|  |  |
| --- | --- |
| **Chemistry\_1999-** | |
| **ID** | 0365 |
| **Biographical** | On the banks of the Nile, the Rosetta branch, I lived an enjoyable childhood in the City of Disuq, which is the home of the famous mosque, Sidi Ibrahim. I was born (February 26, 1946) in nearby Damanhur, the “City of Horus”, only 60 km from Alexandria. In retrospect, it is remarkable that my childhood origins were flanked by two great places – Rosetta, the city where the famous Stone was discovered, and Alexandria, the home of ancient learning. The dawn of my memory begins with my days, at Disuq’s preparatory school. I am the only son in a family of three sisters and two loving parents. My father was liked and respected by the city community – he was helpful, cheerful and very much enjoyed his life. He worked for the government and also had his own business. My mother, a good-natured, contented person, devoted all her life to her children and, in particular, to me. She was central to my “walks of life” with her kindness, total devotion and native intelligence. Although our immediate family is small, the Zewails are well known in Damanhur.  The family’s dream was to see me receive a high degree abroad and to return to become a university professor – on the door to my study room, a sign was placed reading, “Dr. Ahmed,” even though I was still far from becoming a doctor. My father did live to see that day, but a dear uncle did not. Uncle Rizk was special in my boyhood years and I learned much from him – an appreciation for critical analyses, an enjoyment of music, and of intermingling with the masses and intellectuals alike; he was respected for his wisdom, financially well-to-do, and self-educated. Culturally, my interests were focused – *reading, music, some sports and playing backgammon*. The great singer Um Kulthum (actually named Kawkab Elsharq – a superstar of the East) had a major influence on my appreciation of music. On the first Thursday of each month we listened to Um Kulthum’s concert – “waslats” (three songs) – for more than three hours. During all of my study years in Egypt, the music of this unique figure gave me a special happiness, and her voice was often in the background while I was studying mathematics, chemistry… etc. After three decades I still have the same feeling and passion for her music. In America, the only music I have been able to appreciate on this level is classical, and some jazz. Reading was and still is my real joy.  As a boy it was clear that my inclinations were toward the physical sciences. Mathematics, mechanics, and chemistry were among the fields that gave me a special satisfaction. Social sciences were not as attractive because in those days much emphasis was placed on memorization of subjects, names and the like, and for reasons unknown (to me), my mind kept asking “how” and “why”. This characteristic has persisted from the beginning of my life. In my teens, I recall feeling a thrill when I solved a difficult problem in mechanics, for instance, considering all of the tricky operational forces of a car going uphill or downhill. Even though chemistry required some memorization, I was intrigued by the “mathematics of chemistry”. It provides laboratory phenomena which, as a boy, I wanted to reproduce and understand. In my bedroom I constructed a small apparatus, out of my mother’s oil burner (for making Arabic coffee) and a few glass tubes, in order to see how wood is transformed into a burning gas and a liquid substance. I still remember this vividly, not only for the science, but also for the danger of burning down our house! It is not clear why I developed this attraction to science at such an early stage.  After finishing high school, I applied to universities. In Egypt, you send your application to a central Bureau (Maktab El Tansiq), and according to your grades, you are assigned a university, hopefully on your list of choice. In the sixties, Engineering, Medicine, Pharmacy, and Science were tops. I was admitted to Alexandria University and to the faculty of science. Here, luck played a crucial role because I had little to do with Maktab El Tansiq’s decision, which gave me the career I still love most: science. At the time, I did not know the depth of this feeling, and, if accepted to another faculty, I probably would not have insisted on the faculty of science. But this passion for science became evident on the first day I went to the campus in Maharem Bek with my uncle – I had tears in my eyes as I felt the greatness of the university and the sacredness of its atmosphere. My grades throughout the next four years reflected this special passion. In the first year, I took four courses, mathematics, physics, chemistry, and geology, and my grades were either excellent or very good. Similarly, in the second year I scored very highly (excellent) in Chemistry and was chosen for a group of seven students (called “special chemistry”), an elite science group. I graduated with the highest honors – “Distinction with First Class Honor” – with above 90% in all areas of chemistry. With these scores, i was awarded, as a student, a stipend every month of approximately £13, which was close to that of a university graduate who made £17 at the time!  After graduating with the degree of Bachelor of Science, I was appointed to a University position as a demonstrator (“Moeid”), to carry on research toward a Masters and then a Ph.D. degree, and to teach undergraduates at the University of Alexandria. This was a tenured position, guaranteeing a faculty appointment at the University. In teaching, I was successful to the point that, although not yet a professor, I gave “professorial lectures” to help students after the Professor had given his lecture. Through this experience I discovered an affinity and enjoyment of explaining science and natural phenomena in the clearest and simplest way. The students (500 or more) enriched this sense with the appreciation they expressed. At the age of 21, as a Moeid, I believed that behind every universal phenomenon there must be beauty and simplicity in its description. This belief remains true today.  On the research side, I finished the requirements for a Masters in Science in about eight months. The tool was spectroscopy, and I was excited about developing an understanding of how and why the spectra of certain molecules change with solvents. This is an old subject, but to me it involved a new level of understanding that was quite modern in our department. My research advisors were three: The head of the inorganic section, Professor Tahany Salem and Professors Rafaat Issa and Samir El Ezaby, with whom I worked most closely; they suggested the research problem to me, and this research resulted in several publications. I was ready to think about my Ph.D. research (called “research point”) after one year of being a Moeid. Professors El Ezaby (a graduate of Utah) and Yehia El Tantawy (a graduate of Penn) encouraged me to go abroad to complete my Ph.D. work. All the odds were against my going to America. First, I did not have the connections abroad. Second, the 1967 war had just ended and American stocks in Egypt were at their lowest value, so study missions were only sent to the USSR or Eastern European countries. I had to obtain a scholarship directly from an American University. After corresponding with a dozen universities, the University of Pennsylvania and a few others offered me scholarships, providing the tuition and paying a monthly stipend (some $300). There were still further obstacles against travel to America (“Safer to America”). It took enormous energy to pass the regulatory and bureaucratic barriers.  Arriving in the States, I had the feeling of being thrown into an ocean. The ocean was full of knowledge, culture, and opportunities, and the choice was clear: I could either learn to swim or sink. The culture was foreign, the language was difficult, but my hopes were high. I did not speak or write English fluently, and I did not know much about western culture in general, or American culture in particular. I remember a “cultural incident” that opened my eyes to the new traditions I was experiencing right after settling in Philadelphia. In Egypt, as boys, we used to kid each other by saying “I’ll kill you”, and good friends often said such phrases jokingly. I became friends with a sympathetic American graduate student, and, at one point, jokingly said “I’ll kill you”. I immediately noticed his reserve and coolness, perhaps worrying that a fellow from the Middle East might actually do it!  My presence – as the Egyptian at Penn – was starting to be felt by the professors and students as my scores were high, and I also began a successful course of research. I owe much to my research advisor, Professor Robin Hochstrasser, who was, and still is, a committed scientist and educator. The diverse research problems I worked on, and the collaborations with many able scientists, were both enjoyable and profitable. My publication list was increasing, but just as importantly, I was learning new things literally every day – in chemistry, in physics and in other fields. The atmosphere at the Laboratory for Research on the Structure of Matter (LRSM) was most stimulating and I was enthusiastic about researching in areas that crossed the disciplines of physics and chemistry (sometimes too enthusiastic!). My courses were enjoyable too; I still recall the series 501, 502, 503 and the physics courses I took with the Nobel Laureate, [Bob Schrieffer](https://www.nobelprize.org/nobel_prizes/physics/laureates/1972/index.html). I was working almost “day and night,” and doing several projects at the same time: The Stark effect of simple molecules; the [Zeeman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1902/index.html) effect of solids like NO2– and benzene; the optical detection of magnetic resonance (ODMR); double resonance techniques, etc. Now, thinking about it, I cannot imagine doing all of this again, but of course then I was “young and innocent”.  The research for my Ph.D. and the requirements for a degree were essentially completed by 1973, when another war erupted in the Middle East. I had strong feelings about returning to Egypt to be a University Professor, even though at the beginning of my years in America my memories of the frustrating bureaucracy encountered at the time of my departure were still vivid. With time, things change, and I recollected all the wonderful years of my childhood and the opportunities Egypt had provided to me. Returning was important to me, but I also knew that Egypt would not be able to provide the scientific atmosphere I had enjoyed in the U.S. A few more years in America would give me and my family two opportunities: First, I could think about another area of research in a different place (while learning to be professorial!). Second, my salary would be higher than that of a graduate student, and we could then buy a big American car that would be so impressive for the new Professor at Alexandria University! I applied for five positions, three in the U.S., one in Germany and one in Holland, and all of them with world-renowned professors. I received five offers and decided on Berkeley.  Early in 1974 we went to Berkeley, excited by the new opportunities. Culturally, moving from Philadelphia to Berkeley was almost as much of a shock as the transition from Alexandria to Philadelphia – Berkeley was a new world! I saw Telegraph Avenue for the first time, and this was sufficient to indicate the difference. I also met many graduate students whose language and behavior I had never seen before, neither in Alexandria, nor in Philadelphia. I interacted well with essentially everybody, and in some cases I guided some graduate students. But I also learned from members of the group. The obstacles did not seem as high as they had when I came to the University of Pennsylvania because culturally and scientifically I was better equipped. Berkeley was a great place for science – the BIG science. In the laboratory, my aim was to utilize the expertise I had gained from my Ph.D. work on the spectroscopy of pairs of molecules, called dimers, and to measure their coherence with the new tools available at Berkeley. Professor Charles Harris was traveling to Holland for an extensive stay, but when he returned to Berkeley we enjoyed discussing science at late hours! His ideas were broad and numerous, and in some cases went beyond the scientific language I was familiar with. Nevertheless, my general direction was established. I immediately saw the importance of the concept of coherence. I decided to tackle the problem, and, in a rather short time, acquired a rigorous theoretical foundation which was new to me. I believe that this transition proved vital in subsequent years of my research.  I wrote two papers with Charles, one theoretical and the other experimental. They were published in *Physical Review*. These papers were followed by other work, and I extended the concept of coherence to multidimensional systems, publishing my first independently authored paper while at Berkeley. In collaboration with other graduate students, I also published papers on energy transfer in solids. I enjoyed my interactions with the students and professors, and at Berkeley’s popular and well-attended physical chemistry seminars. Charles decided to offer me the IBM Fellowship that was only given to a few in the department. He strongly felt that I should get a job at one of the top universities in America, or at least have the experience of going to the interviews; I am grateful for his belief in me. I only applied to a few places and thought I had no chance at these top universities. During the process, I contacted Egypt, and I also considered the American University in Beirut (AUB). Although I visited some places, nothing was finalized, and I was preparing myself for the return. Meanwhile, I was busy and excited about the new research I was doing. Charles decided to build a picosecond laser, and two of us in the group were involved in this hard and “non-profitable” direction of research (!); I learned a great deal about the principles of lasers and their physics.  During this period, many of the top universities announced new positions, and Charles asked me to apply. I decided to send applications to nearly a dozen places and, at the end, after interviews and enjoyable visits, I was offered an Assistant Professorship at many, including Harvard, Caltech, Chicago, Rice, and Northwestern. My interview at Caltech had gone well, despite the experience of an exhausting two days, visiting each half hour with a different faculty member in chemistry and chemical engineering. The visit was exciting, surprising and memorable. The talks went well and I even received some undeserved praise for style. At one point, I was speaking about what is known as the FVH, picture of coherence, where F stands for [Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html), the famous Caltech physicist and Nobel Laureate. I went to the board to write the name and all of a sudden I was stuck on the spelling. Half way through, I turned to the audience and said, “you know how to spell Feynman”. A big laugh erupted, and the audience thought I was joking – I wasn’t! After receiving several offers, the time had come to make up my mind, but I had not yet heard from Caltech. I called the Head of the Search Committee, now a colleague of mine, and he was lukewarm, encouraging me to accept other offers. However, shortly after this, I was contacted by Caltech with a very attractive offer, asking me to visit with my family. We received the red carpet treatment, and that visit did cost Caltech! I never regretted the decision of accepting the Caltech offer.  My science family came from all over the world, and members were of varied backgrounds, cultures, and abilities. The diversity in this “small world” I worked in daily provided the most stimulating environment, with many challenges and much optimism. Over the years, my research group has had close to 150 graduate students, postdoctoral fellows, and visiting associates. Many of them are now in leading academic, industrial and governmental positions. Working with such minds in a village of science has been the most rewarding experience – Caltech was the right place for me.  My biological children were all “made in America”. I have two daughters, Maha, a Ph.D. student at the University of Texas, Austin, and Amani, a junior at Berkeley, both of whom I am very proud. I met Dema, my wife, by a surprising chance, a fairy tale. In 1988 it was announced that I was a winner of the King Faisal International Prize. In March of 1989, I went to receive the award from Saudi Arabia, and there I met Dema; her father was receiving the same prize in literature. We met in March, got engaged in July and married in September, all of the same year, 1989. Dema has her M.D. from Damascus University, and completed a Master’s degree in Public Health at UCLA. We have two young sons, Nabeel and Hani, and both bring joy and excitement to our life. Dema is a wonderful mother, and is my friend and confidante.  The journey from Egypt to America has been full of surprises. As a Moeid, I was unaware of the Nobel Prize in the way I now see its impact in the West. We used to gather around the TV or read in the newspaper about the recognition of famous Egyptian scientists and writers by the President, and these moments gave me and my friends a real thrill – maybe one day we would be in this position ourselves for achievements in science or literature. Some decades later, when President Mubarak bestowed on me the Order of Merit, first class, and the Grand Collar of the Nile (“Kiladate El Niel”), the highest State honor, it brought these emotional boyhood days back to my memory. I never expected that my portrait, next to the pyramids, would be on a postage stamp or that the school I went to as a boy and the road to Rosetta would be named after me. Certainly, as a youngster in love with science, I had no dreams about the honor of the Nobel Prize.  Since my arrival at Caltech in 1976, our contributions have been recognized by countries around the world. Among the awards and honors are:   |  | | --- | | Special Honors | | King Faisal International Prize in Science (1989). | | First Linus Pauling Chair, Caltech (1990). | | Wolf Prize in Chemistry (1993). | | Order of Merit, first class (Sciences & Arts), from President Mubarak (1995). | | Robert A. Welch Award in Chemistry (1997). | | Benjamin Franklin Medal, Franklin Institute, USA (1998). | | Egypt Postage Stamps, with Portrait (1998); the Fourth Pyramid (1999). | | Nobel Prize in Chemistry (1999). | | Grand Collar of the Nile, Highest State Honor, conferred by President Mubarak (1999). | |  | | Prizes and Awards | | Alfred P. Sloan Foundation Fellow (1978-1982). | | Camille and Henry Dreyfus Teacher-Scholar Award (1979-1985). | | Alexander von Humboldt Award for Senior United States Scientists (1983). | | National Science Foundation Award for especially creative research (1984; 1988; 1993). | | Buck-Whitney Medal, American Chemical Society (1985). | | John Simon Guggenheim Memorial Foundation Fellow (1987). | | Harrison Howe Award, American Chemical Society (1989). | | Carl Zeiss International Award, Germany (1992). | | Earle K. Plyler Prize, American Physical Society (1993). | | Medal of the Royal Netherlands Academy of Arts and Sciences, Holland (1993). | | Bonner Chemiepreis, Germany (1994). | | Herbert P. Broida Prize, American Physical Society (1995). | | Leonardo Da Vinci Award of Excellence, France (1995). | | Collége de France Medal, France (1995). | | Peter Debye Award, American Chemical Society (1996). | | National Academy of Sciences Award, Chemical Sciences, USA (1996). | | J.G. Kirkwood Medal, Yale University (1996). | | Peking University Medal, PU President, Beijing, China (1996). | | Pittsburgh Spectroscopy Award (1997). | | First E.B. Wilson Award, American Chemical Society (1997). | | Linus Pauling Medal Award (1997). | | Richard C. Tolman Medal Award (1998). | | William H. Nichols Medal Award (1998). | | Paul Karrer Gold Medal, University of Zürich, Switzerland (1998). | | E.O. Lawrence Award, U.S. Government (1998). | | Merski Award, University of Nebraska (1999). | | Röntgen Prize, (100th Anniversary of the Discovery of X-rays), Germany (1999). | |  | | Academies and Societies | | American Physical Society, Fellow (elected 1982). | | National Academy of Sciences, USA (elected 1989). | | Third World Academy of Sciences, Italy (elected 1989). | | Sigma Xi Society, USA (elected 1992). | | American Academy of Arts and Sciences (elected 1993). | | Académie Européenne des Sciences, des Arts et des Lettres, France (elected 1994). | | American Philosophical Society (elected 1998). | | Pontifical Academy of Sciences (elected 1999). | | American Academy of Achievement (elected 1999). | | Royal Danish Academy of Sciences and Letters (elected 2000) | |  | | Honorary Degrees | | Oxford University, UK (1991): M.A., h.c. | | American University, Cairo, Egypt (1993): D.Sc., h.c. | | Katholieke Universiteit, Leuven, Belgium (1997): D.Sc., h.c. | | University of Pennsylvania, USA (1997): D.Sc., h.c. | | Université de Lausanne, Switzerland (1997): D.Sc., h.c. | | Swinburne University, Australia (1999): D.U., h.c. | | Arab Academy for Science & Technology, Egypt (1999): H.D.A.Sc. | | Alexandria University, Egypt (1999): H.D.Sc. | | University of New Brunswick, Canada (2000): Doctoris in Scientia, D.Sc., h.c. | | Universita di Roma “La Sapienza”, Italy (2000): D.Sc., h.c. | | Université de Liège, Belgium (2000): Doctor *honoris causa*, D., h.c. |   From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1999*, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 2000  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 1999 **Addendum, January 2006** After the awarding of the Nobel Prize in 1999, I continued to serve as a faculty member at the California Institute of Technology (Caltech) as the [Linus Pauling](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1954/index.html) Chair Professor of Chemistry and Professor of Physics, and the Director of the Physical Biology Center for Ultrafast Science and Technology (UST) and the NSF Laboratory for Molecular Sciences (LMS). Current research is devoted to dynamical chemistry and biology, with a focus on the physics of elementary processes in complex systems. A major research frontier is the new development of “4D ultrafast diffraction and microscopy”, making possible the imaging of transient structures in space and time with atomic-scale resolution.  I have also devoted some time to giving public lectures in order to enhance awareness of the value of knowledge gained from fundamental research, and helping the population of developing countries through the promotion of science and technology for the betterment of society. Because of the unique East-West cultures that I represent, I wrote a book *Voyage Through Time – Walks of Life to the Nobel Prize* hoping to share the experience, especially with young people, and to remind them that it is possible! This book is in 12 editions and languages, so far.  Since the awarding of the Nobel Prize, the following are some of the awards and honors received:   |  | | --- | | Special Honors | | Postage Stamp, issued by the country of Ghana (2002) | | Ahmed Zewail Fellowships, University of Pennsylvania (2000–) | | Highest Order of the State from United Arab Emirates, Sudan, Tunisia, Lebanon (2000–) | | Ahmed Zewail Prize, American University in Cairo (2001–) | | Ahmed Zewail Prize for Creativity in the Arts, Opera House, Cairo (2004–) | | Zewail Foundation for Knowledge and Development, Cairo (2004–) | | Ahmed Zewail Prizes for Excellence and Leadership, ICTP, Trieste, Italy (2004–) | | Ahmed Zewail Award for Ultrafast Science and Technology, American Chemical Society (2005–). | |  | | Prizes and Awards | | Faye Robiner Award, Ross University School of Medicine, New York (2000) | | Golden Plate Award, American Academy of Achievement (2000) | | City of Pisa Medal, City Mayor, Pisa, Italy (2000) | | Medal of “La Sapienza” (“wisdom”), University of Rome (2000) | | Médaille de l’Institut du Monde Arabe, Paris, France (2000) | | Honorary Medal, Universite Du Centre, Monastir, Tunisia (2000) | | Honorary Medal, City of Monastir, from The Mayor, Tunisia (2000) | | Distinguished Alumni Award, University of Pennsylvania (2002) | | G.M. Kosolapoff Award, The American Chemical Society (2002) | | Distinguished American Service Award, ADC, Washington D.C. (2002) | | Sir C.V. Raman Award, Kolkata, India (2002) | | Arab American Award, National Museum, Dearborn, Michigan (2004) | | Gold Medal (Highest Honor), Burgos University, Burgos, Spain (2004) | | Grand Gold Medal, Comenius University, Bratislava, Slovak Republic (2005) | |  | | Academies and Societies | | American Association for the Advancement of Science (AAAS), Fellow (elected 2000) | | Chemical Society of India, Honorary Fellow (elected 2001) | | Indian Academy of Sciences (elected 2001) | | The Royal Society of London, Foreign Member (elected 2001) | | Sydney Sussex College, Honorary Fellow, Cambridge, U.K. (elected 2002) | | Indian National Science Academy, Foreign Fellow (elected 2002) | | Korean Academy of Science and Technology, Honorary Foreign Member (elected 2002) | | African Academy of Sciences, Honorary Fellow (elected 2002) | | Royal Society of Chemistry, Honorary Fellow, U.K. (elected 2003) | | Russian Academy of Sciences, Foreign Member (elected 2003) | | The Royal Swedish Academy of Sciences, Foreign Member (elected 2003) | | The Royal Academy of Belgium, Foreign Member (elected 2003) | | St. Catherine’s College, Honorary Fellow, Oxford, U.K. (elected 2004) | | European Academy of Sciences, Honorary Member, Belgium (elected 2004) | | The Literary & Historical Society, University College, Honorary Fellow, Dublin, Ireland (elected 2004) | | The National Society of High School Scholars, Honorary Member Board of Advisors, U.S.A. (elected 2004) | | Academy of Sciences of Malaysia, Honorary Fellow (elected 2005) | | French Academy of Sciences, Foreign Member (elected 2005) | |  | | Honorary Degrees | | Jadavpur University, India (2001): D.Sc., h.c. | | Concordia University, Montréal, Canada (2002): LLD, h.c. | | Heriot-Watt University, Scotland (2002): D.Sc., h.c. | | Pusan National University, Korea (2003): M.D., h.c. | | Lund University, Sweden (2003) : D.Ph., h.c. | | Bogaziçi University, Istanbul, Turkey (2003): D.Sc., h.c. | | École Normale Supérieure, Paris, France (2003): D.Sc., h.c. | | Oxford University, United Kingdom (2004): D.Sc., h.c. | | Peking University, People’s Republic of China (2004): H.D.D. | | Autonomous University of the State of Mexico, Toluca, Mexico (2004): D., h.c. | | University of Dublin, Trinity College, Ireland (2004): D.Sc., h.c. | | Tohoku University, Sendai, Japan (2005): H.D.D. | | American University of Beirut, Lebanon (2005): D.H.L. | | University of Buenos Aires, Argentina (2005): D., h.c. | | National University of Cordoba, Argentina (2005): D., h.c. |   For more updated biographical information, see: Zewail, Ahmed, *Voyage through Time. Walks of Life to the Nobel Prize.* American University in Cairo Press, Cairo, 2002.  *Ahmed Zewail died on 2 August 2016.* |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0365 |
| **Interview** |  |
| **Q2** | **Professor Zewail, let me first congratulate you to this year’s Nobel Prize in Chemistry for your pioneering work on femtosecond spectroscopy. Would you like to tell how you got started in this interesting field that you have developed?** |
|  | Well, you know, Professor Forsén, you know everybody thinks some times that science is done as a master plan and that somebody like me came from Mars and figured out everything and so on, but that’s really not the way it worked. We started at Caltech asking very simple questions: how do we understand the molecule dynamics or the dynamics of molecules, emotions and so on, molecules but in a coherent fashion. These molecules, there are millions and billions of them in the sample that we want to study, and they are all at random. And there are no relationship among them and we would like to just order them somehow so they all can behave at the end as if they are a single molecule at rejectory.  So in the beginning we were trying to develop new laser techniques to see how these molecules, if we prepare them coherent then study them in a coherent fashion that then we can learn about the intrinsic processes such as how does the energy move in the molecules, how molecules changing their orientation and so on. So we did some of the analogues of the powerful nuclear magnetic resonance techniques which you are very familiar with and used coherent pulses and pulse sequences. But then around 1980 I was asking the question: can we do the same thing in a single isolated molecule, completely isolated from the rest of the world? The environment is not there. So the question of using then a combination of molecular beams and ultra fast lasers came to mind and we started to initiate this research area which I had no idea what’s involved in terms of molecule beams but we had my students and I had to get into this area. And fundamentally we’re thinking about if we now excite an isolated molecule, no collision no /- – -/, that if we excite certain vibration in the molecule how would the energy move into all the other modes in the molecule.  And that was really the beginning of femtochemistry science …  There are many theories and many ideas of how this, and the typical one it goes back to your [Arrhenius](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1903/arrhenius-facts.html), namely that the energy will go in a random way. But to the pleasant surprise of all of us is that the energy was not randomising on the short time scale. We found actually that the energy goes from this mode to another mode and comes back even though we have millions, we have a whole forest of modes in the molecule, but the molecule is behaving very selectively and coherently and the molecules are very complex that we were studying. And that was really the beginning of femtochemistry science, because it immediately indicated to us that if we can excite molecules and initiate this in molecules on a very short time scale prior to their vibration and rotational and scrambling of energy and everything we should be seeing a whole new world of ordered coherence in them and by -87 we made what the Nobel citation called ‘the breakthrough’. Namely that by so doing we’re able to see the motions of the real atoms inside the molecules and with atomic scale resolution of the dynamics. |
| **Q14** | **Professor Zewail, the resolution at which one can study chemical reactions has been constantly decreasing over the past century. Do you think it will be possible, or is it even necessary to go further, right there in the future field of autochemistry?** |
|  | This is a legitimate question actually, because I wrote something about this, but the Nobel citation, which was very well written, actually pointed out that this is hint of the, if you like, there is a Guinness time. The reason for that it’s fundamental. It is not that we have to keep shortening the time. It turns out all molecular and biological systems have speeds of the atoms move inside them, the fastest possible speeds are determined by their molecular vibrations and this speeds is about a kilometre per second. One thousand metre per second, which means that if you have femtosecond time resolution or 10 to the -15 or 10 -14 second you have a distance resolution of about a 10th of an Ångström. That means really that you are now frozen all the chemical or biological structures on that times kill you can’t do any better.  What I think the OCT or sub-femtosecond is going to be important and I suggested this in one review article would be to go into the motion of the electrons. So, who knows it may be that we will see one day how we can localise also the electron just like we did with the nuclei. But I think that will be beyond the realm of chemistry and biology, you will be now addressing issues in physics looking at semi conductors and high energy physics and so on. But that’s … I don’t want to commit myself and then proven to be wrong later but that’s basically what I think about it at this point. |
| **Q14** | **How do you see fusion directions in the studies of chemical and biological phenomena or time is loosen or the aura of femtoseconds, would it be possible to make sort of a snap shots x-ray the fraction in femtoseconds time scales or electron defraction. How do you see the future let’s say the next 10, 15, 20 years?** |
|  | I’m glad you asked this question, Professor Forsén, firstly because I thought in my Nobel Lecture I pointed that I was delighted that the Swedish Academy of Science did not quote anything about my current work right now, because the current work that my group is focusing on is actually both the time resolve electrons and possibly x-rays to be able to get the architecture of these molecules, the molecular structures themselves, of very complex biological systems. That’s the ultimate goal. We have succeeded so far to do this defraction type experiment on smaller molecules and look at intermediate transit structure but you can imagine the power of this maybe in 10 years, when we are able to look say at the intermediates of proteins process. Or the motions of some of the DNA structures that we are interested in. The effect of salvation does the hydrophobic forces play a fundamental role in the process of folding. There was just a numerous questions I would like to ask and we really have to look at these snap shots at early times. |
| **Q14** | **Femtochemistry provides really strong possibilities for new drugs, do you think it will ever be possible to study at that level in a living cell, perhaps study enzyme reactions?** |
|  | Well. that will be really a fantastic advance. Of course the difficulty usually we get into this at this point in time is that in the cell there are the medium itself and the water and so on but one advantage of the femtosecond time resolution, and we demonstrated this in a number of studies, is that even in liquid type environment same coherence I was talking about and the motions that we see we can actually isolate because the protonation from the solvent is not turned on yet on that timescale. So, some people have started to use actually femtosecond methods to image processes in cells. There has been work on DNA, in fact I’m involved in this with my colleagues at Caltech in the looking at the transport of electrons in DNA. A lot of work has been done on proteins including cytocromoxides and others so, you’ll see a variety of applications in the coming years. |
| **Q10** | **I know that you have been or you have a strong desire to promote science and help young scientist’s in your country of birth, Egypt. What are your plans in this direction? Could you tell something about that?** |
|  | Yes, I do feel quite strongly about this that probably one of the things that unfortunately this age now to get a Nobel Prize is to really use part of it to help the young people get excited about science. In the States, in the world at large but also with particular focus on Egypt because I came from Egypt and I owe Egypt a lot to what I am now. And I do feel that there are tremendous amount of talent in Egypt, human resources. Human resources are just tremendous in Egypt but we need the science base, we need the correct science base. How to get these people to interact with each other. When I give a lecture on Egypt there are thousands of people in the lecture hall, so obviously they would like to go to science and they would love to do science, but you really have to get the correct science base in order for them to interact, so we are hoping to have maybe a little Caltech maybe a little /- – -/ do you know that just to start something there that really both Egyptians and colleagues from the western world and so on can interact with each other, give the students the exchange and do science at the frontiers. |
| **Q9** | **I think the Nobel Prize will help you in this respect?** |
|  | I think the Nobel Prize helps for a number of reasons. Number one, if I can be frank, there is these people will feel by getting a Nobel Prize that I’m one of them, that it is possible to contribute on the world map of science and technology. That’s not only in the hands of the Swedes and the Americans and the British and so on. That they could if they worked hard and they have the abilities and so on, they can achieve even on the highest level. And the other thing also which I’m hoping for is that the government in Egypt is willing and interested in promoting science and technology and this is an ideal time now to be able to do something. |
| **Q1** | **Are there certain fields of science which you think might be especially well suited to young scientist in Egypt?** |
|  | I think in Egypt and all over the world you know, David, there are many books I read talking about the end of science. There are many, many books I’ve read and I think this is quite naïve actually because we all just try to uncover something. But the universe at large is full of questions that we still don’t know anything about and there will be always young people brilliant who are going to make new discoveries. I mean if you think in 1999, now this is an ending year of this millennium and you just think about the world of the very small, thinking about manipulating the atoms and the molecules. Think about the whole world of biological complex sciences. We still don’t understand the way a protein folds the way it does. I mean it’s an amazing thing. So, all of this is opened up, you go to the very big, you go to astronomy, we still don’t understand how the big bang and evolving all the way to the human species and so on. So all of this is going to be a very, very exciting to the new people. But for me to sit down here, even as a Nobel Laureate and make a prediction about which science I think that will be a mistake. |
| **Q10** | **Scientists are supposed to be creative and surely they certainly are and you were a most creative man. What are your best creative moments, is it in the shower when you are listening to music in the early mornings can you say something about that?** |
|  | Yes, it’s a crazy actually moments you just never know. Well, one of the things I enjoy most is to be left alone with a book. I am not one of the new media experts working all the time with my computers and the PowerPoint’s and things of that sort. So, I’m an old fashioned still in this regard but these are the moment where I really can be creative, if I am, to be left alone with just a book and piece of paper and to be thinking. And I think actually one of the messages to the young people building on David’s question, is the internet and all this wonderful thing we have, these are not thinking machines. These are information gathering, but you should have some time to think and that’s very important. |
| **Q9** | **Nobel Laureates usually complain that getting the Nobel Prize perturbs their life infinitely and how do you try to get away from, to find your sort of precious moments or creativity in the future?** |
|  | Well, I’ll tell you a story and I’ll tell you how I’m trying to do it. When they called me with the Nobel call from Secretary General of the Swedish Academy it was twenty minutes to six and he said well that was well hope I’m not disturbing you but I am the Secretary General of the Swedish Academy. Of course you can imagine I was frozen in time when he said that but then he made a very famous statement, something to the effect that this is the last 20 minutes of peace of your life. And he was right at six o’clock exactly until today I just have not had the time to be in equilibrium with myself. We receive thousands of e-mails and faxes and so on. But I want to really focus on doing this two things I really would like to focus on science and the excitement of science and to help with science. So, that’s my intention to focus my efforts there and I hope I will be able to do so. |
| **Q6** | **Back to your previous answer going to the internet, there’s a lot of talk about information overload that there’s so much information flowing out especially with computers and things that people get sort of too much and can’t think any longer. Do you think that that could be a problem for scientists that it could eventually even become detrimental that there so much information that it becomes difficult to sort of concentrate on a specific area?** |
|  | Yes, I do have a concern however, humanity has a great way of adapting and I’m sure scientists of the future, probably after I leave this planet, earth will have a new way of dealing with the internet but I do have a concern in the transition period, namely that … I’ll give you an example, when I was a graduate student and we get a journal, let’s say in your case molecular biology or if you’re not chemical physics or something. You know we are dying to take the whole journal and find out the different areas of science. Professor Forsén is aware of many areas that going on in from physical chemistry to molecular biology but because of the internet nowadays people are getting more and more specialised and so you found that I found with my students they don’t necessarily look at journals any more, but they print right away from the internet what’s relevant to what’s he doing you see. Yes and you’re probably a living example. |
| **Q1** | **We have actually received a question from Egypt from a student at the American University in Cairo and he says Sir, addressing you, in your judgment, who was the best chemist, physicist, biologist in this millennium?** |
|  | Millennium, yes, not century, well I think that’s a million dollar type question. But I would like to tell this young person who’s asking the question from the American University in Cairo, is that in science doesn’t work this way. Scientists contribute in a variety of ways and I don’t think I can singular one even including Einstein, that I can say that he’s the best. We don’t work like the best basketball player and the best musician and so on. Science is a collective effort. I built on the efforts of a previous scientist, others will build on the work I’m doing and if I look at the whole scope from chemistry to biology to physics, it’s just the list is too long to mention just one and it’s not fair to the others. |
| **ID** | **0366** |
| Biographical | I suppose I am not the first Nobelist who, on the occasion of receiving this Prize, wonders how on earth, by what strange alchemy of family background, teachers, friends, talents and especially accidents of history and of personal life he or she arrived at this point. I have browsed in previous volumes of “Les Prix Nobel” and I know that there are others whose eventual destinies were foreshadowed early in their lives – mathematical precocity, champion bird watching, insatiable reading, mechanical genius. Not in my case, at least not before my late teens. On the contrary: An early photo of my older sister and myself, taken at a children’s costume party in Vienna – I look about 7 years old – shows me dressed up in a dark suit and a black top hat, toy glasses pushed down my nose, and carrying a large sign under my arm with the inscription “Professor Know-Nothing”.  Here then is my attempt to convey to the reader how, at age 75, I see my life which brought me to the present point: a long-retired professor of theoretical physics at the University of California, still loving and doing physics, including chemical physics, mostly together with young people less than half my age; moderately involved in the life of my community of Santa Barbara and in broader political and social issues; with unremarkable hobbies such as listening to classical music, reading (including French literature), walking with my wife Mara or alone, a little cooking (unjustifiably proud of my ratatouille); and a weekly half hour of relaxed roller blading along the shore, a throwback to the ice-skating of my Viennese childhood. My three daughters and three grandchildren all live in California and so we get to see each other reasonably often.  I was naturalized as an American citizen in 1957 and this has been my primary self-identity ever since. But, like many other scientists, I also have a strong sense of global citizenship, including especially Canada, Denmark, England, France and Israel, where I have worked and lived with a family for considerable periods, and where I have some of my closest friends.  My feelings towards Austria, my native land, are – and will remain – very painful. They are dominated by my vivid recollections of 1 1/2 years as a Jewish boy under the Austrian Nazi regime, and by the subsequent murder of my parents, Salomon and Gittel Kohn, of other relatives and several teachers, during the holocaust. At the same time I have in recent years been glad to work with Austrians, one or two generations younger than I: Physicists, some teachers at my former High School and young people (Gedenkdiener) who face the dark years of Austria’s past honestly and constructively.  On another level, I want to mention that I have a strong Jewish identity and – over the years – have been involved in several Jewish projects, such as the establishment of a strong program of Judaic Studies at the University of California in San Diego.  My father, who had lost a brother, fighting on the Austrian side in World War I, was a committed pacifist. However, while the Nazi barbarians and their collaborators threatened the entire world, I could not accept his philosophy and, after several earlier attempts, was finally accepted into the Canadian Infantry Corps during the last year of World War II. Many decades later I became active in attempts to bring an end to the US-Soviet nuclear arms race and became a leader of unsuccessful faculty initiatives to terminate the role of the University of California as manager of the nuclear weapons laboratories at Los Alamos and Livermore. I offered early support to Jeffrey Leiffer, the founder of the student [Pugwash](https://www.nobelprize.org/nobel_prizes/peace/laureates/1995/index.html) movement which concerns itself with global issues having a strong scientific component and in which scientists can play a useful role. Twenty years after its founding this organization continues strong and vibrant. My commitment to a humane and peaceful world continues to this day. I have just joined the Board of the Population Institute because I am convinced that early stabilization of the world’s population is important for the attainment of this objective.  After these introductory general reflections from my present vantage point I would now like to give an idea of my childhood and adolescence. I was born in 1923 into a middle class Jewish family in Vienna, a few years after the end of World War I, which was disastrous from the Austrian point of view. Both my parents were born in parts of the former Austro-Hungarian Empire, my father in Hodonin, Moravia, my mother in Brody, then in Galicia, Poland, now in the Ukraine. Later they both moved to the capital of Vienna along with their parents. I have no recollection of my father’s parents, who died relatively young. My maternal grandparents Rappaport were orthodox Jews who lived a simple life of retirement and, in the case of my grandfather, of prayer and the study of religious texts in a small nearby synagogue, a Schul as it was called. My father carried on a business, Postkartenverlag Brueder Kohn Wien I, whose main product was high quality art postcards, mostly based on paintings by contemporary artists which were commissioned by his firm. The business had flourished in the first two decades of the century but then, in part due to the death of his brother Adolf in World War I, to the dismantlement of the Austrian monarchy and to a worldwide economic depression, it gradually fell on hard times in the 1920s and 1930s. My father struggled from crisis to crisis to keep the business going and to support the family. Left over from the prosperous times was a wonderful summer property in Heringsdorf at the Baltic Sea, not far from Berlin, where my mother, sister and I spent our summer vacations until Hitler came to power in Germany in 1933. My father came for occasional visits (the firm had a branch in Berlin). My mother was a highly educated woman with a good knowledge of German, Latin, Polish and French and some acquaintance with Greek, Hebrew and English. I believe that she had completed an academically oriented High School in Galicia. Through her parents we maintained contact with traditional Judaism. At the same time my parents, especially my father, also were a part of the secular artistic and intellectual life of Vienna.  After I had completed a public elementary school, my mother enrolled me in the Akademische Gymnasium, a fine public high school in Vienna’s inner city. There, for almost five years, I received an excellent education, strongly oriented toward Latin and Greek, until March 1938, when Hitler Germany annexed Austria. (This so-called Anschluss was, after a few weeks, supported by the great majority of the Austrian population). Until that time my favorite subject had been Latin, whose architecture and succinctness I loved. By contrast, I had no interest in, nor apparent talent for, mathematics which was routinely taught and gave me the only C in high school. During this time it was my tacit understanding that I would eventually be asked to take over the family business, a prospect which I faced with resignation and without the least enthusiasm.  The Anschluss changed everything: The family business was confiscated but my father was required to continue its management without any compensation; my sister managed to emigrate rather promptly to England; and I was expelled from my school.  In the following fall I was able to enter a Jewish school, the Chajes Gymnasium, where I had two extraordinary teachers: In physics, Dr. Emil Nohel, and in mathematics Dr. Victor Sabbath. While outside the school walls arbitrary acts of persecution and brutality took place, on the inside these two inspired teachers conveyed to us their own deep understanding and love of their subjects. I take this occasion to record my profound gratitude for their inspiration to which I owe my initial interest in science. (Alas, they both became victims of Nazi barbarism).  I note with deep gratitude that twice, during the Second World War, after having been separated from my parents who were unable to leave Austria, I was taken into the homes of two wonderful families who had never seen me before: Charles and Eva Hauff in Sussex, England, who also welcomed my older sister, Minna. Charles, like my father, was in art publishing and they had a business relationship. A few years later, Dr. Bruno Mendel and his wife Hertha of Toronto, Canada, took me and my friend Joseph Eisinger into their family. (They also supported three other young Nazi refugees). Both of these families strongly encouraged me in my studies, the Hauffs at the East Grinstead County School in Sussex and the Mendels at the University of Toronto. I cannot imagine how I might have become a scientist without their help.  My first wife, Lois Kohn, gave me invaluable support during the early phases of my scientific career; my present wife of over 20 years, Mara, has supported me in the latter phases of my scientific life. She also created a wonderful home for us, and gave me an entire new family, including her father Vishniac, a biologist as well as a noted photographer of pre-war Jewish communities in Eastern Europe, and her mother Luta. (They both died rather recently, well into their nineties).  After these rather personal reminiscences I now turn to a brief description of my life as a scientist.  When I arrived in England in August 1939, three weeks before the outbreak of World War II, I had my mind set on becoming a farmer (I had seen too many unemployed intellectuals during the 1930s), and I started out on a training arm in Kent. However, I became seriously ill and physically weak with meningitis, and so in January 1940 my “acting parents”, the Hauffs, arranged for me to attend the above-mentioned county school, where – after a period of uncertainty – I concentrated on mathematics, physics and chemistry.  However, in May 1940, shortly after I had turned 17, and while the German army swept through Western Europe and Britain girded for a possible German air-assault, Churchill ordered most male “enemy aliens” (i.e., holders of enemy passports, like myself) to be interned (“Collar the lot” was his crisp order). I spent about two months in various British camps, including the Isle of Man, where my school sent me the books I needed to study. There I also audited, with little comprehension, some lectures on mathematics and physics, offered by mature interned scientists.  In July 1940, I was shipped on, as part of a British convoy moving through U-boat-infested waters, to Quebec City in Canada; and from there, by train, to a camp in Trois Rivieres, which housed both German civilian internees and refugees like myself. Again various internee-taught courses were offered. The one which interested me most was a course on set-theory given by the mathematician Dr. Fritz Rothberger and attended by two students. Dr. Rothberger, from Vienna, a most kind and unassuming man, had been an advanced private scholar in Cambridge, England, when the internment order was issued. His love for the intrinsic depth and beauty of mathematics was gradually absorbed by his students.  