education in public schools, matriculating from the High School in 1869. He was appointed by President Grant to the U.S. Naval Academy and, after graduation as Ensign in 1873 and a two-years’ cruise in the West Indies, he became an instructor in physics and chemistry at the Academy under Admiral Sampson. In 1879, he was posted to the Nautical Almanac Office, Washington, to work with Simon Newcomb, but in the following year, he obtained leave of absence to continue his studies in Europe. He visited the Universities of Berlin and Heidelberg, and the College de France and École Polytechnique in Paris. He resigned from the Navy and in 1883 returned to America to take an appointment as Professor of Physics in the Case School of Applied Science, Cleveland, Ohio. In 1890 he accepted a similar position at Clark University, Worcester, Massachusetts, and in 1892 he became Professor of Physics and the first Head of Department at the new University of Chicago. He rejoined the Navy during World War I, and in 1918 returned to Chicago where in 1925 he was appointed to the first of the Distinguished Service Professorships. Michelson resigned in 1929 to work at the Mount Wilson Observatory, Pasadena.  During his career, Michelson touched on many departments of physics but, perhaps due to a special instinct which he appeared to possess, he excelled in optics. He performed early measurements of the velocity of light with amazing delicacy and in 1881 he invented his interferometer for the purpose of discovering the effect of the Earth’s motion on the observed velocity. In cooperation with Professor E.W. Morley, and using the interferometer, it was shown that light travels at a constant speed in all inertial systems of reference. The instrument also enabled distances to be measured with greater accuracy by means of the length of light-waves. At the request of the International Committee of Weights and Measures, Michelson measured the standard metre in terms of wavelength of cadmium light. He invented the echelon spectroscope and during his wartime service in the Navy he performed research work on devices for naval use – he developed a rangefinder which was adapted as part of U.S. Navy equipment. On his return to civilian life, Michelson became more interested in astronomy and in 1920, using light interference and a highly developed version of his earlier instrument, he measured the diameter of the star Betelgeuse: this was the first determination of the size of a star that could be regarded as accurate.  Michelson has contributed numerous papers to many scientific periodicals and among his more substantial works are the classics, Velocity of Light (1902) Light Waves and their Uses (1899-1903); and Studies in Optics (1927).  Michelson was honoured by memberships of many learned societies throughout America and ten European countries, and he received honorary science and law degrees from ten American and foreign universities. He was President of the American Physical Society (1900), the American Association for the Advancement of Science (1910-1911), and the National Academy of Sciences (1923-1927). He was also a Fellow of the Royal Astronomical Society, the Royal Society of London and the Optical Society, an Associate of l’Académie Française and among the many awards he has received are the Matteucci Medal (Societá Italiana), 1904; Copley Medal (Royal Society), 1907; Elliot Cresson Medal (Franklin Institute), 1912; Draper Medal (National Academy of Sciences), 1916; Franklin Medal (Franklin Institute) and the Medal of the Royal Astronomical Society, 1923; and the Duddell Medal (Physical Society), 1929.  Michelson married Edna Stanton of Lake Forest, Illinois in 1899. They had one son and three daughters. He died in 1931. |
| **Autobiographical** |  |
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| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0219 |
| **Biographical** | Joseph John Thomson was born in Cheetham Hill, a suburb of Manchester on December 18, 1856. He enrolled at Owens College, Manchester, in 1870, and in 1876 entered Trinity College, Cambridge as a minor scholar. He became a Fellow of Trinity College in 1880, when he was Second Wrangler and Second Smith’s Prizeman, and he remained a member of the College for the rest of his life, becoming Lecturer in 1883 and Master in 1918. He was Cavendish Professor of Experimental Physics at Cambridge, where he succeeded [Lord Rayleigh](https://www.nobelprize.org/nobel_prizes/physics/laureates/1904/index.html), from 1884 to 1918 and Honorary Professor of Physics, Cambridge and Royal Institution, London.  Thomson’s early interest in atomic structure was reflected in his *Treatise on the Motion of Vortex Rings* which won him the Adams Prize in 1884. His *Application of Dynamics to Physics and Chemistry* appeared in 1886, and in 1892 he had his *Notes on Recent Researches in Electricity and Magnetism* published. This latter work covered results obtained subsequent to the appearance of James Clerk Maxwell’s famous “Treatise” and it is often referred to as “the third volume of Maxwell”. Thomson co-operated with Professor J. H. Poynting in a four-volume textbook of physics, *Properties of Matter* and in 1895 he produced *Elements of the Mathematical Theory of Electricity and Magnetism,* the 5th edition of which appeared in 1921.  In 1896, Thomson visited America to give a course of four lectures, which summarised his current researches, at Princeton. These lectures were subsequently published as *The Discharge of Electricity through Gases* (1898). On his return from America, he achieved the most brilliant work of his life – an original study of cathode rays culminating in the discovery of the electron, which was announced during the course of his evening lecture to the Royal Institution on Friday, April 30, 1897. His book, *Conduction of Electricity through Gases,* published in 1903 was described by Lord Rayleigh as a review of “Thomson’s great days at the Cavendish Laboratory”. A later edition, written in collaboration with his son, George, appeared in two volumes (1928 and 1933).  Thomson returned to America in 1904 to deliver six lectures on electricity and matter at Yale University. They contained some important suggestions as to the structure of the atom. He discovered a method for separating different kinds of atoms and molecules by the use of positive rays, an idea developed by Aston, Dempster and others towards the discovery of many isotopes. In addition to those just mentioned, he wrote the books, *The Structure of Light* (1907), *The Corpuscular Theory of Matter* (1907), *Rays of Positive Electricity* (1913), *The Electron in Chemistry* (1923) and his autobiography, *Recollections and Reflections* (1936), among many other publications.  Thomson, a recipient of the Order of Merit, was knighted in 1908. He was elected Fellow of the Royal Society in 1884 and was President during 1916-1920; he received the Royal and Hughes Medals in 1894 and 1902, and the Copley Medal in 1914. He was awarded the Hodgkins Medal (Smithsonian Institute, Washington) in 1902; the Franklin Medal and Scott Medal (Philadelphia), 1923; the Mascart Medal (Paris), 1927; the Dalton Medal (Manchester), 1931; and the Faraday Medal (Institute of Civil Engineers) in 1938. He was President of the British Association in 1909 (and of Section A in 1896 and 1931) and he held honorary doctorate degrees from the Universities of Oxford, Dublin, London, Victoria, Columbia, Cambridge, Durham, Birmingham, Göttingen, Leeds, Oslo, Sorbonne, Edinburgh, Reading, Princeton, Glasgow, Johns Hopkins, Aberdeen, Athens, Cracow and Philadelphia.  In 1890, he married Rose Elisabeth, daughter of Sir George E. Paget, K.C.B. They had one son, now [Sir George Paget Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1937/index.html), Emeritus Professor of Physics at London University, who was awarded the [Nobel Prize for Physics](https://www.nobelprize.org/nobel_prizes/physics/laureates/1937/index.html) in 1937, and one daughter. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0220 |
| **Biographical** | Philipp von Lenard was born at Pozsony[1](https://www.nobelprize.org/prizes/physics/1905/lenard/biographical/#not_1) (Pressburg) in Austria-Hungary on June 7, 1862. His family had originally come from the Tyrol. He studied physics successively at Budapest, Vienna, Berlin and Heidelberg under Bunsen, Helmholtz, Königsberger and Quincke and in 1886 took his Ph.D. at Heidelberg.  From 1892 he worked as a Privatdozent and assistant to Professor Hertz at the University of Bonn and in 1894 was appointed Professor Extraordinary at the University of Breslau. In 1895 he became Professor of Physics at Aix-la-Chapelle and in 1896 Professor of Theoretical Physics at the University of Heidelberg. In 1898 he was appointed Professor Ordinarius at the University of Kiel.  Lenard’s first work was done in the field of mechanics, when he published a paper on the oscillation of precipitated water drops and allied problems and in 1894 he published the *Principles of Mechanics* left behind by Hertz.  Soon he became interested in the phenomena of phosphorescence and luminescence. This was a development of the mysterious attraction which weak light appearing in darkness had had for him since his boyhood, when he had, with his school fellows, warmed fluorine crystals to make them luminescent; and now he took up, with the astronomer W. Wolf, the study of the luminosity of pyrogallic acid when it is mixed with alkali and bisulphite for developing photographs. He found that its luminosity depended on the oxidation of the pyrogallic acid. At this time he also carried out studies of magnetism with bismuth and, in collaboration with V. Klatt, who had been his first teacher of physics in his native town, he studied, at the Modern College at Pressburg, the so-called self-luminous substances such as calcium sulphide on which Klatt had been working for some years. Together they found that calcium sulphide, after previous illumination, exerts light in the dark, but only if it contains at least some traces of heavy metals, such as copper and bismuth, which form crystals on which the colour and the intensity and durations of the luminosity depend; if it is quite pure, it is not luminous. This work with Klatt was the beginning of work in a field which occupied Lenard for the next 18 years.  In 1888, when he was working at Heidelberg under Quincke, Lenard had done his first work with cathode rays. He investigated the view then held by Hertz that these rays were analogous to ultraviolet light and he did an experiment to find out whether cathode rays would, like ultraviolet light, pass through a quartz window in the wall of a discharge tube. He found that they would not do this; but later, in 1892, when he was working as an assistant to Hertz at the University of Bonn, Hertz called him to see the discovery he had made that a piece of uranium glass covered with aluminium foil and put inside the discharge tube became luminous beneath the aluminium foil when the cathode rays struck it. Hertz then suggested that it would be possible to separate, by means of a thin plate of aluminium, two spaces, one in which the cathode rays were produced in the ordinary way and the other in which one could observe them in a pure state, which had never been done. Hertz was too busy to do this and gave Lenard permission to do it and it was then that he made the great discovery of the “Lenard window”.  After many experiments with aluminium foil of various thicknesses he was able to publish, in 1894, his great discovery that the plate of quartz that had, until then, been used to close the discharge tube, could be replaced by a thin plate of aluminium foil just thick enough to maintain the vacuum inside the tube, but yet thin enough to allow the cathode rays to pass out. It thus became possible to study the cathode rays, and also the fluorescence they caused, outside the discharge tube and Lenard concluded from the experiments that he then did that the cathode rays were propagated through the air for distances of the order of a decimetre and that they travel in a vacuum for several metres without being weakened. Although Lenard at first followed Hertz in believing that the cathode rays were propagated in the ether, he later abandoned this view as a result of the work of [Jean Perrin](https://www.nobelprize.org/nobel_prizes/physics/laureates/1926/index.html) in 1895, [Sir J.J. Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1906/index.html) in 1897 and [W. Wien](https://www.nobelprize.org/nobel_prizes/physics/laureates/1911/index.html) in 1897, which proved the corpuscular nature of the cathode rays.  Later Lenard extended the work of Hertz on the photoelectric effect. Working in a high vacuum, he analysed the nature of this effect, showing that when ultraviolet light falls on a metal it takes from the metal electrons which are then propagated in the vacuum, in which they can be accelerated or retarded by an electric field, or their paths can be curved by a magnetic field. By exact measurements he showed that the number of electrons projected is proportional to the energy carried by the incident light, whilst their speed, that is to say, their kinetic energy, is quite independent of this number and varies only with the wavelength and increases when this diminishes.  These facts conflicted with current theory and were not explained until 1905, when [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/index.html) produced his quantitative law and developed the theory of quanta of light or photons, which was verified much later by [Millikan](https://www.nobelprize.org/nobel_prizes/physics/laureates/1923/index.html). But Lenard never forgave Einstein for discovering and attaching his own name to this law.  