Later I was moved around among various other camps in Quebec and New Brunswick. Another fellow internee, Dr. A. Heckscher, an art historian, organized a fine camp school for young people like myself, whose education had been interrupted and who prepared to take official Canadian High School exams. In this way I passed the McGill University junior Matriculation exam and exams in mathematics, physics and chemistry on the senior matriculation level. At this point, at age 18, I was pretty firmly looking forward to a career in physics, with a strong secondary interest in mathematics.  I mention with gratitude that camp educational programs received support from the Canadian [Red Cross](https://www.nobelprize.org/nobel_prizes/peace/laureates/1917/index.html) and Jewish Canadian philanthropic sources. I also mention that in most camps we had the opportunity to work as lumberjacks and earn 20 cents per day. With this princely sum, carefully saved up, I was able to buy Hardy’s Pure Mathematics and Slater’s Chemical Physics, books which are still on my shelves. In January 1942, having been cleared by Scotland Yard of being a potential spy, I was released from internment and welcomed by the family of Professor Bruno Mendel in Toronto. At this point I planned to take up engineering rather than physics, in order to be able to support my parents after the war. The Mendels introduced me to Professor Leopold Infeld who had come to Toronto after several years with [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/index.html). Infeld, after talking with me (in a kind of drawing room oral exam), concluded that my real love was physics and advised me to major in an excellent, very stiff program, then called mathematics and physics, at the University of Toronto. He argued that this program would enable me to earn a decent living at least as well as an engineering program.  However, because of my now German nationality, I was not allowed into the chemistry building, where war work was in progress, and hence I could not enroll in any chemistry courses. (In fact, the last time I attended a chemistry class was in my English school at the age of 17.) Since chemistry was required, this seemed to sink any hope of enrolling. Here I express my deep appreciation to Dean and head of mathematics, Samuel Beatty, who helped me, and several others, nevertheless to enter mathematics and physics as special students, whose status was regularized one or two years later.  I was fortunate to find an extraordinary mathematics and applied mathematics program in Toronto. Luminous members whom I recall with special vividness were the algebraist Richard Brauer, the non-Euclidean geometer, H.S.M. Coxeter, the aforementioned Leopold Infeld, and the classical applied mathematicians John Lighton Synge and Alexander Weinstein. This group had been largely assembled by Dean Beatty. In those years the University of Toronto team of mathematics students, competing with teams from the leading North-American Institutions, consistently won the annual Putman competition. (For the record I remark that I never participated). Physics too had many distinguished faculty members, largely recruited by John C. McLennan, one of the earliest low temperature physicists, who had died before I arrived. They included the [Raman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1930/index.html) specialist H.L. Welsh, M.F. Crawford in optics and the low-temperature physicists H.G. Smith and A.D. Misener. Among my fellow students was [Arthur Schawlow](https://www.nobelprize.org/nobel_prizes/physics/laureates/1981/index.html), who later was to share the Nobel Prize for the development of the laser.  During one or two summers, as well as part-time during the school year, I worked for a small Canadian company which developed electrical instruments for military planes. A little later I spent two summers, working for a geophysicist, looking for (and finding!) gold deposits in northern Ontario and Quebec.  After my junior year I joined the Canadian Army. An excellent upper division course in mechanics by A. Weinstein had introduced me to the dynamics of tops and gyroscopes. While in the army I used my spare time to develop new strict bounds on the precession of heavy, symmetrical tops. This paper, “Contour Integration in the Theory of the Spherical Pendulum and the Heavy Symmetrical Top” was published in the Transactions of American Mathematical Society. At the end of one year’s army service, having completed only 2 1/2 out of the 4-year undergraduate program, I received a war-time bachelor’s degree “on – active – service” in applied mathematics.  In the year 1945-6, after my discharge from the army, I took an excellent crash master’s program, including some of the senior courses which I had missed, graduate courses, a master’s thesis consisting of my paper on tops and a paper on scaling of atomic wave-functions.  My teachers wisely insisted that I do not stay on in Toronto for a Ph.D, but financial support for further study was very hard to come by. Eventually I was thrilled to receive a fine Lehman fellowship at Harvard. Leopold Infeld recommended that I should try to be accepted by [Julian Schwinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html), whom he knew and who, still in his 20s, was already one of the most exciting theoretical physicists in the world.  Arriving from the relatively isolated University of Toronto and finding myself at the illustrious Harvard, where many faculty and graduate students had just come back from doing brilliant war-related work at Los Alamos, the MIT Radiation Laboratory, etc., I felt very insecure and set as my goal survival for at least one year. The Department Chair, [J.H. Van Vleck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/index.html), was very kind and referred to me as the Toronto-Kohn to distinguish me from another person who, I gathered, had caused some trouble. Once Van Vleck told me of an idea in the band-theory of solids, later known as the quantum defect method, and asked me if I would like to work on it. I asked for time to consider. When I returned a few days later, without in the least grasping his idea, I thanked him for the opportunity but explained that, while I did not yet know in what subfield of physics I wanted to do my thesis, I was sure it would not be in solid state physics. This problem then became the thesis of Thomas Kuhn, (later a renowned philosopher of science), and was further developed by myself and others. In spite of my original disconnect with Van Vleck, solid state physics soon became the center of my professional life and Van Vleck and I became lifelong friends.  After my encounter with Van Vleck I presented myself to Julian Schwinger requesting to be accepted as one of his thesis students. His evident brilliance as a researcher and as a lecturer in advanced graduate courses (such as waveguides and nuclear physics) attracted large numbers of students, including many who had returned to their studies after spending “time out” on various war-related projects.  I told Schwinger briefly of my very modest efforts using variational principles. He himself had developed brilliant new Green’s function variational principles during the war for wave-guides, optics and nuclear physics (Soon afterwards Green’s functions played an important role in his Nobel-Prize-winning work on quantum electrodynamics). He accepted me within minutes as one of his approximately 10 thesis students. He suggested that I should try to develop a Green’s function variational method for *three*-body scattering problems, like low-energy neutron-deuteron scattering, while warning me ominously, that he himself had tried and failed. Some six months later, when I had obtained some partial, very unsatisfactory results, I looked for alternative approaches and soon found a rather elementary formulation, later known as Kohn’s variational principle for scattering, and useful for nuclear, atomic and molecular problems. Since I had circumvented Schwinger’s beloved Green’s functions, I felt that he was very disappointed. Nevertheless he accepted this work as my thesis in 1948. (Much later L. Fadeev offered his celebrated solution of the three-body scattering problem).  My Harvard friends, close and not so close, included [P.W. Anderson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/index.html), [N. Bloembergen](https://www.nobelprize.org/nobel_prizes/physics/laureates/1981/index.html), H. Broida (a little later), K. Case, F. De Hoffman, J. Eisenstein, R. Glauber, T. Kuhn, R. Landauer, [B. Mottelson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1975/index.html), G. Pake, F. Rohrlich, and C. Slichter. Schwinger’s brilliant lectures on nuclear physics also attracted many students and Postdocs from MIT, including J. Blatt, M. Goldberger, and J.M. Luttinger. Quite a number of this remarkable group would become lifelong friends, and one – J.M. “Quin” Luttinger – also my closest collaborators for 13 years, 1954-66. Almost all went on to outstanding careers of one sort or another.  I was totally surprised and thrilled when in the spring of 1948 Schwinger offered to keep me at Harvard for up to three years. I had the choice of being a regular post-doctoral fellow or dividing my time equally between research and teaching. Wisely – as it turned out – I chose the latter. For the next two years I shared an office with Sidney Borowitz, later Chancellor of New York University, who had a similar appointment. We were to assist Schwinger in his work on quantum electrodynamics and the emerging field theory of strong interactions between nucleons and mesons. In view of Schwinger’s deep physical insights and celebrated mathematical power, I soon felt almost completely useless. Borowitz and I did make some very minor contributions, while the greats, especially Schwinger and [Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html), seemed to be on their way to unplumbed, perhaps ultimate depths.  For the summer of 1949, I got a job in the Polaroid laboratory in Cambridge, Mass., just before the Polaroid camera made its public appearance. My task was to bring some understanding to the mechanism by which charged particles falling on a photographic plate lead to a photographic image. (This technique had just been introduced to study cosmic rays). I therefore needed to learn something about solid state physics and occasionally, when I encountered things I didn’t understand, I consulted Van Vleck.  It seems that these meetings gave him the erroneous impression that I knew something about the subject. For one day he explained to me that he was about to take a leave of absence and, “since you are familiar with solid state physics”, he asked me if I could teach a course on this subject, which he had planned to offer. This time, frustrated with my work on quantum field theory, I agreed. I had a family, jobs were scarce, and I thought that broadening my competence into a new, more practical, area might give me more opportunities.  So, relying largely on the excellent, relatively recent monograph by F. Seitz, “Modern Theory Of Solids”, I taught one of the first broad courses on Solid State Physics in the United States. My “students” included several of my friends, N. Bloembergen, C. Slichter and G. Pake who conducted experiments (later considered as classics) in the brand-new area of nuclear magnetic resonance which had just been opened up by [E. Purcell](https://www.nobelprize.org/nobel_prizes/physics/laureates/1952/index.html) at Harvard and [F. Bloch](https://www.nobelprize.org/nobel_prizes/physics/laureates/1952/index.html) at Stanford. Some of my students often understood much more than I, they were charitable towards their teacher.  At about the same time I did some calculations suggested by Bloembergen, on the recently discovered, so-called Knight shift of nuclear magnetic resonance, and, in this connection, returning to my old love of variational methods, developed a new variational approach to the study of wavefunctions in periodic crystals.  Although my appointment was good for another year and a half, I began actively looking for a more long-term position. I was a naturalized Canadian citizen, with the warmest feelings towards Canada, and explored every Canadian university known to me. No opportunities presented themselves. Neither did the very meager US market for young theorists yield an academic offer. At this point a promising possibility appeared for a position in a new Westinghouse nuclear reactor laboratory outside of Pittsburgh. But during a visit it turned out that US citizenship was required and so this possibility too vanished. At that moment I was unbelievably lucky. While in Pittsburgh, I stayed with my Canadian friend Alfred Schild, who taught in the mathematics department at the Carnegie Institute of Technology (now Carnegie Mellon University). He remarked that F. Seitz and several of his colleagus had just left the physics department and moved to Illinois, so that – he thought – there might be an opening for me there. It turned out that the Department Chair, Ed Creutz was looking rather desperately for somebody who could teach a course in solid state physics and also keep an eye on the graduate students who had lost their “doctor-fathers”. Within 48 hours I had a telegram offering me a job!  A few weeks later a happy complication arose. I had earlier applied for a National Research Council fellowship for 1950-51 and now it came through. A request for a short postponement was firmly denied. Fortunately, Ed Creutz agreed to give me a one-year leave of absence, provided I first taught a compressed course in solid state physics. So on December 31, 1950 (to satisfy the terms of my fellowship) I arrived in Copenhagen.  Originally I had planned to revert to nuclear physics there, in particular the structure of the deuteron. But in the meantime I had become a solid state physicist. Unfortunately no one in Copenhagen, including [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html), had even heard the expression “Solid State Physics”. For a while I worked on old projects. Then, with an Indian visitor named Vachaspati (no initial), I published a criticism of Froehlich’s pre-BCS theory of superconductivity, and also did some work on scattering theory.  In the spring of 1951, I was told that an expected visitor for the coming year had dropped out and that the Bohr Institute could provide me with an Oersted fellowship to remain there until the fall of 1952. Very exciting work was going on in Copenhagen, which eventually led to the great “Collective Model of the Nucleus” of A. Bohr and B. Mottelson, both of whom had become close friends. Furthermore my family and I had fallen in love with Denmark and the Danish people. A letter from Niels Bohr to my department chair at Carnegie quickly resulted in the extension of my leave of absence till the fall of 1952.  In the summer of 1951, I became a substitute teacher, replacing an ill lecturer at the first summer school at Les Houches, near Chamonix in France, conceived and organized by a dynamic young French woman, Cécile Morette De Witt. As an “expert” in solid state physics, I offered a few lectures on that subject. [Wolfgang Pauli](https://www.nobelprize.org/nobel_prizes/physics/laureates/1945/index.html), who visited, when he learned of my meager knowledge of solids, mostly metallic sodium, asked me, true to form, if I was a professor of physics or of sodium. He was equally acerbic about himself. Some 50 years old at the time, he described himself as “a child-wonder in menopause” (“ein Wunderkind in den Wechseljahren”). But my most important encounter was with Res Jost, an assistant of Pauli at the ETH in Zurich, with whom I shared an interest in the so-called inverse scattering problem: given asymptotic information, (such as phase-shifts as function of energy), of a particle scattered by a potential V(r), what quantitative information can be inferred about this potential? Later that year, we both found ourselves in Copenhagen and addressed this problem in earnest. Jost, at the time a senior fellow at the Institute for Advanced Study in Princeton, had to return there before we had finished our work. A few months later, in the spring of 1952, I received an invitation from Robert Oppenheimer, to come to Princeton for a few weeks to finish our project. In an intensive and most enjoyable collaboration, we succeeded in obtaining a complete solution for S-wave scattering by a spherical potential. At about the same time I.M. Gel’fand in the Soviet Union published his celebrated work on the inverse problem. Jost and I remained close lifelong friends until his death in 1989.  After my return to Carnegie Tech in 1952, I began a major collaboration with N. Rostoker, then an assistant of an experimentalist, later a distinguished plasma theorist. We developed a theory for the energy band structure of electrons for periodic potentials, harking back to my earlier experience with scattering, Green’s functions and variational methods. We showed how to determine the bandstructure from a knowledge of purely geometric structure constants and a small number (~ 3) of scattering phase-shifts of the potential in a single sphericalized cell. By a different approach this theory was also obtained by J. Korringa. It continues to be used under the acronym KKR. Other work during my Carnegie years, 1950-59, includes the image of the metallic [Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html) Surface in the phonon spectrum (Kohn anomaly); exponential localization of Wannier functions; and the nature of the insulating state.  My most distinguished colleague and good friend at Carnegie was G.C. Wick, and my first PhD’s were D. Schechter and V. Ambegaokar. I also greatly benefitted from my interaction with T. Holstein at Westinghouse.  In 1953, with support from Van Vleck, I obtained a summerjob at Bell Labs as assistant of [W. Shockley](https://www.nobelprize.org/nobel_prizes/physics/laureates/1956/index.html), the co-inventor of the transistor. My project was radiation damage of Si and Ge by energetic electrons, critical for the use of the recently developed semiconductor devices for applications in outer space. In particular, I established a reasonably accurate energy threshold for permanent displacement of a nucleus from its regular lattice position, substantially smaller than had been previously presumed. Bell Labs at that time was without question the world’s outstanding center for research in solid state physics and for the first time, gave me a perspective over this fascinating, rich field. [Bardeen](https://www.nobelprize.org/nobel_prizes/physics/laureates/1956/index.html), [Brattain](https://www.nobelprize.org/nobel_prizes/physics/laureates/1956/index.html) and Shockley , after their invention of the transistor, were the great heroes. Other world class theorists were C. Herring, G. Wannier and my brilliant friend from Harvard, P.W. Anderson. With a few interruptions I was to return to Bell Labs every year until 1966. I owe this institution my growing up from amateur to professional.  In the summer of 1954 both Quin Luttinger and I were at Bell Labs and began our 13-year long collaborations, along with other work outside our professional “marriage”. (Our close friendship lasted till his death in 1997). The all-important impurity states in the transistor materials Si and Ge, which govern their electrical and many of their optical properties, were under intense experimental study, which we complemented by theoretical work using so-called effective mass theory. In 1957, I wrote a comprehensive review on this subject. We (mostly Luttinger) also developed an effective Hamiltonian in the presence of magnetic fields, for the complex holes in these elements. A little later we obtained the first non-heuristic derivation of the Boltzman transport equation for *quantum mechanical* particles. There followed several years of studies of many-body theories, including Luttinger’s famous one-dimensional “Luttinger liquid” and the “Luttinger’s theorem” about the conservation of the volume enclosed by a metallic Fermi surface, in the presence of electron electron interaction. Finally, in 1966, we showed that superconductivity occurs even with purely repulsive interactions – contrary to conventional wisdom and possibly relevant to the much later discovery of high-Tc superconductors.  In 1960, when I moved to the University of California San Diego, California, my scientific interactions with Luttinger, then at Columbia University, and with Bell Labs gradually diminished. I did some consulting at the nearby General Atomic Laboratory, interacting primarily with J. Appel. My university colleagues included G. Feher, B. Maple, B. Matthias, S. Schultz, H. Suhl and J. Wheatley, – a wonderful environment. During my 19-year stay there I typically worked with two postdocs and four graduate students. A high water mark period were the late 1960s, early 1970s, including N. Lang, D. Mermin, M. Rice, L.J. Sham, D. Sherrington, and J. Smith.  I now come to the development of density functional theory (DFT). In the fall of 1963, I spent a sabbatical semester at the École Normale Supérieure in Paris, as guest and in the spacious office of my friend Philippe Nozières. Since my Carnegie days I had been interested in the electronic structure of alloys, a subject of intense experimental interest in both the physics and metallurgy departments. In Paris I read some of the metallurgical literature, in which the concept of the effective charge e\* of an atom in an alloy was prominent, which characterized in a rough way the transfer of charge between atomic cells. It was a *local* point of view in *coordinate space,* in contrast to the emphasis on *delocalized* waves in *momentum space,* such as Bloch-waves in an average periodic crystal, used for the rough description of substitutional alloys. At this point the question occurred to me whether, in general, an alloy is *completely* or only partially characterized by its electronic density distribution n(r): In the back of my mind I knew that this was the case in the Thomas-Fermi approximation of interacting electron systems; also, from the “rigid band model” of substitutional alloys of neighboring elements, I knew that there was a 1-to-1 correspondence between a weak perturbing potentialdeltav(r) and the corresponding small changedeltan(r) of the density distribution. Finally it occurred to me that for a single particle there is an explicit elementary relation between the potential v(r) and the density, n(r), of the groundstate. Taken together, these provided strong support for the conjective that the density n(r) completely determines the external potential v(r). This would imply that n(r) which integrates to N, the total number of electrons, also determines the total Hamilton H and hence *all* properties derivable from H and N, e.g. the wavefunction of the 17th excited state,psi17 (r1,…,rN)! Could this be true? And how could it be decided? Could two different potentials, v1(r) and v2(r), with associated different groundstatespsi1 (r1,…,rN) and2 (r1,…,rN) give rise to the *same* density distribution? It turned out that a simple 3-line argument, using my beloved Rayleigh Ritz variational principle, confirmed the conjecture. It seemed such a remarkable result that I did not trust myself.  By this time I had become friends with another inhabitant of Nozière’s office, Pierre Hohenberg, a lively young American, recently arrived in Paris after a one-year fellowship in the Soviet Union. Having completed some work there he seemed to be “between” problems and I asked if he would be interested in joining me. He was. The first task was a literature search to see if this simple result was already known; apparently not. In short order we had recast the Rayleigh-Ritz variational theorem for the groundstate energy in terms of the density n (r) instead of the many electron wave functionpsi, leading to what is now called the Hohenberg Kohn (HK) variational principle. We fleshed out this work with various approximations and published it.  Shortly afterwards I returned to San Diego where my new postdoctoral fellow, Lu J. Sham had already arrived. Together we derived from the HK variational principle what are now known as the Kohn-Sham (KS) equations, which have found extensive use by physicists and chemists, including members of my group.  Since the 1970s I have also been working on the theory of surfaces, mostly electronic structure. The work with Lang in the early 1970s, using DFT, picked up and carried forward where J. Bardeen’s thesis had left off in the 1930s.  In 1979, I moved to the University of California, Santa Barbara to become the initial director of the National Science Foundation’s Institute for Theoretical Physics (1979-84). I have continued to work with postdoctoral fellows and students on DFT and other problems that I had put aside in previous years. Since the middle 1980s, I have also had increasing, fruitful interactions with theoretical chemists. I mention especially Robert Parr, the first major theoretical chemist to believe in the potential promise of DFT for chemistry who, together with his young co-workers, has made major contributions, both conceptual and computational.  Since beginning this autobiographical sketch I have turned 76. I enormously enjoy the continuing progress by my younger DFT colleagues and my own collaboration with some of them. Looking back I feel very fortunate to have had a small part in the great drama of scientific progress, and most thankful to all those, including family, kindly “acting parents”, teachers, colleagues, students, and collaborators of all ages, who made it possible.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1998*, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1999  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.  *Walter Kohn passed away on 19 April 2016.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | **0366** |
| Interview |  |
| Q9 | **Professor Kohn, thank you for seeing us today, it’s very nice to meet you. You got the Prize in Chemistry 1998 and that took a few … that was quite a number of years after you did your remarkable discovery. How have you dealt with this prize, this award and how has it, how has it affected your life?** |
|  | I’ll answer this in two parts. I mean one is simply how has receiving the Nobel Prize affected my life and the other one is, the second part is how has the fact that I’ve suddenly been transformed into a chemist affected my life and it’s, in both cases really very positive. Insofar as the first half of your question is concerned I think most of my colleagues who have received a Nobel Prize have probably had the similar experience. The fact that you receive the Nobel Prize suddenly means that broadly speaking people take you more seriously suddenly and that means that if one has some principles, some convictions and is not totally satisfied with the world as it is, one now has …  … here is the leading country in the world, but look at its health system …  Let me first say an opportunity, but really in certain sense a responsibility to do something with it. Not to let it be. I mean whether one’s concern is for example as an American I’m very concerned about the fact that here is the leading country in the world, but look at its health system. So health system is on the one hand in many areas the best in the world, on the other hand, if you look at the fraction of people, including millions of children who are getting very poor, sometimes no health care, it’s shocking. So as an example of something that I felt very upset about for many years. |
| Q15 | **Do you speak out about that?** |
|  | Yes, you know, I speak out on it and, and the same about several other areas that concern me, so. |
| Q15 | **And during your speech in -98 you said that for example working for peace is something of importance.** |
|  | Yes, and I continue to do this quite actively. I’ll just mention something very concrete, I’m a research Professor at the University of California, the Santa Barbara Campus, and the University of California is, has been managing the two, *the* two nuclear weapons laboratories in America, I think I’m correct in saying that every advance in the effectiveness of nuclear weapons – and one has to stop for a moment and ask oneself whether the word “advance” is the appropriate word – was done under the management of the University of California. It’s my University. It bothers me a lot and so the …  This management of the weapons laboratories by the University of California with some little glitches and some modifications is negotiated typically for a period of five years and then in the past has always been renewed. But it has to go through a renewal process. So the renewal process is just in progress now and I’ve spent a lot of time on this. I have spoken not only on my Campus but I’ve had a video made and the video has been used in Los Angeles at UCLA and so I had this what I call the opportunity and I personally felt the responsibility to speak up on this question. |
| Q11 | **And the other part is that you got it for chemistry, but you are not a chemist.** |
|  | No I, yes that’s quite correct and well, that’s the situation, I’m actually very comfortable with it. Chemists do use my work extensively and they appreciate it and they are, because they appreciate it they are without exception very friendly towards me. They would be in a position extremely easily to embarrass me terribly, but no-one has ever tried that. I can tell you a slightly amusing story.  The day after I received the Prize of course it was quite a sensation in my little town in Santa Barbara, so I walked across the Campus and two students, two women students, are walking in the opposite direction, and they must have seen my picture somewhere, perhaps in the student newspaper, and so one of them turns around and comes back and says to me “Are you the guy who won the Nobel Prize?” and I said “Yes, I am” and so they both gave me a very friendly hug and then they kept going again, but then one came back again and said “Do you mind, we are just going to a chemistry exam, can we ask you a question?”  … at a certain theoretic level chemistry and physics are very close to each other …  So yes I was in a very tight spot! So I said “well try me” and I started praying very hard because I knew that the more elementary the questions would be the less likely that I could answer them! And so then this one young woman began to ask a question and then I recognised an interesting fact. You see my … the fact that I got the Nobel Prize in Chemistry reflects the fact that at a certain theoretic level chemistry and physics are very close to each other, but also at the lowest level they’re very close to each other and these girls didn’t really know the difference between physics and chemistry. So the question they had for me was in fact a physics question. So I gave them a brilliant answer! They were very impressed. |
| Q11 | **Is it that these areas that you getting the Prize in, so to speak, maybe they do overlap more and more in the modern scientific world?** |
|  | Yes, I would say yes, but I would not go along with some kind of world picture that all sciences will become unified. I think different sciences, and of course each science evolves over time, but by and large they do have their own characteristics which differ, and I personally think that’s for the good because then science as a whole is enriched by this difference. Vive la difference! |
| Q1 | **If you advise students today at the Campus, or people who are … who would listen to this interview who want to go into scientific research, what would you give them, what kind of advice would you give them?** |
|  | First of all I would like to get to know the person a little bit before I offer any advice, because promising young scientists are already rather exceptional people and they are all different from each other and … |
| Q7 | **Then a specific quality that you have to have, do you have to endure long nights of hard work and days?** |
|  | Well, many yes, and some no. So I wanted to make a caveat before I say anything, because what I say is perhaps broadly sensible, but there will be some cases where it will be absolutely the wrong advice, and well let me think about this, I’d say perhaps two or three words of advice. Number one: Be prepared for some great excitement and great personal rewards but mostly disappointments, and you must last through disappointments and you have to be prepared for them. So that’s one piece of advice.  … I myself feel very much like a global citizen and it’s one of the best parts of my life …  Another piece that I would offer is, but again I would come back and say, I know people who would not say that, but in general I think scientists have a great opportunity to combine their intellectual or in some cases mechanical abilities on the one hand with playing a useful role in society, and whether it’s their town, or their country or whether it’s global, and of course scientists are a very global group and of course Nobel himself sort of highlights this globality of science, and I think I myself feel very much like a global citizen and it’s one of the best parts of my life. I mean I’m just coming from two hours conversations with students from all over, you know, the kind of students that come here literally all over the world and it’s very few things that are more richer, that richer experience. |
| Q6 | **Professor, you said you, you know, the scientists feel like a citizen of the world often and certainly one can describe you as a citizen of the world. You were born in Austria, but your childhood was made traumatic as you had to flee during the Nazi takeover of Austria. Could you tell us something about that?** |
|  | I had traumatic experiences in my childhood. The traumatic experiences were related with the so-called Anschluss, the joining of Austria to Germany during the Hitler time in 1938. Before that, looking back at least, I feel I had a generally good childhood. So my problems were … many people of course suffered under the Hitler regime, my family and I suffered like all other people who were in the same situation because of our religion. We were Jewish. Now the … was that traumatic? Yes, it was terribly traumatic, I mean the entire family was totally uprooted, destabilised, maltreated, I was thrown out of my school in a day, as I remember, and so there were things of that sort. Now I want to say first of all that my parents did succeed to help both my sister and myself to emigrate, we both emigrated first to England, unfortunately they were unable to emigrate and they were eventually murdered.  I did not ever even think about returning, it was unbearable for me …  Both my sister and I had a wonderful experience in England, a family that we had not ever encountered personally had some business relationship with my father took us into their home and treated us as family members and were absolutely wonderful. My sister eventually returned to Vienna after she married an Austrian, another Austrian émigré. I did not ever even think about returning, it was unbearable for me, and after, so then I had bizarre experiences, in retrospect right I was threatened with extermination by the Austrians and Germans but on the other hand the English regarded me as a potential spy for the Germans, and in turn me and thousands of others who are close to years, and so I had bizarre experiences of this sort. Of course compared to the horror of people who had to stay behind I was in my personal body, so to speak, I was fortunate, the loss of my parents was of course disaster. |
| Q6 | **So after the years in England you went to Canada, you emigrated to Canada, or was sent to Canada. What happened there?** |
|  | Well, in Canada I, together with I think approximately within five and ten thousand other people in similar situations, was put into an internment camp. You may wonder how did they happen to have this internment camp ready, well they had it ready because they arranged, the Canadians, with the British at the British request to build these internment camps in order to be prepared for the thousands of German prisoners of war they expected. Well the war didn’t go that way, in the initial period the British, the little British expeditionary force in France they were incredibly lucky to get away with their skin and they came back to Britain and I think a single prisoner came back with their skin, so there were these camps waiting and here were we, the suspected spy.  So, yes we were put in these camps and I want to say right away we were treated very humanely. It was sometimes difficult because there were a few real Nazis in there, a small minority, but they were very in the early phases of the war, the war was going splendidly for the Germans and so whenever there was a great victory on the East Coast, right, these people would have their celebrations, and of course the rest of us got very upset about this. So it was unpleasant, nothing really serious, nobody lost his life but few people lost a few teeth! Otherwise, there was a wonderful man there I encountered several times in my life these people that were there, this was a man by the name of Heckscher, a German art historian, and we encountered him as this dedicated person, very capable, very intelligent and he was going to establish a school in camp for young people like me. Himself was about 20 years older, in his late thirties, and he inspired respect by everybody including the officers who treated him pretty much as their equal. He was not Jewish but he had been … his family had been in the business of helping camp prisoner of war camp inmates for the second generation, his father had done this in the First World War and so he took over the family tradition. So he established a very good camp school.  There were some brilliant people in the camp and so I divided my time roughly half to studying and the other half we were given the opportunity, but not required, to do work of different kinds, so the work, I had two kinds that I liked best was making camouflage nets for the British Armed Forces, that’s very calming for the nerves, you stand there and you have sort of three movements that you learn in five minutes and then you repeat them tens of thousands of times and of course you no longer know you are doing this, and you can dream, whatever, talk to your neighbour. And the other activity in Canada, you will be surprised, was lumber work, cutting trees. So of course I mean from time to time we felt a little sorry for ourselves, I mean why are we in this camp and why can’t we go out and so on, but … |
| Q5 | **When did you set your heart on science then?** |
|  | Perhaps the most influential person was somebody I encountered after the Anschluss in Austria. I was expelled from my school, there was some temporary arrangements, complicated, I skipped them for a few months, the Anschluss was in March, next September some of us who had high grades in school were given the opportunity to enter into a newly organised, that’s not quite correct, but let me just say to enter into a re-organised school which was exclusively for Jewish students and with Jewish teachers. Many of them did not survive. The director was a physicist and so he directed the school and he taught physics. Then his Latin teacher was arrested and didn’t come back, and so the director who had taken Latin in school also taught Latin, and he didn’t know much Latin but he came into class with the books that were supposed to be read, some of the classic Latin poets, Ovid, Virgil, and a dictionary. So he taught us from the books and the dictionary … |
| Q5 | **Which way was he influencing you to become a physicist?** |
|  | Well, he taught physics also. He influenced me both very much just admirable personality generally and inexplicably it was evident to me from the beginning that this man really knew science. Really understood science. Then about ten years later I found out that he had been an assistant of Einstein’s and he was an incredible man. I think without meeting him, and I had another very good teacher, a mathematician in the same school, without these two gentlemen I probably would not have become a scientist. My interests before that time, I was very enthusiastic about something but it wasn’t science, it was Latin. |
|  |  |
| **ID** | **0367** |
| Biographical | My early life was spent in Burnham-on-Sea, Somerset, a small seaside resort town (population around 5000) on the west coast of England. I was born on October 31, 1925 and lived there with my parents until shortly after the end of the Second World War in 1946. No member of my family was involved in any scientific or technical activity. Indeed, I was the first to attend a university.  My father, Keith Pople, owned the principal men’s clothing store in Burnham. In addition to selling clothes in the shop, he used to drive around the surrounding countryside with a car full of clothes for people in remote farms and villages. He was resourceful and made a fair income, considering the economic difficulties during the depression of the 1930s. My great-grandfather had come to Burnham around 1850 and set up a number of local businesses. He had a large family and these were split up among his children. As a result, I had relatives in many of the other businesses in the town. My grandfather inherited the clothing shop and this passed to my father when he returned from the army at end of the First World War.  My mother, Mary Jones, came from a farming background. Her father had moved from Shropshire as a young man and had farmed near Bath for most of his life. I suspect that he would have preferred to be a teacher, for he had a large collection of books and encyclopedias. He wanted my mother to be a schoolteacher, but this did not happen. Instead, she became a tutor to children in a rich family and, later, a librarian in the army during the first war. Most of her relatives were farmers in various parts of Somerset and Wiltshire so, as small children, my younger brother and I spent much time staying on farms.  Both of my parents were ambitious for their children; from an early age I was told that I was expected to do more than continue to run a small business in this small town. Education was important and seen as a way of moving forward. However, difficulties arose in the choice of school. There was a good preparatory school in Burnham but, as part of the complex English class system, it was not open to children of retail tradesmen, even if they could afford the fees. The available alternative was unsatisfactory and my parents must have agonized over what to do. Eventually, they decided to send us to Bristol Grammar School (BGS) in the nearest big city thirty miles away. BGS was the prime day school for boys, catering mainly to middle class families resident in the city, although it received a government grant for accepting about thirty boys a year from the state elementary schools. I went there in the spring of 1936 at the age of ten. Some arrangement had to be made for boarding and I used to return home by train each weekend. This I found unappealing and eventually I persuaded my parents to allow me to commute daily – two miles by bicycle, twenty-five miles by train and one mile on foot. I continued to do this during the early part of the war, a challenging experience during the many air attacks on Bristol. Often, we had to wend our way past burning buildings and around unexploded bombs on the way to school in the morning. Many classes had to be held in damp concrete shelters under the playing fields. In spite of all these difficulties, the school staff coped well and I received a superb education.  At the age of twelve, I developed an intense interest in mathematics. On exposure to algebra, I was fascinated by simultaneous equations and rapidly read ahead of the class to the end of the book. I found a discarded textbook on calculus in a wastebasket and read it from cover to cover. Within a year, I was familiar with most of the normal school mathematical curriculum. I even started some research projects, formulating the theory of permutations in response to a challenge about the number of possible batting orders of the eleven players in a cricket team. For a very short time, I thought this to be original work but was mortified to find *n!* described in a textbook. I then attempted to extend *n!* to fractional numbers by various interpolation schemes. Despite a lot of effort, this project was ultimately unsuccessful; I was angry with myself when I learned of Euler’s solution some years later. However, these early experiences were valuable in formulating an attitude of persistence in research.  All this mathematical activity was kept secret. My parents did not comprehend what I was doing and, in class, I often introduced deliberate errors in my exercises to avoid giving an impression of being too clever. My grades outside of mathematics and science were undistinguished so I usually ended up several places down in the monthly class order. This all changed suddenly three years later when the new senior mathematics teacher, R.C. Lyness, decided to challenge the class with an unusually difficult test. I succumbed to temptation and turned in a perfect paper, with multiple solutions to many of the problems. Shortly afterwards, my parents and I were summoned to a special conference with the headmaster at which it was decided that I should be prepared for a scholarship in mathematics at Cambridge University. During the remaining two years at BGS, I received intense personal coaching from Lyness and the senior physics master, T.A. Morris. Both were outstanding teachers. The school, like many others in Britain, attached great importance to the placement of students at Oxford or Cambridge. Most such awards were in the classics and I think that the mathematics and science staff were very anxious to compete. Ironically, during the last two years at BGS, I abandoned chemistry to concentrate on mathematics and physics. In 1942, I travelled to Cambridge to take the scholarship examination at Trinity College, received an award and entered the university in October 1943.  In the middle of the war, most young men of my age were inducted into the armed forces at the age of seventeen. However, a small group of students in mathematics, science and medicine was permitted to attend university before taking part in wartime research projects such as radar, nuclear explosives, code-breaking and the like. This was a highly successful project and many of my predecessors in earlier years made important contributions to the war effort. The plan was to complete all degree courses in only two years, followed by secondment to a government research establishment. In my case, I completed Part II of the mathematical tripos in May 1945, just as the European war was ending. In fact, it was hard to concentrate on the examinations because of the noisy celebrations going on in the streets outside. The government no longer had need for my services and the university was under great pressure to make room for the deluge of exservicemen as they were demobilized from the armed forces. So, I had to leave Cambridge and take up industrial employment for a period. This was with the Bristol Aeroplane Company, close to where I had attended school. There was little to do there and I had a period of enforced idleness as changing employment was illegal at the time (part of the obsession for a planned economy in postwar Britain).  In 1945, I had little idea of what my future career might be. My interest in pure mathematics began to wane; after toying with several ideas, I finally resolved to use my mathematical skills in some branch of science. The choice of a particular field was postponed, so I devoted much of my time to pestering government offices for permission to return to Cambridge and resume my studies. In the late summer of 1947, I finally received a letter informing me that an unexpectedly large number of students had failed their examinations and a few places were available. So, in October 1947, I returned to Cambridge to begin a career in mathematical science.  Cambridge in 1947 had greatly changed since 1943. The university was crowded with students in their late twenties who had spent many years away at the war. In addition, the lectures were given by the younger generation who had also been away on research projects. There was a general air of excitement as these people turned their attention to new scientific challenges. I remained as a mathematics student but spent the academic year 1947-8 taking courses in as many branches of theoretical science as I could manage. These included quantum mechanics (taught in part by [Dirac](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/index.html)), fluid dynamics, cosmology and statistical mechanics. Most of the class opted for research in fundamental areas of physics such as quantum electrodynamics which was an active field at the time. I felt that challenging the likes of [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/index.html) and Dirac was overambitious and decided to seek a less crowded (and possibly easier) branch of science. I developed an interest in the theory of liquids, particularly as the statistical mechanics of this phase had received relatively little attention, compared with solids and gases. I approached Fred Hoyle, who was giving the statistical mechanics lectures (following the death of R.H. Fowler). However, his current interests were in the fields of astrophysics and cosmology, which I found rather remote from everyday experience. I next approached Sir John Lennard-Jones (LJ), who had published important papers on a theory of liquids in 1937. He held the chair of theoretical chemistry at Cambridge and was lecturing on molecular orbital theory at the time. When I approached him, he told me that his interests were currently in electronic structure but he would very possibly return to liquid theory at some time. On this basis, we agreed that I would become a research student with him for the following year. Thus, after the examinations in June 1948, I began my career in theoretical chemistry at the beginning of July. I had almost no chemical background, having last taken a chemistry course at BGS at the age of fifteen. Other important events took place in my life at this time. In late 1947, I was attempting to learn to play the piano and rented an instrument for the attic in which I lived in the most remote part of Trinity College. The neighbouring room was occupied by the philosopher Ludwig Wittgenstein, who had retired to live in primitive and undisturbed conditions in the same attic area. There is some evidence that my musical efforts distracted him so much that he left Cambridge shortly thereafter. In the following year, I sought out a professional teacher. The young lady I contacted, Joy Bowers, subsequently became my wife. We were married in Great St. Mary’s Church, Cambridge in 1952, after a long courtship. Like many other Laureates, I have benefit immeasurably from the love and support of my wife and children. Life with a scientist who is often changing jobs and is frequently away at meetings and on lecture tours is not easy. Without a secure home base, I could not have made much progress. The next ten years (1948-1958) were spent in Cambridge. I was a research student until 1951, then a research fellow at Trinity College and finally a lecturer on the Mathematics Faculty from 1954 to 1958. Cambridge was an extraordinarily active place during that decade. I was a close observer of the remarkable developments in molecular biology, leading up to the double helix papers of [Watson](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1962/index.html) and [Crick](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1962/index.html). At the same time, the X-ray group of [Perutz](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1962/index.html) and [Kendrew](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1962/index.html) (introduced to the Cavendish Laboratory by [Lawrence Bragg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1915/index.html)) were achieving the first definitive structures of proteins. Elsewhere, Hoyle, Bondi and Gold were arguing their case for a cosmology of continuous creation, ultimately disproved but vigorously presented. Looking through the list of earlier Nobel laureates, I note a large number with whom I became acquainted and with whom I interacted during those years as they passed through Cambridge.  In the theoretical chemistry department, LJ was professor and Frank Boys started as lecturer in September 1948. I began research with some studies of the water molecule, examining the nature of the lone pairs of electrons. This was an initial step towards a theory of hydrogen bonding between water molecules and a preliminary, rather empirical study of the structure of liquid water. This fulfilled my initial objective of dealing with properties of liquids and gained me a Ph.D. and a research fellowship at Trinity College. This highly competitive stage accomplished, I was able to relax a bit and formulate a more general philosophy for future research in chemistry. The general plan of developing mathematical models for simulating a whole chemistry was formulated, at least in principle, some time late in 1952. It is the progress towards those early objectives that is the subject of my Nobel lecture.  At that early date, of course, computational resources were limited to hand calculators and very limited access to motorized electric machines. So my early notes show attempts to simplify theories enough to turn them into practical possibilities. The work paralleling studies of Pariser and Parr led to what became known as PPP theory. This was not a complete model but rather one applicable to systems with only one significant electron per atom. It did fit the general form of conjugated hydrocarbons and achieved some notoriety. In 1953, Bob Parr came to Cambridge to spend a year with Frank Boys. We shared an office and had many valuable discussions; he was to have a major influence on my future. I talked about PPP theory when I began to speak at international meetings in 1955.  