In the course of his work Lenard had, for the purpose of accelerating the speed of the electrons and measuring their energy, invented a photoelectric cell which was the first model of the “3-electrode lamp” which is so important today in radioelectric technique. The only difference between these two cells was that in Lenard’s cell the electrons were taken from the cathode by light, whereas on the “3-electrode lamp” the cathode is a white-hot filament capable of sending into the vacuum currents of much higher intensity.  In 1902 Lenard showed that an electron must have a certain minimum energy before it could produce ionisation when it passed through a gas.  In 1903 he published his conception of the atom as an assemblage of what he called “dynamides”, which were very small and were separated by wide spaces; they had mass and were imagined as electric dipoles connected by two equal charges of contrary sign and their number was equal to the atomic mass. The solid matter in the atom was, he thought, about one thousand millionth of the whole atom. This work contributed much to [Lorentz](https://www.nobelprize.org/nobel_prizes/physics/laureates/1902/index.html)‘ theory of electrons.  In his later years Lenard studied the nature and origin of the lines of the spectrum. Developing the work of Rydberg, Kayser and Runge, who had shown that the lines of the spectrum of a metal can be arranged in two or more different series and that there is a marked mathematical relationship between the wavelengths of these series, Lenard showed that in each series a definite modification of the atom has occurred and that these modifications determine the series and are differentiated by the number of electrons lost.  Lenard was an experimentalist of genius, but more doubtful as a theorist. Some of his discoveries were great ones and others were very important, but he claimed for them more than their true value. Although he was given many honours (for instance, he received Honorary Doctorates of the Universities of Christiania, now Oslo, in 1911, Dresden in 1922 and Pressburg in 1942, the Franklin Medal in 1905, the Eagle Shield of the German Reich in 1933, and was elected Freeman of Heidelberg in the same year), he believed that he was disregarded and this probably explains why he attacked other physicists in many countries. He became a convinced member of Hitler’s National Socialist Party and maintained unreserved adherence to it. The party responded by making him the Chief of Aryan or German Physics. Among his publications are several books: *Ueber Aether und Materie* (second edition 1911), *Quantitatives über Kathodenstrahlen* (1918), *Ueber das Relativitätsprinzip* (1918) and *Grosse Naturforscher* (second edition 1930).  Von Lenard, who was married to Katharina Schlehner, died on May 20, 1947 at Messelhausen. |
| **Autobiographical** |  |
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| **Interview** |  |

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| **Physics\_1999-** | |
| **ID** | 0221 |
| **Biographical** | John William Strutt, third Baron Rayleigh, was born on November 12, 1842 at Langford Grove, Maldon, Essex, as the son of John James Strutt, second Baron, and his wife Clara Elizabeth La Touche, eldest daughter of Captain Richard Vicars, R. E. He was one of the very few members of higher nobility who won fame as an outstanding scientist.  Throughout his infancy and youth he was of frail physique; his education was repeatedly interrupted by ill-health, and his prospects of attaining maturity appeared precarious. After a short spell at Eton at the age of 10, mainly spent in the school sanatorium, three years in a private school at Wimbledon, and another short stay at Harrow, he finally spent four years with the Rev. George Townsend Warner (1857) who took pupils at Torquay.  In 1861 he entered Trinity College, Cambridge, where he commenced reading mathematics, not at first equal in attainments to the best of his contemporaries, but his exceptional abilities soon enabled him to overtake his competitors. He graduated in the Mathematical Tripos in 1865 as Senior Wrangler and Smith’s Prizeman. In 1866 he obtained a fellowship at Trinity which he held until 1871, the year of his marriage.  A severe attack of rheumatic fever in 1872 made him spend the winter in Egypt and Greece. Shortly after his return his father died (1873) and he succeeded to the barony, taking up residence in the family seat, Terling Place, at Witham, Essex. He now found himself compelled to devote part of his time to the management of his estates (7000 acres). The combination of general scientific knowledge and acumen with acquired knowledge of agriculture made his practice in estate management in many respects in advance of his time. Nevertheless, in 1876 he left the entire management of the land to his younger brother.  From then on, he could devote his full time to science again. In 1879 he was appointed to follow James Clerk Maxwell as Professor of Experimental Physics and Head of the Cavendish Laboratory at Cambridge. In 1884 he left Cambridge to continue his experimental work at his country seat at Terling, Essex, and from 1887 to 1905 he was Professor of Natural Philosophy in the Royal Institution of Great Britain, being successor of Tyndall.  He served for six years as President of a Government Committee on Explosives, and from 1896 to 1919 he was Scientific Advisor to Trinity House. He was Lord Lieutenant of Essex from 1892 to 1901.  Lord Rayleigh’s first researches were mainly mathematical, concerning optics and vibrating systems, but his later work ranged over almost the whole field of physics, covering sound, wave theory, colour vision, electrodynamics, electromagnetism, light scattering, flow of liquids, hydrodynamics, density of gases, viscosity, capillarity, elasticity, and photography. His patient and delicate experiments led to the establishment of the standards of resistance, current, and electromotive force; and his later work was concentrated on electric and magnetic problems. Lord Rayleigh was an excellent instructor and, under his active supervision, a system of practical instruction in experimental physics was devised at Cambridge, developing from a class of five or six students to an advanced school of some seventy experimental physicists. His *Theory of Sound* was published in two volumes during 1877-1878, and his other extensive studies are reported in his *Scientific Papers* – six volumes issued during 1889-1920. He has also contributed to the *Encyclopaedia Britannica.*  He had a fine sense of literary style; every paper he wrote, even on the most abstruse subject, is a model of clearness and simplicity of diction. The 446 papers reprinted in his collected works clearly show his capacity for understanding everything just a little more deeply than anyone else. Although a member of the House of Lords, he intervened in debate only on rare occasions, never allowing politics to interfere with science. His recreations were travel, tennis, photography and music.  