In addition to the PPP work, I started theoretical work on other topics in physical chemistry. I began supervision of research students in 1952, beginning with David Buckingham, who completed a masterly thesis on properties of compressed gases. He was the first of a long list of remarkably able and dedicated students who have worked with me over the years. In 1954, LJ was succeeded as professor of theoretical chemistry by Christopher Longuet-Higgins, who was joined by Leslie Orgel shortly afterwards. I continued to spend a lot of time in the chemistry department, although by then I had undertaken new teaching responsibilities as a lecturer in mathematics. The department was crowded and active in those years. Among the many visitors were [Linus Pauling](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1954/index.html), [Robert Mulliken](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1966/index.html), Jack Kirkwood, Clemens Roothaan and Bill Schneider. Frank Boys was also managing a lively group of students.  At the end of 1955, I developed an interest in nuclear magnetic resonance, which was then emerging as a powerful technique for studying molecular structure. At the urging of Bill Schneider, I agreed to spend two summers (1956 and 1957) at the National Research Council in Ottawa, Canada, working on the theoretical background of NMR. This was extremely stimulating for, at that time, we were measuring the spectra and interpreting the nuclear spin behaviour of many standard chemicals for the first time. My time there with Bill and Harold Bernstein led to a book, *High Resolution Nuclear Magnetic Resonance,* which was well received. This area was the main emphasis of my research during the final years in Cambridge.  By 1958, I had become dissatisfied with my mathematics teaching position at Cambridge. I had clearly changed from being a mathematician to a practicing scientist. Indeed, I was increasingly embarassed that I could no longer follow some of the more modern branches of pure mathematics, in which my undergraduate students were being examined. I therefore resolved to seek a new job with greater scientific content. After some hesitation, I accepted a position as head of the new Basics Physics Division at the National Physical Laboratory near London. This involved direction of experimental work and a considerable amount of administration. When I took the job, I hoped that the administrative burden would not be large enough to interfere with my research programme. Although I was given plenty of help, this turned out not to be so and I had a rather fallow period while I was there.  In the spring of 1961, I organized an international conference in Oxford, along with Charles Coulson and Christopher Longuet-Higgins. Bob Parr was an invited speaker and, during a break, he urged me to come and spend a sabbatical year at Carnegie Institute of Technology in Pittsburgh. This was an attractive suggestion and I arranged to come for the academic year 1961-2 with my family. By this time, Joy and I had three children and were expecting a fourth. We arrived in September, accompanied by a charming young Swedish *au pair,* Elisabeth Fahlvik. One of the most delightful side-effects of winning the Nobel Prize is the opportunity to meet her again after a gap of over thirty-six years.  By the time we arrived in Pittsburgh, Bob Parr had decided to leave for Johns Hopkins University and he did, in fact, leave in January. Nevertheless, we had a delightful year, travelling as a family over much of the eastern part of the U.S.A. During this period, I made up my mind to abandon my administrative job and seek an opportunity to devote as much time as possible to chemical research. I was approaching the age of forty, with a substantial publication record, but had not yet held any position in a chemistry department. When we returned to England in June, 1962, it was not clear where we might go for there were opportunities both in the U.K. and the U.S.A. Eventually, after much debate, we decided to return to Pittsburgh in 1964. Leaving England was a painful decision and we still have some regrets about it. However, at that time, the research environment for theoretical chemistry was clearly better in the U.S.  On my return to Pittsburgh, I resolved to go back to the fundamental problems of electronic structure that I had contemplated abstractly many years earlier. Prospects of really implementing model chemistries had improved because of the emerging development of high-speed computers. I was late in recognizing the role that computers, would play in the field – I should not have been, for Frank Boys was continually urging the use of early machines back in Cambridge days. However, by 1964, it was clear that the development of an efficient computer code was one of the major tasks facing a practical theoretician and I learned the trade with enthusiasm. Mellon Institute, where I had an adjunct appointment, acquired a Control Data machine in 1966 and my group was able to make rapid progress in the dingy deep basement of that classic building. In 1967, Carnegie Tech and Mellon Institute merged to become Carnegie-Mellon University (CMU) and I remained on the faculty there until 1993. Almost all of the work honored by the Nobel Foundation was done at CMU. That institution deserves much of the credit for their continuing support and encouragement over many years.  The scientific details of the Pittsburgh work are related, in part, in the accompanying lecture. Over the years, we were able to keep abreast with the rapid developments in computer technology. Around 1971, the work was moved to a Univac 1108 machine and then, in 1978, we were fortunate enough to acquire the first VAX/780 minicomputer from the Digital Equipment Corporation for use entirely within the chemistry department. This became a valuable workhorse as we began to distribute programs to the general chemical community. In more recent years, of course, the techniques have become available on small work stations and personal computers. The astonishing progress made in computer technology has had profound consequences in so many branches of theoretical science.  Our children were mostly brought up and educated in the Churchill suburb east of Pittsburgh. Each summer, we took them back to England for an extended period. By 1979, all had gone away and Joy and I decided to move again to Illinois, where our daughter had settled. In 1981, we set up house in Rogers Park, Chicago and then moved to Wilmette in 1988. Our family is now scattered in Chicago, Houston, Pittsburgh and Cork, Ireland. We have been blessed with ten grandchildren (an eleventh expected), who greatly enrich our lives in many ways.  From 1981 to 1993, I continued to run my research group in Pittsburgh, commuting frequently and communicating with my students by telephone and modem. Northwestern University kindly offered me an adjunct appointment and I became a full member of their faculty in 1993. I am very grateful to them for the opportunity to continue my research programme and interact with other members of the chemistry department.  I have had many opportunities to visit universities all over the world in the past fifty years. Among the most rewarding have been frequent trips to Australia and New Zealand, where Joy and I have wintered no fewer than nine times since 1982. The campus of the Australian National University, where Leo Radom became Professor after spending time with me as a postdoctoral fellow from 1968 to 1972, has become a second academic home – a great place for relaxed contemplation.  Israel and Germany are other countries with which I have become closely associated, having visited and collaborated many times. In the 1980s, I held a von Humboldt Award, which allowed me to spend some time in Erlangen, where I collaborated with Paul Schleyer on a large number of applications of the theory. In Israel, I have visited and lectured at all universities, including a period as Visiting Professor at the Technion, Haifa. In 1992, I was fortunate enough to receive the Wolf Prize in Chemistry at a ceremony in the Knesset.  I must emphasize that my contribution to quantum chemistry has depended hugely on work by others. The international community in our field is a close one, meeting frequently and exchanging ideas freely. I am delighted to have had students, friends and colleagues in so many nations and to have learned so much of what I know from them. This Nobel Award honours them all.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1998*, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1999  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.  *John Pople died on March 15, 2004.* |
| Autobiographical |  |
| Podcast |  |
| **Telephone**  **interview** | **0367** |
| **Interview** |  |
| Q2 | **How did you come to end up in science? Was there a family background in science? What started you?** |
|  | My family has no scientific background. As a child I lived in a small town in the West of England. My father was a shop keeper. He owned and ran the men’s clothing store in this town. My mother’s family were mostly farmers from different parts of England. So I had no professional scientific background in my family. As a child I did become extremely interested in mathematics at about the age of 11 or 12. I was by that time already keeping my father’s accounts, large sums to be added up in pounds, shilling and pence, which is quite complicated mathematics. And I became very facile in doing this sort of thing.  I used to make sometimes deliberate errors in mathematics classes so that I would not appear to be too smart …  But then at the age of 12, when I started being exposed to algebra in school, I became extremely fascinated and spent much of my time teaching myself at the age of 12 more advanced parts of mathematics. In fact, I taught myself calculus using an old book which I found in a waste basket and I took it out of the waste basket and read it from cover to cover. So by the age of 12 I was already actively thinking about mathematical problems. I was thinking about permutations and extending tables of factorials to non-integer values, various projects which I formulated and tried to solve, not always successfully, but I was doing research projects in mathematics when I was quite young. So that was really my introduction to academic subjects. Actually at that age I was quite secretive about it. I did not tell my teachers that I had taught myself calculus. And for a while I was reluctant for this to be known, and I used to make sometimes deliberate errors in mathematics classes so that I would not appear to be too smart. One feels the pressure from one’s peers at that stage. But then after two or three years I sort of went public with this. Then the school authorities, this is at Bristol Grammar School which did have a very fine mathematics teacher, they told me that I should study for the scholarship exam for Cambridge. So I spent the last two or three years really preparing for this competitive intake at Trinity College Cambridge, which is where mathematicians tend to go in Britain, because they have a great history in development of mathematics.  So I went to Cambridge to take the scholarship examinations in the middle of the War, in 1942. At that time it was not too easy to go to University because almost all young men, I was 17 at the time, were conscripted into the army. But there was a programme for a small number of mathematics students to go to university, to Cambridge, to take a mathematics degree in a very short compressed period and then go into government service in some form of operations, research. This turned out to be a very successful programme. Some of the people who were in the programme before me that were older than me had made major contributions to the British war effort. Freeman Dyson was one such person. He was in exactly the same programme two years older than me, and he was in Cambridge between 1941 and 1943, and I think he went on to become an adviser to the air force in the latter part of the war. In my case I went to Cambridge in 1943 and by the time I’d completed my degree in 1945 the war was just ending so I never really did anything useful in the war time. |
| Q2 | **Then you went on to PhD studies?** |
|  | Yes, in 1945 as the war was ending I was, the government didn’t quite know what to do with me because the war effort was winding down. So I was sent off into industry and I worked for the Bristol Aeroplane Company for two years, not doing anything very significant, until I could get back to Cambridge to start a research career. And so I returned in 1947. I spent one year doing sort of post graduate courses in all branches of applied mathematics and then it was after that that I decided to become a theoretical scientist. And at that point I picked on chemistry as a field that I would enter. But until then I had no background in chemistry. I’d given up chemistry at a quite early stage in high school. I decided this was a good opportunity. It turned out well. |
| Q2 | **Also you started working on quantum chemical methods and development on computational techniques?** |
|  | Yes. In the early days a felt that a theoretical chemist should really address all branches of chemistry and I did work as I told you on liquids, statistical mechanics or liquids. And I also started doing some work on quantum mechanics, there’s early papers on that by 1952. And then I did the nuclear magnetic resonance. So I used to switch from one topic to another every two or three years. But that became more and more difficult I think because as fields grow you need a great deal of technical expertise.  So in 1958 I took an administrative job which was for several years which I didn’t like very much. And when I gave that up I decided that I would return to full time research, with the main emphasis on quantum mechanics, of electronic structure of molecules, which is the most fundamental part of theoretical chemistry, basic understanding of how molecules, why molecules behave as they do. So that is the time at which I moved with my family to the United States. And the projects for which the Nobel Prize was awarded were started around that time. |
| Q14 | **How would you see also the future development of quantum chemistry? Will faster computers influence quantum chemistry?** |
|  | Progress has taken place in both respects. One has been able to achieve more efficient or new methods, or more efficient ways of handling the same method. Certain theoretical computations may start off being rather expensive and hard to use except for very small molecules. But then one can do a lot of research on new mathematical schemes which will get the same answer with far fewer operations. That has been a major focus of work and that has enabled the techniques to be applied not only to very small molecules but to many molecules, larger molecules, and to begin to make really significant predictions about chemical behaviour. And that will certainly continue. Computers are clearly going to get faster and faster and we are continually working on ways of making our methods used to fewer and fewer multiplications to get bigger and bigger systems. So this is very much a development along the same path. You can never tell whether a totally new method will come in and demolish all existing procedures, but I don’t see it at the moment. |
| Q14 | **Would you see for example a kind of fusion of molecular dynamics and quantum chemistry?** |
|  | Yes, that’s right. The good quantum chemistry, high level theories can still only be applied to systems of up to maybe 100 electrons. And it’s important to be able to examine systems which are in solution for example, where you have to take account of all the rest of the material. So there’s important work going on on kind of multiple techniques where you use a quantum mechanical study of, a good quantum mechanical study of a central part of a molecule or system, that you need to study carefully. And it’s surrounded by a solvent or the rest of a large protein or something which are simulated in a cruder fashion so that you can still get the general effects of the medium on the chemistry that’s going on. |
| Q18 | **At this meeting many things have also happened. Two days ago there was a joint announcement by groups from the States and Britain and other countries that have completed the sequence for 99 or 97% of the human genome. It’s regarded by many as a milestone in the human history, science history. And probably rightly so. I know that you take an interest also in biological systems and would you like to comment on these recent events that we have witnessed?** |
|  | Yes, well that is certainly the complete knowledge of the human genome would be a big step forward. I think from a scientific point of view one may well desire to have the sequences of many more species before we can begin to really get a further understanding of evolutionary processes and how they have taken place. Biology is really a giant study in the history of particular chemical reactions which has gone on for three to four billion years over a complicated fashion. And the fundamentally interesting point is how did this come about and how did it develop in its various branches. And we’ve heard something of this of course in the talks this morning. So I think this is a big development. It’s clearly the way that the future of the science will go. I’m sure there’ll be the sort of political pressure to work primarily on the human condition rather than other species, which might be more informative, and on which one might be able to do it as was pointed out this morning, on which it might be possible to do more actual experiments. |
| Q18 | **You have had a lot of students over the years, what advice have you given them during their carreer?** |
|  | For my students? Well I’ve had a very fine set of students. I’ve always tried to advise them to handle research in an innovative but critical manner. It’s all about, research is all about getting new ideas. It’s always desirable to question existing ideas. It’s always desirable to formulate your ideas in as simple a fashion as possible, to test them as carefully and as fully as possible for the whole series of steps which I think constitute good research. And many of my students, I think, are very proud of their subsequent careers, have done very well. I’ve got to the age where I actually find myself going to dinners for retirement of people who were students of mine. So after one’s own retirement one starts attending other people’s retirements or 60th birthdays and all those things. |
|  |  |
| **Chemistry\_1999** |  |
| ID | **0368** |
| Biographical | The first 21 years of my life were spent in Provo, Utah, then a city of about 15,000 people, beautifully situated at the foot of the Wasatch Mountains. Hardy Mormon pioneers had settled the area only 70 years before my birth in 1918. Provo was a well-designed city with stable neighbourhoods, a pride in its past and a spirit of unbounded opportunity. The geographical isolation and lack of television made world happenings and problems seem remote.  My father, Dell Delos Boyer, born in 1879 in Springville, Utah, came from the Pennsylvania Boyers, who in turn came from an earlier Bayer ancestry in what is now Holland and Germany. A small portion of my Boyer DNA has been traced to John Alden, famous as a Mayflower pilgrim who wooed for another and won for himself. Dad’s education, at what was then the Brigham Young Academy, was delayed by the ill health he had endured in much of his youth. Through his ambition, and the sacrifices of his family, he acquired training in Los Angeles to become an osteopathic physician. He served humanity well. More by example than by word, my father taught me logical reasoning, compassion, love of others, honesty, and discipline applied with understanding. He also taught me such skills such as pitching horseshoes and growing vegetables. Dad loved to travel. Family trips to Yellowstone and to what are now national parks in Southern Utah, driving the primitive roads and cars of that day, were real adventures. Father became a widower when the youngest of my five siblings was only eight. Fifteen years later he married another fine woman. They shared many happy times, and she cared for him during a long illness as he died from prostate cancer at the age of 82. Prostate cancer also took the life of my only brother when he was 76. If our society continues to support basic research on how living organisms function, it is likely that my great grandchildren will be spared the agony of losing family members to most types of cancer.  Recently I scanned notes on a diary that my mother, Grace Guymon, wrote in her late teens, when living near Mancos, Colorado. The Guymons were among the Huguenots who fled religious persecution in France. My French heritage has been mixed with English and other nationalities as the Guymons descended. Mother’s diary revealed to me more about her vitality and charm than I remembered from her later years, which were clouded by Addison’s disease. She died in 1933, at the age of 45, just weeks after my fifteenth birthday. Discoveries about the adrenal hormones, that could have saved her life, came too late. Her death contributed to my later interest in studying biochemistry, an interest that has not been fulfilled in the sense that my accomplishments remain more at the basic than the applied level. Mother made a glorious home environment for my early years. During her long illness and after her death, all of the children helped with family chores. One of my less pleasant memories is of getting up in the middle of the night to use our allotted irrigation time to water the garden.  The large, gracious home provided by Mother and Dad at 346 North University Avenue has been replaced by a pizza parlor, although an inspection a few months ago revealed that the irrigation ditch for our garden area (now a parking lot) can still be found. Mother had a talent for home decorating. I often read from a set of the *Book of Knowledge or Harvard Classics* while lying in front of the fireplace, with a mantel designed and decorated by her. Staring into the glowing coals as a fire dims provided a wonderful milieu for a youthful imagination. I also remember such things as picnics in Provo Canyon, and the anticipation that I might get to lick the dasher after cranking the ice-cream freezer. My older brother, Roy, and I had a play-fight relationship. I still carry a scar on my nose from when I plunged (he pushed me!) through the mirror of the dining room closet. I am told that I had a bad temper, and remember being banished to the back hall until civility returned. Perhaps this temper was later sublimated into drive and tenacity, traits that may have come in part from my mother.  The great depression of the 1930s left lasting impressions on all our family. Father’s patients became non-paying or often exchanged farm produce or some labor for medical care. Mother saved pennies to pay the taxes. The burden of paper routes and odd jobs to provide my spending money made it painful when my new Iver Johnson bicycle was stolen. We were encouraged to be creative. I recall mother’s tolerance when she allowed me, at an early age, to take off the hinges and doors of cupboards if I would put them back on. My first exposure to chemistry came when I was given a chemistry set for Christmas. It competed for space in our basement with a model electric trains and an “Erector” set. After school the neighborhood yards were filled with shouts of play; games of “kick-the-can,” “run-sheepy-run,” “steal-the-sticks,” as well as marbles, baseball and other activities. In our back yard we built tree houses, dug underground tunnels and secret passages, and made a small club house. The mountains above our house offered other outlets for adventuresome teenage boys. Days were spent in an abandoned cabin or sleeping under the sky in the shadow of Provo peak. We even took cultures of sour dough bread to the mountains and baked delicious biscuits in an a rusty stove. Mountain hikes instilled in me a life-long urge to get to the top of any inviting summit or peak.  Provo public schools were excellent. At Parker Elementary School, a few blocks from my home, I fell in love with my 3rd grade teacher, Miss McKay. Students who learned more easily were allowed to skip a grade, and I entered the new Farrer Junior High school at a younger age than my classmates. This handicapped me in two types of sporting events, athletics and courting girls. Girls did not want to dance with little Paul Boyer; boys were quite unimpressed with my physique. As I grew my status among fellows improved. Once I got into a scuffle in gym class, the instructor had the “combatants” put on boxing gloves, and I gave more than I received. It wasn’t until late high school and early college that I gained enough size and skill to make me welcome on intramural basketball teams.  I was one of about 500 students of Provo High School, where the atmosphere was friendly, and scholarship and activities were encouraged by both students and faculty. I participated on debating teams and in student government, and served as senior class president. I still have a particularly high regard for my chemistry teacher, Rees Bench. I was pleased when he wrote in my Yearbook for graduation, “You have proven yourself as a most outstanding student.” I graduated while still 16, and thought myself quite mature. I wish I had saved a copy of my valedictorian address. I suspect it may have sparkled with naivete.  It was always assumed that I would go to college. The Brigham Young University (BYU) campus was just a few blocks from my home and tuition was minimal. It was a small college of about 3,500 students, less than a tenth of its present size. As in high school, I enjoyed social and student government activities. Friendships abounded. New vistas were opened in a variety of fields of learning. Chemistry and mathematics seemed logical studies to emphasize, although I had little concept as to where they might lead. A painstaking course in qualitative and quantitative analysis by John Wing gave me an appreciation of the need for, and beauty of, accurate measurement. However, the lingering odor of hydrogen sulfide, used for metal identification and separation, called unwanted attention to me in later classes. “Prof” Joe Nichol’s enthusiasm for general chemistry was superbly conveyed to his students. Professor Charles Maw excelled in transferring a knowledge of organic chemistry to his students. Biochemistry was not included in the curriculum.  Summers I worked as a waiter and managerial assistant at Pinecrest Inn, in a canyon near Salt Lake City. One summer a college friend and I lived there in a sheep camp trailer while managing a string of saddle horses for the guests to use. A different type of education came when as a member of a medical corps in the National Guard I spent several weeks in a military camp in California.  As my senior year progressed several career paths were considered; employment as a chemist in the mining industry, a training program in hotel management, the study of osteopathic or conventional medicine, or some type of graduate training. Little information was available about the latter possibility; but a few chemistry majors from BYU had gone on to graduate school. I have a tendency to be lucky and make the right choices based on limited information. A notice was posted of a Wisconsin Alumni Research Foundation (WARF) Scholarship for graduate studies. My application was approved, and the stage was set for a later phase of my career.  Before leaving Provo, a most important and fortunate event occurred. A beautiful and talented brunette coed, with one year of college to finish, indicated a willingness to marry me. She came from a large and loving family, impoverished financially by her father’s death when she was 2 years old. She had worked and charmed her way nearly through college. My savings were limited and hers were negative. But it was clear that my choice was to have her join with me in the Wisconsin adventure or take my chances when I returned a year later. It was an easy decision. Paul, who had just turned 21, and Lyda Whicker, 20, were married in my father’s home on August 31, 1939. Five days later we left by train to Wisconsin for my graduate study.  A few months after our arrival our new marriage almost ended. I was admitted to the student infirmary with diagnosed appendicitis. Through medical mismanagement my appendix ruptured and I became deathly ill. Sulfanilamides, discovered a few years earlier by [Domagk](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1939/index.html), saved my life. Last summer I read an outstanding book, *The Forgotten Plague: How the Battle Against Tuberculosis Was Won and Lost,* by Frank Ryan. The book gives a stirring account, the first I have read, of Domagk’s research and how he was not allowed to leave Hitler’s Germany to receive the 1939 Nobel Prize.  Fortunately, the Biochemistry Department at the University of Wisconsin in Madison was outstanding and far ahead of most others in the country. A new wing on the biochemistry building had recently been opened. The excitement of vitamins, nutrition and metabolism permeated the environment. Steenbock had recently patented the irradiation of milk for enrichment with vitamin D. Elvehjem’s group had discovered that nicotinic acid would cure pellagra. Petersen’s group was identifying and separating bacterial growth factors. Link’s group was isolating and identifying a vitamin K antagonist from sweet clover. Patents for the use of dicoumarol as a rat poison and as an anticoagulant sweetened the coffers of the WARF, the Foundation that supported my scholarship. Among younger faculty an interest in enzymology and metabolism was blossoming.  Married graduate students were rare, and the continuing economic depression made jobs hard to find. But my remarkable wife soon found a good job, and I settled into graduate studies. During our Wisconsin years she gained a perspective of art while employed in Madison’s leading art retail outlet. It was years later before Lyda finished a college degree, became a professional editor at UCLA, and worked with me on the eighteen-volume series of *The Enzymes*. Our contacts in graduate school and through Lyda’s employment gave us life-long friends; one was Henry Lardy, from South Dakota farm country. He and I were assigned to work under Professor Paul Phillips. Henry was highly talented, and it was my good fortune to work along side him. Phillips’ main interests were in reproductive and nutritional problems of farm animals. Henry developed an egg yolk medium for sperm storage that revolutionized animal breeding.  We were encouraged by Phillips to explore metabolic and enzyme interests. I did not realize that it was unusual to be able step across the hall and attend a symposium on respiratory enzymes in which such biochemical giants as [Otto Meyerhof](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1922/index.html), [Fritz Lipmann](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1953/index.html), and [Carl Cori](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1947/index.html) spoke. Evening research discussion groups with keen young faculty such as Marvin Johnson and Van Potter, centered on enzymes and metabolism, broadened and sharpened our perspectives. One evening I presented my and Henry’s evidence for the first known K+ activation of an enzyme, pyruvate kinase. Henry kept score on the interruptions for questions or discussions-some 35 as I recall. This superb training environment set the base for my career.  My Ph.D. degree was granted in the spring of 1943, the nation was at war, and I headed for a war project at Stanford University. A few weeks after my arrival in California, on my birthday, July 31, our daughter Gail was born. I became somewhat more involved in home duties and more deeply in love with Lyda.  The wartime Committee on Medical Research sponsored a project at Stanford University on blood plasma proteins, under the direction of J. Murray Luck, founder of the nonprofit Annual Review of Biochemistry and other Reviews. Concentrated serum albumin fractionated from blood plasma was effective in battlefield treatment of shock. When heated to kill microorganisms and viruses, the solutions of albumin developed cloudiness from protein denaturation. The principal goal of our research project was to find some way to stabilize the solutions so that they would not show this behavior. Our small group found that acetate gave some stabilization and butyrate was better. This led to the discovery that long chain fatty acids would remarkably stabilize serum albumin to heat denaturation, and would even reverse the denaturation by heat or concentrated urea solutions. Other compounds with hydrophobic portions and a negative charge, such as acetyl tryptophan, were also effective. Our stabilization method was quickly adopted and is still in use. From the Stanford studies I gained experience with proteins and a growing respect for the beauty of their structures.  In marked contrast to the University of Wisconsin, Biochemistry was hardly visible at Stanford in 1945, consisting of only two professors in the chemistry department. The war project at Stanford was essentially completed, and I accepted an offer of an Assistant Professorship at the University of Minnesota, which had a good biochemistry department. But my local War Draft Board in Provo, Utah, had other plans and I became a member of the U.S. Navy. The Navy did not know what to do with me, the war with Japan was nearly over, and I became what is likely the only seaman second-class that has had a nearly private laboratory at the Navy Medical Research Institute in Bethesda, Maryland. In less than a year I returned to civilian life. In the spring of 1946 I, my wife, and now two daughters, Gail and Hali, became Minnesotans. But I had unknowingly acquired a latent California virus to be expressed years later.  Minnesota has generally competent and honest public officials, good support of the schools and cultural amenities, and an excellent state university. It was a fine place to rear a family, and soon our third child, Douglas, was born. A golden era for biochemistry was just starting. The NIH and NSF research grants were expanding at a rate equal to, or even ahead, of the growing number of meritorious applications. The G.I. bill provided financial support that brought excellent and mature graduate students to campus. New insights into metabolism, enzyme action, and protein structure and function were being rapidly acquired.  Housing was almost unavailable in the post war years. Initially we coped with an isolated, rat-infested farm house. In 1950, after my academic competence seemed satisfactorily established, we built a home not far from the St. Paul campus where the Department of Biochemistry was located. I served as contractor, plumber, electrician, finish carpenter etc. My warm memories of this home include looking at a sparkling, snow-covered landscape, while seated at the desk in the bedroom corner that served as my study, and struggling with the interpretation of some puzzling isotope exchanges accompanying an enzyme catalysis. The understanding that developed was rewarding and perhaps one of my best intellectual efforts. However, it did not seem that the approach would give answers to major problems.  During my early years at Minnesota I conducted an evening enzyme seminar. One participant in our lively discussions was a promising graduate student from another department, Bo Malmstrom, who became a renowned scientist in his field, and is now a retired professor from the University of Göteborg. In 1952 my family spent a memorable summer at the Woods Hole Marine Biological Laboratories on Cape Cod. A sabbatical period on a Guggenheim Fellowship in Sweden in 1955 was especially rewarding. There I did research at both the Wenner-Gren Institute of the University of Stockholm with Olov Lindberg and Lars Ernster, and at the [Nobel Medical Institute](http://www.nobelprizemedicine.org/), working with [Hugo Theorell](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1955/index.html)‘s group. Professor Theorell received a Nobel Prize that year, exposing us to the splendor and formality of the Nobel festivities.  Along the way, I was gratified to receive the Award in Enzyme Chemistry of the American Chemical Society in 1955. In 1959-60 I served as Chairman of the Biochemistry Section of the American Chemical Society. In 1956 I accepted a Hill Foundation Professorship and moved to the medical school campus of the University of Minnesota in Minneapolis. Much of my group’s research was on enzymes other than the ATP synthase. But solving how oxidative phosphorylation occurred remained one the most challenging problems of biochemistry, and I could not resist its siren call. Mildred Cohn reported that mitochondria doing oxidative phosphorylation catalyzed an exchange of the phosphate and water oxygens, an intriguing capacity. An able physicist and a pioneer in mass spectrometry, Alfred Nier, made gaseous 18O and facilities available to me, and some experiments were run using this heavy isotope of oxygen. However, much of our effort over several years was directed toward attempting to detect a possible phosphorylated intermediate in ATP (adenosine triphosphate) synthesis using 32P as a probe. The combined efforts of some excellent graduate students and postdocs, most of whom went on to rewarding academic careers, culminated in the discovery of a new type of phosphorylated protein, a catalytic intermediate in ATP formation with a phosphoryl group attached to a histidine residue.  By then, time and queries had stimulated the latent California virus. Change was underway. In the summer of 1963, I and a group of graduate students and postdocs who came with me, activated laboratories in the new wing of the chemistry building at the University of California in Los Angeles (UCLA), located on a beautiful campus at the foot of the Santa Monica mountains. We soon found that the enzyme-bound phosphohistidine we had discovered was an intermediate in the substrate level phosphorylation of the citric acid cycle. It was not a key to oxidative phosphorylation. The experience reminds me of a favorite saying: Most of the yield from research efforts comes from the coal that is mined while looking for diamonds.  In 1965 I accepted the Directorship of a newly created Molecular Biology Institute (MBI) at UCLA, in part because of my disappointment that oxidative phosphorylation had resisted our efforts. A building that was promised failed to materialize, but through luck and persistence adequate funds were obtained, partly from private resources, and promising faculty were recruited. The objective was to promote basic research on how living cells function at the molecular level. I believe the best research is accomplished by a faculty member with a small group of graduate students and postdocs, who freely design, competently conduct and intensely evaluate experiments. To spend time with such a group I soon found ways to reduce my administrative chores. Probes of oxidative phosphorylation continued, and, as 1971 approached, we hit pay dirt. We recognized the first main postulate of what was to become the binding change mechanism for ATP synthesis, namely that energy input was not used primarily to form the ATP molecule, but to promote the release of an already formed and tightly bound ATP.  In the following decade, the other two main concepts of the mechanism were revealed, namely that the three catalytic sites participate sequentially and cooperatively, and that our, and other, data could be best explained by what was termed a rotational catalysis. These previously unrecognized concepts in enzymology provided motivation and excitement within my research group. Richard Cross, a postdoctoral fellow trained with Jui Wang at Yale, capably probed tightly bound ATP. Jan Rosing, a gifted experimentalist from Bill Slater’s group in Amsterdam, and Celik Kayalar, an intelligent, innovative graduate student from Turkey, formed a productive pair that unveiled essential facets of cooperative catalysis. David Hackney, a postdoc from Dan Koshland’s stable of budding scientists at Berkeley, was an intellectual leader in our 18O experimentation that led to rotational catalysis. Dan Smith, Michael Gresser, Linda Smith, and Chana Vinkler (from Israel) as postdocs, and Lee Hutton, Gary Rosen and Glenda Choate as graduate students, established the participation of bound intermediates in rapid mixing and quenching experiments, and conducted 18O exchange experiments that clarified and supported our mechanistic postulates.  In ensuing years, other aspects of the complex ATP synthase were explored that solidified our feeling that the binding change mechanism was likely valid and general, and promoted its acceptance in the field. I will resist telling you here about the number, properties, and function of the six nucleotide binding sites, of the probes that agreed with rotational catalysis, of the unraveling of the complex Mg2+ and ADP inhibition, of the generality of the mechanism and other synthase properties revealed by studies with chloroplasts, *E. coli*, and Kagawa’s thermophilic bacterium. It was a pleasure to work on such problems with Teri Melese, a postdoc who excelled in enthusiasm as well as capability, and Zhixiong Xue, an exceptional graduate student that I first met while leading a biochemical delegation to China, with Raj Kandpal a scholarly postdoc from India, with the productive postdocs John Wise (from Alan Senior’s lab) and Rick Feldman (from David Sigman’s lab), with Janet Wood during her sabbatical, and with June-Mei Zhou and Ziyun Du (on leave from Academia Sinica laboratories in China) as well as Dan Wu, Steven Stroop, and Karen Guerrero as graduate students. Special mention should be made of three excellent Russian researchers, Vladimir Kasho, Yakov Milgrom and Marat Murataliev, from the laboratory of Vladimir Skulachev, a respected leader in bioenergetics. With the latter two I am now writing what will likely be my last paper reporting research results. Other welcome postdocs, visitors, and graduate students at UCLA worked with other problems, including the Na+,K+ -ATPase that Skou first isolated, and the related Ca++ transporting ATPase of the sarcoplasmic reticulum. During these active years it was a pleasure to receive peer recognition in the form of the Rose Award of the American Society for Biochemistry and Molecular Biology, the preeminent society in my field (I served as its President many years earlier).  An unexpected benefit of my career in biochemistry has been travel. The information exchanged and gained at scientific conferences and visits has been tremendously important for progress in my laboratory. My travelophilic wife and I thoroughly enjoyed being guests of the Australian and South African biochemical societies while visiting their countries. Meetings or laboratory visits in Japan, Sweden, France, Germany, Russia, Italy, Wales, Argentina, Iran, and elsewhere gave us a world perspective. Manuscripts that have to be produced, sometimes a bit unwillingly, offer the challenge to present speculation and perspective often not welcome by editors of prestigious journals. It was in a volume from a conference dedicated to one of the giants of the bioenergetics field, Efraim Racker, that the designation “the binding change mechanism” was introduced. Conferences at the University of Wisconsin provided opportunity to publish thoughts about rotational catalysis that had not been enthusiastically endorsed at Gordon Conferences, where information is exchanged without publication. These travels have strong scientific justification. They provided the opportunity for exchange of information, to test new ideas, to gain new perspective, and to avoid unnecessary experiments. The milieu encourages innovation and planning, as well as providing a stimulus and vitality that fosters research progress.  Other events that make up a lifetime continued. Through fortunate circumstances, Lyda and I obtained a building lot at a price that a professor could afford, in the hills north of UCLA, overlooking the city and ocean. The home we built (I was again contractor and miscellaneous laborer) has served as a focal point for family activities, and a temporary residence for grandchildren attending UCLA. The home meant much for my research, as I could readily move between home and lab, and the ambiance created was supportive for study and writing.  The study of life processes has given me a deep appreciation for the marvel of the living cell. The beauty, the design, and the controls honed by years of evolution, and the ability humans have to gain more and more understanding of life, the earth and the universe, are wonderful to contemplate. I firmly believe that our present and future knowledge of all that we are and what surrounds us depends on the tools and approaches of science. I was struck by how well [Harold Kroto](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1996/index.html), one of last year’s Nobelists, presented what are some of my views in his biographical sketch. As he stated, “I am a devout atheist–nothing else makes sense to me and I must admit to being bewildered by those, who in the face of what appears to be so obvious, still believe in a mystical creator.” I wonder if in the United States we will ever reach the day when the man-made concept of a God will not appear on our money, and for political survival must be invoked by those who seek to represent us in our democracy.  It is disappointing how little the understanding that science provides seems to have permeated into society as a whole. All too common attitudes and approaches seem to have progressed little since the days of Galileo. Religious fundamentalists successfully oppose the teaching of evolution, and by this decry the teaching of critical thinking. We humans have a remarkable ability to blind ourselves to unpleasant facts. This applies not only to mystical and religious beliefs, but also to long-term environmental consequences of our actions. If we fail to teach our children the skills they need to think clearly, they will march behind whatever guru wears the shiniest cloak. Our political processes and a host of human interactions are undermined because many have not learned how to gain a sound understanding of what they encounter.  The major problem facing humanity is that of the survival of our selves and our progeny. In my less optimistic moments, I feel that we will continue to decimate the environment that surrounds us, even though we know of our folly and of what has happened to others. Humans could become quite transient occupants of planet earth. The most important cause of our problem is over population, which nature, as with other species, will deal with severely. I hear the cry from capable environmental leaders and organizations for movement toward sustainable societies. They are calling for sensible approaches to steer us away from impending disaster. But their voices remain largely unheard as those with power, and those misled by religious or nationality concerns, become immersed in unimportant, self-centered and short range pursuits.  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.  *Paul D. Boyer died on 2 June 2018.* |
| Autobiographical |  |
| Podcast |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q2 | **So then you went to university, was that in Wisconsin or?** |
|  | Paul D. Boyer: That was in Wisconsin. One of the professors at the University of Wisconsin had discovered that you can put vitamin D into milk by shining ultraviolet light so they patented it and the monies from that, they set up for scholarships and when I finished at the University of Utah, there was no course in biochemistry. I hardly knew that biochemistry existed but I was interested in, I knew enough to know that it was there but I didn’t know nearly the extent of the field, but there was a scholarship notice on the bulletin board that the University of Wisconsin, there was a scholarship that you could apply for to study biochemistry. |
| Q4 | **Sounds good, but this is also where you began with your work on the enzymes in energy metabolism in more serious ways?** |
|  | Paul D. Boyer: Most of my career I spent working on these big molecules, they’re called enzymes, they do catalysis, they’re ones that make chemical reactions go and at that stage, I worked actually on an interesting enzyme in the Nobel Medical Institute and had a nice little publication with Professor Theorell from there. It was actually, well I won’t tell you what it was. |
| Q4 | **No, no. But so then, when did you actually do the work on the ATP synthase? When did that begin?** |
|  | Paul D. Boyer: That began in early 1970s. We had spent a considerable effort in the 1960s, in this process, as you know, of oxidated phosphorylation, the ATP, there’s lots of it made, it’s a very important compound. We thought from the paradigms in biochemistry, the way that we knew from other biochemical things that there should be an intermediate in the thing, so we spent lots of years looking for intermediate. Now I have a tendency to be lucky, because we did find a substance that we thought was an intermediate oxidated phosphorylation in making the ATP as we breathe oxygen. It turned out to be, though, an intermediate in a lower cycle, not the big problem, but the small problem, but it was a new chemical compound, never seen before in nature and it’s phosphohistidine, the first time it had been described and so then we’d at least, as I’ve sometimes said, we were reaching for a gold but we got a brass. |
| Q14 | **So then maybe if we now look at the work today in bioenergetics, what would you say is the most important questions that should be looked at today in this field?** |
|  | Paul D. Boyer: The immediate questions, well the enzyme that I worked with is ATP synthase. Now, to tell you what I think the most important questions are, I need to tell you a bit more about the molecule itself. The mitochondrion, the inter-cellular organelle of which this is made, has a membrane in which the enzyme can, I know you’re well acquainted with this, and that’s through this membrane, you develop with the oxidations and electrochemical grating the charge across the membrane that wants to push positively charged protons through the membrane.  Now, we worked mostly on the part of the enzyme that was above the membrane. This is the membrane, we worked on the part that was above this. That’s where the adenosine and triphosphatase push is, as you know, and so we found that the protons, when they go through the membrane, cause the internal part of this to rotate like a motor and change, as you know, with the outer sub-units. I’d say one of the biggest problems in bioenergetics at the present time relates to this proton translocation. It’s not known how the proton translocation makes the bottom part turn. We know that it turns and does this, so one of the immediate important problems is try to get the structure of that bottom part and to deduce how it turns. Now that holds, then, not only for this one but for a whole host of bioenergetic processes that use membranes and translocation of ions across the membrane. |
| Q14 | **And did the hormone denominate, to some extent, these processes?** |
|  | Paul D. Boyer: Yes, to look for other things, for active transport of amino acids, you see, and it still is not known how the lactose transporter works in any detail, so there’s these problems in bioenergetics at the same level of explanation that we sought for the ATP synthase. Now those are the, what should I say? Those are the visible problems, those are the ones that are continuation of present aspects to get more explanations of them. Now, that doesn’t mean they aren’t important because our ATP synthase work was a continuation of trying to understand how oxidated phosphorylation occurred and then in terms of the bioenergetics and the mechanisms, the whole field of motion and contractility is opening up in a lovely way. We don’t yet, though, know how cells move across each other.  For example, in evolution, you produce, we think of the evolving of life on earth as working into a single cell organism and this is the way it was for billions of years. For billions of years, there were only bacteria, you see, and then we achieve a mollusc cellular organism. In order to do that, we had to have cells to start communicating with each other. Now, communication and control is in one sense bioenergetics, you see, and they had to learn how to move with respect to one another. How do cells move during development, you see? What is this type of thing? How do you use it to that? And the control of that movement, you see. The control of that movement makes it so that that movement produces an ear on this side some way. |
| Q18 | **That may come for the future. Ok. Well, is there anything you would like to add that we haven’t talked about that we should discuss?** |
|  | Paul D. Boyer: Oh yes. If you push a button on a professor, he’s always willing to talk. I would like to comment on two things. One, the Nobel Prizes. The responsibility of the Nobel committee is, as I’m sure you’re aware, they’re writing the history of science. They choose just one to the problem but the ones they choose become then much better known in nature so I think this is one of the prominent functions of the Nobel; but the other one relates to the public understanding of science. I think the fact that there is a Nobel Prize, that it gets in the newspapers, they’ve done a good job, up until my selection, of goods selections, you see, so that it’s been well received on it. This has helped the interaction of science and society because I feel that the privilege of being a scientist at the present time in our society is a remarkably fine thing, both for the scientist and humanity.  I think within a grouping of a university setting, where you can get a grouping of students and post-doctor of follows, that are sitting around a table here with 10 or 12 of you trying to understand the experiments you’ve just completed, trying to devise the experiments you want to do next, discussing them in an atmosphere where it’s bringing the best thoughts of these people. Out of this then comes the milieu and the things that makes it possible for me to think of a new approach at a different time, you see. I think that grouping of scholars in a university supporting now by society, they give us billion, they give us monies to buy instruments, is the finest thing that’s been developed to try to understand nature, to try to get the answers of science and then, so I appreciate that, I think that this is something that we need to tell humanity we appreciate.  Now, the other part of that is, is that what is produced out of basic research has done so much to change the world. Now, either you want to understand how the world operates and let it change and let it enrich the lives of the other people. To me, when I can listen to a fine piece of creative music, that’s an enrichment that somebody else gave me but I think for the general public, when they can learn about how the oxygen that they breathe, what it does or how the last aspect of astronomy, I think they appreciate it. I think there’s a beauty to it so I think we return to the … |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | **0369** |
| **Biographical** | I was born in Halifax, Yorkshire on January 7th, 1941 to Thomas Ernest Walker and Elsie Walker (née Lawton). My father was a stone mason, and a talented amateur pianist and vocalist. I was brought up with my two younger sisters, Judith and Jennifer, in a rural environment overlooking the Calder valley near Elland, and then in Rastrick. I received an academic education at Rastrick Grammar School, specializing in Physical Sciences and Mathematics in the last three years. I was a keen sportsman, and became school captain in soccer and cricket. In 1960, I went to St. Catherine’s College, Oxford, and received the B.A. degree in Chemistry in 1964.  In 1965, I began research on peptide antibiotics with E. P. Abraham in the Sir Willian Dunn School of Pathology, Oxford, and was awarded the D. Phil. degree in 1969. During this period, I became aware of the spectacular developments made in Cambridge in the 1950s and early 1960s in Molecular Biology through a series of programmes on BBC television given by [John Kendrew](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1962/index.html), and published in 1966 under the title “The Thread of Life”. These programmes made a lasting impression on me, and made me want to know more about the subject. Two books, “Molecular Biology of the Gene” by [J. D. Watson](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1962/index.html), first published in 1965, and William Hayes’ “Bacterial Genetics” helped to assuage my appetite for more information. My knowledge of this new field was extended by a series of exciting lectures for graduate students on protein structure given in 1966 by David Phillips, the new Professor of Molecular Biophysics at Oxford. Another series of lectures given by Henry Harris, the Professor of Pathology and published in book form under the title “Nucleus and Cytoplasm”, provided more food for thought.  Then followed a period of five years working abroad, from 1969-1971, first at The School of Pharmacy at the University of Wisconsin, and then from 1971-1974 in France, supported by Fellowships from NATO and EMBO, first at the CNRS at Gif-sur-Yvette and then at the Institut Pasteur.  Just before Easter in 1974, I attended a research workshop in Cambridge entitled “Sequence Analysis of Proteins”. It was sponsored by EMBO (The European Molecular Biology Organization), and organised by Ieuan Harris from the Medical Research Council’s Laboratory of Molecular Biology (LMB) and by Richard Perham from the Cambridge University Department of Biochemistry. At the associated banquet, I found myself sitting next to someone that I had not met previously, who turned out to be [Fred Sanger](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1958/index.html). In the course of our conversation, he asked if I had thought about coming back to work in England. I jumped at the suggestion, and with some trepidation, approached Ieuan Harris about the possibility of my joining his group. After discussions with Fred Sanger, it was agreed that I could come to the Protein and Nucleic Acid Chemistry (PNAC) Division at the LMB for three months from June 1974. More than 23 years later, I am still there.  It goes without saying that this encounter with Fred Sanger and Ieuan Harris transformed my scientific career. In 1974, the LMB was infused throughout its three Divisions with a spirit of enthusiasm and excitement for research in molecular biology led by [Max Perutz](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1962/index.html) (the Chairman of the Laboratory), Fred Sanger, [Aaron Klug](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1982/index.html), [Francis Crick](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1962/index.html), Sidney Brenner, Hugh Huxley, John Smith and [César Milstein](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1984/index.html), which was coupled with extraordinary success. For example, along the corridor from my laboratory Fred was inventing his methods for sequencing DNA, immediately across the corridor César Milstein and Georges Köhler were inventing monoclonal antibodies, and elsewhere in the building, Francis Crick and Aaron Klug and their colleagues were revealing the structures of chromatin and transfer RNA. Fred’s new DNA sequencing methods were applied first to the related bacteriophages fX174 and G4, and then to DNA from human and bovine mitochondria. I analyzed the sequences of the proteins from G4 and from mitochondria using direct methods. These efforts led to the discovery of triple overlapping genes in G4 where all three DNA phases encode proteins, and to the discovery that subunits I and II of cytochrome c oxidase were encoded in the DNA in mitochondria. Later on, I helped to uncover details of the modified genetic code in mitochondria.  In 1978, I decided to apply protein chemical methods to membrane proteins, since this seemed to be both a challenging and important area. Therefore, in search of a suitable topic, I read the literature extensively. The enzymes of oxidative phosphorylation from the inner membranes of mitochondria were known to be large membrane bound multi-subunit complexes, but despite their importance, they had been studied hardly at all from a structural point of view. Therefore, the same year, I began a structural study of the ATP synthase from bovine heart mitochondria and from eubacteria. These studies resulted eventually in a complete sequence analysis of the complex from several species, and in the atomic resolution structure of the F catalytic domain of the enzyme from bovine mitochondria, giving new insights into how ATP is made in the biological world. Michael Runswick has worked closely with me throughout this period, and has made contributions to all aspects of our studies.  In 1959, I received the A. T. Clay Gold Medal. I was awarded the Johnson Foundation Prize by the University of Pennsylvania in 1994, in 1996, the CIBA Medal and Prize of the Biochemical Society, and The Peter Mitchell Medal of the European Bioenergetics Congress, and in 1997 The Gaetano Quagliariello Prize for Research in Mitochondria by the University of Bari, Italy. In 1995, I was elected a Fellow of the Royal Society. In 1997, I was made a Fellow of Sidney Sussex College, Cambridge and became an Honorary Fellow of St. Catherine’s College, Oxford.  I married Christina Westcott in 1963. We have two daughters, Esther, aged 21 and Miriam, aged 19. At present, both of them are university students, studying Geography and English, respectively, at Nottingham-Trent and Leeds Universities. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | **0370** |
| **Biographical** | I was born on the 8th of October 1918 into a wealthy family in Lemvig, a town in the western part of Denmark. The town is nicely situated on a fjord, which runs across the country from the Kattegat in East to the North Sea in West. It is surrounded by hills, and is only 10 km, i.e. bicycling distance, from the North Sea, with its beautiful beaches and dunes. My father Magnus Martinus Skou together with his brother Peter Skou were timber and coal merchants.  We lived in a big beautiful house, had a nice summer house on the North Sea coast. We were four children, I was the oldest with a one year younger brother, a sister 4 years younger and another brother 7 years younger. The timber-yard was an excellent playground, so the elder of my brothers and I never missed friends to play with. School was a minor part of life.  When I was 12 years old my father died from pneumonia. His brother continued the business with my mother Ane-Margrethe Skou as passive partner, and gave her such conditions that there was no change in our economical situation. My mother, who was a tall handsome woman, never married again. She took care of us four children and besides this she was very active in the social life in town.  When I was 15, I went to a boarding school, a gymnasium (high school) in Haslev a small town on Zealand, for the last three years in school (student exam). There was no gymnasium in Lemvig.  Besides the 50-60 boys from the boarding section of the school there were about 400 day pupils. The school was situated in a big park, with two football fields, facilities for athletics, tennis courts and a hall for gymnastics and handball. There was a scout troop connected to the boarding section of the school. I had to spend a little more time preparing for school than I was used to. My favourites were the science subjects, especially mathematics. But there was plenty of time for sports activities and scouting, which I enjoyed. All the holidays, Christmas, Easter, summer and autumn I spent at home with my family.  After three years I got my exam, it was in 1937. I returned to Lemvig for the summer vacation, considering what to do next. I could not make up my mind, which worried my mother. I played tennis with a young man who studied medicine, and he convinced me that this would be a good choice. So, to my mother’s great relief, I told her at the end of August that I would study medicine, and started two days later at the University of Copenhagen.  The medical course was planned to take 7 years, 3 years for physics, chemistry, anatomy, biochemistry and physiology, and 4 years for the clinical subjects, and for pathology, forensic medicine, pharmacology and public health. I followed the plan and got my medical degree in the summer of 1944.  I was not especially interested in living in a big town. On the other hand it was a good experience for a limited number of years to live in, and get acquainted with the capital of the country, and to exploit its cultural offers. Art galleries, classical music and opera were my favourites.  For the first three years I spent the month between the semesters at home studying the different subjects. For the last 4 years the months between the semesters were used for practical courses in different hospital wards in Copenhagen.  It was with increasing anxiety that we witnessed to how the maniac dictator in Germany, just south of our border, changed Germany into a madhouse. Our anxiety did not become less after the outbreak of the war. In 1914 Denmark managed to stay out of the war, but this time, in April 1940 the Germans occupied the country. Many were ashamed that the Danish army were ordered by the government to surrender after only a short resistance. Considering what later happened in Holland, Belgium and France, it was clear that the Danish army had no possibility of stopping the German army.  The occupation naturally had a deep impact on life in Denmark in the following years, both from a material point of view, but also, what was much worse, we lost our freedom of speech. For the first years the situation was very peculiar. The Germans did not remove the Danish government, and the Danish government did not resign, but tried as far as it was possible to minimize the consequences of the occupation. The army was not disarmed, nor was the fleet. The Germans wanted to use Denmark as a food supplier, and therefore wanted as few problems as possible.  The majority of the population turned against the Germans, but with no access to weapons, and with a flat homogeneous country with no mountains or big woods to hide in, the possibility of active resistance was poor. So for the first years the resistance only manifested itself in a negative attitude to the Germans in the country, in complicating matters dealing with the Germans as far as possible, and in a number of illegal journals, keeping people informed about the situation, giving the information which was suppressed by the German censorship. There was no interference with the teaching of medicine.  The Germans armed the North Sea coast against an invasion from the allied forces. Access was forbidden and our summer house was occupied. My grandmother had died in 1939, and we four children inherited what would have been my father’s share. For some of the money my brother and I bought a yacht, and took up sailing, and this has since been an important part of my leisure time life. After the occupation the Germans had forbidden sailing in the Danish seas except on the fjord where Lemvig was situated, and another fjord in Zealand.  The resistance against the Germans increased as time went on, and sabotage slowly started. Weapons and ammunition for the resistance movement began to be dropped by English planes, and in August 1943 there were general strikes all over the country against the Germans with the demand that the Government stopped giving way to the Germans. The Government consequently resigned, the Germans took over, the Danish marine sank the fleet and the army was disarmed. An illegal Frihedsråd (the Danish Liberation Council) revealed itself, which from then on was what people listened to and took advice from.  Following this, the sabotage against railways and factories working for the Germans increased, and with this arrests and executions. One of our medical classmates was a German informer. We knew who he was, so we could take care. He was eventually liquidated by unknowns. We feared a reaction from Gestapo against the class, and stayed away from the teaching.  The Germans planned to arrest the Jews, but the date, the night between the first and second October 1943 was revealed by a high placed German. By help from many, many people the Jews were hidden. Of about 7000, the Germans caught 472, who were sent to Theresienstadt where 52 died. In the following weeks illegal routes were established across the sea, Øresund, to Sweden, and the Jews were during the nights brought to safety. From all sides of the Danish society there were strong protests against the Germans for this encroachment on fellow-countrymen.  In May and June 1944, we managed to get our exams. A number of our teachers had gone underground, but their job was taken over by others. We could not assemble to sign the Hippocratic oath, but had to come one by one at a place away from the University not known by others.  I returned to my home for the summer vacation. The Germans had taken over part of my mother’s house, and had used it for housing Danes working for the Germans. This was extremely unpleasant for my mother, but she would not leave her house and stayed. I addressed the local German commander, and managed to get him to move the “foreigners” from the house at least as long as we four children were home on holiday.  The Germans had forbidden sailing, but not rowing, so we bought a canoe and spent the holidays rowing on the fjord.  After the summer holidays I started my internship in a hospital in Hjørring in the northern part of the country. I first spent 6 months in the medical ward, and then 6 months in the surgical ward. I became very interested in surgery, not least because the assistant physician, next in charge after the senior surgeon, was very eager to teach me how to make smaller operations, like removing a diseased appendix. I soon discovered why. When we were on call together and we during the night got a patient with appendicitis, it happened–after we had started the operation–that he asked me to take over and left. He was then on his way to receive weapons and explosives which were dropped by English planes on a dropping field outside Hjørring. I found that this was more important than operating patients for appendicitis, but we had of course to take care of the patients in spite of a war going on. He was finally caught by the Gestapo, and sent to a concentration camp, fortunately not in Germany, but in the southern part of Denmark, where he survived and was released on the 5th of May 1945, when the Germans in Denmark surrendered.  I continued for another year in the surgical ward. It was here I became interested in the effect of local anaesthetics, and decided to use this as a subject for a thesis. Thereafter I got a position at the Orthopaedic Hospital in Aarhus as part of the education in surgery.  In 1947 I stopped clinical training, and got a position at the Institute for Medical Physiology at Aarhus University in order to write the planned doctoral thesis on the anaesthetic and toxic mechanism of action of local anaesthetics.  During my time in Hjørring I met a very beautiful probationer, Ellen Margrethe Nielsen, with whom I fell in love. I had become ill while I was on the medical ward, and spent some time in bed in the ward. I had a single room and a radio, so I invited her to come in the evening to listen to the English radio, which was strictly forbidden by the Germans – but was what everybody did.  After she had finished her education as a nurse in 1948, she came to Aarhus and we married. In 1950 we had a daughter, but unfortunately she had an inborn disease and died after 1 1/2 years. Even though this was very hard, it brought my wife and I closer together. In 1952, and in 1954 respectively we got two healthy daughters, Hanne and Karen.  The salary at the University was very low, so partly because of this but also because I was interested in using my education as a medical doctor, I took in 1949 an extra job as doctor on call one night a week. It furthermore had the advantage that I could get a permission to buy a car and to get a telephone. There were still after war restrictions on these items.  I was born in a milieu which politically was conservative. The job as a doctor on call changed my political attitude and I became a social democrat. I realized how important it is to have free medical care, free education with equal opportunities, and a welfare system which takes care of the weak, the handicapped, the old, and the unemployed, even if this means high taxes. Or as phrased by one of our philosophers, N.F.S. Grundtvig, “a society where few have too much, and fewer too little”.  We lived in a flat, so the car gave us new possibilities. We wanted to have a house, and my mother would give us the payment, but I was stubborn, and wanted to earn the money myself. In 1957 we bought a house with a nice garden in Risskov, a suburb to Aarhus not far away from the University.  I am a family man, I restricted my work at the Institute to 8 hours a day, from 8 to 4 or 9 to 5, worked concentratedly while I was there, went home and spent the rest of the day and the evening with my wife and children. All weekends and holidays, and 4 weeks summer holidays were spent with the family. In 1960 we bought an acre of land on a cliff facing the beach 45 minutes by car from Aarhus, and built a small summer house. From then on this became the centre for our leisure time life. We bought a dinghy and a rowing boat with outboard motor and I started to teach the children how to sail, and to fish with fishing rod and with net.  Later, when the girls grew older, we bought a yacht, the girls and I sailed in the Danish seas, and up along the west coast of Sweden. My wife easily gets seasick, but joined us on day tours. Later the girls took their friends on sail tours.  In wintertime the family skied as soon as there was snow. A friend of mine, Karl Ove Nielsen, a professor of physics, took me in the beginning of the 1960s at Easter time on an 8-day cross country ski tour through the high mountain area in Norway, Jotunheimen. We stayed overnight in the Norwegian Tourist Association’s huts on the trail, which were open during the Easter week. It was a wonderful experience, but also a tour where you had to take all safety precautions. It became for many years a tradition. Later the girls joined us, and they also took some of their friends. When the weather situation did not allow this tour, we spent a week in more peaceful surroundings either in Norway with cross-country skiing or in the Alps with slalom. We still do, now with the girls, their husbands and the grandchildren. Outside the sporting activities, I spend much time listening to classical music, and reading, first of all biographies.  When the children left home, one for studying medicine and the other architecture, my wife worked for several years as a nurse in a psychiatric hospital for children, then engaged herself in politics. She was elected for the County Council for the social democrats, and spent 12 years on the council, first of all working with health care problems. She was also elected to the county scientific ethical committee, which evaluates all research which involves human beings. Later she was elected co-chairman to the Danish Central Scientific Ethical Committee, which lay down the guidelines for the work on the local committees, and which is an appeal committee for the local committees as well as for the doctors. She has worked 17 years on the committees and has been lecturing nurses and doctors about ethical problems.  I had no scientific training when I started at the Institute of Physiology in 1947. It took me a good deal of time before I knew how to attack the problem I was interested in and get acquainted with this new type of work. The chairman, Professor Søren L. Ørskov was a very considerate person, extremely helpful, patient, and gave me the time necessary to find my feet. During the work I got so interested in doing scientific work that I decided to continue and give up surgery. The thesis was published as a book in Danish in 1954, and written up in 6 papers published in English. The work on the local anaesthetics, brought me as described in the following paper to the identification of the sodium-potassium pump, which is responsible for the active transport of sodium and potassium across the cell membrane. The paper was published in 1957. From then on my scientific interest shifted from the effect of local anaesthetics to active transport of cations.  In the 1940s and the first part of the 1950s, the amount of money allocated for research was small. Professor Ørskov, fell chronically ill. His illness developed slowly so he continued in his position, but I, as the oldest in the department after him, had partly to take over his job. This meant that besides teaching in the semesters I had to spend two months per year examining orally the students in physiology.  The identification of the sodium-potassium pump gave us contact to the outside scientific world. In 1961, I met R.W. Berliner at an international Pharmacology meeting in Stockholm. He mentioned the possibility of obtaining a grant from National Institutes of Health (NIH). I applied and got a grant for two years. The importance of this was not only the money, but that it showed interest in the work we were doing.  In 1963, Professor Ørskov resigned and I was appointed professor and chairman. In the late 1950s and especially in the 1960s, more money was allocated to the Universities, and also more positions. Due to the work with the sodium-potassium pump, it became possible to attract clever young people, and the institute staff in a few years increased from 4 to 20-25 scientists. This had also an effect on the teaching. I got a young doctor, Noe Næraa, who had expressed ideas about medical teaching, to accept a position at the Institute. He started to reorganise our old fashioned laboratory course, we got new modern equipment, and thereafter we also reorganised the teaching, made it problem-oriented with teaching in small classes. My scientific interest was membrane physiology, but I wanted also to find people who could cover other aspects of physiology, so we ended up with 5-6 groups who worked scientifically with different physiological subjects.  In 1972 we got a new statute for the Universities, which involved a democratization of the whole system. The chairman was no longer the professor (elected by the board of chairmen which made up the faculty), but he/she was now elected by all scientists and technicians in the Institute and could be anybody, scientist or technician. This was of course a great relief for me because I could get rid of all my administrative duties. A problem was, however, that I got elected as chairman, but later others took over. In the beginning it was very tedious to work with the system, not least because everybody thought that they should be asked and take part in every decision. Later we learned to hand over the responsibility to an elected board at the Institute.  In these years the money to the Institutes came from the Faculty, which got it from the University (which got it from the State). The money was then divided inside the institute by the chairman, and later by the elected board. It was usually sufficient to cover the daily expenses of the research. External funds were only for bigger equipment. Besides research-money we had a staff of very well trained laboratory assistants, whose positions–as well as the positions of the scientific staff–was paid by the University. The institute every year sent a budget for the coming year, to the faculty, who then sent a budget for the faculty to the University, and the University to the State.  This way of funding had the great advantage that there was not a steady pressure on the scientists for publication and for sending applications for external funds. It was a system that allowed everybody to start on his/her own project, independently, and test their ideas. Nobody was forced from lack of money to join a group which had money and work on their ideas. It was also a system which could be misused, by people who were not active scientifically. With an elected board it proved difficult to handle such a situation. Not least because the very active scientists tried to avoid being elected – i.e. it could be the least active who actually decided. In practice, however, the not very active scientists usually accepted to do an extra job with the teaching, thus relieving the very active scientists from part of the teaching burden.  In the 1980s this was changed, the money for science was transferred to centralized (state) funds, and had to be applied for by the individual scientist. Not an advantage from my point of view. Applications took a lot of time, it tempted a too fast publication, and to publish too short papers, and the evaluation process used a lot of manpower. It does not give time to become absorbed in a problem as the previous system.  My research interest was concentrated around the structure and function of the active transport system, the Na+,K+-ATPase. A number of very excellent clever young scientists worked on different sides of the subject, either their own choice or suggested by me. Each worked independent on his/her subject. Scientists who took part in the work on the Na+,K+-ATPase and who made important contributions to field were, P.L. Jørgensen (purification and structure), I. Klodos (phosphorylation), O. Hansen (effect of cardiac glycosides and vanadate), P. Ottolenghi (effect of lipids), J. Jensen (ligand binding), J. G. Nørby (phosphorylation, ligand binding, kinetics), L. Plesner (kinetics), M. Esmann (solubilization of the enzyme, molecular weight, ESR studies), T. Clausen (hormonal control), A.B. Maunsbach and E. Skriver from the Institute of Anatomy in collaboration with P.L. Jørgensen (electron microscopy and crystallization), and I. Plesner from the Department of Chemistry (enzyme kinetics and evaluation of models). We also had many visitors.  We got many contacts to scientists in different parts of the world, and I spent a good deal of time travelling giving lectures. In 1973 the first international meeting on the Na+,K+-ATPase was held in New York. The next was 5 years later in Århus, and thereafter every third year. The proceedings from these meetings have been a very valuable source of information about the development of the field.  My wife joined me on many of the tours and we got friends abroad. Apart from the scientific inspiration the travelling also gave many cultural experiences, symphony concerts, opera and ballet, visits to Cuzco and Machu Picchu in Peru, to Uxmal and Chichén Itzá on the Yucatan Peninsula, and to museums in many different countries. Not to speak of the architectural experiences from seeing many different parts of the world. And not least it gave us good friends.  It is not always easy to keep your papers in order when travelling. Sitting in the airport in Moscow in the 1960s waiting for departure to Khabarovsk in the eastern part of Siberia, we–three Danes on our way to a meeting in Tokyo–realized that we had forgotten our passports at the hotel in town. There were twenty minutes to departure and no way to get the passports in time. We asked Intourist what to do. There was only one boat connection a week from Nakhodka, where we should embark to Yokohama, so they suggested that we should go on, they would send the passports after us. I had once had a nightmare, that I should end my days in Siberia. When we after an overnight flight arrived in Khabarovsk we were met by a lady who asked if we were the gentlemen without passports. We could not deny, and she told us that they would not arrive until after we had left Khabarovsk by train to Nakhodka. But they would send them by plane to Vladivostok and from there by car to Nakhodka. To our question if we could leave Siberia without our passports the answer was no. When the train the following morning stopped in Nakhodka, a man came into the sleeping car and asked if we were the gentlemen without passports. To our “yes” he said “here you are”, and handed over the passports. Amazing. We had an uncomplicated boat trip to Yokohama.  It was not as easy some years later in Argentina. I had been at a meeting in Mendoza, had stopped in Cordoba on the way, had showed passport in and out of the airports without problems. Returning to Buenos Aires to leave for New York, the man at the counter told me that my passport had expired three months earlier, and according to rules I had to return direct to my home country. I argued that I was sure I could get into the U.S., but he would not give way. We discussed for half an hour. Finally shortly before departure he would let me go to New York if he could reserve a plane out of New York to Denmark immediately after my arrival. He did the reservation, put a label on my ticket with the time of departure, and by the second call for departure I rushed off, hearing him saying “You can always remove the label”. In New York, I stepped to the rear end of the line, hoping the man at the counter would be tired when it was my turn. He was not. I asked if I had to return to Denmark. “There is always a way out” was his answer, “No, go to the other counter, sign some papers, pay 5 dollars, and I let you in”.  In 1977, I was offered the chair of Biophysics at the medical faculty. It was a smaller department, with 7 positions for scientists, of which 5 were empty, which meant that we could get positions for I. Klodos and M. Esmann, who had fellowships. Besides J. G. Nørby and L. Plesner moved with us. The two members in the Institute, M. J. Mulvany and F. Cornelius became interested in the connection between pump activity and vasoconstriction, and reconstitution of the enzyme into liposomes, respectively, i.e. all in the institute worked on different sides of the same problem, the structure and function of the Na+,K+-ATPase. We got more space, less administration, and I was free of teaching obligations.  We all got along very well, lived in a relaxed atmosphere, inspiring and helping each other, cooperating, also with the Na+,K+-ATPase colleagues left in the Physiological Institute. And even if we all worked on different sides of the same problem, there were never problems of interfering in each others subjects, or about priority.  In 1988, I retired, kept my office, gave up systematic experimental work and started to work on kinetic models for the overall reaction of the pump on computer. For this I had to learn how to programme, quite interesting, and amazing what you can do with a computer from the point of view of handling even complicated models. And even if my working hours are fewer, being free of all obligations, the time I spent on scientific problems are about the same as before my retirement.  I enjoy no longer having a meeting calendar, I enjoy to go fly-fishing when the weather is right, and enjoy spending a lot of time with my grandchildren. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0371 |
| **Biographical** | I was born in Alice, Texas on August 23, 1933. My father was a Methodist minister, and my mother was what we then called a housewife. I have a sister, Mary, who is some years my elder. In those days, Methodist ministers moved often, and as a child I lived in a succession of mostly small towns in south Texas: Alice, Brady, San Antonio, Kingsville, Del Rio, Brownsville, McAllen, Austin, then back to San Antonio. During this time the church hierarchy recognized that my father was an able administrator capable of organizing people to get things done and gifted at resolving conflicts. From the time I was about nine my father was no longer pastor of a church but rather a supervisor of church activities over a district. This was a great relief to me as I was spared being the center of judgmental attention as the “preacher’s kid.”  By the time I reached adulthood my father was universally revered as a fair, kind, and gentle man with an acute mind. His most enduring monument will be the San Antonio Medical Center as he worked hard and effectively to start the Methodist Hospital there, which really started the center.  When I was nine years old, my parents gave me a chemistry set. Within a week, I had decided to become a chemist and never wavered from that choice. As I grew my interest in chemistry grew more intense, if not more sophisticated. Of course there was no chemistry in the school program until high school.  I was not a particularly distinguished student as a child. My grades were good but obtained more by steady work than any brilliance on my part. I vividly remember my father telling me that one of my elementary school teachers had told him that I was not brilliant but I was a steady hard worker. Somehow the further I progressed in school, the easier it became to do well.  It was a great delight when I finally got to study chemistry in high school. My teacher, Mrs. Lorena Davis, saw that I was keenly interested and did her best to foster and nourish that interest. As only one year of chemistry was offered then, I had no formal course in the subject to take my final year in high school. Mrs. Davis offered me special projects to satisfy my appetite for chemistry. I remember most constructing a Cottrell Precipitator. I was shocked to see Mrs. Davis, who didn’t smoke, light up a cigarette and blow smoke into the precipitator to demonstrate that it worked.  When it came time to choose a college, I got interested in Rice Institute. It had an excellent reputation as being a good school for a dedicated student. I was also impressed by how well its football team was doing. My parents loved my choice because at that time Rice charged no tuition, and they would have been hard pressed to send me to a university that did. While my father held the highest administrative office, not counting the Bishop, in the Southwest Texas Conference, he did not make much money.  At that time there was a high failure rate at Rice. With no tuition, students were expected to prove themselves worthy or make way for someone else. However, I was ready for the challenge that Rice presented and prospered academically. Socially, my fellow students were ready for the challenge that I presented and worked hard to convert a rather straight-laced, serious boy into someone they could stand to be around.  By a quirk of fate, the most colorful professors I encountered in my first years taught subjects other than chemistry. I liked my first and second year chemistry professors (in fact I later developed a closer relation with my second year professor, John T. Smith), but they were not particularly colorful. It was not until my third year when I had John E. Kilpatrick for Physical Chemistry and George Holmes Richter for Organic Chemistry that the chemistry department began to pull ahead in the colorfulness race. John Kilpatrick came to class, sat in the middle of the table in front, lit a cigarette, took an enormous drag, and began to speak. No smoke came out! Richter enlivened his lectures by describing the pharmacological effects of various organic chemicals. Richter was a fine teacher of Organic Chemistry, but that was of little use to me since I had an almost unnatural aversion to Organic Chemistry. Kilpatrick was the most welcoming to students of any person I ever encountered with absolutely no regard for the amount of time he spent with a student. This, happily for him, made the time he devoted self-limiting, because I would think about whether I had an hour or two to spare before dropping by to see him.  The most impressive chemistry teacher I had was Richard Turner, whom I first encountered in a senior Natural Products course. (The curriculum was cleverly constructed so that it was impossible to avoid a second encounter with Organic Chemistry.) It was his enthusiastic discussion of barriers to internal rotation and the pioneering work of Kenneth Pitzer in the area that made me resolve to go to University of California, Berkeley, and work with Pitzer. This is a decision I have never regretted.  While I was at Berkeley, Pitzer was the Dean of the College of Chemistry and a very busy man. Nevertheless he was always completely accessible to his graduate students, and always genuinely delighted to see me when I interrupted his work. When our conversation reached its conclusion, he graciously got me out of his office. I was grateful for this as well because at the time, as you can see from my comments about visiting John Kilpatrick, I had trouble with leave-takings. I think that I received an excellent education in how to do research from Pitzer. The most important work I did at Berkeley was on Pitzer’s extension of the Theory of Corresponding States. Over the years, I have remained in contact with Ken and Jean Pitzer. Indeed, we were able to collaborate again in research some years later when he was president of Rice University.  My years at Berkeley were some of the happiest of my life primarily because it was during this time that I met and married my wife, Jonel. Our union seemed pre-ordained when we discovered that our ancestors came from the tiny town of Center Point, Texas (pop. 300).  At that time, there seemed to be an unwritten rule that Pitzer’s students should do experiment as well as theory. This suited me, because I had always been interested in experiments. Pitzer suggested that I investigate the matrix isolation infrared spectrum of disiloxane in order to establish whether the SiO-Si bond was linear or bent. If I had tried to do these experiments involving liquid hydrogen without help, I believe there is a good chance an explosion would have resulted. However, a fellow student, Dolphus Milligan, helped me tremendously with these experiments and with his aid I was able to collect the necessary data, which indicated that Si-O-Si is somewhat bent from linearty.  Pitzer was able to help me get a post-doctoral position with E. Bright Wilson at Harvard. At that time, Wilson had developed a method for measuring barriers to internal rotation using microwave spectroscopy and I was still interested in internal rotation barriers. It seemed a perfect situation. I enjoyed Harvard scientifically. Wilson’s personality was very different from Pitzer’s. Although he was born in Tennessee, he personified the New England virtues of upright integrity and serious concern about all aspects of life. His disapproval of superstition in all forms was well-known; none of us would dare mention in his presence the gremlins we all suspected inhabited his microwave spectrometer. Wilson above all was a fine, decent, caring person who wanted the best for his students.  The atmosphere in Mallinkrodt Laboratory at Harvard was somewhat different from that of Lewis Hall at Berkeley. Perhaps it was because the graduate system and expectations for graduate students were different. At that time, a student was expected to complete his Ph.D. at Berkeley in three years while at Harvard it took many students five or even more years. Compared with the laid-back Berkeley graduate students of my day, Harvard students seemed intense and often eccentric. The big exception was [Dudley Herschbach](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1986/index.html), who was modest, relaxed, and friendly, and the most brilliant intellect I had encountered in someone my own age.  In those days faculty hiring was done with few formalities. Somewhat out-of- the-blue, I got an offer to come back to Rice as an Assistant Professor. The prospect of returning to a warm climate and familiar surroundings full of many happy memories was delightful and with no negotiations I happily accepted.  I inherited George Bird’s graduate students and his microwave spectrometer, which was more sensitive than Wilson’s. Of these two strokes of good luck, Bird’s students proved the greater treasure. My very first student was Jim Kinsey. He accomplished so much in the first year that I was at Rice that he graduated. The work we did together on the microwave spectrum of ClO2 and the treatment of fine and hyperfine structure set me up for a productive period of studying the spectra of stable free radicals.  I have remained at Rice from 1958 until today. In my professional and research career, I have played a variety of roles and worked in several areas of Physical Chemistry, too varied to describe further. A great deal of my research has been collaborative involving other principals both at Rice and elsewhere. I have enjoyed quite a few very pleasant research associations over the years. Outside Rice I have collaborated with C.A. Coulson, Roger Kewley, Takeshi Oka, Ken Evenson, John Brown, Eizi Hirota, Shuji Saito, Anthony Merer, Wolfgang Urban, Harry Kroto and Leon Phillips. Among the Rice Faculty, I have enjoyed collaborations with John Kilpatrick, Frank Tittel (for the last 25 years), Phil Brooks, Rick Smalley, Graham Glass and Bruce Weisman. The Nobel Prize in Chemistry was awarded to Rick Smalley, Harry Kroto, and myself for the fruits of one of these collaborations, the discovery of the fullerenes.  I must point out that we do not claim this discovery is ours alone. James Heath and Sean O’Brien, who were graduate students at the time, have equal claim to this discovery. Both Jim and Sean were equal participants in the scientific discussions that directed the course of this work and actually did most of the experiments. The early experiments that Sean and Jim did not do were carried out by Yuan Liu and Qing-Ling Zhang. At an early stage, Frank Tittel became involved in this work. At a later stage, F.D. Weiss and J.L. Elkind did the shrink wrap experiments, which were among the strongest evidence for the fullerene hypothesis. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q2 | Professor Curl, I believe I read something about you, that you got your first chemistry box when you were quite a small child? Was that how it all started? |
|  | Robert F. Curl Jr.: That’s right.  Robert F. Curl Jr.: Yes, it really is, I got this chemistry set I think it was for Christmas it’s a little hard to remember for sure. I had a little room over the garage that I could play with it in and essentially what I did was all of the little experiments that they suggested in the booklet that came with it and I hadn’t exhausted possibility for mixing up chemicals so I tried mixing every possible combination to see what happened. And I became fascinated with chemistry and decided essentially then to become a chemist. |
| Q12 | You didn’t scare your parents, did anything that sort of upset them? |
|  | Robert F. Curl Jr.: I did upset my mother on one occasion, this was much later when I was in high school, I was doing some experiment on her stove and boiled over some nitric acid and ate the enamel off the stove and did bother her quite a bit. In fact, she talked about it for years. |
| Q12 | Is that something, if you have children in your family or grandchildren or so on, do you encourage that kind of you know experimenting and being so curious? |
|  | Robert F. Curl Jr.: I encouraged … I’m beginning to despair because neither one of my two sons who have gone on to be quite, you know, they’re adults and actually middle aged now and been quite successful, are not at all interested in that sort of thing. I would buy a little kit to do electronics or something and they would not be very interested doing any of the little experiments |
| Q9 | Not even after you got a Nobel Prize? |
|  | Robert F. Curl Jr.: They were of course adult by that time. My grandchildren have not been particularly interested so far. I have one yet coming that’s not yet five that may turn out to might be interested in doing something but no, it hadn’t happened. |
| Q15 | Did you think that there was a possibility for you to get the prize when you discovered what you discovered together with your colleagues and put them together and started to work together? |
|  | Robert F. Curl Jr.: No, I always have judged the interest in things on the attitude of organic chemists. They’re, in the United States at least, the organic chemists are the ones who create the atmosphere for chemistry. And they … I remember in the years following doing this experiment that I would go and give a talk about the work that we had done on carbonate some place and I’d give this talk and people would be interested. They would inevitably at the end of the talk, there would be some organic chemists who would raise his hand and say can I see a sample of this material? And then I would have to explain that no we were only talking about a few thousands molecules in a big machine and that we’ve never actually seen a large sample and they would immediately lose interest.  And so I couldn’t conceive that there would be a prize if the organic chemists were not interested in it. And what changed things was the work of Wolfgang Krätschmer and Donald Huffman who made and developed a method for making microscopic samples of these materials. Then the organic chemists could get their hands on it and it was at that point that I thought there might be a prize for either them or us. |
| Q18 | I was thinking now and when patent is becoming or has been but is becoming more and more important and sometimes it also become a big quarrel between the scientist and maybe the university or the company that you’re working for, when you do your major discovery, how can one get around that or has it badly influenced the social aspect of research do you think? |
|  | Robert F. Curl Jr.: I don’t know that it in terms of academic research I don’t know that it has. For one thing, certainly in the past, most university professors were not working in things that were immediately patentable. That’s changing, people are more interested in doing things that are of practical importance. In my university, occasionally we get into an interesting paradox because by university regulation the PhD final oral examination is public, it has to be publicly announced and anyone can come and for some of us, some of the professors who are working with very commercially interesting things they find themselves in a really serious bind. |
| Q9 | You did say after having received this award that people ask you all sorts of questions and you’re sort of supposed to know all the answers to it, all different issues and what goes on with humankind? |
|  | Robert F. Curl Jr.: You get used to saying I don’t know. I hated that because you know you’re trained to avoid ignorance to try to know things and so to constantly have to say I don’t know is, you know, it’s hard, it takes a while to get used to that. |
| Q9 | Has it changed your working life though, this award? |
|  | Robert F. Curl Jr.: Yes, it changed it both positive and negative ways. The positive way has been that it’s been easier to get money and co-workers I think. The negative way is that you get a lot of e-mails of course not a whole lot but enough to be significant of email correspondence about things that you would really not particularly want get involved with. Things like getting messages from students.  There are two kinds of messages from students, the one kind is the one where the student really seems to be genuinely interested and has some question that shows that they’ve actually thought about something and then there’s the student who has a homework assignment and feels that this would be a good way to get help on their assignment. And so the first kind I enjoy, the second kind you know I don’t want to feel caught on a crux because you don’t want to discourage any serious student, but you don’t see how it actually helps their development for you to supply them with answers to their assignment, so. |
| Q15 | Any specific … do you think that scientists of your calibre, those who have received the award from the Nobel Foundation, have any specific responsibilities? |
|  | Robert F. Curl Jr.: I don’t think we have any more responsibility than the average citizen has, we don’t, you know, we haven’t become brilliant overnight. Getting the Nobel Prize did not increase our intelligence, let’s put it that way. And it certainly didn’t make us experts in all areas. So, it does give you a certain ability to get people to listen. And so, therefore you have a responsibility as any citizen would have if you feel that there is something you have to say that people really need to listen to, then you should definitely go ahead and say it and try to do what you can to either improve things or try and help overt disaster. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0372 |