Lord Rayleigh, a former Chancellor of Cambridge University, was a Justice of the Peace and the recipient of honorary science and law degrees. He was a Fellow of the Royal Society (1873) and served as Secretary from 1885 to 1896, and as President from 1905 to 1908. He was an original recipient of the Order of Merit (1902), and in 1905 he was made a Privy Councillor. He was awarded the Copley, Royal, and Rumford Medals of the Royal Society, and the [Nobel Prize](https://www.nobelprize.org/nobel_prizes/physics/laureates/1904/index.html) for 1904.  In 1871 he married Evelyn, sister of the future prime minister, the Earl of Balfour, and daughter of James Maitland Balfour and his wife Blanche, the daughter of the second Marquis of Salisbury. They had three sons, the eldest of whom was to become Professor of Physics at Imperial College of Science and Technology, London.  Lord Rayleigh died on June 30, 1919, at Witham, Essex. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0222 |
| **Biographical** | Antoine Henri Becquerel was born in Paris on December 15, 1852, a member of a distinguished family of scholars and scientists. His father, Alexander Edmond Becquerel, was a Professor of Applied Physics and had done research on solar radiation and on phosphorescence, while his grandfather, Antoine César, had been a Fellow of the Royal Society and the inventor of an electrolytic method for extracting metals from their ores. He entered the Polytechnic in 1872, then the government department of Ponts-et-Chaussées in 1874, becoming ingénieur in 1877 and being promoted to ingénieur-en-chef in 1894. In 1888 he acquired the degree of docteur-ès-sciences. From 1878 he had held an appointment as an Assistant at the Museum of Natural History, taking over from his father in the Chair of Applied Physics at the Conservatoire des Arts et Metiers. In 1892 he was appointed Professor of Applied Physics in the Department of Natural History at the Paris Museum. He became a Professor at the Polytechnic in 1895.  Becquerel’s earliest work was concerned with the plane polarization of light, with the phenomenon of phosphorescence and with the absorption of light by crystals (his doctorate thesis). He also worked on the subject of terrestrial magnetism. In 1896, his previous work was overshadowed by his discovery of the phenomenon of natural radioactivity. Following a discussion with Henri Poincaré on the radiation which had recently been discovered by Röntgen (X-rays) and which was accompanied by a type of phosphorescence in the vacuum tube, Becquerel decided to investigate whether there was any connection between X-rays and naturally occurring phosphorescence. He had inherited from his father a supply of uranium salts, which phosphoresce on exposure to light. When the salts were placed near to a photographic plate covered with opaque paper, the plate was discovered to be fogged. The phenomenon was found to be common to all the uranium salts studied and was concluded to be a property of the uranium atom. Later, Becquerel showed that the rays emitted by uranium, which for a long time were named after their discoverer, caused gases to ionize and that they differed from X-rays in that they could be deflected by electric or magnetic fields. For his discovery of spontaneous radioactivity Becquerel was awarded half of the Nobel Prize for Physics in 1903, the other half being given to Pierre and Marie Curie for their study of the Becquerel radiation.  Becquerel published his findings in many papers, principally in the Annales de Physique et de Chimie and the Comptes Rendus de l’Academie des Sciences.  He was elected a member of the Academie des Sciences de France in 1889 and succeeded Berthelot as Life Secretary of that body. He was a member also of the Accademia dei Lincei and of the Royal Academy of Berlin, amongst others. He was made an Officer of the Legion of Honour in 1900.  He was married to Mlle. Janin, the daughter of a civil engineer. They had a son Jean, b. 1878, who was also a physicist: the fourth generation of scientists in the Becquerel family.  Antoine Henri Becquerel died at Le Croisic on August 25, 1908. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0223 |
| **Biographical** | Pierre Curie was born in Paris, where his father was a general medical practitioner, on May 15, 1859. He received his early education at home before entering the Faculty of Sciences at the Sorbonne. He gained his Licenciateship in Physics in 1878 and continued as a demonstrator in the physics laboratory until 1882 when he was placed in charge of all practical work in the Physics and Industrial Chemistry Schools. In 1895 he obtained his Doctor of Science degree and was appointed Professor of Physics. He was promoted to Professor in the Faculty of Sciences in 1900, and in 1904 he became Titular Professor.  In his early studies on crystallography, together with his brother Jacques, Curie discovered piezoelectric effects. Later, he advanced theories of symmetry with regard to certain physical phenomena and turned his attention to magnetism. He showed that the magnetic properties of a given substance change at a certain temperature – this temperature is now known as the Curie point. To assist in his experiments he constructed several delicate pieces of apparatus – balances, electrometers, piezoelectric crystals, etc.  Curie’s studies of radioactive substances were made together with his wife, whom he married in 1895. They were achieved under conditions of much hardship – barely adequate laboratory facilities and under the stress of having to do much teaching in order to earn their livelihood. They announced the discovery of radium and polonium by fractionation of pitchblende in 1898 and later they did much to elucidate the properties of radium and its transformation products. Their work in this era formed the basis for much of the subsequent research in nuclear physics and chemistry. Together they were awarded half of the Nobel Prize for Physics in 1903 on account of their study into the spontaneous radiation discovered by Becquerel, who was awarded the other half of the Prize.  Pierre Curie’s work is recorded in numerous publications in the Comptes Rendus de l’Académie des Sciences, the Journal de Physique and the Annales de Physique et Chimie.  Curie was awarded the Davy Medal of the Royal Society of London in 1903 (jointly with his wife) and in 1905 he was elected to the Academy of Sciences.  His wife was formerly Marie Sklodowska, daughter of a secondary-school teacher at Warsaw, Poland. One daughter, Irene, married Frederic Joliot and they were joint recipients of the [Nobel Prize for Chemistry in 1935](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1935/index.html). The younger daughter, Eve, married the American diplomat H. R. Labouisse. They have both taken lively interest in social problems, and as Director of the United Nations’ Children’s Fund he received on its behalf the [Nobel Peace Prize in Oslo in 1965](https://www.nobelprize.org/nobel_prizes/peace/laureates/1965/index.html). She is the author of a famous biography of her mother, Madame Curie (Gallimard, Paris, 1938), translated into several languages.  Pierre was killed in a street accident in Paris on April 19, 1906. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