| **Biographical** | I was the kid with the funny name in my form. That is one of the earliest memories I have of school (except for being forced to finish school dinners). Other kids had typical Lancashire names such as Chadderton, Entwistle, Fairhurst, Higginbottom, Mottershead and Thistlethwaite though I must admit that there were the odd Smith, Jones and Brown. My name at that time was Krotoschiner (my father changed it to Kroto in 1955 so it is now occasionally thought, by some, to be Japanese). I felt as though I must have come from outer space – or maybe they did! I now realise that I had made a continual subconscious effort to blend as best I could into the environment by making my behaviour as identical as possible to that of the other kids. This was not easy indeed it was almost impossible with a couple of somewhat eccentric parents (in particular an extrovertly gregarious mother) who were born in Berlin and came to Britain as refugees in their late 30’s.  Bolton is a once prosperous but then (the fifties) decaying northern English town which is rightfully proud of its legendary contributions to the industrial revolution – the likes of Samuel Crompton and Richard Arkwright were Boltonians. Indeed we lived in Arkwright St. and I shall always remember walking to school each morning past the windows of cotton mills through which I could see the vast rows of massive looms and spinning frames operated by women who had been working from at least six o’clock in the morning, if not earlier.  My efforts to merge into the background meant, among other things such as fighting (literally) for survival, speaking only English (all real Englishmen expect others to speak English) – though I allowed myself to absorb just enough German to understand what my parents were saying about me when they spoke German. One specific memory was that when I did particularly poorly at French one year my Father gave me a very large French dictionary for my birthday – was I pleased!!!  My name seems to have its origins in Silesia where my father’s family originated and there is a town in Poland now called Krotoszyn (then Krotoschin). My father’s family came from Bojanowo and set up a shop in Berlin where my father was born in 1900. The original family house, which was then a shop, still exists in the main square in Bojanowo. I have an old photograph which shows the sign “I. Krotoschiner” in gothic characters emblazened over the window. I visited the town recently and, apart from cars rather than horsedrawn carts and the sign, little has changed – the Hotel Centralny is now the Restauracja Centralny and the aerials on the roofs are still there!  My father, who originally wanted to be a dress designer but somehow ended up running a small business printing faces and other images on toy balloons, had to leave Berlin in 1937 and my mother (who was not Jewish) followed a few months later. I always felt that my parents had a really raw deal, as did almost everyone born in Europe at the turn of the Century. The First World War took place while they were teenagers, then the Depression struck and Hitler came to power while they were young adults. They had to leave their home country and then the Second World War broke out and they had to leave their home again. When my father was 45 he had to find a new profession, when he was 55 he set up his business again and when he was 65 he realised I was not going to take it over. He sold the business and retired in his early 70’s.  I do not know how my father managed to catch the train to take him over the border into Holland in 1937. For as long as I knew him he was always late for everything; he invariably missed every train or bus he was supposed to catch. He told me that this was because he was called up in 1917 to go to the Front but arrived at the station just as the train was pulling out. When he asked the station master what he should do, he was told to go home. From then on he decided to make a point of missing trains and buses, but seems to have made one exception, in 1937. My parents managed to set up their small business again in London but the effort was, of course, short lived due to the outbreak of the War in September 1939. I was born in Wisbech (a very small town in Cambridgeshire to which my mother was evacuated) on Oct 7th 1939 in the first month of the War so I was a war baby. My father was interned on the Isle of Man because he was considered to be an enemy alien; my mother (who was also an alien, but presumably assumed not to be an enemy one) was moved (with me – when I was about one year old) from London to Bolton in 1940. After the war my father became an apprentice engineer and because he was so good with his hands he managed to get a job as a fully qualified toolmaker at an engineering company in months rather than years.  In 1955, with help from friends in England and Germany from before the war, he set up his own small factory again, this time to make balloons as well as print them. I spent much of my school holidays working at the factory. I was called upon to fill in everywhere, from mixing latex dyes to repairing the machinery and replacing workers on the production line. I only now realise what an outstanding training ground this had been for the development of the problem solving skills needed by a research scientist. I am also sure that what I was doing then would contravene present-day health and safety at work regulations. I would have been considered too young and inexperienced to do the sort of maintenance work that I was often called upon to do. I did the stocktaking twice-a-year using a set of old scales with sets of individual gram weights (weighing balloons 10 at-a-time to obtain their average weights), my head, log tables and a slide rule to determine total numbers of various types of balloons. No paradise of microprocessor controlled balances then. After each stocktaking session I invariably felt that I never wanted to see another balloon as long as I lived.  My parents had lost almost everything and we lived in a very poor part of Bolton. However they did everything they could to get me the best education they could. As far as they were concerned this meant getting me into Bolton School, a school with exceptional facilities and teachers. As a consequence of misguided politically motivated educational policies this school has become an independent school and it bothers me that, were I today in the same financial position as my parents had been when I was a child, I would not be able to send my children to this school. Though I did not like exams or homework anymore than other kids, I did like school and spent as much time as I could there. At first I particularly enjoyed art, geography, gymnastics and woodwork. At home I spent much of the time by myself in a large front room which was my private world. As time went by it filled up with junk and in particular I had a Meccano set with which I “played” endlessly. Meccano which was invented by Frank Hornby around 1900, is called Erector Set in the US. New toys (mainly Lego) have led to the extinction of Meccano and this has been a major disaster as far as the education of our young engineers and scientists is concerned. Lego is a technically trivial plaything and kids love it partly because it is so simple and partly because it is seductively coloured. However it is only a toy, whereas Meccano is a real engineering kit and it teaches one skill which I consider to be the most important that anyone can acquire: This is the sensitive touch needed to thread a nut on a bolt and tighten them with a screwdriver and spanner just enough that they stay locked, but not so tightly that the thread is stripped or they cannot be unscrewed. On those occasions (usually during a party at your house) when the handbasin tap is closed so tightly that you cannot turn it back on, you know the last person to use the washroom never had a Meccano set.  At no point do I ever remember taking religion very seriously or even feeling that the biblical stories were any different from fairy stories. Certainly none of it made any sense. By comparison the world in which I lived, though I might not always understand it in all aspects, always made a lot of sense. Nor did it make much sense that my friends were having a good time in a coffee bar on Saturday mornings while I was in schul singing in a language I could not understand. Once while my father and I were fasting, I remember my mother having some warm croissants – and did they smell good! I decided to have one too – ostensibly a heinous crime. I waited for a 10 ton “Monty Python” weight to fall on my head! It didn’t. Some would see this lack of retribution as proof of a merciful God (or that I was not really Jewish because my mother wasn’t), but I drew the logical (Occam’s razor) conclusion that there was “nothing” there. There are serious problems confronting society and a “humanitarian” God would not have allowed the unaccountable atrocities carried out in the name of any philosophy, religious or otherwise, to happen to anyone let alone to his/her/its chosen people. The desperate need we have for such organisations as Amnesty International has become, for me, one of the pieces of incontrovertible evidence that no divine (*mystical*) creator (other than the simple Laws of Nature) exists.  The illogical excuses, involving concepts such as free will(!), convoluted into confusing arguments by clerics and other self-appointed guardians of universal morality, have always seemed to me to be just so much fancy (or actually clumsy) footwork devised to explain why the fascinating and beautifully elegant world I live in operates exactly the way one would expect it to in the absence of a mystical power. Of course the excuses have been honed and polished over millennia to retain a hold over those unwilling or unable to accept that, as a Croatian friend of mine once neatly put it, “When you’ve had it you’ve had it”.  The humanitarian philosophies that have been developed (sometimes under some religious banner and invariably in the face of religious opposition) are human inventions, as the name implies – and our species deserves the credit. I am a devout atheist – nothing else makes any sense to me and I must admit to being bewildered by those, who in the face of what appears so obvious, still believe in a mystical creator. However I can see that the promise of infinite immortality is a more palatable proposition than the absolute certainty of finite mortality which those of us who are subject to free thought (as opposed to free will) have to look forward to and many may not have the strength of character to accept it.  *[After all this, I have ended up a supporter of ideologies which advocate the right of the individual to speak, think and write in freedom and safety (surely the bedrock of a civilised society). I have very serious personal problems when confronted by individuals, organisations and regimes which do not accept that these freedoms are fundamental human rights. I feel one must oppose those who claim that the “good” of the community must come before that of the individual – this claim is invariably used to justify oppression by the state. Furthermore there has never been any consensus on what the “good” of the community actually consists of, whereas for individuals there is little difficulty. Thus I am a supporter of Amnesty International, a humanist and an atheist. I believe in a secular, democratic society in which women and men have total equality, and individuals can pursue their lives as they wish, free of constraints – religious or otherwise. I feel that the difficult ethical and social problems which invariably arise must be solved, as best they can, by discussion and am opposed to the crude simplistic application of dogmatic rules invented in past millennia and ascribed to a plethora of mystical creators – or the latest invention; a single creator masquerading under a plethora of pseudonyms. Organisations which seek political influence by co-ordinated effort disturb me and thus I believe religious and related pressure groups which operate in this way are acting antidemocratically and should play no part in politics. I also have problems with those who preach racist and related ideologies which seem almost indistinguishable from nationalism, patriotism and religious conviction.]*  My art teacher, Mr Higginson, would give me special tuition at lunch times or after school was over. My father made me finish all my homework and I had to stay up until it was not only complete but passed his inspection – midnight if necessary. As time progressed, for reasons which I am not sure I understand, I gravitated towards chemistry, physics and maths (in that order) and these became my specialist subjects in the 6th form. I was keen on sport, and in school I concentrated on gymnastics whilst outside school I played as much tennis as I could. I patterned my backhand (and my haircut) on that of Dick Savitt and my service on that of Neil Fraser. At one time I remember wanting to be Wimbledon champion but decided that this goal was going to be a bit hard to achieve as I seemed to be having too much difficulty winning.  I started to develop an unhealthy interest in chemistry during enjoyable lessons with Dr. Wilf Jary who fascinated me most with his ability, when using a gas blowpipe to melt lead, to blow continuously without apparently stopping to breath in. I, like almost all chemists I know, was also attracted by the smells and bangs that endowed chemistry with that slight but charismatic element of danger which is now banned from the classroom. I agree with those of us who feel that the wimpish chemistry training that schools are now forced to adopt is one possible reason that chemistry is no longer attracting as many talented and adventurous youngsters as it once did. If the decline in hands-on science education is not redressed, I doubt that we shall survive the 21st century. I became ever more fascinated by chemistry – particularly organic chemistry – and was encouraged by the sixth form chemistry teacher (Harry Heaney, now Professor at Loughborough) to go to. Sheffield University because he reckoned it had, at the time, the best chemistry department in the UK (and perhaps anywhere) – a friendly interview with the amazing Tommy Stephens (compared with a most forbidding experience at Nottingham) settled it.  I was born during the war so I just escaped military service. As all the normal places at Oxbridge were already assigned for the next two years to reemerging national servicemen, I needed to achieve scholarship level to get to Cambridge. This turned out to be a bit difficult as I had been assigned a college with an examination syllabus orthogonal to the one that I had studied. Ian McKellen, the actor, who was in the same year at school, only seems to have needed to remember his lines from his part as Henry V in the school play!  The first day that I arrived in Sheffield, I walked past a building which had a nameplate saying it was the Department of Architecture and was bemused – did people do that at University? I had somehow missed this possibility because general careers advice was non-existent at that time. With hindsight I am sure that with the advice available today I would have done something like architecture which would have conflated my art and technology interests. At Sheffield I did as much as I could. Initially I lived with a family in Hillsborough, near to the Sheffield Wednesday football ground and occasionally watched them – very occasionally as I am a Bolton Wanderers supporter. I played as much tennis as I could which helped to get me a room in a hall of residence (Crewe Hall). I played for the university tennis team and we got to the UAU (Universities Athletics Union) final twice – the team would probably have been champions without me – which they were in 1964. I wanted to continue with some form of art, which was really my passion, and became art editor of “Arrows” (the student magazine which we published each term), specialising in designing the magazine’s covers and the screenprinted advertising posters. Whilst a research student I won a *Sunday Times* bookjacket design competition – the first important (national) prize I was to get for a very long time. Later my cover design for the departmental teaching and research brochure *“Chemistry at Sussex”* was featured in “Modern Publicity” (an international annual of the best in professional graphic design) – I consider this to be one of my best publications.  In the 1960s almost everybody could play the guitar well enough to play and sing two or three songs at a party so I had a go at that too and learned just enough chords (about half-a-dozen) to play some simple songs at local student folk clubs. I also decided that I should do some administration in the Students’ Union and from secretary of the tennis team I somehow ended up as President of the Athletics Council. During my last year at University (1963-64) I spent some 2-3 hours of each day attending to administration in the sports office in the Union. That year’s involvement in embryonic politics was enough to last a lifetime. I managed to do enough chemistry in between the tennis, some snooker and football, designing covers and posters for “Arrows”, painting murals as backdrops for balls and trying to play the guitar, to get a first class honours BSc degree (1958-61) and a PhD (1961-64) as well as some job offers. I also got married.  I had been keen on organic chemistry when I arrived at Sussex (at the behest of Harry Heaney I had bought Fieser and Fieser’s Organic Chemistry and read much of it while at school – it was a good read), but as the university course progressed I started to get interested in quantum mechanics and when I was introduced to spectroscopy (by Richard Dixon, who was to become Professor at Bristol) I was hooked. It was fascinating to see spectroscopic band patterns which showed that molecules could count. I had a problem as I really liked organic chemistry (I guess I really liked drawing hexagons) but in the end I decided to do a PhD in the Spectroscopy of Free Radicals produced by Flash Photolysis – with Richard Dixon. George Porter was Professor of Physical Chemistry at that time so there was a lot of flashing going on at Sheffield.  In 1964 I had several job offers but Marg(aret) and I decided that we wanted to live abroad for a while and Richard Dixon had inveigled an attractive offer of a postdoctoral position for me from Don Ramsay at the National Research Council in Ottawa. In 1964 Marg and I left Liverpool, on the Empress of Canada, for Montreal and then went on to Ottawa by train. I arrived at the famous No. 100, Sussex Drive, NRC, Ottawa, where [Gerhard Herzberg](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1971/index.html) (GH) had created the mecca of spectroscopy with his colleagues Alec Douglas, Cec Costain, Don Ramsay, Boris Stoicheff and others. At the time NRC was the only national research facility worldwide that was recognised as a genuine success. I suspect that this was because the legendary Steacie had left researchers to do the science they wanted; now unfortunately – as almost everywhere else – administrators decide what should be done. I remember easily making friends with all the other postdocs who congregated each morning and afternoon in the historical room 1057 – the spectroscopy tea/coffee area. The atmosphere was, in retrospect, quite exhilarating and many there, including: Reg Colin, Cec Costain, Fokke Creutzberg, Alec Douglas, Werner Goetz, Jon Hougen, Takeshi Oka and Jim Watson and their families became our lifelong close friends. As I look back I realise that Cec Costain, Jon Hougen, Takeshi Oka and Jim Watson were to exert enormous direct and indirect influence on my scientific development. I gradually learned to recognise who was good at what and what (if anything) I was good at. To paraphrase Clint Eastwood “A (scientist’s) gotta know his limitations”- and in this somewhat daunting company I learned mine. Although I knew that my level of knowledge and understanding was limited when I arrived, I was never made to feel inferior. This encouraging atmosphere was, in my opinion, the most important quality of the laboratory and permeated down directly from GH, Alec and Cec – it was a fantastic, free environment. The philosophy seemed to be to make state-of-the-art equipment available and let budding young scientists loose to do almost whatever they wanted. Present research funding policies appear to me to be opposed to this type of intellectual environment. I have severe doubts about policies (in the UK and elsewhere) which concentrate on “relevance” and fund only those with foresight when it is obvious that many (including me) haven’t got much. There are as many ways to do science as there are scientists and thus when funds are scarce good scientists have to be supported even if they do not know where their studies are leading. Though it seems obvious (at least to me) that unexpected discoveries must be intrinsically more important than predictable (applied) advances it is now more difficult than ever before to obtain support for more non-strategic research.  In 1965 after a further year of flash photolysis/spectroscopy in Don Ramsay’s laboratory, where I discovered a singlet-singlet electronic transition of the NCN radical and worked on pyridine which turned out to have a nonplanar excited state (still to be fully published!), I transferred to Cec Costain’s laboratory because I had developed a fascination for microwave spectroscopy. There I worked on the rotational spectrum of NCN3. Sometimes Takeshi Oka would be on the next spectrometer-working next to someone with such an exceptional blend of theoretical and experimental expertise did not help to alleviate the occasional sense of inadequacy. I really learned quantum mechanics (as did we all) from an intensive course that Jon Hougen gave at Carleton University. Whenever I was in difficulty theoretically (which was most of the time) Jim Watson helped me out – when he was not busy helping everyone else out. Gradually I realised that many in the field were stronger at physics than chemistry and in retrospect I subconsciously recognised that there might be a niche for me in spectroscopy research if I could exploit my relatively strong chemistry background.  In 1966, after two years at NRC, John Murrell (who had taught me quantum chemistry at Sheffield) offered me a postdoctoral position at Sussex. We were quite keen to live in the US, however, and I managed to get a postdoctoral position at Bell Labs (Murray Hill) with Yoh Han Pao (later Professor at Case Western) to carry out studies of liquid phase interactions by laser Raman spectroscopy. David Santry (now Professor at McMaster) was also working with Yoh Han at that time and each evening Dave and I carried out CNDO theoretical calculations on the electronic transitions of small molecules and radicals. I learned programming (Fortran) from Dave who threw me in at the deep end by showing me how to modify and correct the programs and then left me to see if I could do it myself.  During the year I received another letter from John Murrell to say that the position that had been available at Sussex the previous year was still available but would not be so for much longer. Thus Marg, Stephen (who had been born in Ottawa) and I came back to the UK- my annual salary dropped from $14000 to 1400 pounds, ouch! Marg had to find part-time employment as soon as possible although pregnant with our second son, David (we were poorer – but we were happier …. ! ! ! ). I was just about to start writing off for some positions back in the US and had just located the address of Buckminster Fuller’s research group (I was interested in the way that predesigned urban sub-structures might be welded into an efficient large urban complex) when John Murrell offered me a permanent lectureship at Sussex which I accepted.  I remember thinking I would give myself five years to make a go of research and teaching and if it was not working out I would re-train to do graphic design (my first love) or go into scientific educational TV (I had had an interview with the BBC before we went to Canada). I started to build up a microwave laboratory to probe unstable molecules and Michael Lappert encouraged me to use his photoelectron spectrometer to carry out work independently.  By 1970 I had carried out research in the electronic spectroscopy of gas phase free radicals and rotational microwave spectroscopy, I had built He-Ne and argon ion lasers to study intermolecular interactions in liquids, carried out theoretical calculations and learned to write programs. At Sussex I carried on liquid phase Raman studies, rebuilt a flash photolysis machine and built a microwave spectrometer and started to do photoelectron spectroscopy. I had applied for a Hewlett Packard microwave spectrometer and SERC, in its infinite wisdom, decided to place the equipment at Reading (where my co-applicant, a theoretician (!), worked) so requiring me and my group (the experimentalists) to travel each month to Reading to make our measurements! However by 1974, after three further attempts to get my own spectrometer (with help in consolidating my proposal from David Whiffen), the SERC finally gave in and I got one of my own at Sussex. The first molecule we studied was the carbon chain species HC5N – to which the start of my role in the discovery of C60 can be traced directly.  The discovery of C60 in 1985 caused me to shelve my dream of setting up a studio specialising in scientific graphic design (I had been doing graphics semiprofessionally for years and it was clear that the computer was starting to develop real potential as an artistically creative device). That was the downside of our discovery. I decided to probe the consequences of the C60 concept. In 1990 when the material was finally extracted by Krätschmer, Lamb, Fostiropoulos and Huffman, I and my colleagues Roger Taylor and David Walton, decided to exploit the synthetic chemistry and materials science implications. I began to realise that I might never fulfill my graphics aspirations. In 1991 I was fortunate enough to be awarded a Royal Society Research Professorship which enables me to concentrate on research by allowing me to do essentially no teaching. However I like teaching so I continue to do some. I have discovered that since I stopped teaching 1st and 2nd-year students, home-grown graduate students are few and far between.  In 1995, together with Patrick Reams a BBC producer, I inaugurated the Vega Science Trust to create science films of sufficiently high quality for network television broadcast (BBC2 and BBC Prime). Our films not only reflect the excitement of scientific discovery but also the intrinsic concepts and principles without which fundamental understanding is impossible. The Trust also seeks to preserve our scientific cultural heritage by recording scientists who have not only made outstanding contributions but also are outstanding communicators. The trust, whose activities are coordinated by Gill Watson, has now made some 20 films of Royal Institution (London) Discourses archival programmes and interviews.  I have been asked many questions about our Nobel Prize and have many conflicting thoughts about it. I have particular regrets about the fact that the contributions of our student co-workers Jim Heath, and Sean O’Brien as well as Yuan Liu receive such disparate recognition relative to that accorded to ours (e.g. Bob, Rick and me). I also have regrets with regard to the general recognition accorded to the amazing breakthrough that Wolfgang Krätschmer and Don Huffman made with their students Kostas Fostiropoulos and Lowell Lamb in extracting C60 using the carbon arc technique and which did so much to ignite the explosive growth of Fullerene Science. I have heard some scientists say that young scientists need prizes such as the Nobel Prize as an incentive. Maybe some do, but I don’t. I never dreamed of winning the Nobel Prize – indeed I was very happy with my scientific work prior to the discovery of C60 in 1985. The creation of the first molecules with carbon/phosphorus double bonds and the discovery of the carbon chains in space seemed (to me) like nice contributions and even if I did not do anything else as significant I would have felt quite successful as a scientist. A youngster recently asked what advice I would give to a child who wanted to be where I am now. One thing I would not advise is to do science with the aim of winning any prizes let alone the Nobel Prize that seems like a recipe for eventual disillusionment for a lot of people. [Over the years I have given many lectures for public understanding of science and some of my greatest satisfaction has come in conversations with school children, teachers, lay people, retired research workers who have often exhibited a fascination for science as a cultural activity and a deep and understanding of the way nature works.] I believe competition is to be avoided as much as possible. In fact this view applies to any interest – I thus have a problem with sport which is inherently competitive. My advice is to do something which interests you or which you enjoy (though I am not sure about the definition of enjoyment) and do it to the absolute best of your ability. If it interests you, however mundane it might seem on the surface, still explore it because something unexpected often turns up just when you least expect it. With this recipe, whatever your limitations, you will almost certainly still do better than anyone else. Having chosen something worth doing, *never give up and try not to let anyone down*.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1996*, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1997  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 1996 **Addendum, July 2012** Introduction After the “eventful” two week period in September 1985 at Rice my whole research strategy changed essentially overnight. Instead of spending my weekends on graphics as I had always intended, I started working even harder on science than before. At my request Rick agreed that I must come back to work with his group to try to prove our conjecture. In the event I returned some nine or ten times over the next 1½ year period from September 1985 to April 1987, each time for a period of 2-3 weeks. The original structural conjecture was probed exhaustively during this period by joint Rice/Sussex experiments, by the independent Rice studies and also independent experimental and theoretical work by our group at Sussex.  Scientific Attitudes It is important to realize that there are occasional moments in the life of a scientist when one has to be bold and I and the Rice team were conscious that this was one of those moments. We had proposed a possible structure to explain our discovery of a stable molecule with sixty carbon atoms but really had only this number to go on – and our intuition. I had the strong gut feeling that it was so beautiful a solution that it just had to be right. I do not remember during this early period thinking it could be wrong. I am sure that the other members of the team who had also lived through the exciting period of discovery had the same feeling. I decided however that I certainly must be ethical about this. I had a strong desire to work as hard as I could to prove the conjecture was right, but more importantly if it were not correct I definitely wanted to falsify the conjecture myself – I really did not want anyone else to prove that we were wrong. During the five-year period 1985, when we discovered C60, and 1990 when the Krätschmer and Huffman team extracted it, I worked with the Rice team, the Rice team worked independently and we worked independently at Sussex to assemble as much experimental and theoretical evidence as possible for the veracity of our original structural proposal. Indeed at Sussex we were only just pipped-at-the-post in confirming the structure unequivocally by the beautiful paper of Krätschmer and colleagues.  My attitude over this was strongly coloured by some earlier interactions that I had had with (Sir) Fred Hoyle over his claim that he had found evidence for bacteria in interstellar space. In earlier times Hoyle had been a well-known and well-respected scientist, especially in the UK, and had often been on radio and TV pontificating on scientific and other issues. It has been argued that his original prediction of the reasons that there is enough carbon in the Universe for Life to exist was worthy of a Nobel Prize – arguably (and I would argue it) the only so-called “Anthropic” conjecture of any value whatsoever. However as time evolved he had, over many years, published some highly contentious conjectures which had received widespread publicity because of his reputation based on the excellent early work on the synthesis of the chemical elements in stars by nuclear fusion.  At least three of his conjectures were surprising, to say the least, and ran counter to arguments based on highly reliable scientific understanding. One of his proposals was that the archaeopteryx fossils were fakes and a second was that some epidemics were caused by bacteria which had been injected into the Earth’s atmosphere from outer space. Both proposals were strongly criticised by leading paleontologists and epidemiologists respectively. I had noted this from the sidelines and not taken a particularly strong interest in Hoyle’s odd proposals until he ventured into my own field, spectroscopy, and published a claim, with a colleague Chandra Wickramasinghe who was a professor of mathematics, that a single broad and almost featureless infrared band was evidence for bacteria in space! It seemed to me at the time that he was trying to find evidence to support the fundamental idea behind a very popular science fiction book he had written many years before entitled “The Black Cloud” which I had read as a student and enjoyed. I studied the claim which was also published in a book entitled “Proofs that Life is Cosmic” carefully and found that it was based on fit between a bone fide astrophysical observation, which consisted of a stellar spectrum published with a linear wavelength scale, and Hoyle’s laboratory data on freeze dried bacteria with an original scale linear in cm-1. In correlating the two spectra, which differed mainly in the fact that wavelength is proportional to 1/cm-1, an error had somehow occurred. Furthermore the error bars on the astrophysical data indicated that the fit in the comparison plot was some 1000 or more times better than could be expected, even if bacteria were responsible for the astrophysically observed data! When I carried out the comparison analysis I found the fit was statistical and that comparison only supported the existence of some mix of C/N/O/H-containing species, as conventional wisdom based on copious radioastronomical data suggested. The errors unequivocally falsified the claim and my attempts to publish my findings were not accepted for publication and curiously vanished.  At some stage Hoyle and co-workers presented their “Bacteria in Space” claim at a Royal Society meeting on Halley’s Comet and in the ensuing discussion after I presented my analysis of their data Hoyle suggested that the shift that I and others had found must have been due to a “Draughtsman’s Error”! The organisers of this meeting refused to include this part of the discussion in the Proceedings Volume of the meeting. I felt this was not right and could only help to propagate unproven claims in the popular press as having scientific validity when they had not. I feel this sort of thing is starting to be a serious general problem at the interface between the scientific community and society as the pressure to justify scientific results and funding by highlighting results with “hyperflated” application claims in radio and TV science programmes, magazines, newspaper articles, interviews and research reports becomes more and more common. I must point out that I did not criticise the concept of bacteria in space, I criticised only the claim that there was evidence to support the claim. From then on I felt that if one ventured a hypothesis, one was bound by ethical principles, as a scientist, to do everything possible to prove or disprove the hypothesis oneself, and not suggest falsification is for the critics, especially in the case of highly contentious proposals. Of course this is a much more general issue as scientists are in the vanguard of the champions of natural philosophy who must face the onslaught of the purveyors of mystical concepts who claim revelation as the basis of truth.  The 4 out of 5 Rule Days A few days after the C60 discovery paper was sent off to *Nature*, the first experiments yielding confirmatory evidence started to arrive. Within another two weeks Martyn Poliakoff sent me an article by David Jones in which I found some simple and highly convincing theoretical supporting evidence for the cage structure. Very soon, and even before the paper was published, we had enough evidence to consider the structure not just plausible but, using one of Rick’s favourite adjectives, “compelling”. Within a few months we had assembled sufficient circumstantial, experimental and theoretical, evidence to indicate that we must be correct and our structure became highly convincing to any scientists “disinterested” enough to carefully scrutinise all our evidence. A number of groups did not seem to fit into the “disinterested” category and published papers suggesting that not only was our structural conjecture wrong but even that our experiment was in error – in particular that our result that C60 was special was an experimental artefact.  My view was that if C60 were not a cage then the conjecture would have fallen at the first (conjectural and/or experimental) hurdle. During this period I developed what I call my “4 out of 5 rule”:  If one makes a new observation, then develop a hypothesis to explain it. Then carry out several further experiments – five would be a good number – to check it out. If 4-out-of-5 confirm your hypothesis then you are *almost* certainly right; if only 1-out-of-5 fits, you are *almost* certainly wrong – in both cases the accent is on *almost.*  In fact statistical analysis suggests that if only one experiment doesn’t fit, there is a *ca* 99.98% probability that you are correct. Within about a year the amount of evidence that indicated we were right was overwhelming, at least to the discovery team whose reputation depended on it, and also to many other groups who contributed supporting theory and measurements. Contrary to the claims made by some, our proposal of the Buckminsterfullerene structure was fully justified. I am sure that anyone who had as we had, carried out such an exhaustive set of exciting experiments and then alighted on, to our complete amazement, the soccer ball structure as a possibility, would also have been similarly bowled over by the idea and proposed it as a possibility in the original paper. Had any of our numerous studies either experimental or theoretical, during the next five years, falsified the conjecture, we would have withdrawn it – all in fact supported the proposed structure. Perhaps one might argue that the title of our paper, “C60: Buckminsterfullerene”, was a bold act, if so I take full responsibility.  Experiments at Sussex between Sept 1985 and Sept 1990 Based on the Work of Hintenberger et al Several interesting and important developments took place at the University of Sussex between September 1985, when C60 was discovered with the Rice Group, and September 1990 when the brilliant paper on its extraction was submitted to *Nature* by Wolfgang Krätschmer, Lowell Lamb, Kostas Fostiropoulos and Donald Huffman. During this period a parallel series of experiments to those of Krätschmer et al was carried out at Sussex.  A key reason for carrying out the experiment at Rice in the first place was an intriguing set of results obtained by Hintenberger and colleagues between 1958 and 1963 that showed, by mass spectrometry, that carbon species with as many as 33 carbon atoms were produced in a carbon arc discharge. At Sussex, after the initial C60 discovery in 1985, I had a hole drilled in an old carbon-arc evapourator we had, so that we could deposit carbon on a silica wafer at various argon pressures. The idea was to follow up the Hintenberger et al experiments by recreating roughly the same conditions, that we had achieved with the Rice nozzle as cheaply, as simply as possible with an electric arc discharge. At this point I conjectured that as the argon pressure was increased we might be able to use the electron microscope that was available at Sussex to see the formation of roundish carbon particles which I conjectured might provide some circumstantial evidence for C60 formation. I thought that the assembly processes that created C60 might also lead to the formation of large spheroidal soot-like carbon particles. What we found was that the smooth carbon coating obtained under very low pressure changed, more-or-less suddenly, at ca 70-80 µm pressure of argon creating an undulating blistered rough surface of the kind I vaguely expected.  This observation was encouraging as it seemed to be some sort of confirmation the idea might be valid and that C60 might be forming. Here I made a fundamental mistake – and not for the first time! I assumed that C60 would only be formed in minuscule amounts and only detectable, if at all, by the most sensitive analytical technique available *i.e.* mass spectrometry. After all, how could C60 be easily made when it had avoided detection until nearly the end of the 20th century, and then only fleetingly, when its two more famous siblings, diamond and graphite, had been known since time immemorial. It is now hard, more than twenty five years later, when C60 is in every school science textbooks to realize that C60 was, prior to 1990, considered by some to be highly suspicious character and indeed by some (see above) even an imposter. Indeed some still claim to this day that we had no right to make the claims contained in the *Nature* paper (see “Candid Science” by Istvan Hargittai).  A Funding Problem During this period together with my Sussex colleague Geoff Cloke, an expert in metal vapour deposition, I tried to obtain funds for an in-situ quadrupole mass spectrometer to monitor the electric discharge process directly and see if C60 could be detected. Having already obtained significant support from EPSRC to build a Rice-type cluster beam system with another Sussex colleague Tony Stace, I had to go elsewhere for the £12k I needed for the mass spectrometer. (NB: I had by the way tried to get Rick to buy one to monitor alternative possible C60 creation experiments that Jim Heath and I had been probing – but to no avail). In the event this modest proposal was turned down by Shell, BP and also the Royal Society. All indicated it was an interesting proposal but none was prepared to cough up £12k.  The Royal Society committee was so impressed it suggested we apply to EPSRC! We did have some funding from “British Gas” that a former student Steve Wood had managed to obtain for us to probe the idea I had that C60 must be a key constituent in a sooting flame. It is interesting to point out in this context that Mitsubishi now makes C60 in bulk quantities commercially by combustion of methane! Our small group had a lot of work on its hands! Unfortunately we did not get the quadruple MS which had unfortunate consequences for us at Sussex.  Krätschmer et al Enter the Scene Then at some point a photocopy of a conference presentation abstract was sent to me by the astrophysicist Michael Jura a friend and colleague at UCLA. Michael had been to a conference in Capri where Wolfgang Krätschmer had presented a paper in which he and his colleagues presented intriguing evidence that they had detected four vibrational infrared bands of C60. At the top of the copy Mike had written, in his inimitable scrawl, “Harry, do you believe this?”  I must admit I found it very hard to believe. If Krätschmer and colleagues were correct I had “screwed up big time”. Instead of the minute amounts, needing the mass spectrometric sensitivity, that I had assumed were being formed in our evaporator, we must have already been making samples in which ca 1% of the deposit was C60; enough to detect by infrared spectroscopy! At just this moment, as luck would have it, Jonathan Hare was working for a DPhil with me. He had come to work on astrophysically related experimental problems. We immediately wheeled out the old modified evapourator and Jon started to make carbon films in an attempt to repeat the Krätschmer-Huffman experiments. On 22nd November 1989 we saw the first IR spectrum of C60 in his films at Sussex. Unfortunately then Jon had to spend time rebuilding the ancient apparatus. By the 5th March with UG project student Amit Sarkar he had worked out how to reproduce the IR spectrum reliably and we realized we must have some C­60 in our hands!  Then Jon wrote to Krätschmer to tell him that we had reproduced his results. I felt we were honour-bound that Wolfgang should be made aware that we were working on the problem. Although we had gone back to it because of their results it did not seem unethical as I had already been exploring this avenue and had been thwarted by being turned down for funding as indicated above and furthermore they had published their preliminary observations.  Fleeting Sightings of C60, the Orson Welles Character of the Third Form of Carbon Story The dream I had always had was to prove our C60 conjecture by detecting the single 13C line NMR spectrum that C60 should exhibit as all sixty carbon atoms are equivalent. I had a quite a consistent track record in one-line assignments: In the 1970’s we had identified CH2=PH on the basis of one microwave line, then also HC5N, HC7N and HC9N all on the basis of single radio lines and of course C60 on the basis of one mass spectrometric line. I understand that these breakthroughs had led to my being called “One-Line Kroto” by the Monash microwave group! I took it as a complement, but I am not sure that that was the way it was meant! In July 1990 Jon gave a sample to Alla’a Abdul-Sada to check the mass spectrum and he obtained a 720 mass signal so we knew that we were on the right track. In discussion with Jon my thought was that as C60 looked like benzene from 20 angles (with its 20 hexagons) maybe it would be soluble in this solvent – not thinking of course that maybe the benzene line might overlap the C60 signal!  In the event one Monday morning (6th August 1990) Jon placed a small phial containing benzene in which his soot sample had been washed. It was a deep burgundy red.  I was apprehensive and wondered whether a suspension of essentially invisible tiny microscopic particles might scatter and give the appearance of a red solution. On the following Thursday (9th) we tried to obtain a mass spectrum of the extract but our sampling procedure needed to be refined.  Black (and Red!) Friday The next day – Friday August 10th, (Black Friday) I had a call from *Nature* – that’s the journal – Philip Ball asked me if I would referee a paper by Wolfgang Krätschmer and colleagues on C60. Without really thinking I said of course I would as I felt I was as expert as anybody else on this issue. One never realizes that a hurricane is coming: A fax arrived at 12.10 and as I read the title “Fullerite, a New Form of Carbon” my heart sank, then as I read further down the abstract – it got worse – I saw the words a *“wine red solution”* glaring at me from the fax and there in front of me on my desk was the Jon’s phial with the wine-red solution staring me in the face. They also had a fantastic photograph of C60 crystals together with some all-important X-ray data that showed that they had obtained crystals consisting 1nm diameter spheroidal molecules – it was all totally convincing. I knew instinctively that it was correct.  I wondered whether to commit suicide or go for lunch. What the hell – as any student knows – there is not a lot of difference between lunch in a university canteen and suicide – so I went for lunch. After lunch on returning to my office I called Philip at *Nature* to tell him that this was proof positive that they definitely had C60 and asked him to call Krätschmer and say I waived anonymity and congratulate him and his colleagues. Philip asked who else did I recommend as a referee and I suggested that Bob Curl, the Rice Group’s consilieri, would be the best. I consider this paper one of classic chemistry papers of the 20th century in that they had conjectured they might have C60 from earlier electronic spectroscopy studies of carbon particles and then had used the four infrared vibrational modes that Group Theory indicated would be fingerprint bands as the key step in tracking it down. I think their study should be used in all chemistry courses as an iconic example of the way Group Theory can be a powerful tool in science – indeed I doubt there has ever been a more important or perfect example. One might think that the Group Theoretical derivation would be difficult but it turns out to be fairly straight-forward as almost all terms cancel out.  The Single NMR Line Anyway, what to do now – if anything? We had been so close and I felt that we had really been thwarted by the funding system. It would have certainly been unfair to Krätschmer and colleagues had we won this race but I felt it had certainly been unfair to us too in the circumstances of not getting the support I needed to probe the electric arc avenue. However, as I carefully re-read the paper I thought about the fact that there was no mass spectrum in the manuscript and we had a 720 signal and in particular there was no NMR line – my dream-line! I subsequently learned that Krätschmer et al did have the crucial mass spectrometric data but there had been some understandable problems associated with presenting it. My friend and former Sussex colleague Ken Seddon had encouraged me long before to just go all out for the nmr line – if only I had heeded his advice! In the event after all the trials and tribulations, especially failing to get the financial support I needed for essentially the same experiment as Krätschmer et al., I felt that we were justified in continuing. I decided we must drive on to obtain my coveted NMR line – after all coming in second to Krätschmer et al’s brilliant work was not that bad especially as Jon had made the most important breakthrough ever made in my laboratory by extracting C60, one week prior to the arrival of that fateful fax from *Nature*.  One Line to Prove it All beyond Reasonable Doubt Jon gave all of his precious sample to my Sussex colleague Roger Taylor who, with the help of Jim Hanson, developed the chromatographic technique that is now the standard procedure for separating members of the fullerene family. Roger found that Jon’s red solution contained at least two molecules; C60 and C70. The sample was red because of C70, though present at significantly lower concentration, had a stronger spectrum and its colour masked the stunningly beautiful delicate magenta of C60.  The precious single line nmr spectrum of C60 and the confirmatory five lines of C70 were detected by Tony Avent – who should have been a co-author of the resulting paper.  In our preliminary manuscript one of C70‘s five lines had not been identified correctly as it lay very close to the benzene line; this was however corrected in the final manuscript that was published.  The Third Key Paper in the Buckyball Saga As “luck” would have it at just this moment I happened to be travelling to a conference in Freiburg and then was to go on to another conference on Brioni. With the preliminary manuscript in my hand, I felt I must stop off in Heidelberg to see Wolfgang Krätschmer and show him the manuscript as I wanted to make sure that he was comfortable with the wording in our manuscript. After all the trials and tribulations I of course wanted to claim as much credit as possible for our Sussex team without taking anything away from the Krätschmer-Huffman work: In particular we had obtained a 720 mass signal and solvent extracted C60 independently prior to the arrival of the heartbreaking fax. Furthermore we had chromatographically separated C60 and C70 and confirmed both the structures by NMR. Wolfgang was most gracious and said he was happy with the manuscript. In the event our preliminary unpublished manuscript, which gave full details of how to separate C60 and C70, as well as nmr data on C60 and our preliminary data on C70 propagated like wildfire. It found its way into the hands of others such as Robert Whetton and Francois Diederich, both of whom had in earlier times been critical of our Buckminsterfullerene structure proposal. They followed our recipe and, not surprisingly, confirmed our results.  The Aftermath … 1990 onwards After the Krätschmer-Huffman breakthrough I decided to delay my aim of doing graphics more seriously and spend about five years researching the implications of the discovery. With Roger Taylor and David Walton I set up an intensive research initiative, the Sussex Nanoscience and Nanotechnology Centre, and proceeded to explore the chemistry and chemical physics of the Fullerenes. Of course as is well known Sumio Iijima explored the material produced in the K-H soot generator and found nanotubes were being created. These structures had been observed by Morinobu Endo and coworkers several years previously in 1976 but here suddenly was a way to make them in sufficient quantities to study them in detail. The Fullerene breakthrough had not only opened up a whole new area of chemistry, presently averaging ca 1000 papers per year, but a vast new area of nanoscience and nanotechnology as the nanotubes turned out to have fascinating electrical and mechanical properties promising new materials with exceptional strength and outstanding electromagnetic behaviour. At Sussex we played quite an important part as our group succeeded in making important contributions to the use of C60 and C70 as synthons. We also explored the ramifications of the nanotube breakthrough and made contributions to our understanding of how they were formed, especially in the presence of a catalyst and in condensed phase.  At the beginning of 1996 much to my total amazement I was offered a knighthood which I duly accepted and later that year in October it was announced that Bob Curl, Rick Smalley and I were to be awarded the Nobel Prize. From then onwards many things changed. I had always been heavily involved in educational initiatives but now the knighthood and the prize made it a bit easier to get the funding needed to explore the way new educational technologies involving the Internet might improve the general understanding of science. It also gave me an opportunity to represent the views of many in the scientific community more widely. I set up the Vega Science Trust which created science programmes – at first for TV and then to stream on the internet at [www.vega.org.uk](http://www.vega.org.uk/). Numerous great programmes can be found there including numerous interviews with Nobel Laureates.  In 2004 I retired from my position at the University of Sussex and took up a position at Florida State University. This was something that I had never thought about but of course one seldom makes relatively momentous decisions such as emigrating to another country unless one has to. In this case FSU was not only keen for me to continue research but also to explore new ways of using the Internet for educational outreach. This led to the creation of the GEOSET project ([www.geoset.info](http://www.geoset.info/) and [www.geoset.fsu.edu](http://www.geoset.fsu.edu/)) which aims to create a globally distributed cache of educational material accessible free worldwide and created by the best teachers on the planet. As this project started, a wonderful bonus surfaced; this was the fact that our students are a great source of imaginative educational material. Not only that, their presentations have become part of their resumés and in particular the use of the URLs of their presentations, when inserted into references and applications, ensure that their individual abilities in presentation and what they find interesting and how they think become much more transparent than is possible when reading through a pile of arid paperwork. I suspect that these sorts of presentations will soon become “de rigeur” requirements – even just to make the shortlist for jobs, fellowships, awards and scholarships.  Ever since I had carried out radioastronomy research in the mid 1970s and had started to give relatively popular general lectures on astrophysical chemistry I had found that I had received quite a lot of invitations to lecture around the world. The conflation of astronomy with chemistry turns out to be an excellent recipe for teaching chemical physics in particular my research speciality spectroscopy. After the prize in 1996 the number of invitations multiplied until now they arrive at a rate of almost one a day. Particularly important are the Lindau Nobel Symposia where I always go when invited as I feel that many of the young people there will in the future attain positions of significant social responsibility and I always aim in some part of my presentations to make the audience think!  In general we try to accommodate as many student events as possible as I think it is important that young people realize that Nobel Laureates are no different from other people and in general no smarter and Lindau is one of the best places for this.  The response of young people in India, China, Japan and Korea when a Nobel Laureate is to give a lecture is often phenomenal and certainly should be a lesson to the West. At one venue in China the students stood 5 abreast all the way down the aisles of the lecture theatre during the whole presentation and some told me they had arrived at 7 am to get a seat for my lecture at 10 am! I also present Buckyball Workshops for very young children. Earlier ones were carried at British Association meetings in the UK with Jon Hare but many have been held all over the world: Florida, Texas, California, Sweden, Malaysia, India, Japan, China and even by Internet to Iceland and to 2000 kids across the whole of Australia.  I decided to do as many lectures as possible especially for schools as I gradually have felt it necessary to communicate with a significant group of young people who, on arriving at our Universities – which I consider oases of intelligence in a sea of ignorance – develop an astute analytical approach to all aspects of life. [Richard Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/) in his small and interesting book “The Meaning of it All” discusses this group of students. When I first read this chapter I did not think it was as large a number as Feynman suggested, but latterly I have found it to be very large. Especially in my general science lectures I highlight the fact that Natural Philosophy (the basic cultural concept that subsumes science) “is the only philosophical construct we have devised to determine truth with any degree of reliability”. I point out that the ethical purpose of education must be the schooling of young people in the ways of deciding what they are being told or what they believe is actually true. Without knowledge-based on evidence, anything goes. Indeed almost anything does go and as [Bertrand Russell](https://www.nobelprize.org/nobel_prizes/literature/laureates/1950/) says “man is a credulous creature and without good reason to believe he is satisfied with bad” In fact I would suggest man is highly susceptible to being convinced that comforting mystical concepts, for which there is no adequate foundation, are true – even though a moment’s rational deliberation indicates that they must be palpably false. As President Kennedy once said: “The great enemy of the truth is very often not the lie, deliberate, contrived and dishonest, but the myth, persistent, persuasive, and unrealistic. Belief in myths allows the comfort of opinion without the discomfort of thought.”  The complexity of living 9 months in the US and the 3 summer months based in the UK together with the feeling that I should speak to this constituency of young people has made life so complicated that my wife Margaret has shouldered the arduous burden of managing the logistical issues as well as the day-to-day problems of survival. Hardly a week goes past when we do not have to travel to a venue somewhere in the world. For several years now I have averaged some 70-80 lectures per year away from our home town and often in another country. I try to go to as many student events as possible because I feel able to give a measure of support to many students disconcerted by the way that analytical thought undermines the unsubstantiated, and unsubstantiatable, mystical dogmas that many have been brought up to accept before they have developed the analytical skills to ask questions about their veracity. As Abelard said “By doubting we come to enquire and by enquiry we arrive at truth”. There is almost no widespread infrastructure available for freethinking young person commensurate with the plethora of churches, mosques synagogues, temples and shrines populated by the purveyors of mystical dogma. When disconcerting questions arise as they do quite naturally in the doubting mind it may cause complex problems both at an intellectual level for sensitive students and also on a day-to-day personal level especially within families for whom mystical issues may be very important.  I still find a bit of time to do what I feel most comfortable and able to do which is art and graphics – not as much as I would like and I really only have time to do the odd poster and logo when commissioned such as these recent ones for the Alliance Française Tallahassee, the Kroto Research Institute in Sheffield and an Internet Buckyball Workshop for 2000 small children across the whole Continent of Australia – earlier ones are at [www.kroto.info](http://www.kroto.info/).  Our younger son David created the characters of the little boy Benjy and his dog Bruno, my wife Margaret and our older son Stephen refined the storyline and I pulled the graphics together for publishing. The little boy and his dog become smaller by a factor of ten every time they encounter an object or animal that has a geodesic polyhedral structure in which pentagonal and hexagonal domains are involved such as in the case of a soccer ball, the eyes of a fly, viruses etc. They finally become so small that they end up swimming along the veins of one of Benji’s friends. It is a book which attempts to give small children an idea of the scale in a similar way to various “Powers of Ten” efforts. It is to appear in English in due course.  At FSU where I have been since 2004-2005 I have been able to carry out interesting research in metal organic framework (MOF) materials and cluster science. I have also been able to plant GEOSET seeds in several institutions around the world. The gateway site is at [www.geoset.info](http://www.geoset.info/). Fortunately my new colleagues at FSU, in particular Naresh Dalal and Alan Marshall, have been great co-workers and Tony Cheetham now at Cambridge has also helped me in the daunting task of starting research off again. It is pretty difficult getting a research programme up-and-running the first time when one is young, but doing it a second time from scratch when you have the knowledge of how difficult it was the first time, makes it seem twice as hard.  *Sir Harold Kroto died on 30 April 2016.* |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q2 | So your basic degree was in chemistry and then you specialised in spectroscopy after that? |
|  | Sir Harold Kroto: My best subjects at school were geography and art and then sciences. We weren’t encouraged to consider art as a possible career. We were encouraged to consider going to Oxford and Cambridge, to do Latin or something like that. I went to quite a good school. It was more easy in those days if you passed exams to go to certain schools. This school is now really more of a public school or what you call in the States a private school. I was able to go to this rather good school called Bolton School. I got a few little scholarships worth eight pounds a month or something like that to help. But I was good at chemistry, and gradually … |
| Q2 | Microwave spectroscopy became your speciality at some point? |
|  | Sir Harold Kroto: Yes. What happened at university is … I was introduced to quantum mechanics and the experimental data that underpinned quantum mechanics is spectroscopy and I was in chemistry and I was taught spectroscopy by Richard Dixon as an undergraduate. And I somehow got quite fascinated by it. The fact that a molecule could count, you know there were regular series, beautiful patterns which I got interested in. Although I was very interested in organic chemistry at that time. I thought … When I wanted to do my PhD I’d like to do spectroscopy. I was in Sheffield where George Porter was, who got the Nobel Prize for flash photolysis, and so it was very exciting time because George had set up all this flash photolysis apparatus. There was that apparatus that belonged to him and Richard Dixon and I worked for Richard but I saw George all the time. He and Richard were quite influential in this.  I enjoyed it. I could play tennis and I could do graphics for the university magazine. I could play the guitar because in those days all the kids at university could play the guitar and sing in folk groups. I was having a good time. and then went to Ottawa to do more flash photolysis. Electronic spectroscopy. After one year in one lab I moved to work with Cec Costain, to do microwave spectroscopy, which is actually a lot simpler than the others. Electronic spectroscopy you have electronic motion, you have vibrational motio,n but in rotational you just have the rotation. So in a sense it’s simpler but more precise. And I was a chemist. So you could use microwave spectroscopy to study moderately complex molecules. I started to realise that it would allow me to do chemistry and identify the molecules and study them by what we call microwave spectroscopy. |
| Q10 | I read here that in the mid 60s you went to the University of Sussex. Was that for your further academic career? |
|  | Sir Harold Kroto: Yes, because I went to do a post doc in Canada at the National Research Council for two years. 1964 to 66. In 67 I went to Bell Telephone for a year. My boss there, Johan Powell, then went to Case Western Reserve, and I had an offer of a post doctoral tutorial fellow. Slightly above a post doctoral fellowship at Sussex. When Johan left to go to Case I thought I’ll go back to Sussex. And that’s where I went and after half the year I was fortunately, or unfortunately, offered a permanent position there which I took. I spent five years trying to do something as a lecturer at Sussex and if it didn’t work I’d probably go and do night school and go into graphics and design. After five years things were starting to tick over. But I would say it took seven years to get really going which is a long time. |
| Q2 | Was it very hectic? You were busy all the time? |
|  | Sir Harold Kroto: Well, it wasn’t. It was hectic, but I had an idea that this would work. A student would have an idea. Results just seemed to pour out during that period in a way that they haven’t happened ever since. |
| Q4 | What is carbine? |
|  | Sir Harold Kroto: Carbine. It’s supposed to be a solid made from polyines and whenever you try to condense polyines they blow up, so if carbine exists at all it can only exist for a very short time before it blows up and cross links to form a highly exothermic reaction in which things blow up. And I’ve seen these explosions anyway, so I had problems with it. But I thought they were graphite sheets. Sheets of graphite and you could rationalise that a sheet with an even number of atoms would be a little bit more stable than a sheet with an odd number of atoms. I looked at it but I didn’t pay a lot of attention to this. I thought that looks like what these things are. I think I should have been smarter than that. I feel I should have looked at that in more detail. Most of the experiment had been carried out. They hadn’t done the reactions with hydrogen and nitrogen that I’d wanted to do. There was another group at Bell Labs that also did the same experiment. In the Exxon work they had seen C60. But anyway the Bell Labs had seen C60 and picked it off. In our experiments we carried out some experiments which really changed the conditions. Really pushing this experiment. Not let’s do this in random and then rush to the next one. We found that the 60 signal could be made extremely strong. I’m sure if Exxon or Bell had persevered and changed the reaction conditions in the way that we did they would have seen the same results and they would have discovered C60. They would have said this thing is so strong, what structural explanation is there for this strong signal. And they didn’t do that. So I wouldn’t be sitting here being asked about it because they would have discovered it. |
| Q15 | Can I round off by asking … Now it seems that there are coming some practical applications that you make materials out of …maybe not balls but at least tubes of this kind of materials. Do you have any ideas about this or would you say it’s more of the beauty of nature? |
|  | Sir Harold Kroto: I think there’s still some major problems to be solved. There is no doubt that the round cages are beautiful and they’ve changed our understanding of graphite and how graphite behaves, that it can close up into a cage. The nanotubes which were discovered by Japanese Sumio Igima. They’re very interesting. They’re elongated cages. They bear the same relationship to a dome or a ball as a tube does to these so called nanotubes. They’re fascinating. They can conduct like metals you know inorganic super conductor. They’re extremely strong. Probably the strongest materials ever made. But to actually use those properties is a major problem which has yet to be solved. And maybe quite difficult. I’m a bit apprehensive that the applications of nanotubes and C60 will be in the near future still have big technological problems. There’s always the hint of exciting promise. Getting that promise to the marketplace is another matter. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0373 |
| **Biographical** | I was born in Akron, Ohio on June 6, 1943, one year to the day before D-Day, the allied invasion at Normandy. The youngest of four children, I was brought up in a wonderfully stable, loving family of strong Midwestern values. When I was three my family moved to Kansas City, Missouri where we lived in a beautiful large home in a lovely upper-middle class neighborhood. I grew up there (at least to the extent one can be considered to be grown up on leaving for college at age 18) and was convinced that Kansas City, Missouri was the exact center of the known universe.  My mother, Esther Virginia Rhoads, was the third of six children of Charlotte Kraft and Errett Stanley Rhoads, a wealthy manufacturer of furniture in the Kansas City area. She liked the unusual name Errett so much that she gave it to me as my middle name. She picked the name Richard after the crusading English king (the Lion-Hearted), but being a good American and suitably suspicious of royalty, she was fond of calling me “Mr. President” instead. She had big plans for me, and loved me beyond all reason.  My father, Frank Dudley Smalley, Jr., was the second of four children born to Mary Rice Burkholder and Frank Dudley Smalley (Sr.), a railroad mail clerk in Kansas City. Although my father went by the name of June (short for Junior), he never quite forgave his father for not having given him a name of his own, and for not having aspired to more in life. My father started work as a carpenter, and then as a printer’s devil, working for the local newspaper, *The Kansas City Star,* and later for a farm implement trade journal, *Implement and Tractor.* By the time he retired in 1963 he had long since risen to be CEO of this company, and a group of several others that published trade journals in the booming agriculture industry throughout the Western Hemisphere. He was incredibly industrious, talented, and fascinated with both business and technology. He had a wonderfully analytic mind, and loved argument, open discussion, and homespun philosophy. During the depression in the early 1930’s he married my mother (who fell in love with his blue eyes) and was promptly laid off from work. The story of his career is one of total dedication to both his work and his family, a dedication that held steady through a series of tribulations, many of which I am only now beginning to appreciate. He loved me too, but he could see himself in me, and knew my failings through and through. Until late in life I was never quite good enough for my father, and I suppose that is part of what drives me even now, well after his death in 1992.  My interest in Science had many roots. Some came from my mother as she finished her B.A. Degree studies in college while I was in my early teens. She fell in love with science, particularly as a result of classes on the Foundations of Physical Science taught by a magnificent mathematics professor at the University of Kansas City, Dr. Norman N. Royall, Jr. I was infected by this professor second hand, through hundreds of hours of conversations at my mother’s knees. It was from my mother that I first learned of Archimedes, Leonardo da Vinci, Galileo, Kepler, Newton, and Darwin. We spent hours together collecting single-celled organisms from a local pond and watching them with a microscope she had received as a gift from my father. Mostly we talked and read together. From her I learned the wonder of ideas and the beauty of Nature (and music, painting, sculpture, and architecture). From my father I learned to build things, to take them apart, and to fix mechanical and electrical equipment in general. I spent vast hours in a woodworking shop he maintained in the basement of our house, building gadgets, working both with my father and alone, often late into the night. My mother taught me mechanical drawing so that I could be more systematic in my design work, and I continued in drafting classes throughout my 4 years in high school. This play with building, fixing, and designing was my favorite activity throughout my childhood, and was a wonderful preparation for my later career as an experimentalist working on the frontiers of chemistry and physics.  The principal impetus for my entering a career in science, however, was the successful launching of Sputnik in 1957, and the then current belief that science and technology was going to be where the action was in the coming decades. While I had been a rather erratic student for many years, I suddenly became very serious with my education at the beginning of my junior year in the fall of 1959. I set up a private study in the partly furnished, unheated attic of our home, and began to spend long hours in solitude studying and reading (and smoking cigarettes). This happened to be the year when I began to study chemistry for the first time. Luckily, these years were some of the best ever for the public school system in Kansas City, and my local high school, Southwest High, was one of the most effective anywhere in the US as measured by scores on standard achievement tests, and the fraction of students going on to college. My teacher, Victor E. Gustafson, was a great inspiration. He had just begun to teach the preceding year, and was full of love for his subject and for teaching, and had an as yet unblunted ambition to reach even the slowest of students. In addition, this was the first class I had ever taken with my sister, Linda, who was a year older than I, and was a far better student than I had ever been. The result was that by the end of the year, my sister and I finished with the top two grades in the class. We hardly ever missed a question on an exam. It was an exhilarating experience for me, and still ranks as the single most important turning point in my life, even from my current perspective of nearly four decades later. It was the proof of an existence theorem. After my junior year, I knew I could be successful at science. The next year I did equally well in physics with a wonderful professor, J.C. Edwards, but my soul had already been imprinted by my exposure to chemistry the year before.  My mother’s youngest sibling, Dr. Sara Jane Rhoads, was one of the first women in the United States to ever reach the rank of full Professor of Chemistry. After earning her Ph.D. in 1949 with William von Eggers Doering, who was then at Columbia University, she devoted her life to teaching and research in the Department of Chemistry of the University of Wyoming. She received the Garvan Medal of the American Chemical Society in 1982 for her contributions to physical organic chemistry, particularly in the study of the Cope and Claisen rearrangements. She was the only scientist in our extended family and was one of the brightest and, in general, one of the most impressive human beings I have ever met. She was my hero. I used to call her, lovingly, “The Colossus of Rhoads”. Her example was a major factor that led me to go into chemistry, rather than physics or engineering. One of the most enjoyable memories of my early life was the summer (1961) I spent working in her organic chemistry laboratory at the University of Wyoming. It was at her suggestion that I decided to attend Hope College that fall in Holland, Michigan. Hope had then (and still has now) one of the finest undergraduate programs in chemistry in the United States.  At Hope College I spent two years in fruitful study, but decided to transfer to the University of Michigan in Ann Arborafter my favorite professor, Dr. J. Harvey Kleinheksel, died of a heart attack, and the organic chemistry professor with whom I had hoped to do research, Dr. Gerrit Van Zyl, announced his retirement. While the next two years in Ann Arbor were successful, I had become so entangled in a stormy love affair with a lovely girl back at Hope College, that I was not able to concentrate as much on science as I should have. I did, however, learn a lot. Most of all I learned from my fellow students, and particularly from John Seely Brown, a graduate student in mathematics who lived in an apartment down the hall in a small house off campus (he is currently Director of Xerox’s Palo Alto Research Center, PARC). John displayed an audacity of thought and intellectual ambition that I have rarely seen in any individual. My fellow housemates and I were infected with the notion that we could master any subject, and at times we did manage to at least feel that we got close.  By the time of my graduation in 1965, the job market for scientists in the United States was at an all-time high, and even chemistry graduates with just a BS degree were in great demand. Rather than proceeding directly to graduate school, I decided to take a job in the chemical industry in order to buy a bit of time to see what I really wanted to do in science, and to live a little in the “real” world. It turned out to be a terrific decision.  In the fall of 1965 I began work full time in Woodbury, New Jersey at a large polypropylene manufacturing plant owned by the Shell Chemical Company. I began as a chemist working in the quality control laboratory for the plant, a 24 hour a day operation that in the mid 60’s was quite a wonderland of high technology. My first boss was a chemist named Donald S. Brath. He taught his young professionals that “chemists can do anything”, and the time I worked under him was a wonderfully broadening experience. I was teamed up with chemical engineers at the plant to study problems with the quality of the polymer product. The Ziegler-Natta catalyst system then in use by Shell to produce isotactic polypropylene was no where near as efficient as those currently in use, and the level of inorganics remaining in the polymer was high. Much of what we were concerned with in those days revolved around this problem of high “ash” content and how it affected the downstream applications. These were fascinating days, involving huge volumes of material, serious real-world problems, with large financial consequences. I loved it.  After two years I moved up to the Plastics Technical Center at the same site in Woodbury, and devoted myself to developing analytical methods for various aspects of polyolefins, and of the materials involved in their manufacture, modification, and processing. Although I found my work at Shell highly enjoyable, I realized it was time to get on to graduate school, so I began to study seriously and to send out applications. At the time I was most interested in quantum chemistry, and received several offers for graduate assistantships in excellent schools. I was close to accepting an offer from the Theoretical Chemistry Institute at the University of Wisconsin when the automatic graduate student deferments from the Draft into the US military were eliminated. This was in early 1968, during a major buildup phase in the Vietnam War, and I decided it would be more prudent to remain at Shell for a while since my industrial deferment was still in effect.  In my off hours over the past few years I had met Judith Grace Sampieri, who was a wonderful young secretary at Shell. We were married on May 4 of 1968. Soon thereafter, even the industrial deferment was lost, and we decided that I might as well reapply for graduate school. Since Judy’s family lived in New Jersey, I decided to apply to Princeton University, and was accepted. In the late fall of 1968 I was reclassified 1A for the draft and reported to the processing center in Newark for my physical. At the end of the day I ended up in the group who had passed. We were told to put our affairs in order since we would soon be called up. However, in a great stroke of luck, within a week, my wife told me she was pregnant, and within just a few more weeks my draft board reclassified me to some status I do not remember, save that it meant I would not be drafted. On June 9, 1969 Judy and I were blessed with the birth of a beautiful child, Chad Richard. Later that summer, I held him in my lap as Neil Armstrong first stepped out onto the Moon.  In the fall of 1969 I moved my new family up to Princeton to begin studies and research for the Ph.D. in the Department of Chemistry. I was lucky enough to be in the first group of graduate students to work with Elliot R. Bernstein who was just starting as an Assistant Professor at Princeton, after having spent a few years postdoctoral work at the University of Chicago with Clyde A. Hutchison III, following doctoral training with G. Wilse Robinson at CalTech. Elliot’s research at the time involved detailed optical and microwave spectral probes of pure and mixed molecular single crystals cooled in liquid helium. I knew nothing about it at the time I joined the group. I was certain that it was going to be both experimentally and theoretically complex and challenging, but it seemed likely to be worth the effort. My research project was the detailed study of 1,3,5-triazine, a heterocyclic benzene analog that we expected would provide a poignant testing ground for theories of the Jahn Teller effect. In the end we found that the crystal field surrounding each molecule was insufficiently symmetrical to provide the tests we originally sought, but much was learned. Most importantly from my standpoint, I learned from Elliot Bernstein a penetrating, intense style of research that I had never known before, and I learned a great deal about the chemical physics of condensed phase and molecular systems.  In the summer of 1973 we moved to the south side of Chicago so I could begin a postdoctoral period with Donald H. Levy at the University of Chicago. Levy had studied gas-phase magnetic resonance with Alan Carrington, and had been doing some of the most impressive research anywhere in the world with microwave/optical double resonance and the Hanle effect on NO2 and other open-shell small molecules. These were the earliest days when tunable dye lasers were beginning to transform molecular spectroscopy, and Levy’s group was in the lead. The optical spectrum of NO2 was the most troublesome problem for molecular spectroscopists. Even though it had only three atoms, the visible spectrum had far more structure than anyone could understand. But since NO2 was readily available and it displayed an extensive absorption spectrum just where the new lasers could readily operate (500-640 nm), it was a favorite object for study. Don Levy and one of his students, Richard Solarz, had made some major advances with NO2 earlier that summer, so after I arrived in Chicago I began to consider what I could do next. My biggest problem was that my training at Princeton had been in condensed matter spectroscopy, and the ultrahigh resolution gas-phase spectral techniques being used by the Levy group were going to take months to understand. The detailed physics of rotating polyatomic molecules with spin is extremely complex. I was familiar only with the physics of molecules frozen still in a crystal lattice near absolute zero.  When we first arrived in Chicago, Don Levy was in Germany for a several month-long visit, so I had an opportunity to do some extended reading and to prepare for the final oral exam for the Ph.D. degree back in Princeton. At that time in the Chemistry Department at Princeton, the final oral exam consisted of a defense of three original research proposals. I spent many hours in the Univ. Chicago chemistry department library reading recent journal articles, searching for possible topics for these research proposals. On one day I read a new paper by [Yuan Lee](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1986/index.html) and Stuart Rice on the crossed beam reaction of fluorine with benzene (*J. Chem. Phys.* 59, 1427 (1973)] in one of Yuan’s “universal” molecular beam apparatuses. It was the sort of experiment that was to lead to Yuan Lee sharing the Nobel Prize in 1986 with [John Polanyi](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1986/index.html) and [Dudley Herschbach](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1986/index.html). I was deeply struck by a passage in the paper which said that the supersonic expansion used to make the benzene molecular beam was strong enough to cool out essentially all rotational degrees of freedom. That was just what I needed. Since I didn’t understand rotating molecules yet, perhaps I could just stop them from rotating in the first place!  As a result of this exciting day in the Chicago library, one of the proposals I presented to the Princeton Ph.D. committee later that fall was to use a supersonic expansion to cool NO2 to the point that only a single rotational state was populated, and then to use a tunable dye laser to study the now greatly simplified spectrum. I had found in further reading that the current supersonic expansion techniques actually would not get cold enough, so I added the further use of an electric resonance “state-selector” to do a final sorting out of just a single rotational state for study. I recommended, in fact, that the 10 meter state-selector beam machine of Lennard Wharton at Chicago could be used.  When Levy returned from Germany, I told him of this proposal, and we discussed it in some depth. He was intrigued, but was concerned that too much of the NO2 would dimerize to N204 before sufficient cooling was obtained. A few weeks later we discussed it again, and became sufficiently excited to walk down the hall and ask Lennard Wharton what he thought. Len lit up like a light bulb.  Wharton argued that we should first do the experiment on NO2 expanded in a supersonic free jet, and leave the much more elaborate state-selected experiment for later. I told him that wouldn’t be cold enough – the lowest rotational temperature reported for a polyatomic molecule in a supersonic beam that I was aware of at that time was 30 K still way too hot to achieve the simplification we needed. Wharton smiled wryly and swiveled in his chair to reach a research notebook from the shelf behind him. After reading a few pages he looked up and asked “would 3 K be cool enough?”. He had already built a liquid hydrogen cryopumped supersonic beam source with argon, and in the research notebook had measured data for the velocity distribution showing the translational temperature was cooled to 3 K. That, I knew from my Ph.D. proposal, would be quite cool enough in the case of NO2 to collapse the rotational population to just a few levels. We would simply mix in a percent or so of NO2 into the argon and make a “seeded” supersonic beam. This would avoid the N204 formation that concerned Don Levy, and may just possibly cool the rotational degrees of freedom to near the translational temperature of the argon carrier gas. Thus began the collaboration that led to supersonic beam laser spectroscopy.  On the night of August 8, 1974 (the night Nixon resigned from the US Presidency) we recorded the first jet cooled spectrum of NO2. The next morning Don Levy saw the spectrum for the first time, and immediately recognized its significance. Molecular physics had changed. Now we could study at least small polyatomic molecules with at the same penetrating level of detail previously attained only for atoms and diatomics.  A year later, Lennard Wharton came back from a trip to France where he had visited with Roger Campargue and learned of the concept of the “zone of silence” that exists in an expanding gas at sufficiently high densities. While this zone is surrounded by shock waves where the gas is heated to very high temperatures, within the zone the expanding gas is exactly as cold and unperturbed as it would be if the gas expanded into a perfect vacuum, forming no shock waves at all. Campargue had learned to fabricate a ultrasharp edged “skimmer” that could penetrate the “Mach disc” at end of the zone and transmit the gas streaming along the center line of the zone of silence to form the most intense, coldest supersonic beams ever produced. Wharton told Don Levy and me that using helium in such an apparatus we could easily get cowl to 1 K and perhaps even lower. I was stunned. I knew that 1 Kwas low enough to freeze out the rotational motion of even medium-sized molecules such as benzene and naphthalene, and all such molecules could now be studied without rotational congestion.  Later that same day in a hallway conversation Len Wharton and I realized we didn’t need the skimmer. The probe laser beam could easily penetrate the shock waves without perturbation, and we could image just the fluorescence from the laser-excited ultracold molecules in the zone of silence. We quickly built a new apparatus that incorporated these ideas. With the spectroscopic insight of Don Levy and with a series of graduate students we published the pioneering papers on not only jet cooled spectra of ordinary molecules such as NO2, and tetrazine, but also on the first van der Waals complexes with helium (e.g. HeI2), and with the vital collaboration of Daniel Auerbach the first supersonic beam study of a metal atom-rare gas complex, NaAr.  In the summer of 1976 my family and I moved to Houston, Texas where I had accepted a position as assistant professor in the chemistry department at Rice University. I knew of Rice principally because of the beautiful laser spectroscopy that was being done there by Robert F. Curl, and I wanted to collaborate with him much the same as I had with Don Levy. The first supersonic beam apparatus I set up was a free jet machine similar to that I had used in Chicago, but adapted to use pulsed dye lasers in the ultraviolet so that we could study more ordinary molecules such as benzene. My first proposal to the National Science Foundation was for a much larger, more ambitious apparatus that would for the first time use pulsed supersonic nozzles. With these pulsed devices mounted in a large chamber I expected we could attain a 10-100 fold increase in beam intensity and cooling, and by synchronizing with the pulsed lasers in both the visible and ultraviolet be able to study a vast array of large molecules, radicals, and clusters. Being the second apparatus we constructed, it was called “AP2”.  With AP2 we quickly succeeded in setting the world’s record for rotational cooling of a polyatomic molecule (0.17 K). We invented resonant two-photon ionization (R2PI) with time of flight mass spectrometric detection as a means of probing the spectrum of molecules in the supersonic beam. We used this to probe the structure and molecular dynamics of large aromatic molecules, particularly focussing on the question of intramolecular vibrational redistribution. We also developed a means of producing fragments of polyatomic molecules (free radicals such as benzyl and methoxy) by directing a pulsed laser into a specially designed pulsed supersonic nozzle, and studying these cooled in the supersonic beam.  In the late 1970s in collaboration with Andrew Kaldor and his group at Exxon we had extended the capabilities of AP2 so that we could study a large uranium containing molecule (a hexafluoroacetylacetonate-, tetahydrofurancomplexed form of UO2). These were the days of the oil crisis, when there was widespread belief that nuclear fission using uranium was going to be the only long-term alternative. Exxon was working intensely on laser-based isotope separation schemes, and Kaldor was heading up a group to pursue the molecular route. Our experiment on AP2 ultimately revealed a beautiful sharpening of the infrared multiphoton dissociation spectrum of this volatile UO2 complex cooled in the supersonic beam, just what Exxon was looking for. Unfortunately, we began to succeed with these experiments only after the nuclear release “event” at Three Mile Island on March 28, 1979. Within a year, Exxon made a corporate level decision to get out of the isotope separation business. But Kaldor had become so impressed with the capabilities of AP2 that he wanted his own at the corporate laboratories in Linden in any event. Under contract to Exxon, we developed a smaller version of the apparatus, and built two versions. One was kept at Rice and lived on for many years with a very productive science history. Logically, it was called AP3. The clone of AP3 was shipped to Exxon in late 1982.  After a few years of intensive research we found a way to use a pulsed laser directed into a nozzle to vaporize any material, allowing for the first time the atoms of any element in the periodic table to be produced cold in a supersonic beam. Most importantly, we developed a way to control the clustering of these atoms to small aggregates, which then were cooled in the supersonic expansion. Now for the first time it was possible to roam the periodic table and make detailed study of the properties of nanometer-scale particles consisting of a precise number of atoms. The field of metal and semiconductor cluster beams was born. We shipped Exxon this new accessory to their AP3 clone, and both groups then rapidly began to develop the new field.  As is now well known, the Kaldor group was the first to put carbon in a laser vaporization cluster beam apparatus, and see the amazing even-numbered distribution of carbon clusters that we now know to be the fullerenes. Within a year we repeated the same experiment, but now on an improved version of AP2 that had been modified for the study of semiconductor clusters. The story of what we discovered on this apparatus in September of 1985 has been told many times.  The subsequent development of my research in metal and semiconductor clusters, and the fullerenes is too involved to recount here. Increasingly, the tubular variant of the fullerenes has dominated our activities. Now our motto is “if it ain’t tubes, we don’t do it”. We are convinced that major new technologies will be developed over the coming decades from fullerene tubes, fibers, and cables, and we are moving as fast as possible to bring this all to life.  Several years ago AP2 was dismantled and sold off in pieces to other research groups, and the main chamber where the first pulsed nozzle experiments were performed was sold off to a scrap metal dealer along the Houston Ship Channel. Now there are no supersonic beam machines of any type in the laboratory. Times change.  But life and science go on. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0374 |
| **Biographical** | I was born in Amsterdam on December, 3, 1933, the son of Anna Gurk and Jozef Crutzen. I have one sister who still lives in Amsterdam with her family. My mother’s parents moved to the industrial Ruhr region in Germany from East Prussia towards the end of the last century. They were of mixed German and Polish origin. In 1929 at the age of 17, my mother, moved to Amsterdam to work as a housekeeper. There she met my father. He came from Vaals, a little town in the southeastern corner of the Netherlands, Bordering Belgium and Germany and very close to the historical German city of Aachen. He died in 1977. He had relatives in the Netherlands, Germany and Belgium. Thus, from both parents I inherited a cosmopolitan view of the world. My mother, now 84 years old, still lives in Amsterdam, mentally very alert, but since a few months ago, wheelchair-bound. Despite having worked in several countries outside The Netherlands since 1958, I have remained a Dutch citizen.  In May, 1940, The Netherlands were overrun by the German army. In September of the same year I entered elementary school, “de grote school” (the big school), as it was popularly called. My six years of elementary school largely overlapped with the 2nd World War. Our school class had to move between different premises in Amsterdam after the German army had confiscated our original school building. The last months of the war, between the fall of 1944 and Liberation Day on May, 5, 1945, were particularly horrible. During the cold “hongerwinter” (winter of famine) of 1944-1945, there was a severe lack of food and heating fuels. Also water for drinking, cooking and washing was available only in limited quantities for a few hours per day, causing poor hygienic conditions. Many died of hunger and disease, including several of my schoolmates. Some relief came at the beginning of 1945 when the Swedish Red Cross dropped food supplies on parachutes from airplanes. To welcome them we waved our red, white and blue Dutch flags in the streets. I had of course not the slightest idea how important Sweden would become later in my life. We only had a few hours of school each week, but because of special help from one of the teachers, I was allowed together with two other schoolmates to continue to the next and final class of elementary school; unfortunately, all the others lost a year. More or less normal school education only became possible again with the start of the new school year in the fall of 1945.  In 1946, after a successful entrance exam, I entered the “Hogere Burgerschool” (HBS), “Higher Citizen School”, a 5 year long middle school, which prepared for University entrance. I finished this school in June, 1951, with natural sciences as my focal subjects. However, we all also had to become proficient in 3 foreign languages: French, English and German. I got considerable help in learning languages from my parents: German from my mother, French from my father. During those years, chemistry definitely was not one of my favourite subjects. They were mathematics and physics, but I also did very well in the three foreign languages. During my school years I spent considerable time with a variety of sports: football, bicycling, and my greatest passion, long distance skating on the Dutch canals and lakes. I also played chess, which in the Netherlands is ranked as a “denksport” (thought sport). I read widely about travels in distant lands, about astronomy, as well as about bridges and tunnels. Unfortunately, because of a heavy fever, my grades in the final exam of the HBS were not good enough to qualify for a university study stipend, which was very hard to obtain at that time, only 6 years after the end of the 2nd world war and a few years after the end of colonial war in Indonesia, which had been a large drain on Dutch resources. As I did not want to be a further financial burden on my parents for another 4 years or more (my father, a waiter, was often unemployed; my mother worked in the kitchen of a hospital), I chose to attend the Middelbare Technische School (MTS), middle technical school, now called the higher technical school (HTS), to train as a civil engineer. Although the MTS took 3 years, the second year was a practical year during which I earned a modest salary, enough to live on for about 2 years. From the summer of 1954 until February, 1958, with a 21-month interruption for compulsory military service in The Netherlands, I worked at the Bridge Construction Bureau of the City of Amsterdam. In the meanwhile, on a vacation trip in Switzerland, I met a sweet girl, Terttu Soininen, a student of Finnish history and literature at the University of Helsinki. A few years later I was able to entice her to marry me. What a great choice I made! She has been the center of a happy family; without her support, I would never have been able to devote so much of my time to studies and science. After our marriage in February, 1958, we settled in Gävle, a little town about 200 km north of Stockholm, where I had found a job in a building construction bureau. In December of that same year our daughter Ilona was born. In March, 1964, she got a little sister, Sylvia. Ilona is a registered nurse. Her son Jamie Paul is 12 years old. Sylvia is a marketing assistant in München, Germany. All were present in Stockholm, Uppsala and Gävle during the Nobel week. We had a happy and unforgettable time.  All this time I had longed for an academic career. One day, at the beginning of 1958, I saw an advertisement in a Swedish newspaper from the Department of Meteorology of Stockholm Högskola (from 1961, Stockholm University) announcing an opening for a computer programmer. Although I had not the slightest experience in this subject, I applied for the job and had the great luck to be chosen from among many candidates. On July 1, 1959, we moved to Stockholm and I started with my second profession. At that time the Meteorology Institute of Stockholm University (MISU) and the associated International Meteorological Institute (IMI) were at the forefront of meteorological research and many top researchers worked in Stockholm for extended periods. Only about a year earlier the founder of the institutes, Prof. Gustav Rossby, one of the greatest meteorologists ever, had died suddenly and was succeeded by Dr. Bert Bolin, another famous meteorologist, now “retired” as director of the Intergovernmental Panel on Climate Change (IPCC). At that time Stockholm University housed the fastest computers in the world (BESK and its successor FACIT).  With the exception of participation in a field campaign in northern Sweden, led by Dr. Georg Witt to measure the properties of noctilucent clouds, which appear during summer at about 85 km altitude in the coldest parts of atmosphere, and some programming work related to this, I was until about 1966 mainly involved in various meteorological projects, especially helping to build and run some of the first numerical (barotropic) weather prediction models. I also programmed a model of a tropical cyclone for a good friend, Hilding Sundquist, now a professor at MISU. At that time programming was a special art. Advanced general computer languages, such as Algol or Fortran, had not been developed, so that all programmes had to be written in specific machine code. One also had to make sure that all operations yielded numbers in the range -1 The great advantage of being at a university department was that I got the opportunity to follow some of the lecture courses that were offered at the university. By 1963 I could thus fulfill the requirement for the filosofie kandidat (corresponding to a Master of Science) degree, combining the subjects mathematics, mathematical statistics, and meteorology. Unfortunately, I could include neither physics nor chemistry in my formal education, because this would have required my participation in time consuming laboratory exercises. In this way I became a pure theoretician. I have, however, always felt close to experimental work, which I have strongly supported during my later years as director of research at the National Center of Atmospheric Research (NCAR) in Boulder, Colorado (1977-1980) and at the Max-Planck-Institute for Chemistry in Mainz, Germany (since 1980).  Being employed at the meteorological research institute, it was quite natural to take a meteorological topic for my filosofie licentiat thesis (comparable to a Ph.D. thesis). Building on my earlier experience further development of a numerical model of a tropical cyclone had been proposed to me. However, around 1965 I was given the task of helping a scientist from the U.S. to develop a numerical model of the oxygen allotrope distribution in the stratosphere, mesosphere and lower thermosphere. This project got me highly interested in the photochemistry of atmospheric ozone and I started an intensive study of the scientific literature. This gave me an understanding of the status of scientific knowledge about stratospheric chemistry by the latter half of the 1960’s, thus setting the “initial conditions” for my scientific career. Instead of the initially proposed research project, I preferred research on stratospheric chemistry, which was generously accepted. At that time the main topics of research at the Meteorological Institute at the University of Stockholm were dynamics, cloud physics, the carbon cycle, studies of the chemical composition of rainwater, and especially the “acid rain” problem which was largely “discovered” at MISU through the work of Svante Odén and Erik Eriksson. Several researchers at MISU, among them Prof. Bolin and my good friend and fellow student Henning Rodhe, now Professor in Chemical Meteorology at MISU, got heavily involved in the issue which drew considerable political interest at the first United Nation Conference on the Environment in Stockholm in 1972. However, I wanted to do pure science related to natural processes and therefore I picked stratospheric ozone as my subject, without the slightest anticipation of what lay ahead. In this choice of research topic I was left totally free. I can not overstate how I value the generosity and confidence which were conveyed to me by my supervisors Prof. Georg Witt, an expert on the aeronomy of the upper atmosphere, and the head of MISU Prof. Bert Bolin. They were always extremely helpful and showed great interest in the progress of my research. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q12 | Dr Crutzen, I would like to begin to ask you something about your family background, where you grew up and where you went to school and so on? |
|  | Paul J. Crutzen: I’m born in Amsterdam in 1933; well until 1958 I was basically all the time in Amsterdam. My family was a worker’s family, my father was a waiter, a restaurant waiter, my mother did some work in hospital. Life was not always easy and of course from 1940 to 1945 there was the World War and the Netherlands were occupied by the German Nazi troops. So we had that experience also. So even after the war, of course, things were a little slim. Because of course everything had to be built up again. |
| Q2 | But so how did your interest in science then begin? Was it at an early age at school already or did it come later? |
|  | Paul J. Crutzen: Firstly, I did quite well in school. It was easy for me to learn, especially natural sciences but also languages. I was early interested in languages because my father was very good in French and he wanted to show this off on me so, we had some competition in that way, you can almost say. And my mother was born in Germany so I could speak German pretty good. |
| Q17 | So you were multi-lingual from the beginning? |
|  | Paul J. Crutzen: Yes, I was basically from home to bi-lingual, but then through school and through contacts with my father also French and then English and later of course Swedish when I moved to Sweden. But natural sciences I became interested in, I don’t know how, it’s just by reading books and the first books maybe were about explorations you know, the people, the Jules Verne books, the stories about the march to the North and to the South Pole and so on. And I remember at home we had a fantastic picture book about Yellowstone National Park, it was black and white of course, but that book fascinated me so much and things like that pushed me sort of in the scientific area |
| Q2 | Then I understand that you did not immediately start to work in science, you were educated as an engineer from the beginning and also worked as an engineer as I understand it? |
|  | Paul J. Crutzen: Well ,subject which I was also interested in was bridges, of course in Holland you have plenty of bridges. So, that I read a lot about. When I did my final exams for entrance into the university, the last year of our high school, I was very sick and had to do my exams with very high fever over a two-week period and so I had to do it that way because if I didn’t do the exams I would have to wait another year. So, there were no other opportunities. |
| Q10 | But then you also moved to Sweden after this period of studies I realise? |