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| **Physics\_1999-** | |
| **ID** | 0224 |
| **Biographical** | Marie Curie, *née* Maria Sklodowska, was born in Warsaw on November 7, 1867, the daughter of a secondary-school teacher. She received a general education in local schools and some scientific training from her father. She became involved in a students’ revolutionary organization and found it prudent to leave Warsaw, then in the part of Poland dominated by Russia, for Cracow, which at that time was under Austrian rule. In 1891, she went to Paris to continue her studies at the Sorbonne where she obtained Licenciateships in Physics and the Mathematical Sciences. She met Pierre Curie, Professor in the School of Physics in 1894 and in the following year they were married. She succeeded her husband as Head of the Physics Laboratory at the Sorbonne, gained her Doctor of Science degree in 1903, and following the tragic death of Pierre Curie in 1906, she took his place as Professor of General Physics in the Faculty of Sciences, the first time a woman had held this position. She was also appointed Director of the Curie Laboratory in the Radium Institute of the University of Paris, founded in 1914.  Her early researches, together with her husband, were often performed under difficult conditions, laboratory arrangements were poor and both had to undertake much teaching to earn a livelihood. The discovery of radioactivity by Henri Becquerel in 1896 inspired the Curies in their brilliant researches and analyses which led to the isolation of polonium, named after the country of Marie’s birth, and radium. Mme. Curie developed methods for the separation of radium from radioactive residues in sufficient quantities to allow for its characterization and the careful study of its properties, therapeutic properties in particular.  Mme. Curie throughout her life actively promoted the use of radium to alleviate suffering and during World War I, assisted by her daughter, Irene, she personally devoted herself to this remedial work. She retained her enthusiasm for science throughout her life and did much to establish a radioactivity laboratory in her native city – in 1929 President Hoover of the United States presented her with a gift of $ 50,000, donated by American friends of science, to purchase radium for use in the laboratory in Warsaw.  Mme. Curie, quiet, dignified and unassuming, was held in high esteem and admiration by scientists throughout the world. She was a member of the Conseil du Physique Solvay from 1911 until her death and since 1922 she had been a member of the Committee of Intellectual Co-operation of the League of Nations. Her work is recorded in numerous papers in scientific journals and she is the author of *Recherches sur les Substances Radioactives* (1904), *L’Isotopie et les Éléments Isotopes* and the classic *Traité’ de Radioactivité* (1910).  The importance of Mme. Curie’s work is reflected in the numerous awards bestowed on her. She received many honorary science, medicine and law degrees and honorary memberships of learned societies throughout the world. Together with her husband, she was awarded half of the Nobel Prize for Physics in 1903, for their study into the spontaneous radiation discovered by Becquerel, who was awarded the other half of the Prize. In 1911 she received a second [Nobel Prize, this time in Chemistry](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1911/index.html), in recognition of her work in radioactivity. She also received, jointly with her husband, the Davy Medal of the Royal Society in 1903 and, in 1921, President Harding of the United States, on behalf of the women of America, presented her with one gram of radium in recognition of her service to science.  For further details, cf. [Biography of Pierre Curie](https://www.nobelprize.org/nobel_prizes/physics/laureates/1903/pierre-curie-bio.html). Mme. Curie died in Savoy, France, after a short illness, on July 4, 1934. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0225 |
| **Biographical** | Hendrik Antoon Lorentz was born at Arnhem, The Netherlands, on July 18, 1853, as the son of nursery-owner Gerrit Frederik Lorentz and his wife *née* Geertruida van Ginkel. When he was four years old, his mother died, and in 1862 his father married Luberta Hupkes. In those days the grade school did not only have school hours in the morning and in the afternoon, but also in the evening, when teaching was more free (in a sense resembling the Dalton method). In this way, when in 1866 the first highschool (H.B.S.) at Arnhem was opened, Hendrik Lorentz, as a gifted pupil, was ready to be placed in the 3rd form. After the 5th form and a year of study of the classics, he entered the University of Leyden in 1870, obtained his B.Sc. degree in mathematics and physics in 1871, and returned to Arnhem in 1872 to become a night-school teacher, at the same time preparing for his doctoral thesis on the reflection and refraction of light. In 1875, at the early age of 22, he obtained his doctor’s degree, and only three years later he was appointed to the Chair of Theoretical Physics at Leyden, newly created for him. In spite of many invitations to chairs abroad, he always remained faithful to his Alma Mater. From 1912 onward, when he accepted a double function at Haarlem as Curator of Teyler’s Physical Cabinet and Secretary of the “Hollandsche Maatschappij der Wetenschappen” (Dutch Society of Sciences), he continued at Leyden as Extraordinary Professor, delivering his famous Monday morning lectures for the rest of his life. The far-seeing directors of Teyler’s Foundation thus enabled his unique mind to be freed from routine academic obligations, permitting him to spread his wings still further in the highest secluded realms of science, which are attainable by so few.  From the start of his scientific work, Lorentz took it as his task to extend James Clerk Maxwell’s theory of electricity and of light. Already in his doctor’s thesis, he treated the reflection and refraction phenomena of light from this standpoint which was then quite new. His fundamental work in the fields of optics and electricity has revolutionized contemporary conceptions of the nature of matter.  In 1878, he published an essay on the relation between the velocity of light in a medium and the density and composition thereof. The resulting formula, proposed almost simultaneously by the Danish physicist Lorenz, has become known as the Lorenz-Lorentz formula.  