|  | Paul J. Crutzen: I met a Finish girl on the mountain top in Switzerland. She was vacationing and I was sort of hiking in Switzerland and we started corresponding and I visited her and then in 1958 we married and then we decided to move half way between Finland and Holland and that is Sweden, and I learnt Swedish, which is a reasonably easy language to learn. It’s not very difficult, the grammar is relatively simple. So I worked in a house construction company for about 1,5 years and then I saw an advertisement by the University for Stockholm, Meteorology Department. They were looking for a programmer, computer programmer, and although I had no background in that area and … but I thought well must have something to do with mathematics which I liked very much.  I applied and among quite many candidates, they picked me for some reason. That was my big luck, because I then started working at the Institute of Meteorology of the university as a programmer, but they allowed me to go to courses in mathematical statistics and then meteorology, very theoretical courses. Because I could not afford doing the more experimental courses which take time, you have to spend a considerable amount of hours in the lab and I couldn’t afford that. One of the reasons why I became a theoretician is because of practical reasons. |
| Q2 | So this is when your interest in atmospheric chemistry rised in this environment of meteorology or did it come later? |
|  | Paul J. Crutzen: It came later and initially I studied as I said mathematics, mathematical statistics and then finally I decided, let me try out meteorology. I must say it was a little bit of a shock because mathematics is so clean and mathematical statistics … and meteorology is a very much also an intuitive science. You have to bring in, you apply of course the laws of dynamics and thermal dynamics, but then there’s so many … the system around us is so complex, you have to be very, very intuitive to pick the right things.  And at the Institute after a while, and it’s about 10 years or so, well a little less than 10 years, I became a programmer for a US scientist who was coming to get his PhD at the Institute of Meteorology. So I did the programming for him, the subject was always ozone, was one of the first models of the virtual distribution of ozone which we developed. And while I was doing that I started studying photo chemistry and I got suddenly fascinated and in spectroscopy and so on. That became then my … and one of the very important things I found out is that people just repeated themselves. It is obvious that, these sentences that this and this is true. |
| Q15 | Yes of course. But this is an interesting question: Did the society immediately see the serious consequences of this just because you were a scientist’s projecting something or that was accepted? |
|  | Paul J. Crutzen: That was surprising. It was surprising because I mean the ideas were on the table and very soon of course no measures were immediately taken. The industry was hoping to build these planes. But it was clear that major studies had to be conducted before this expansion of the aircraft lead would take place. And we did several years of research, three or four years on this topic. But at the end suddenly there came another problem which already existed namely the release of chlorine into the stratosphere and by the chlorofluorocarbons gases. |
| Q4 | Why is that so? Why do they grow up can you explain that in simple ways or? |
|  | Paul J. Crutzen: Well, it’s not so simple, you need a blackboard to write some equations down. If we only would consider the chemistry of the nitrogen oxides and indeed ozone would be depleted everywhere. However, low down we also have the emissions of hydrocarbons from automobiles but even more by force and it is the products from the oxidation of the hydrocarbons together with the catalytic action of NOx which then creates ozone and of course you need sunlight. That’s why the photo chemicals were mainly due in summer on the very stable conditions when hydro carbons can accumulate in the lower atmosphere and also nitrogen oxides, then you get these. But this was known already for say Los Angeles area but one did not consider that these reactions basically take place everywhere. |
| Q15 | And would that be enough to heal the hole do you think? |
|  | Paul J. Crutzen: We cannot do more. There have been, it will take 30 even up to a 100 years before the ozone hole will have disappeared. But that’s … nature must have its way now because the chlorofluorocarbons gases are so diluted in the atmosphere that you cannot just take them out and destroy them. It’s too late. And all the ideas which have been spread around so far on mending the ozone layer have been totally unpractical. And mostly many of those are also lounged by former military laboratories who wanted somebody to save the world. But nothing practical. |
| Q15 | Another a worse problem perhaps. So then maybe we should spend a few minutes on the greenhouse effect and your opinions about the carbon dioxide problem and what’s your opinion of our future there? |
|  | Paul J. Crutzen: We have still … maybe you can repeat the question … We have still one, you asked me at the end, have you forgotten something and that of course is one thing that we should also talk about and that is nuclear wind. |
| Q4 | And how could that happen then? Does one know that now? |
|  | Paul J. Crutzen: What we had not considered is that ice particles which form in the stratosphere at low temperatures below minus 80 degrees that on these ice particles reactions take place, which lead to the conversion of relatively stable, we call it reservoir molecules, into very aggressive chlorine atoms and chlorine monoxide radicals which attack ozone very efficiently. And the formation of the ice particles is a, you can almost say natural, process, that is dependent on that’s what is happening over Antarctica because it’s simply cold in winter time, early spring. But the reactions on the ice particles involving chlorine, they are of course coming from human activities. |
| Q14 | You mentioned one other thing you wanted to speak about when we talked about is different scientific topics. I had also another question about what is your major interest right now so to speak, but maybe you want to bring up this question to begin with? |
|  | Paul J. Crutzen: With nuclear wind, well, one of the things I have been very much involved in the past with studying the effects of by a mass burning in the tropics on atmospheric chemistry. It’s normally assumed that the tropics is a very clean part of our environment and people go on vacation to the most wonderful places and of course they are not thinking about pollution there. However, pollution is not only created by industry but also by mass burning and that is happening a lot in the tropics. We have deforestation activities, we have people burn just to cook or they burn to get rid of stubble or old wood or old plant material so they burn everywhere. In the Savannah regions of the world of the one about every year half the area is burnt to get rid of the high dry yellow grass which is not very fancied much by cattle for instance.  So this is a lot of pollution coming into the atmosphere in the tropics so that has been one scientific activity of me and related also to human activities but out of that and my interest in that came totally by chance one day thought that maybe the fires which would be raging following a nuclear war a major nuclear war in cities and oil refineries etc bringing large amounts of soot into the atmosphere. What that would do to the radiation balance of the atmosphere and what we found out and that was actually, that work was initiated by an invitation by *Ambio*, a Swedish journal issued by the Royal Swedish Academy of Sciences. They asked a number of scientists to think about what would be the environmental consequences of nuclear war. Initially I didn’t want to participate in this study. I thought well a nuclear war everybody’s killed five or six times over and I was in the belief that this would be true, but then finally I realised this is not true. You can kill one person or a number of persons six or maybe even more times over really, theoretically, but people are spread all around the world.  And you cannot wipe out human race just by nuclear war, you can make an awful world, but to wipe out humans altogether is a story which is not true. But then one day when I thought about it what is happening when all the soot gets into the atmosphere from heavy fires deposited higher up in the atmosphere and then we found out that would block sunlight from reaching the earth’s surface and we then would create almost something like an anti-greenhouse effect in other words that, because of that, the earth’s surface would cool very strongly and the higher layers up into the atmosphere where the soot is were warm.  So you get a temperature profile which is totally opposite to what is usual when it is warmer low down than higher up. So you turn it the thermo structure of the atmosphere upside down. And of course photo synthesis is reduced under these circumstances, then additional this became a big international study and additional facts were found for instance that temperatures at the earth’s surface may go down below freezing and that of course would be even during summertime this could happen. And that would of course then be disastrous for food production and so on. So suddenly this idea that mankind or a large part of it could be really at risk by nuclear war suddenly became a hypothetical reality. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0375 |
| **Biographical** | I attended elementary school and high school in Mexico City. I was already fascinated by science before entering high school; I still remember my excitement when I first glanced at paramecia and amoebae through a rather primitive toy microscope. I then converted a bathroom, seldom used by the family, into a laboratory and spent hours playing with chemistry sets. With the help of an aunt, Esther Molina, who was a chemist, I continued with more challenging experiments along the lines of those carried out by freshman chemistry students in college. Keeping with our family tradition of sending their children abroad for a couple of years, and aware of my interest in chemistry, I was sent to a boarding school in Switzerland when I was 11 years old, on the assumption that German was an important language for a prospective chemist to learn. I remember I was thrilled to go to Europe, but then I was disappointed in that my European schoolmates had no more interest in science than my Mexican friends. I had already decided at that time to become a research chemist; earlier, I had seriously contemplated the possibility of pursuing a career in music – I used to play the violin in those days. In 1960, I enrolled in the chemical engineering program at UNAM, as this was then the closest way to become a physical chemist, taking math-oriented courses not available to chemistry majors.  After finishing my undergraduate studies in Mexico, I decided to obtain a Ph.D. degree in physical chemistry. This was not an easy task; although my training in chemical engineering was good, it was weak in mathematics, physics, as well as in various areas of basic physical chemistry – subjects such as quantum mechanics were totally alien to me in those days. At first I went to Germany and enrolled at the University of Freiburg. After spending nearly two years doing research in kinetics of polymerizations, I realized that I wanted to have time to study various basic subjects in order to broaden my background and to explore other research areas. Thus, I decided to seek admission to a graduate program in the United States. While pondering my future plans, I spent several months in Paris, where I was able to study mathematics on my own and I also had a wonderful time discussing all sorts of topics, ranging from politics, philosophy, to the arts, etc., with many good friends. Subsequently, I returned to Mexico as an Assistant Professor at the UNAM and I set up the first graduate program in chemical engineering. Finally, in 1968 I left for the University of California at Berkeley to pursue my graduate studies in physical chemistry.  During my first year at Berkeley, I took courses in physics and mathematics, in addition to the required courses in physical chemistry. I then joined the research group of Professor George C. Pimentel, with the goal of studying molecular dynamics using chemical lasers, which were discovered in his group a few years earlier.  George Pimentel was also a pioneer in the development of matrix isolation techniques, which is widely used in the study of the molecular structure and bonding of transient species. He was an excellent teacher and a wonderful mentor; his warmth, enthusiasm, and encouragement provided me with inspiration to pursue important scientific questions.  My graduate work involved the investigation of the distribution of internal energy in the products of chemical and photochemical reactions; chemical lasers were well suited as tools for such studies. At the beginning I had little experience with the experimental techniques required for my research, such as handling vacuum lines, infrared optics, electronic instrumentation, etc. I learned much of this from my colleague and friend Francisco Tablas, who was a postdoctoral fellow at that time. Eventually I became confident enough to generate original results on my own: my earliest achievement consisted of explaining some features in the laser signals – that at first sight appeared to be noise – as “relaxation oscillations,” predictable from the fundamental equations of laser emission.  My years at Berkeley have been some of the best of my life. I arrived there just after the era of the free-speech movement. I had the opportunity to explore many areas and to engage in exciting scientific research in an intellectually stimulating environment. It was also during this time that I had my first experience dealing with the impact of science and technology on society. I remember that I was dismayed by the fact that high-power chemical lasers were being developed elsewhere as weapons; I wanted to be involved with research that was useful to society, but not for potentially harmful purposes.  After completing my Ph.D. degree in 1972, I stayed for another year at Berkeley to continue research on chemical dynamics. Then, in the fall of 1973, I joined the group of Professor F. Sherwood (Sherry) Rowland as a postdoctoral fellow, moving to Irvine, California. Sherry had pioneered research on “hot atom” chemistry, investigating chemical properties of atoms with excess translational energy and produced by radioactive processes. Sherry offered me a list of research options: the one project that intrigued me the most consisted of finding out the environmental fate of certain very inert industrial chemicals – the chlorofluorocarbons (CFCs) – which had been accumulating in the atmosphere and which at that time were thought to have no significant effects on the environment. This project offered me the opportunity to learn a new field – atmospheric chemistry – about which I knew very little; trying to solve a challenging problem appeared to be an excellent way to plunge into a new research area. The CFCs are compounds similar to others that Sherry and I had investigated from the point of view of molecular dynamics; we were familiar with their chemical properties, but not with their atmospheric chemistry.  Three months after I arrived at Irvine, Sherry and I developed the “CFC-ozone depletion theory.” At first the research did not seem to be particularly interesting – I carried out a systematic search for processes that might destroy the CFCs in the lower atmosphere, but nothing appeared to affect them. We knew, however, that they would eventually drift to sufficiently high altitudes to be destroyed by solar radiation. The question was not only what destroys them, but more importantly, what the consequences are. We realized that the chlorine atoms produced by the decomposition of the CFCs would catalytically destroy ozone. We became fully aware of the seriousness of the problem when we compared the industrial amounts of CFCs to the amounts of nitrogen oxides which control ozone levels; the role of these catalysts of natural origin had been established a few years earlier by Paul Crutzen. We were alarmed at the possibility that the continued release of CFCs into the atmosphere would cause a significant depletion of the Earth’s stratospheric ozone layer. Sherry and I decided to exchange information with the atmospheric sciences community: we went to Berkeley to confer with Professor Harold Johnston, whose work on the impact of the release of nitrogen oxides from the proposed supersonic transport (SST) aircraft on the stratospheric ozone layer was well known to us. Johnston informed us that months earlier Ralph Cicerone and Richard Stolarski had arrived at similar conclusions concerning the catalytic properties of chlorine atoms in the stratosphere, in connection with the release of hydrogen chloride either from volcanic eruptions or from the ammonium perchlorate fuel planned for the space shuttle.  We published our findings in Nature, in a paper which appeared in the June 28, 1974 issue. The years following the publication of our paper were hectic, as we had decided to communicate the CFC – ozone issue not only to other scientists, but also to policy makers and to the news media; we realized this was the only way to insure that society would take some measures to alleviate the problem.  To me, Sherry Rowland has always been a wonderful mentor and colleague. I cherish my years of association with him and my friendship with him and his wife, Joan. While he was on sabbatical leave in Vienna during the first six months of 1974, we communicated via mail and telephone. There were many exchanges of mail during this short period of time, which illustrated the frantic pace of our research at that time while we continued to refine our ozone depletion theory. Soon after, Sherry and I published several more articles on the CFC-ozone issue; we presented our results at scientific meetings and we also testified at legislative hearings on potential controls on CFCs emissions.  In 1975, I was appointed as a member of the faculty at the University of California, Irvine. Although I continued to collaborate with Sherry, as an assistant professor I had to prove that I was capable of conducting original research on my own. I thus set up an independent program to investigate chemical and spectroscopic properties of compounds of atmospheric importance, focusing on those that are unstable and difficult to handle in the laboratory, such as hypochlorous acid, chlorine nitrite, chlorine nitrate, peroxynitric acid, etc.  Although my years at Irvine were very productive, I missed not doing experiments myself because of the many responsibilities associated with a faculty position: teaching courses, supervising graduate students, meetings, etc. After spending seven years at Irvine as Assistant and then Associate Professor, I decided to move to a non-academic position. I joined the Molecular Physics and Chemistry Section at the Jet Propulsion Laboratory in 1982. I had a smaller group – only a few postdoctoral fellows – but I also had the luxury of conducting experiments with my own hands, which I enjoyed very much. Indeed, I spent many hours in the laboratory in those years, conducting measurements and developing techniques for the study of newly emerging problems. Around 1985, after becoming aware of the discovery by Joseph Farman and his co-workers of the seasonal depletion of ozone over Antarctica, my research group at JPL investigated the peculiar chemistry which is promoted by polar stratospheric clouds, some of which consist of ice crystals. We were able to show that chlorine-activation reactions take place very efficiently in the presence of ice under polar stratospheric conditions; thus, we provided a laboratory simulation of the chemical effects of clouds over the Antarctic. Also, in order to understand the rapid catalytic gas phase reactions that were taking place over the South Pole, experiments were carried out in my group with chlorine peroxide, a new compound which had not been reported previously in the literature and which turned out to be important in providing the explanation for the rapid loss of ozone in the polar stratosphere.  In 1989 I returned to academic life, moving to the Massachusetts Institute of Technology, where I have continued with research on global atmospheric chemistry issues.  Although I no longer spend much time in the laboratory, I very much enjoy working with my graduate and postdoctoral students, who provide me with invaluable intellectual stimulus. I have also benefited from teaching; as I try to explain my views to students with critical and open minds, I find myself continually being challenged to go back and rethink ideas. I now see teaching and research as complementary, mutually reinforcing activities.  When I first chose the project to investigate the fate of chlorofluorocarbons in the atmosphere, it was simply out of scientific curiosity. I did not consider at that time the environmental consequences of what Sherry and I had set out to study. I am heartened and humbled that I was able to do something that not only contributed to our understanding of atmospheric chemistry, but also had a profound impact on the global environment.  One of the very rewarding aspects of my work has been the interaction with a superb group of colleagues and friends in the atmospheric sciences community. I truly value these friendships, many of which go back 20 years or more, and which I expect to continue for many more years to come. I feel that this Nobel Prize represents recognition for the excellent work that has been done by my colleagues and friends in the atmospheric chemistry community on the stratospheric ozone depletion issue.  \* This autobiography was provided by the Laureate in November 2007.  Copyright © The Nobel Foundation 2007 **Addendum, May 2006** My wife is Guadalupe Alvarez; I am no longer married to Luisa Molina. My son Felipe finished his medical school studies and is now a physician in Boston.; In 2005 I moved from MIT to the University of California, San Diego, and also to Mexico City, where I created a new center for strategic studies in energy and environment.  My current work is related to air quality and global change issues. I collaborate with colleagues from many other disciplines on the problem of rapidly growing cities with serious air pollution problems. In Mexico City we have succeeded in improving air quality significantly, although much work remains to be done. My research group in San Diego is investigating chemical properties of atmospheric particles. The goal is to better understand the effect of these particles on clouds and climate. |
| **Autobiographical** |  |
| **Podcast** | Clare Brilliant: Welcome to Nobel Prize Conversations and this encore presentation of our April 2014 talk with chemistry laureate Mario Molina. I’m Claire. Brilliant, and I’m here with our host Adam Smith. Hi, Adam.  Adam Smith: Hi, Clare.  Brilliant: With 1995 chemistry laureate Mario Molina we wrap up our series of six classic podcasts.  Smith: And hasn’t it been interesting to hear this variety of podcasts from the past? Some of them are completely timeless, but others seem to have aged considerably in 10 years.  Brilliant: How is it to listen to yourself now 10 years later?  Smith: Maybe I’m one of the more things that hasn’t aged so well over the 10 years. It’s strange because you think you’re the same, you don’t change, but actually when you listen to yourself and the questions you ask, you think, gosh, things are a bit different. It was a “the past is a different country”, as L.P. Hartley said in The Go-Between and it really can bring it home.  Brilliant: When you spoke to Mario in 2014, he’d already been living with the prize for nearly 20 years, and he’d really embraced a role as a leader in communicating about climate change. In this episode, you spend quite a lot of time talking about a 2014 paper called ‘What We Know’, and then you and I actually had the privilege of meeting Mario when we attend an event with him in 2018. I was really struck by his positivity, despite some of the enormous challenges posed by climate change.  Smith: He was extremely good at getting that optimism across in 2018, wasn’t he? I remember audiences had become more worried about climate change than they were back in 2014 when we recorded this. But they seemed to come out of his talks more optimistic than they were when they went in, which is an incredible achievement.  Brilliant: He was talking about such a difficult, and I guess, to most people, scary subject, but somehow you came out with a message of hope and I think that does come across in this interview as well. Sadly, Mario Molina is no longer with us as he died in 2020, but do you think the messages from this podcast are still relevant today?  Smith: The world doesn’t see many people like Mario Molina with such optimism and the engaging power to convince people to come together. I really think that for that reason, this podcast stands the test of time and deserves another listen. It’s really worth taking seriously what Mario Molina has to say. Let’s listen to this encore presentation, the last in our series with Mario Molina.  Mario Molina: Hello.  Adam Smith: To start out so you have just released from the American Academy for the Advancement of Science, this new report called ‘What We Know’, which you have chaired.  Molina: Right.  Smith: This is another report on global warming, and there have been many reports on global warming of course. Can you tell me what’s the need for this new report?  Molina: Yes, the purpose is not really to issue a new report, it’s to communicate with the public at large, particularly in the United States, in a more efficient way. We’re not seeking to bring the science up to date or anything like that. We’re just attempting to have a better communication strategy highlighting just a few steps, maybe three at most four. The report is just a background so that it’s documented where this comes from, but the main work to be done will be to somehow through the media and interviews just to stress briefly these three or four important points about climate change that we want the public to be aware of.  Smith: So it’s clarifying the message.  Molina: It is clarifying the message. We’re working with professional communicators, understanding that we scientists often do not communicate well. That’s the main purpose and difference from other reports.  Smith: The choice of title is very emphasise then: what we know. It’s just trying to communicate in simple, straightforward language what is currently understood about global warming. Is that right?  Molina: That’s right.  Smith: Okay. What is currently understood? What’s the main message?  Molina: The main message is that there is a very clear consensus among experts and we quote numbers like, more than 90-97% of climate change experts agree that climate change is taking place and it’s most likely of human origin. That’s the main message. Then a second message is that it’s already happening. It’s not just something for the end of the century. The third, perhaps what is the newer message if you want, is that we’re dealing with risks. We are not necessarily worried mostly about the most likely chain of events, but working with economists in particular, we reached the conclusion that the society should react based on the on risks, namely that there will be some abrupt changes with very important consequences for society at large or for many groups if you want that risks that are unacceptable. Even if they’re not very likely, if you have something of a 10 or 20% risk or more of real calamities to happen, that should be avoided. The science is rather uncertain about this, but this is based on scientific insights and these changes can indeed happen. Perhaps also the fourth important message is that we can deal with this, we can do something about this in contrast to ideas that have been spread out that, oh, this would be just incredibly expensive and we can’t stop in any way using fossil fuels. We believe that’s the wrong message. The message is that we can deal with the problem at the reasonable cost, certainly smaller than what it would cost to shoot some of these abrupt changes that take place.  Smith: That’s a hopeful message to end with.  Molina: That’s right. Yes.  Smith: There’s a lot in there. If we start first of all with this idea of climate change is happening and it is most likely caused by human activity. A lot of people seem to get quite hung up on that most likely piece and say, well, maybe it’s something we are doing, or maybe it’s something we’re not doing. That seems to stop other action, the conversation can in a way stop there. Why do you think everybody’s got so excited by this question of whether or not it is truly anthropogenic?  Molina: When we elaborate on this, we use metaphors. When you drive your car, you wear seat belts and have airbags and not because you have a certainty that you will crash. There are so many things along these lines that prompt society to act. If you have a tumour and your physician tells you, oh, there’s only 80% chance that it’s cancer, just don’t worry about it, go home. It doesn’t make any sense to require certainty, but we clarify that indeed science is a complex system, and we have a very conservative, if you want, group of scientists behind this report precisely to be able to document these statements. There have been lots of exaggerations and unclear statements precisely because they were aware about people misunderstanding the message and scientists are just not happy with making statements that are actually not correct.  Smith: Yes. We are very used to dealing with uncertainty in our own lives. Yet on the climate change issue, as I say, there seems to be more scope for the discussion of the importance of that certainty. For instance, you have the well-publicised climate change sceptics who seem to hold a lot of public opinion, have a lot of public interest in them. People listen to them. That’s odd in a way.  Molina: Yes. We, of course, we’re very much aware of that. That’s why we want to give out a clear message and as needed, explain it with metaphors. Let me give you an even a more striking example. Extreme events that we’re worried about but that’s bringing climate change closer to home for many people. The metaphor we use is in the US for baseball games, if it’s clear that at some point some of the best known players were taking some drugs, okay? There are many more home runs, but you can’t assign any particular home runs because somebody took some drugs. All you can say is that the likelihood of having home runs clearly increased. That’s what’s happening with extreme events. We can’t attribute any particular one to climate change. That’s even less certain if you want, but overall, the statistics is clear. The idea that you need certainty is that something we want to explain? There are so many other examples, that society makes all sorts of political decisions, there’s almost never certainty withheld issues as well. Why would one want to have certainty with something so clearly worrisome that it would be so much against society a reasonable way of acting to ignore these threats.  Smith: Do you have an answer to the question of why that has become such an issue? Have you an understanding of what it is that is stopping people accepting the idea?  Molina: Oh, yes. We know that there was a very well financed public media campaign from interest groups. We also know that there are some artisan issues. It has to do with politics issue became polarised, and it’s a worry with, particularly with the Republican party in the United States that at some point took extreme positions. But those extreme positions, they’re not questioning whether it’s slightly or not, they’re simply claiming that this is all a hoax and that it’s just because the scientists want more money and so on. That’s why, in this case, we also stress that this is the consensus of experts and also of well-established scientific groups, the national academies, professional societies. You have to claim that all of sciences corrupt, and that begins to be sort of crazy, even the enormous impact that science has had on our lives. There is a whole range of reasons behind scepticism. Perhaps we can address them all as needed. But the most worrisome one is just perhaps the one that said, oh, it’s uncertain that scientists do not agree. That’s why that’s the first message. Scientists do agree that it’s happening, it’s real, and they do agree that it’s a big risk.  Smith: Yes.  Molina: That’s the point. I know it’s a challenge.  Smith: You mentioned that this is directed particularly at the American public. Is this, and I guess American institutions as well, is this a particular problem do you think for America? Is the acceptance of the evidence harder in America than elsewhere?  Molina: Yes. But this, of course, we no longer elaborate in these documents and so on, but what is clear in terms of assessing what needs to be done about these problems? It’s clear that one of the best solutions would be to have an international agreement that would put a price on emissions. By the way, just stepping back for a moment, we don’t prescribe with these reports or messages, they’re not prescriptive. They just say, let’s put it on the table and let society take it seriously, discuss what to do. But coming back to your question, it’s clear that the main bottleneck for this international agreement at the moment is US Congress, because they will certainly not stratify any international agreement dealing with climate change because of the official posture of the Republican party. Without their approval, you can’t ratify an international agreement. Their official posture is to deny climate change science. My reasoning is that it’s something equivalent to still living in the age of astrology. That’s unacceptable for any rational society. I hope this for this reasons, if we make the message clear we should be able to bring the rationality to the system. But that’s the reason why the United States is particularly worrisome. It’s international political arena. We certainly know that countries like China, India and so on are very important for these agreements as well. We have indications that they would be willing to move much further ahead in terms of this sort of international agreements than at least the United States at the moment. That’s the bottleneck in terms of coming with a real solution to the problem.  Smith: That anti-science stance in this case of the Republican party is a very peculiar thing. Because I suppose it’s partly vested interests causing one to have that policy but it’s also an antagonistic attitude to science itself. It’s been pointed out by others that the number of scientists within the political system is extremely small or perhaps even zero. On the face of it, it just seems ludicrous that one would have an antagonistic attitude towards the practice of science.  Molina: Yes. That’s why there’s such a strong division, because in fact some Republicans tend to agree. It’s just that they are at the moment compelled to take the parking line. That’s beginning to change. There are beginning to be some very important divisions within the Republican party. Second point, we work with many Republicans that are not now in Congress, but were in government before, and many of them very much agree with our conclusions. In fact, many important decisions connected with the environmental problems and global environmental problems as well were carried up with Republican administrations. One point that we’re also bringing forth that appears to appeal to the public in the United States is we’ve done this before. The United States has been a leader in many environmental issues. One example is the stratospheric cause issue that I was involved with, and it’s one global problem has been solved very efficiently with leadership from the United States, from Republicans. It has happened before.  Smith: Sorry to dwell on this question of why people find it hard to just accept that action is needed, but is it also the magnitude of the problem? People just feel completely overwhelmed by that?  Molina: Sure. That’s why one of the important messages is that it’s a challenge, but we’re not suggesting solving on very expensive changes. This has been carefully analysed, again, by experts, economists, and we can certainly deal with it. It’s an enormous challenge, but it’s something that is striking that it’s not an enormous cost, because with creativity and with technologies for clean energies being further developed, we do have examples of things that are already happening that would bring us to a solution of this problem, or at least a clear indication of the right path. We don’t have to speculate that this is something that perhaps society could actually deal with. We can cite specific examples.  Smith: Then I wanted to move on to this question of risk. You mentioned that there are the kind of the gradual changes that we are already observing, but you are pointing out that there are risks of truly catastrophic events ahead. Tell me more about those.  Molina: One worry we had is for the scientific community to perceive this as alarm and so on. That’s why we don’t even use the word catastrophe in the document. It’s very carefully avoided, but it’s the equivalent, that you’ll have extremely damaging events. But the risk is based on science. We have indications of changes in climate in the past, and we have very clear indications that there’s more energy in the system now and so on. It is not sheer speculation. The existence of risk is based on science. We are very much aware that it’s one of those issues that is hard to communicate to the public, because they’re often very ill-informed about the nature of risks. This is where our work with professional communicators is also very helpful because they can measure and probe how do people react to all to these various sorts of statements and what’s the normal conception they have about risk? They don’t have a good quantification of risks. If it’s small or large, they often just get confused. We’re, again, perhaps through metaphors or examples, we can explain why is it very important for society to take some sort of insurance if you want. That’s one way to explain it to them for risks that are real, and that’s quite reasonable and accepted by people in general, it’s very quite acceptable to spend some resources on insurance because it has paid off clearly in the past.  Smith: What’s an example of the sort of thing that we’re insuring against?  Molina: Here we’re again very careful and we cite and quote some studies like a national academy story or some IPCC stories, they’re actually quite conservative here. But examples would be say the drying up of the Amazon forest or changes in the ocean circulation. Here a conclusion is the following: that one scientist begin to look very carefully at some of these specific possibilities, they turn out not to be very likely so the main worry remains surprises. That’s why we don’t want to give many very specific examples of things to worry about without the statement look. Once we identify this large releases of methane from permafrost, these are all feasible. Those do not seem very likely, but we have so much experience with surprises, even the extreme events. We’re not quite expected, and they are happening already. I realise it’s a difficult issue to communicate, but again, we want to be very honest and not put on the table issues or possibilities that have already been carefully examined. There are, on the other hand, issues such as the rising sea levels that are happening and happening faster than anticipated with very clear consequences that are already materialising. Once you have higher sea level, then if you have a bad weather or hurricane storms and coastal areas they become much more damaging, even if there’s just a relatively small increase in sea level. That’s one of the main worries if you want.  Smith: I can see it’s a very difficult situation because if you elaborate specific risks and they don’t come to pass, then people on the other side of the fence will turn around and say that didn’t happen. You see, it’s all wrong. In a way, that’s the permanent problem. I suppose that you are trying to be very measured and precise in what you say, and the people who are arguing against you don’t have that limitation. They can just pull, if they want, anything out of the air and say, look, climate change scientists say that this is going to happen and it’s not happening, so it’s all wrong. Which obviously doesn’t hold up as an argument, but it’s good as a soundbite.  Molina: That’s right. That’s why, first of all, in the end we make a strong case that the climate is indeed changing because sceptics, until perhaps very recently, were claiming that, oh, it’s actually the climate goes up and down and so on. That part is clear and that part there is consensus. But the second point, in some sense, we try to rely on the record and the honesty that is established or in many other areas of the experts, the scientific community and so on. If necessary, we can certainly have debates or do whatever is needed with people that think to be experts on the other side. For the public in general, we believe we don’t need to sort of do the signs, the complicated science in great detail. We just need to make the case that the science is clear about the risk.  Smith: What do you hope the report will achieve? What do you hope will be the result?  Molina: The result, we hope, is that the polls will begin to show a shift, which is happening already. We will push the shift in public opinion. It’s happening already because of extreme events that are people just until recently, where all this is just a worry for future generations. It doesn’t affect us at all, but there’s so many people already being affected that there is a change in perception. We believe if we, for the first time if you want, could organise a public relations campaign comparable to the one that was carried out. One the other side, we hope to have an impact on public opinion and subsequently on the perception of politicians about this, of course, because they count on public opinion often to support their decisions. We realise that it’s not something that’s going to happen immediately, but we believe that if a world carried out campaign can in fact move the results of the poll.  Smith: When you compare this problem to the one you worked on earlier on CFCs and reducing CFC emissions, was the same kind of publicity campaign required for that? Or was there just an easier acceptance?  Molina: It was easier because energy is behind so many activities of society that’s clearly more difficult and behind economic road. We had an easier time with the CFCs, but fortunately we were able to act before it became polarised or politicised. We did not have this division along party lines materialising. We were able to act before that, and public opinion was important, but it was really the opinion of decision makers, leaders in government that were able to make things sufficiently important to reach an international agreement. We were not really counting there on public opinion driving the solution to the problem but it was certainly part of it. Whoever had looked at it and so on, it supported. There were of course sceptics as well. Another very important difference if you want, is that we were able to work with industry. At the beginning, they were sceptics, of course, but after the science became very clear the industrial sector collaborated with us. By the way, what’s happening now with climate change is not that the private sector is against it. It’s just some interest groups in the private sector are against it, but some others are in favour. That’s another thing we will try to take advantage of, that we can work with powerful groups that understand the problem. Not all the business minded people think that the problem is a hoax. But again, we clearly had an advantage with the stratospheric on issue in that at some point essentially all of industry was behind the solution to the problem.  Smith: If you can help to achieve this ongoing change in the public opinion in the US, that’s part of the battle. Do you think that in the long term, the combination of a developed world already sort of at high levels of energy usage and an emerging world using more and more energy as time goes on, can reach a solution and in fact, act concertedly to prevent truly damaging global warming?  Molina: Yes. That’s of course a very important question. We believe that it’s possible, but it will require further development in technology so that the options to use less fossil fuel become also more economically attractive. That’s happening. There is a clear tendency for clean energies to become cheaper. Two points here to make, one is we’re very much aware of the need of a good section of the global population that doesn’t have access to energy, and that’s a high priority also. But at the moment, at least for the near future, that would not be, even if they were to use fossil fuels or wood that would not detract from the main effort of the rest of the countries. We are very willing to promote access to energy for poor populations. Obviously, we’re not suggesting that there should be zero emissions. We’re just suggesting to do the most efficient way. By the way, these questions are no longer part of this report, we’re suggesting let’s put all of this on the table and have experts continue discussing that. The second point is that, yes, although there are still some problems ahead, we should put everything on the table. Carbon capture, for example, and storage is at the moment just too expensive but perhaps in the future it might be feasible. One of the more controversial possibilities, which we do not deal with in this particular report, is nuclear energy, because that has a very safe historical record in spite of Fukushima. We know that was a big problem in time, but it’s a form of energy that exists. The price keeps coming down, and it’s not that high according to the recent studies. It’s available. The hope is that we will, again, with time develop even safer technologies. But again, the point there with nuclear energy is that it’s the historical fact that it has worked with relatively little problem compared to fossil fuels and other forms.  Smith: Yes. It’s, again, down to this perception of risk.  Molina: That’s right.  Smith: You find yourself at the forefront of arguments about global climate. As a child, you were a very scientifically interested child, were you not?  Molina: That’s right. Yes.  Smith: What form did your scientific explorations take?  Molina: When I was child, quite young, 10 years old or so, I was already quite interested. I like to read and read the biographies of scientists and so on, and then started to play with the chemistry sets, microscopes. I was fascinated with these games if you want. Then I just continued it. It was not something connected to school, but it was more connected to as a hobby.  Smith: I like the use of the word games that. Was it the tools that you enjoyed? Was it the questions you enjoyed?  Molina: It was probably the questions and doing experiments myself. I was lucky my father’s sister was a chemist, and after I showed a lot of interest in these things, she helped me do more sophisticated experiments. To me, that was just fascinating to actually see things with my own eyes and do the experiments with my own hands. It’s this active learning, which by the way, is a tendency nowadays to change education quite dramatically, to have kids do these sort of things rather than just memorising. This natural curiosity that we have as children, just looking through a microscope and suddenly discovering a new world there with all sorts of little creatures moving that you don’t see with your naked eyes. That sort of thing was fascinating to me. I had a number of good teachers that were sympathetic, if you want to, with this attitude towards science eventually this could materialise. I had decided as a child if it was possible to become a scientist and to do scientific research for a living that’s what I wanted to do.  Smith: You had your aunt who was influencing you, and your parents were nurturing. They gave you the space you needed.  Molina: My parents were nurturing. They were not scientists. My father was a lawyer, but they nurtured it. They supported my interest.  Smith: Yes. Did you ever contemplate any other career?  Molina: I did. As a kid, I liked music. I still like it a lot of course. I used to play the violin. I did contemplate, well, maybe that’s an option. I could become a musician. Somehow my parents got some advice that unless I was willing to spend eight or 10 hours a day just playing the violin that I should just give it up. To me, that was very bad advice because you could of course, get very involved in music without being a musician. Fortunately, even though I gave up eventually playing the violin myself, I never gave up my fondness for music. But if I had a chance again, I would not give it up. I would just do it as a hobby. I think that was just because I did not get the correct advice. Fortunately, that didn’t affect at least learning and listening to music.  Smith: That’s true. You were brought up in Mexico, but also traveling around?  Molina: Yes, actually as a child, because of this interest I had in the sciences, my parents sent me to Switzerland for a couple of years to learn German.  Smith: How old were you?  Molina: I was probably 11.  Smith: Gosh. Yes.  Molina: This was to just boarding school. It was a very good experience because my chemistry teacher in Switzerland was somebody I got along very well with and did experiments. The disappointment I had, which fortunately was not seriously, I thought now that I’m going to Europe, all my friends will also be like me and they will like science a lot. They were just like my Mexican friends. Of course, I had normal friends and played and did things, but my friends at that age did not like anything connected to school. In Europe, it was something I did on my own or with my teachers, not with my friends. That happened much later. Eventually just in college, or actually when I start the college and the PhD, that’s when you begin to, of course, share all these interesting times with your colleagues. As a child, it was very much a lonely activity because again of the normal situation that I hope is changing now of children just disliking school and disliking science because it’s just memorising all sorts of facts and very boring.  Smith: Yes. The opposite of what it is really.  Molina: Exactly. Yes.  Smith: That does indicate you were quite a precocious child then, that you were so turned onto science by the age of 11 that you were sent to a school to nurture those interests. That’s unusual.  Molina: My parents had sort of tradition they wanted to us and my brothers and sisters abroad as young kids to learn a lot, another language and to be exposed to another culture. I was very lucky to be part of that. But in my case, of course, very much along the lines, as you just mentioned of, of pursuing my interest in science.  Smith: You obviously feel very tied to Mexico, and now you divide your time between United States and Mexico.  Molina: That’s right.  Smith: Is that a hard balance to manage?  Molina: It’s, but for many years, again, when I was, for example, just at MIT a researcher, then I went to Mexico just for vacations and so on. But at some point I started to do some work with my MIT colleagues that was connected with air quality in Mexico City, because we wanted to do something very interdisciplinary, not just involving science, but also policy economics and so on. That was remarkably successful because we got our students very interested in this interdisciplinary approach to social problems. Making sure that their basic interest in whatever field they were working remained very strong. But as a consequence of that, I realised I could at some stage that I was going to have more impact or that it was something more satisfactory to go back to Mexico to really do some work in my country of origin. I am born a Mexican and an American citizen, and I remain, of course, very much involved with my activities in the United States. The main example, perhaps been on this scientific advisory board to President Obama. I think fortunately what these issues I work with, they are very much international in scope. It’s both to Mexico and the United States interest to have this international perspective. That’s what brings it all together. Take this climate change approach, even though we’re focusing it to begin with in the United States, it’s something we can expand to Mexico and Latin Americans as well. But in Mexico, it’s really a change in field from science to science policy. The big challenge is how can we change not just regulations, but how can we actually affect the way society functions? It’s a big challenge, but we’ve been able to manage in some cases, like improving air quality in Mexico City and so on. It’s a challenge indeed.  Smith: Is it a model you can export to other cities the successful way you’ve tackled it in Mexico City?  Molina: Yes. In fact, we work closely with all the cities. Beginning with Latin America, we have some projects where we were simultaneously working also with Bogota and Santiago in Chile and Sao Paolo in Brazil. We have a lot to learn from each other. But again, there are other examples, other cities, not just in Latin America. That’s beginning to happen. There’s a lot of effort. There are associations of cities now, for example, dealing with a climate change issue, but in broader terms, in just in terms of improving the quality of life, greener cities where transportation works efficiently and so on and so forth. So this is an international effort that I think is beginning to take that form and becoming stronger and stronger learning from each other what works.  Smith: The collaborative approach that sort of powers science that expanded to encompass everybody.  Molina: That’s right. It’s a challenge because you have to deal with society that is hard to change, but if you find the right leaders, then we think it can be moved.  Smith: It seems to me that in many ways you have transitioned really talking the word leader from being a practicing scientist to almost a sort of scientific leader. Do you miss the actual science, or are you so busy leading that you don’t miss it, you don’t have time?  Molina: I do miss it. I was very much involved very early on doing experiments myself, but then later when having a large group, the experiments were actually carried out by my students. But now the way I looked at it is the even though I missed, if you want being heavily involved in just fundamental science or even applied science, but with a group of students, many of my former students are doing excellent science now as faculties in very well-known universities and so on. In some sense, I feel that I was able to keep that going, and I can do something that has more impact, a larger challenge to this translation of science into policy, which relatively few scientists successfully can do. It’s something we have to educate our students more and more about. That’s one component is this policy aspect. The other one is educational. Just making sure that the educational system keeps evolving and changing to become more and more effective.  Smith: It is a very important point on both counts. I suppose as we were saying earlier, the idea that there is so little science in the political system just argues that there needs to be a greater cohort of scientists who want to be involved in politics.  Molina: That’s right.  Smith: And policy. Sure.  Molina: I agree. Yes.  Smith: Last question. Do you yourself think that at some point you might actually enter politics full time?  Molina: No. I had that option some time ago. Already because having a Nobel Prize gives you all sorts of opportunities if you want. A few of my colleagues have done that. But no, I decided that was not the way I could function best. I could probably have more impact to things in a more important fashion, remaining outside politics but stressing disconnection between science and policy. It’s a different world if you really become a Secretary of Energy or whatever. There are so many other pressures for Secretary of the Environment, it’s just I decided that was not the way I would be most productive.  Smith: It’s been absolutely splendid speaking to you. Thank you very much indeed for giving me your time.  Molina: I’m pleased to do that. Sure.  Smith: Okay. Thanks. Bye.  Brilliant: This podcast was presented by Nobel Prize Conversations. If you’d like to know more about Mario Molina, you can go to nobelprize.org. Where you’ll find a wealth of information about the prizes and the people behind the discoveries.  Nobel Prize Conversations is a podcast series with Adam Smith, a co-production of FILT and Nobel Prize Outreach. The producer for Nobel Prize Talks was Magnus Gylje. The editorial team for this encore production includes Andrew Hart, Olivia Lundqvist and me, Clare Brilliant. Music by Epidemic Sound. You can find previous seasons and conversations on Acast or wherever you listen to podcasts. Thanks for listening. |