Lorentz also made fundamental contributions to the study of the phenomena of moving bodies. In an extensive treatise on the aberration of light and the problems arising in connection with it, he followed A.J. Fresnel’s hypothesis of the existence of an immovable ether, which freely penetrates all bodies. This assumption formed the basis of a general theory of the electrical and optical phenomena of moving bodies.  From Lorentz stems the conception of the electron; his view that his minute, electrically charged particle plays a *rôle* during electromagnetic phenomena in ponderable matter made it possible to apply the molecular theory to the theory of electricity, and to explain the behaviour of light waves passing through moving, transparent bodies.  The so-called Lorentz transformation (1904) was based on the fact that electromagnetic forces between charges are subject to slight alterations due to their motion, resulting in a minute contraction in the size of moving bodies. It not only adequately explains the apparent absence of the relative motion of the Earth with respect to the ether, as indicated by the experiments of Michelson and Morley, but also paved the way for Einstein’s special theory of relativity.  It may well be said that Lorentz was regarded by all theoretical physicists as the world’s leading spirit, who completed what was left unfinished by his predecessors and prepared the ground for the fruitful reception of the new ideas based on the quantum theory.  In 1919, he was appointed Chairman of the Committee whose task it was to study the movements of sea water which could be expected during and after the reclamation of the Zuyderzee in The Netherlands, one of the greatest works of all times in hydraulic engineering. His theoretical calculations, the result of eight years of pioneering work, have been confirmed in actual practice in the most striking manner, and have ever since been of permanent value to the science of hydraulics.  An overwhelming number of honours and distinctions from all over the world were bestowed on Lorentz. International gatherings were presided over by him with exceptional skill, both on account of his amiable and judicious personality and his masterly command of languages. Until his death he was Chairman of all Solvay Congresses, and in 1923 he was elected to the membership of the “International Committee of Intellectual Cooperation” of the League of Nations. Of this Committee, consisting of only seven of the world’s most eminent scholars, he became the President in 1925.  Through his great prestige in governmental circles in his own country, Lorentz was able to convince them of the importance of science for national production. He thus initiated the steps which finally led to the creation of the organisation now generally known under the initials T.N.O. (Dutch for Applied Scientific Research).  Lorentz was a man of immense personal charm. The very picture of unselfishness, full of genuine interest in whoever had the privilege of crossing his path, he endeared himself both to the leaders of his age and to the ordinary citizen.  In 1881 Lorentz married Aletta Catharina Kaiser, whose father, J.W. Kaiser, Professor at the Academy of Fine Arts, was the Director of the Museum which later became the well-known Rijksmuseum (National Gallery) of Amsterdam, and the designer of the first postage stamps of The Netherlands. There were two daughters and one son from this marriage. The eldest daughter Dr. Geertruida Luberta Lorentz is a physicist in her own right and married Professor W.J. de Haas, Director of the Cryogenic Laboratory (Kamerlingh Onnes Laboratory) of the University of Leyden.  Lorentz died at Haarlem on February 4, 1928. |
| **Autobiographical** |  |
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| **Physics\_1999-** | |
| **ID** | 0226 |
| **Biographical** | Pieter Zeeman was born on May 25, 1865, at Zonnemaire, a small village in the isle of Schouwen, Zeeland, The Netherlands, as the son of the local clergyman Catharinus Forandinus Zeeman and his wife, *née* Wilhelmina Worst. After having finished his secondary school education at Zierikzee, the main town of the island, he went to Delft for two years to receive tuition in the classical languages, an adequate knowledge of which was required at that time for entrance to the university. Taking up his abode at the house of Dr. J.W. Lely, conrector of the Gymnasium and brother of Dr. C. Lely (Minister of Public Works and known for initiating and developing the work for reclamation of the Zuyderzee), Zeeman came into an environment which was beneficial for the development of his scientific talents. It was here also that he came into contact with [Kamerlingh Onnes (Nobel Prize in Physics for 1913)](https://www.nobelprize.org/nobel_prizes/physics/laureates/1913/index.html), who was twelve years his senior. Zeeman’s wide reading, which included a proper mastery of works such as Maxwell’s *Heat*, and his passion for performing experiments amazed Kamerlingh Onnes in no small degree, and formed the basis for a fruitful friendship between the two scientists.  Zeeman entered Leyden University in 1885 and became mainly a pupil of Kamerlingh Onnes (mechanics) and Lorentz (experimental physics): the latter was later to share the Nobel Prize with him. An early reward came in 1890 when he was appointed assistant to Lorentz, enabling him to participate in an extensive research programme which included the study of the Kerr effect – an important foundation for his future great work. He obtained his doctor’s degree in 1893, after which he left for F. Kohlrausch’s institute at Strasbourg, where for one semester he carried out work under E. Cohn. He returned to Leyden in 1894 and became “privaat-docent” (extra-mural lecturer) from 1895 to 1897.  In 1897, the year following his great discovery of the magnetic splitting of spectral lines, he was called to a lectureship at the University of Amsterdam; in 1900 came his appointment as Extraordinary Professor. In 1908 [Van der Waals (Nobel Prize in Physics for 1910)](https://www.nobelprize.org/nobel_prizes/physics/laureates/1910/index.html) reached the retiring age of 70 and Zeeman was chosen as his successor, at the same time functioning as Director of the Physics Laboratory. In 1923 a new laboratory, specially erected for him, was put at his disposal, a prominent feature being a concrete block weighing a quarter of a million kilograms, erected free from the floor, as a suitable platform for vibration-free experiments. The institute is now known as the Zeeman Laboratory of Amsterdam University. Many world-famous scientists have visited Zeeman there or worked with him for some time. He remained in this dual function for 35 years – on numerous occasions refusing an invitation to occupy a Chair abroad – until in 1935 he had to resign on account of his pensionable age. An accomplished teacher and of kind disposition he was much loved by his pupils. One of these was C.J. Bakker, who was from 1955 until his untimely death in an aircraft accident in 1960 the General Director of the Organisation Européenne pour la Recherche Nucléaire (CERN) at Geneva. Another worker in his laboratory was S. Goudsmit, who in 1925 with G.E. Uhlenbeck originated the concept of electron spin.  Zeeman’s talent for natural science first became apparent in 1883, when, while still attending the secondary school, he gave an apt description and drawing of an aurora borealis – then clearly to be observed in his country – which was published in *Nature*. (The Editor praised the meticulous observations of «Professor Zeeman in his observatory at Zonnemaire»!)  Zeeman’s main theme of investigation has always concerned optical phenomena. His first treatise *Mesures relatives du phénomène de Kerr*, written in 1892, was rewarded with a Gold Medal from the Dutch Society of Sciences at Haarlem; his doctor’s thesis dealt with the same subject. In Strasbourg he studied the propagation and absorption of electrical waves in fluids. His principal work, however, was the study of the influence of magnetism on the nature of light radiation, started by him in the summer of 1896, which formed a logical continuation of his investigation into the Kerr effect. The discovery of the so-called Zeeman effect, for which he has been awarded the Nobel Prize, was communicated to the Royal Academy of Sciences in Amsterdam – through H. Kamerlingh Onnes (1896) and J.D. van der Waals (1897) – in the form of papers entitled *Over den Invloed eener Magnetisatie op den Aard van het door een Stof uitgezonden Licht* (On the influence of a magnetization on the nature of light emitted by a substance) and *Over Doubletten en Tripletten in het Spectrum teweeggebracht door Uitwendige Magnetische Krachten* (On doublets and triplets in the spectrum caused by external magnetic forces) I, II and III. (The English translations of these papers appeared in *The Philosophical Magazine*; of the first paper a French version appeared in *Archives Néerlandaises des Sciences Exactes et Naturelles*, and in a short form in German in *Verhandlungen der Physikalischen Gesellschaft zu Berlin*.)  The importance of the discovery can at once be judged by the fact that at one stroke the phenomenon not only confirmed Lorentz’ theoretical conclusions with regard to the state of polarization of the light emitted by flames, but also demonstrated the negative nature of the oscillating particles, as well as the unexpectedly high ratio of their charge and mass (*e/m*). Thus, when in the following year the discovery of the existence of free electrons in the form of cathode rays was established by [J.J. Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1906/index.html), the identity of electrons and the oscillating light particles could be established from the negative nature and the *e/m* ratio of the particles. The growing number of observations made by other investigators on studying the effects of using various substances as light emitters – not all of them explicable by Lorentz’ original theory (the so-called «anomalous Zeeman effect» could only adequately be explained at a later date, with the advent of Bohr’s atomic theory, quantum wave mechanics, and the concept of the electron spin) – was assembled by him in his book *Researches in Magneto-Optics* (London 1913, German translation in 1914). Not only has the Zeeman effect thrown much light on the mechanism of light radiation and on the nature of matter and electricity, but its immense importance lies in the fact that even to this day it offers the ultimate means for revealing the intimate structure of the atom and the nature and behaviour of its components. It still serves as the final test in any new theory of the atom.  Already in his second communication Zeeman expressed the opinion that the accepted existence of strong magnetic fields on the surface of the sun could be verified, since these should alter spectral lines derived from the celestial body. (It is typical of Zeeman to extend physical concepts into the realm of celestial phenomena.) In a letter to him (1908) the astronomer G.E. Hale, Director of Mount Wilson Observatory, corroborated this opinion by means of photographs which indicated that in solar vortices the spectral lines indeed appeared to be affected by magnetic fields. Even the theoretical prediction concerning the probable interrelationship between the directions of polarization and those of the magnetic fields was subsequently confirmed by Hale.  With regard to Zeeman’s activities outside the field of the magnetic splitting of spectral lines, mention should first be made of his work on the Doppler effect in optics and in canal rays (laboratory tests). A second field of study was that on the propagation of light in moving media (justification of the existence of the Lorentz-term in the Fresnel drag coefficient). Other investigations were those into the influence of the magnetic moment of the nucleus on the hyperfine structure of spectral lines. He also succeeded, with J. de Gier, in discovering a number of new isotopes (38Ar, 64Ni, amongst others) by means of Thomson’s parabola mass spectrograph. Zeeman’s predilection for testing fundamental laws also found expression in his verification – carried out with an accuracy of 7 – of the equality of heavy and inert masses.  Zeeman was Honorary Doctor of the Universities of Göttingen, Oxford, Philadelphia, Strasbourg, Liège, Ghent, Glasgow, Brussels and Paris. He was also a member or honorary member of numerous learned academies, including the very rare distinction of Associé Etranger of the Académie des Sciences of Paris. He was also member and Chairman of the Commission Internationale des Poids et Mesures, Paris. Appointed member of the Royal Academy of Sciences of Amsterdam in 1898, he served as the Secretary of the Mathematical-Physical Section from 1912 to 1920. Among the other distinctions may be mentioned the Rumford Medal of the Royal Society of London, the Prix Wilde of the Academie des Sciences of Paris, the Baumgartner-Preis of the Akademie der Wissenschaften of Vienna, the Matteucci Medal of the Italian Society of Sciences, the Franklin Medal of the Franklin Institute of Philadelphia, the Henry Draper Medal of the National Academy of Sciences of Washington. He was also made a Knight of the Order of Orange-Nassau and Commander of the Order of the Netherlands Lion.  Outside his field of study Zeeman showed much interest in literature and the stage. An entertaining host, he loved to invite his collaborators and pupils to dine with him at his home, an event preceded by a learned talk in his study and followed by a gathering in the family circle.  Zeeman married Johanna Elisabeth Lebret in 1895; they had one son and three daughters. During the last year of his professorship he suffered from ill-health. He died after a short illness on October 9, 1943. |
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| **Physics\_1999-** | |
| **ID** | 0227 |
| **Biographical** | Wilhelm Conrad Röntgen was born on March 27, 1845, at Lennep in the Lower Rhine Province of Germany, as the only child of a merchant in, and manufacturer of, cloth. His mother was Charlotte Constanze Frowein of Amsterdam, a member of an old Lennep family which had settled in Amsterdam.  When he was three years old, his family moved to Apeldoorn in The Netherlands, where he went to the Institute of Martinus Herman van Doorn, a boarding school. He did not show any special aptitude, but showed a love of nature and was fond of roaming in the open country and forests. He was especially apt at making mechanical contrivances, a characteristic which remained with him also in later life. In 1862 he entered a technical school at Utrecht, where he was however unfairly expelled, accused of having produced a caricature of one of the teachers, which was in fact done by someone else.  He then entered the University of Utrecht in 1865 to study physics. Not having attained the credentials required for a regular student, and hearing that he could enter the Polytechnic at Zurich by passing its examination, he passed this and began studies there as a student of mechanical engineering. He attended the lectures given by Clausius and also worked in the laboratory of Kundt. Both Kundt and Clausius exerted great influence on his development. In 1869 he graduated Ph.D. at the University of Zurich, was appointed assistant to Kundt and went with him to Würzburg in the same year, and three years later to Strasbourg.  In 1874 he qualified as Lecturer at Strasbourg University and in 1875 he was appointed Professor in the Academy of Agriculture at Hohenheim in Württemberg. In 1876 he returned to Strasbourg as Professor of Physics, but three years later he accepted the invitation to the Chair of Physics in the University of Giessen.  After having declined invitations to similar positions in the Universities of Jena (1886) and Utrecht (1888), he accepted it from the University of Würzburg (1888), where he succeeded Kohlrausch and found among his colleagues Helmholtz and Lorenz. In 1899 he declined an offer to the Chair of Physics in the University of Leipzig, but in 1900 he accepted it in the University of Munich, by special request of the Bavarian government, as successor of E. Lommel. Here he remained for the rest of his life, although he was offered, but declined, the Presidency of the Physikalisch-Technische Reichsanstalt at Berlin and the Chair of Physics of the Berlin Academy.  Röntgen’s first work was published in 1870, dealing with the specific heats of gases, followed a few years later by a paper on the thermal conductivity of crystals. Among other problems he studied were the electrical and other characteristics of quartz; the influence of pressure on the refractive indices of various fluids; the modification of the planes of polarised light by electromagnetic influences; the variations in the functions of the temperature and the compressibility of water and other fluids; the phenomena accompanying the spreading of oil drops on water.  Röntgen’s name, however, is chiefly associated with his discovery of the rays that he called X-rays. In 1895 he was studying the phenomena accompanying the passage of an electric current through a gas of extremely low pressure. Previous work in this field had already been carried out by J. Plucker (1801-1868), J. W. Hittorf (1824-1914), C. F. Varley (1828-1883), E. Goldstein (1850-1931), Sir William Crookes (1832-1919), H. Hertz (1857-1894) and [Ph. von Lenard](https://www.nobelprize.org/nobel_prizes/physics/laureates/1905/index.html) (1862-1947), and by the work of these scientists the properties of cathode rays – the name given by Goldstein to the electric current established in highly rarefied gases by the very high tension electricity generated by Ruhmkorff’s induction coil – had become well known. Röntgen’s work on cathode rays led him, however, to the discovery of a new and different kind of rays.  On the evening of November 8, 1895, he found that, if the discharge tube is enclosed in a sealed, thick black carton to exclude all light, and if he worked in a dark room, a paper plate covered on one side with barium platinocyanide placed in the path of the rays became fluorescent even when it was as far as two metres from the discharge tube. During subsequent experiments he found that objects of different thicknesses interposed in the path of the rays showed variable transparency to them when recorded on a photographic plate. When he immobilised for some moments the hand of his wife in the path of the rays over a photographic plate, he observed after development of the plate an image of his wife’s hand which showed the shadows thrown by the bones of her hand and that of a ring she was wearing, surrounded by the penumbra of the flesh, which was more permeable to the rays and therefore threw a fainter shadow. This was the first “röntgenogram” ever taken. In further experiments, Röntgen showed that the new rays are produced by the impact of cathode rays on a material object. Because their nature was then unknown, he gave them the name X-rays. Later, [Max von Laue](https://www.nobelprize.org/nobel_prizes/physics/laureates/1914/index.html) and his pupils showed that they are of the same electromagnetic nature as light, but differ from it only in the higher frequency of their vibration.  Numerous honours were showered upon him. In several cities, streets were named after him, and a complete list of Prizes, Medals, honorary doctorates, honorary and corresponding memberships of learned societies in Germany as well as abroad, and other honours would fill a whole page of this book. In spite of all this, Röntgen retained the characteristic of a strikingly modest and reticent man. Throughout his life he retained his love of nature and outdoor occupations. Many vacations were spent at his summer home at Weilheim, at the foot of the Bavarian Alps, where he entertained his friends and went on many expeditions into the mountains. He was a great mountaineer and more than once got into dangerous situations. Amiable and courteous by nature, he was always understanding the views and difficulties of others. He was always shy of having an assistant, and preferred to work alone. Much of the apparatus he used was built by himself with great ingenuity and experimental skill.  Röntgen married Anna Bertha Ludwig of Zürich, whom he had met in the café run by her father. She was a niece of the poet Otto Ludwig. They married in 1872 in Apeldoorn, The Netherlands. They had no children, but in 1887 adopted Josephine Bertha Ludwig, then aged 6, daughter of Mrs. Röntgen’s only brother. Four years after his wife, Röntgen died at Munich on February 10, 1923, from carcinoma of the intestine. |
| **Autobiographical** |  |
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