| **Telephone**  **interview** |  |
| **Interview** |  |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0376 |
| **Biographical** | I was born on June 28, 1927, the second of three sons, in the small central Ohio town of Delaware, the home of Ohio Wesleyan University. My father and mother had moved there the previous year when he took the position of Professor of Mathematics and Chairman of the Department at Ohio Wesleyan. All of my elementary and high school education was received in the Delaware public schools from an excellent set of teachers. The Delaware school system then believed in accelerated promotion, so that I entered first grade at age 5 and skipped the fourth grade entirely, with the result that I entered high school at 12 and graduated a few weeks before my sixteenth birthday. The college preparatory curriculum was strong on Latin, English, History, Science and Mathematics. The academic side of high school was easy for me, and I enjoyed it. In several summers of my early teens, the high school science teacher entrusted to me during his two week vacations the operation of the local volunteer weather station, an auxiliary part of the U.S. weather service-maximum and minimum temperatures and total precipitation. This was my first exposure to systematic experimentation and data collection.  Our home was filled with books, and all of us were avid readers. My reading at that time ran toward naval history, which was complemented with realistic scale-models and simulated naval battles using an elaborate mathematical system for rating each warship and the effects of combat on them. During my sophomore year in high school, my math teacher, who also coached tennis and basketball, encouraged me to take up tennis – which led me onto the varsity tennis team for my junior and senior years, and into a full decade of intense athletic competition. As a senior, I played on the varsity basketball team.  After graduation from high school in 1943, almost all of my male classmates immediately entered the military services. However, because I was still well under the compulsory draft age of 18, I enrolled at Ohio Wesleyan and attended the university year-round for the next two years. During these war years, only 30 or 40 civilian males were on campus, plus about 200 naval officer trainees and 1,000 women. With so few men available, I played on the University basketball and baseball teams, and wrote much of the sports page for the University newspaper.  My accelerated academic schedule made me eligible for my final year of university in June, 1945, as I approached my 18th birthday. However, with the fighting in the Pacific and the continuing military draft, I enlisted in a Navy program to train radar operators. The Pacific war ended while I was still in basic training near Chicago, and I served the next year in several midwestern Naval Separation Centers, as the 10,000,000 Americans who had preceded me into the military were returned to civilian life. A major amount of this Navy time was devoted to competitive athletics for the Navy base teams, and I emerged after 14 months as a non commissioned officer with a rating of Specialist (Athletics) 3rd class. My first real opportunity to see the rest of the United States came when I was transferred to San Pedro, California for discharge from the Navy.  I then hitchhiked 2000 miles back to Ohio, traveling through Yosemite and Yellowstone Park on the way.  This year away from the academic life convinced me that at age 19, there was little reason for me to seek a quick finish to my undergraduate education. I therefore arranged my schedule to take two more years rather than one to graduate, and continued to play basketball on the university team. My coursework at Ohio Wesleyan emphasized science within a liberal arts curriculum, with more or less equal amounts of chemistry, physics and mathematics, and majors in all three fields. As had been the case in high school, I really enjoyed the academic side of university life.  I do not honestly remember when the decision that I would go to graduate school was made. My father had studied for his Ph.D., and all of us took it for granted that I would, too. Furthermore, both my parents had firm convictions that the University of Chicago, which each had attended, was not just the best choice for graduate work, but the only choice. So I applied to the Department of Chemistry at the University of Chicago for Fall 1948, and was duly admitted. All service veterans were entitled to a certain number of months (27 in my case) of paid university education, and I had not used any of these credits during my undergraduate years at Ohio Wesleyan because faculty children did not pay tuition, and I lived at home. I therefore didn’t apply for any of the teaching assistantships or academic fellowships, and was quite surprised after arriving in Chicago to find that many of my fellow students were being paid by the University to attend graduate school. In subsequent years, I was supported by an Atomic Energy Commission (A.E.C.) national fellowship.  At that time, the Chemistry Department of the University of Chicago had a policy of immediately assigning each new graduate student to a temporary faculty adviser prior to the choice of an individual research topic. My randomly assigned mentor was [Willard F. Libby](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1960/index.html), who had just finished developing the Carbon-14 Dating technique for which he received the 1960 Nobel Prize. Bill Libby (although I never called him anything but “Professor Libby” until I was more than 40 years old) was a charismatic, brusque (on first meeting, “I see you made all A’s in undergraduate school. We’re here to find out if you are any damn good!”) dynamo, with a very wide range of fertile ideas for scientific research. I settled automatically and happily into his research group, and became a radiochemist working on the chemistry of radioactive atoms. Almost everything I learned about how to be a research scientist came from listening to and observing Bill Libby.  The first nuclear reactor had been built by Enrico Fermi in 1942 under the football stands at the University of Chicago, and the post-war university had managed to capture many of the leading scientists from the Manhattan Project into the Physics and Chemistry departments. My impression at the time (and now in retrospect 45 years later) was that this was an unbelievably exciting time in the physical sciences at the University of Chicago. My physical chemistry course was taught by [Harold Urey](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1934/index.html) for two quarters and in the third quarter by Edward Teller; inorganic chemistry was given by [Henry Taube](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1983/index.html); radiochemistry by Libby. I also attended courses on Nuclear Physics given by [Maria Goeppert Mayer](https://www.nobelprize.org/nobel_prizes/physics/laureates/1963/index.html) and by [Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html). (The chemistry student grapevine said, “Go to any lecture that Fermi gives on any subject”). Urey and Fermi already had been awarded Nobel Prizes, and Libby, Mayer and Taube were to receive theirs in the future.  My thesis concerned the chemical state of cyclotron-produced radioactive bromine atoms. The nuclear process not only creates a radioactive atom, but breaks it loose from all of its chemical bonds. These highly energetic atoms exist only in very, very low concentration, but can subsequently be traced by their eventual radioactive decay. Bill Libby gave his graduate students an unusual amount of leeway in how they chose to use their time, and was a superb research superviser – supporting, encouraging, but never letting one forget that intensive critical thought, together with unrelenting hard work on experiments, underlay all progress in our research.  My interest in competitive athletics also continued unabated in graduate school. Because of the atypical structure of its undergraduate college system, the University of Chicago, unlike almost all other American universities, permitted graduate students to compete in intercollegiate athletics. During my first graduate year, I played both basketball and baseball for the University teams. I continued to play baseball for the University during the spring for two more years, and spent both of those summers playing semi-professional baseball for a Canadian team in Oshawa, Ontario. Each winter I also played for several basketball teams around the city of Chicago.  Without a doubt, however, the major extracurricular event of those four years at the University of Chicago was meeting and then marrying on June 7, 1952, Joan Lundberg, also a graduate of the University. We have now shared more than 43 years of married life – and shared is really the descriptive word. I finished my Ph. D. thesis in August of 1952, and we went off to Princeton University in September of that year for my new position of Instructor in the Chemistry Department. Our daughter Ingrid was born in Princeton in the summer of 1953, and our son Jeffrey in Huntington, Long Island, in the summer of 1955.  In each of the years from 1953-55, I spent the summer in the Chemistry Department of the Brookhaven National Laboratory. An early experiment there of putting a powdered mixture of the sugar glucose and lithium carbonate into the neutron flux of the Brookhaven nuclear reactor resulted in a one-step synthesis of radioactive tritium-labeled glucose, an article in *Science*, and a new sub-field of tritium “hot atom” chemistry. The A.E.C. also expressed considerable interest in this tracer chemistry, and offered support for continuation of the research.  In 1956, I moved to an Assistant Professorship at the University of Kansas, which had just completed a new chemistry building including special facilities for radiochemistry. Contract support from the A.E.C. was already approved, and in place when I arrived that summer. Several excellent graduate students interested in radiochemistry joined my research group that summer, and were shortly joined by others and by a series of postdoctoral research associates, including many from Europe and Japan. This research group was very productive for the next eight years, chiefly investigating the chemical reactions of energetic tritium atoms and I moved through the ranks to a full Professorship. Both Ingrid and Jeff grew up knowing the members of the group – meeting everyone at our regular home seminars, and from an early age occasionally visiting the laboratory. During these Kansas years, too, the everyday routine was that the entire family came home for lunch. Later on in California, Ingrid and Jeff each worked regularly (but unpaid) drafting slide and journal illustrations for the chemistry department, and thereby continuing to know the members of my research group.  The Irvine campus of the University of California was scheduled to open for students in September, 1965, and I went there in August, 1964, as Professor of Chemistry and the first Chairman of the Chemistry Department. The A.E.C. support turned out to be truly long-term, surviving this transfer, and then the transformations of the A.E.C. into the Energy Research and Development Administration and then into the Department of Energy. That basic contract finally terminated in 1994, by which time NASA was furnishing the major support for our continuing research.  “Hot atom” chemistry continued to play a major role in our research efforts at the University of California Irvine. However, I have deliberately followed a policy of trying to instill some freshness into our research efforts by every few years extending our work into some new, challenging aspect of chemistry – first, radioactive tracer photochemistry, using tritium and carbon-14; then chlorine and fluorine chemistry using the radioactive isotopes 38Cl and 18F  When I decided in 1970 to retire from the Chemistry department chairmanship, I once again sought some new avenue of chemistry for our investigation. Because the state of the environment had become a significant topic for discussion both by the general public and within our family, I traveled to Salzburg, Austria, for an International Atomic Energy Agency meeting on the environmental applications of radioactivity. Afterward on the train to Vienna, I shared a compartment with an A.E.C. program officer also coming from the [IAEA](https://www.nobelprize.org/nobel_prizes/peace/laureates/2005/index.html) meeting. He learned in our conversation that I was personally interested in atmospheric science because of my early association and admiration for the 14C work of Bill Libby, and further that my research had then been supported by the A.E.C. for the previous 14 years. I in turn learned that one of his A.E.C. responsibilities was the organization of a series of Chemistry-Meteorology Workshops, with the intention of encouraging more cross-fertilization between these two scientific fields.  In due course, I was invited to the second of these workshops in January, 1972, in Fort Lauderdale, Florida, where I heard a presentation about recent measurements by the English scientist, Jim Lovelock, of the atmospheric concentrations of a trace species, the man-made chlorofluorocarbon CCl3F, on the cruise of the *Shackleton* to Antarctica. His shipboard observations showed its presence in both the northern and southern hemispheres, although in quite low concentration. One of the special advantages cited for this molecule was that it would be an excellent tracer for air mass movements because its chemical inertness would prevent its early removal from the atmosphere.  As a chemical kineticist and photochemist, I knew that such a molecule could not remain inert in the atmosphere forever, if only because solar photochemistry at high altitudes would break it down. However, many other possible chemical fates could be imagined, and I wondered whether any of these might occur. In early 1973, my regular yearly proposal was submitted to the A.E.C. and was duly approved and funded by them. In addition to the continuation of several radiochemistry experiments, I also included in the proposal a new direction – asking the question: what would eventually happen to the chlorofluorocarbon compounds in the atmosphere?  Later that year, Mario Molina, who had just completed his Ph. D. work as a laser chemist at the University of California Berkeley, joined my research group as a postdoctoral research associate. Offered his choice among several areas for our collaborative research, Mario chose the one furthest from his previous experience and from my own experience as well, and we began studying the atmospheric fate of the chlorofluorocarbon molecules.  Within three months, Mario and I realized that this was not just a scientific question, challenging and interesting to us, but a potentially grave environmental problem involving substantial depletion of the stratospheric ozone layer. A major part of both of our careers since has been spent on the continuing threads of this original problem.  Since 1973, the work of my research group has progressively involved more atmospheric chemistry and less radiochemistry until now our only important use of radioisotopes is directed toward problems associated with atmospheric chemistry. This research work has been conducted at the University of California Irvine by a strong, hard-working group of postdoctoral and graduate student research associates, together with some able technical specialists.  The chlorofluorocarbon-ozone problem became a highly visible public concern in late 1974, and brought with it many new scientific experiments, and also legislative hearings, extensive media coverage, and a much heavier travel schedule for me. This change came after both Ingrid and Jeff had moved away from home for their own university educations, leaving Joan free to accompany me in these travels. She has attended-and sat through with perceptive interest – countless scientific meetings since 1975. She quickly became quite conversant with the general scientific aspects of ozone depletion, and has been a knowledgeable and trusted confidante through all of the last two decades of ozone research. Ingrid and Jeff, too, have maintained close contact and support during those often controversial years.  In many ways, the understanding of atmospheric chemistry is still in an early stage. The necessary instrumental precision and sensitivity for dealing with chemical species in such low concentrations has only been progressively available over the last two decades, and of course the trace composition of the atmosphere is highly variable around the world. The research group has been heavily involved in a series of regional and global experiments, often since 1988 as participants in comprehensive aircraft-based atmospheric field research. Some of this research involves challenging and interesting scientific puzzles, and some can also be described as directed toward global environmental problems. As with the ozone depletion capability of the chlorofluorocarbons, one does not always know until well into the work whether it belongs to the second category as well as the first. We continue to find fascination in the chemistry of the atmosphere. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** | **376** |
| Q15 | When I was briefed about you and your work from one of the people at the Nobel Museum in Stockholm, they said there are a number of people who have done great jobs who have received a prize, but one of them who has made the most outstanding contribution is Professor Rowland. And it was in the work that you have done regarding the environmental issues. How do you feel when people are saying something like that? |
|  | Sherwood Rowland: I do think that the question of the chlorofluorocarbons in stratospheric ozone turned out to be a very important problem. We didn’t plan it that way but that’s how it turned out. And so it has for many people become a symbol of what the environmental movement can do and I agree with that. The Montreal Protocol is an agreement among many countries and it’s actually working. |
| Q15 | But do you feel it’s a burden almost or is it an excitement to carry that, you have been part of something so big? |
|  | Sherwood Rowland: I think it’s more that that was part of my life and I certainly don’t regret it. I enjoyed it while we were doing it because it was very exciting science. And to have it work out that we have, the Montreal Protocol was added to it, that it was recognised and acted upon. |
| Q3 | Were you already then interested in environmental issues? |
|  | Sherwood Rowland: I’d been interested, I wouldn’t say, in one sense I don’t think that I came into this from the environmental camp. I came into it from the scientific side of an interesting scientific problem of what would happen to these compounds which … they’re man made, they hadn’t been in the atmosphere before, they are very useful technologically and one of their major technological advantages is they don’t react with very many things, in fact almost nothing that they react with. And that’s a technological advantage and it’s the fatal flaw in the earth’s atmosphere is that the only thing that happens to them is up in the stratosphere, relieves chlorine atoms, chlorine atoms attack ozone and you have a problem. And so you can’t go on doing that. You have to quite manufacturing and using these compounds. So there was beyond the science there was political and environmental questions that had to be faced and over a period of time they were. |
| Q3 | But have you become more interested in environmental issues? Are you being … |
|  | Sherwood Rowland: Once we got into this then we began … the laboratory that I set up … I was an experimental chemist so you have a laboratory, and we set up a laboratory to measure chlorofluorocarbons and other molecules in the atmosphere. And if I may say so we have a very good laboratory, and as a result if you collected air sample in Southern Chile and you bring it back and you analyse it, we can find out how much of chlorofluorocarbon-11 is in it. We can find out how much of 200 other molecules are there. And you’ve already collected the sample, you’ve already analysed it, and that takes you into studying of other things that are going on in the atmosphere.  We began to measure for example methane, which is also influenced by man but it is not an industrial product in the general sense. It’s a product of molecules that are released in rice paddies, they’re released by cattle, many other uses. And that becomes … and methane is an important greenhouse gas and so we’ve been measuring that now on a global basis for about 25 years. And then you keep on looking and there are other interesting molecules in the atmosphere relative to what’s going on in Chicago or Oklahoma City or Beijing or Tokyo. And so we have become clearly working on the environment but working on the atmosphere as it exists in various places. And that makes you aware, frequently this turns out to be an environmental problem on a local basis or maybe a regional basis or global. And so you start worrying about how you cut off these problems before they become something that takes effect in cities all over the world. |
| Q18 | Of course what we see by the thinning of the ozone layer is that it’s a man-made problem to a large extent. Can we as human beings reverse that? Can we repair it? Can we do something? |
|  | Sherwood Rowland: In terms of reversing it, nature will take care of that, and I don’t see any way to do it faster. And nature will not take care of it rapidly. They major molecule that’s involved, there are two major molecules, but the longer lived of these two is dichlorodifluoromethane or fluorocarbon-12. Its life time in the atmosphere is about 100 years.  And just to make sure what life time means, it means that at the end, about 1% goes away every year, but what that means is that at the end of a century then 37% of it is still left. And at the end of two centuries there’s 15% and at the end of three centuries there are about 6%. So we’re not going to be free of dichlorodifluoromethane for several hundred years. But we will have rapidly, not rapidly, but progressively less as we go through the coming centuries. Fluorocarbon-11, the other major molecule, has a life time of about half a century. And so it will go away somewhat faster, but will still easily measurable quantities for hundreds of years into the future. |
| Q1 | Would you advise young students to get into this field? I read somewhere that we only know very little yet about the atmospheric field. |
|  | Sherwood Rowland: I would encourage young students to get into something that they find very interesting, whatever that is. And there are a lot of interesting environmental problems. There are some where you can, if you clean up the air in your own city you can see the result fairly quickly. In India for instance, in Delhi, in the last several years they’ve made a remarkable improvement because they have switched away from diesel fuel for trucks and taxis and three wheeled vehicles. So the people in India that are working in Delhi can look up and see blue sky where they didn’t have it before, and they can certainly get a feeling of satisfaction out of that, as well as having solved the scientific problem along the way. |
| Q18 | But do you think we as human beings on this earth could have changed quicker, we could have other kinds of use energies, different kinds of cars, if there was not enough pressure? |
|  | Sherwood Rowland: No, on the question of the stratospheric ozone question has been treated a reasonably satisfactory solution about the best that we could has been reached. And we know that this Montreal Protocol is working. Global warming on the other hand is a continuing problem and it’s one that not everyone is facing. Most countries are not really facing the real magnitude of what that problem is.  The Kyoto Protocol that was agreed, negotiated in 1997 and has gone into effect eight years later, meaning this year, and not everybody clearly in the United States has not agreed to it. But the Kyoto Protocol is just a bare start. In order to reduce the effects of greenhouse gases in the atmosphere, we need not to get rid of six or seven percent of the carbon dioxide, but more like sixty of seventy percent. And since carbon dioxide comes from burning coal, gas and oil, that means that we have to be working very hard on all of the opportunities to find energy substitutes from burning the carbon fuels. |
| Q9 | When you received the prize, were you surprised? Had you expected it? |
|  | Sherwood Rowland: The answer to that is no, I hadn’t expected it. One is sort of aware that it might be considered but the Nobel Prize is rarely given for someone that does work that’s outside the laboratory, amidst physics and chemistry. And whether the destruction of the ozone layer was counted as chemistry or physics or geophysics. If it were geophysics then it probably wouldn’t be eligible. So for much of the time when we knew that it was getting a lot of attention, wasn’t it at all clear that it would qualify for Nobel consideration. |
| Q9 | Did it in any way change your working life? |
|  | Sherwood Rowland: Changed it in some ways. I think for many Nobel Prize winners the change in their life is that they become a public scientist and that happened for [Crutzen](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1995/crutzen-facts.html) and [Molina](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1995/molina-facts.html) and myself back in the middle seventies when the destruction of stratospheric ozone became a front page topic. Then we started doing things like this, of doing television interviews or talking with newspaper and magazine people. So that aspect hit very hard in the middle seventies and again in the middle eighties when the Antarctic ozone hole appeared. So having been a public scientist for a couple of decades certain things weren’t so different. But clearly the Nobel Prize is widely recognised and honoured and it’s sort of as though your name has just been changed. It’s not Sherwood Rowland, it’s Sherwood Rowland Nobel Prize winner. And that certainly is a difference. |
| Q2 | What was it in your life that made you want to become a scientist? |
|  | Sherwood Rowland: I was always good at mathematics and science, but I don’t think, I really don’t think that if one went back to when I was a college student, I majored in chemistry. But I could have imagined if something had come up one might have gone off toward journalism or something of this sort. But I did work for the college newspaper. Maybe if somebody had come to me then and said we want you to do this I would have been side tracked in that direction and who knows? But I went to graduate school in chemistry and found that I was fortunate at the University of Chicago to have been dropped into the middle of a hotbed of outstanding science from people that had come out of World War II with scientific projects to work on, now that they were no longer working on military projects. And that was a very stimulating time period. It became not at all difficult to say this looks like this would be a good career. |
| Q1 | Here in Lindau there are many young people who would like to talk to you, who are here because they are outstanding in their own fields. How do you feel about meeting these people? Is it exciting? |
|  | Sherwood Rowland: I think it is interesting to tell them and to listen to them … when I was in their position that was 50 years ago and a lot of things are different. So that some of the things which if I describe my early years as after I had my PhD it’s like coming from an alien world for them because it is so different from the way science was organised in the time period, 1940s, 1950s. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0377 |
| **Biographical** | I was born in Budapest, Hungary, on May 22, 1927 the son of Julius Olah and Magda Krasznai. My father was a lawyer and to my best knowledge nobody in my family before had interest in science. I grew up between the two world wars and received a rather solid general education, the kind middle class children enjoyed in a country whose educational system had its roots dating back to the Austro-Hungarian Monarchy. I attended a Gymnasium (a combination of junior and senior high school) at one of the best schools in Budapest run by the Piarist Fathers, a Roman Catholic order. A strict and demanding curriculum heavily emphasizing the humanities included 8 years of Latin, with German and French as other obligatory languages. Although we had an outstanding science teacher who later became a professor of physics in the University of Budapest I can not recollect any particular interest in chemistry during my school years. My main interest was in the humanities, particularly history, literature, etc. I was (and still am) and avid reader and believe that getting attached too early to a specific field frequently shortchanges a balanced broad education. Although reading the classics in Latin in school may be not as fulfilling as it would be at a more mature age, few scientists can afford the time for such diversion later in life.  After graduating from high school and having survived the ravages of war in Budapest and realizing the difficulties facing life in a small and war torn country, I started to study chemistry upon entering university, being attracted by the wide diversity it offered.  Classes at the Technical University of Budapest were relatively small. We probably started with a class of 70 or 80, whose numbers were rapidly pared down during the first year to maybe half by rather demanding “do or die” oral examinations, where the ones who failed could not continue. This was a rather cruel process, because laboratory facilities were so limited that only few could be accommodated. At the same time the laboratory training was thorough. For example, in the organic laboratory we did some 40 Gatterman preparations. It certainly gave a solid foundation.  Organic chemistry particularly intrigued me and I was fortunate later to become a research assistant to Professor Geza Zemplen, the senior professor of organic chemistry in Hungary, who himself was a student of [Emil Fischer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1902/index.html) in Berlin. He established in Hungary a reputable school in organic chemistry. As Fischer, he too expected his students to pay their own way and even paying for the privilege to work in his laboratory. Becoming an assistant to him although meant no remuneration but also no fee. Zemplen had a formidable reputation, and working for him was quite an experience. He also liked partying and these remarkable events in neighboring pubs lasted frequently for days. Certainly one’s stamina developed through these experiences.  Zemplen was a carbohydrate chemist, much interested in glycosides. Early in our association it became clear that my ideas and interest were not always closely matching his. When I suggested that fluorine containing carbohydrates may be of interest in coupling reactions, his reaction was not unexpectedly very negative. To try to pursue fluorine chemistry in post-war Hungary was indeed far fetched. Eventually, however, he gave in. Even basic chemicals needed for the work, such as HF, FSO3H or BF3 were non-existent and I made them myself, with enthusiastic help by some of my early associates (A. Pavlath, S. Kuhn). Laboratory space, particularly hoods (the kind exhausted only by draft caused by a gas burner causing warm air to raise and take some of the obnoxious fumes through a chimney) was very scarce and even by the time I became an assistant professor it was not welcome to “pollute” more important conventional work. However, the Institute which was on the second floor of the chemistry building, had in the back an open balcony, used to store chemicals. In one of his unexpected gestures Zemplen agreed that I can have the use of this balcony. With some effort we enclosed it, installed two old hoods and were soon in business in what was referred to as the “balcony laboratory”. I am not sure that Zemplen even set foot in it. We enjoyed, however, our new quaters and the implicit understanding that our fluorine chemistry and related study of Friedel-Crafts reactions and their intermediates was now officially tolerated.  Some of my publications in the early 50s from Hungary caught the eye of Hans Meerwein. It is still a mystery to me how he came to read them in a Hungrian journal, although there also was a foreign language edition of the Hungarian Chimica Acta. Anyhow, I received an encouraging letter from him and we followed up correspondence (not easy at a time in completely isolated Hungary). He must have sympathized with my difficulties because one day through his efforts I received a cylinder of boron trifluoride. What a precious gift it was!  The Hungarian educational system after the Communist takeover was realigned according to the Soviet example. University research was deemphasized and research institutes were established under the auspices of the Academy of Sciences. I was invited to join the newly established Central Chemical Research Institute of the Hungarian Academy of Sciences in 1954 and was able to establish a small research group in organic chemistry, housed in temporary laboratories of an industrial research institute. With my group, which by now also included my wife, we were able to expand our work and made the best of our possibilities. In October 1956 Hungary revolted against the Soviet rule, but the uprising was soon put down by drastic measures and much loss of life. Budapest was again devastated and the future looked rather dim. In November-December 1956 some 200,000 Hungarians, mostly of the younger generation fled their country. With my family and much of my research group we also decided to follow this path and look for a new life in the West.  I married in 1949 Judith Lengyel, the best thing ever to happen to me in my life. We knew each other from our early youth and are happily married now for more than 45 years. Judy worked initially as a technical secretary at the Technical University. After we were married she enrolled to study chemistry. She probably rightly recalls that I was entirely responsible for this step and she only agreed to get along with her single minded husband who seemed to believe that there is little in life outside chemistry. From my point of view for husband and wife to closely understand each other’s work and may even work together was most desirable. Our older son George John was born in Budapest in 1954. After we fled Hungary in early December of 1956, we reached late in December London where my wife had relatives. We subsequently moved on in the spring of 1957 to Canada, where my mother-in-law lived in Montreal after the war. During our stay in London for the first time I was able to establish personal contact with some of the organic chemists, whose work I knew and admired from the literature. I found them most gracious and helpful. In particular Christopher Ingold and [Alexander Todd](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1957/index.html) extended efforts on behalf of a young, practically unknown Hungarian refugee chemist in a way which I never forget and for which I am always grateful.  Dow Chemical, with its home base at Midland, Michigan was establishing at the time a small exploratory research laboratory 100 miles across the border in Sarnia, Ontario where its Canadian Subsidaries major operations were located. I was offered a position to join this new laboratory and they also hired two of my original Hungarian Collaborators, including Steven Kuhn. We moved to Sarnia in late May of 1957. As our moving expenses where paid we checked in two cardboard boxes containing all of our worldly possessions unto the train from Montreal and started our new life. Our younger son Ronald Peter was born in Sarnia in 1959. There was no possibility for Judy to continue her career at the time. Sacrificing her own career she devoted herself to bring up our children. She rejoined in our research only a decade later in Cleveland after I returned to academic life.  The Sarnia years at Dow were productive. It was during this period in the late 50’s that my initial work on stable carbocations was started. Dow was and is a major user of carbocationic chemistry, such as the Friedel-Crafts type manufacture of etylbenzene for styrene production. My work thus also had practical significance and helped to improve some industrial processes. In return I was treated well and given substantial freedom to pursue my own ideas. Eventually I was promoted to company Scientist, the highest research position without administrative responsibility.  In the spring of ’64 I transferred to Dow’s Eastern Research Laboratories in Framingham, Massachusetts established under Fred McLarrerty’s directorship. The laboratory was subsequently moved to Wayland, just outside Boston. In the summer of 1965 I was invited to join Western Reserve University in Cleveland, Ohio and returned to academic life as professor with the added responsibility of becoming also Department Chairman.  My Cleveland years were both scientifically and personally most rewarding. My wife Judy was able to rejoin me in our research and my research group grew rapidly. The chemistry departments of Western Reserve University and neighboring Case Institute of Technology were practically adjacent, separated only by a parking lot. It became obvious that it would make sense to join the two into a single, stronger department. We achieved this by 1967 with surprisingly little friction and I was asked to serve as the Chair of the joint department till things settled down. It was in 1969 that I was able to give up my administrative responsibility. As I worked hard my research never suffered during this period and as a matter of fact these were probably some of my most productive years.  After 12 years in Cleveland it was time again to move on. Our older son George was approaching the end of his college years and our younger son Ron who was finishing high school set his mind to go to Stanford. He convinced us that it should be nice for the whole family to resettle in California. Coincidentally, in the fall of 1976 Sid Benson, an old friend called me to find out whether I would be interested to join him at the University of Southern California in Los Angeles. After some visits to LA the challenge of trying to build up chemistry in a dynamic university and the attractiveness of life in Southern California convinced us to move. We fell in love with California and we still are. As USC had limited chemistry facilities, it was offered to establish a research institute in the broad area of hydrocarbon research and provide it with its own building and facilities. We moved in May of 1977. Some 15 members of my research group joined the move West. By arrangements worked out we were able to take with us most of the laboratory equipment, chemicals, etc. Two weeks after our arrival with some large moving vans we were back doing chemistry in temporary quarters, while our research institute was constructed. The Institute was established at USC with generous support by Mr. & Mrs. D.P. Loker, friends and great supporters of the University. The Institute was subsequently named after them. Don Loker passed away some years ago, but Katherine still chairs the Institute’s board. Through her and other friends’ generosity a wonderful new addition to our Institute is just completed doubling our space.  As rewarding as the Nobel Prize is personally to any scientist, I feel it is also recognition of all my past and present students and associates (by now numbering close to 200), who contributed over the years so much through their dedicated hard work to our joint effort. It also recognizes fundamental contributions by many colleagues and friends from around the world to a field of chemistry, which is not frequently highlighted or recognized.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1994*, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1995  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 1994 **Addendum, May 2005** It is frequently said that receiving the Nobel Prize put so many obligations and commitments on the winners that their scientific work inevitably suffers. I was much determined that this should not happen to me. Having received the prize at the age 67 also helped, as my lifelong habits were solidly developed. I was determined that the prize should not affect significantly my life and certainly not my research.  I feel that I mostly succeeded. The intervening years were very productive and in many ways most rewarding for my research. Helped by my dedicated younger colleagues and associates and by close collaboration with my colleague Professor Surya Prakash, I was able to not only to continue my research but to extend it into new challenging areas.  A significant part of my previous research was based on the study of positively charged carbon compounds (carbocations) using superacids and their chemistry. The extremely strong acids I used and explored turned out to be many billions or even trillions of times stronger than previously recognized “strong” acids such as concentrated sulfuric acid.  The vastly increased acidity of superacidic systems resulted in the significant new field of superacid chemistry. In the last decade I asked myself whether a similar but more general approach could be used to produce in general electrophiles (electro deficient reagents) of greatly enhanced electron reactivity.  This resulted in the development of the concept of superelectrophilic activation and the study of superelectrophiles, i.e. electrophiles of greatly enhanced reactivity compared with previously known related electrophilic reagents and systems.  The concept of superelectrophiles thus emerged from my previous studies on superacidic carbocation and onium ion systems. It is based on the realization that a variety of electrophiles capable of further interaction (coordination) with strong Bronsted or Lewis acids can be greatly activated by them. Examples include onium and carboxonium ions, acyl cations, halonium, azonium, carbozonium ions, even certain substituted carbocations and the like. This activation produces what is now known as superelectrophiles, that is, electrophiles of doubly electron-deficient (dipositive) nature whose reactivity significantly exceeds that of their parents. Superelectrophiles are the *de facto* reactive intermediates of many electrophilic reactions in superacidic systems (including those involving solid superacids) and even some enzymatic systems and should be differentiated from energetically lower-lying, thus much more stable intermediates, which frequently are observable and even isolable but are not necessarily reactive enough without further activation.  Examples of some superelectrophiles so far studied and their parents are  It should be recognized that superelectrophilic reactions frequently proceed with only “electrophilic assistance” (solvation, association) by the superacids without forming distinct dipositive intermediates. Protosolvolytic activation of electrophiles should always be considered in this context.  Another area of my post-Nobel research, that turned into a major continuing effort, evolved from the realization that our hydrocarbon resources, the marvelous gift of nature in the form of petroleum oil, natural gas and coal, are finite and not renewable.  The rapidly growing world population, which was 1.6 billion at the beginning of the twentieth century, has now well exceeded 6 billion. Even if mankind increasingly would exercise population control, by mid-century we will reach around 9.5-10 billion. This inevitably puts enormous pressure on our resources, not the least on our energy resources. For its survival, mankind needs not only food, clean water, shelter clothing, etc. but also energy. Since the cave man first managed to keep light and fire, our early ancestors burned wood and subsequently other natural sources. The industrial revolution was fueled by coal. The twentieth century added oil and natural gas and introduced atomic energy.  When fossil fuels such as coal, oil, or natural gas (i.e. hydrocarbons) are burned in power plants to generate electricity or to heat our homes and fuel our cars and airplanes, they form carbon dioxide and water. Thus, they are used up and are nonrenewable (at least on the human time scale). To find ways to replace our diminishing natural resources hydrocarbons will to be made by ourselves in a renewable, economical, and environmentally adaptable, clean way. This represents a major challenge for mankind in the twenty-first century.  I have developed a promising new approach for solving not only our long range dependence on decreasing fossil fuels (oil, gas, and coal) but also at the same time to mitigate global climate change (warming) caused significantly by derived greenhouse gases such as carbon dioxide and methane. The approach is based on the use of methanol (CH3OH) as a way to store energy, as well as a convenient fuel and hydrocarbon source. Methanol as a fuel can also be directly used in the new fuel cell we developed jointly with the Jet Propulsion Laboratory of Caltech. It is also a raw material for synthetic (man made) hydrocarbons through its conversion to ethylene or propylene (by catalytic bimolecular dehydration i.e. 2CH3OH->CH2=CH2+2H2O). From these one can produce all the hydrocarbon fuels and products (from gasoline and diesel oil, to plastics, synthetic materials, pharmaceuticals, etc.), which are currently made from oil and natural gas. I call this new approach the “methanol economy”.  Currently methanol is still produced from fossil fuels, predominantly from natural gas through syn-gas (a mixture of CO and H2) by so-called Fisher-Tropsch chemistry, which is however a highly energy vasting process. We have developed new methods to convert still existing natural gas (methane) directly and efficiently to methanol. The true methanol economy, however, will do without natural gas, oil and coal as it is possible to produce methanol by the reaction of carbon dioxide with hydrogen. Exhaust gases from power plants and varied industrial emissions contain considerable amounts of carbon dioxide, which can easily be separated. Rather than just collecting carbon dioxide and storing it underground or at the bottom of the seas (as it is suggested) it can be used to produce methanol. Eventually, atmospheric carbon dioxide itself will be possible to be separated and converted to methanol. As atmospheric carbon dioxide is available to all people on the Earth this will enable mankind to liberate itself from dependence on fossil fuels. Substantial energy is of course necessary to generate the needed hydrogen for methanol production. This energy could come from safe nuclear power plants as well as all alternative energy sources such as sunlight, wind, geothermal, etc. At the same time, this approach will also diminish the danger of global warming by removing and recycling the rising carbon dioxide content of the atmosphere.I am fortunate to have retained my interest and drive to continue research with quite unabated energy. I published in the last decade years a number of books and monographs, notably: “Hydrocarbon Chemistry” (with Arpad Molnar,) 2nd revised ed., Wiley, 2003. “Onium Ions” (with Kenneth Laali, Qi Wang, and Surya Prakash,) Wiley, 1998. “Across Conventional Lines, selected papers of George Olah” (ed. with Surya Prakash), World Scientific Publishing, Singapore, 2003. “Carbocation Chemistry” (ed. with Surya Prakash), Wiley, 2004. “Beyond Oil and Gas: The Methanol Economy” (with Alain Goeppert and Surya Prakash), Wiley-VCH, 2005 (in preparation). I also published about 170 additional research papers and obtained some 15 patents. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| **Q** | family background and early education; his discovery and the Nobel Prize (5.17)  **I’m sitting here in Lindau in Southern Bavaria, this is the 50th anniversary of the Lindau so called Nobelpreisträgertagungen and I am sitting here with George Olah who received the Nobel Prize in Chemistry in 1994. I would like to ask you first, Professor Olah, could you tell us a little bit about how and why you became a scientist.** |
|  | George A. Olah: I was born and grew up and lived in Hungary until I was 29. When I was growing up and going to school, I must confess I had absolutely no interest in science. As a matter of fact it never crossed my mind that science would be an area I would be involved in. I had much interest in history, literature, languages, even philosophy. At the end of World War II, in war-devastated Hungary in central Europe, when the time came to enter university, it certainly became clear to me that I’d better try to get into a profession that I also could make a living. When took my first chemistry class I fell in love with chemistry – don’t ask me why because I can’t explain why you are falling in love, but I am still in love with it. It’s maybe disappointing, but I was not one of these wunderkindwho at age ten already knew exactly what he wanted to do and studied /- – -/ before this.  Maybe I should mention [Eugene Wigner](https://www.nobelprize.org/prizes/physics/1963/wigner/facts/) who was also a Hungarian born physicist. I read once he wrote about his life and when he was finishing high school his father was a businessman sat down with him and asked what he wanted to become. He said that he would like to become a theoretical physicist and his father, who I guess never had heard of theoretical physics, answered ‘And tell me how many jobs in this little country of Hungary are for theoretical physicists?’ His son, who was an honest guy, said ‘I guess two, maybe three.’ At which time the conversation was terminated his father enrolled him as a chemical engineer, but later on he shifted gears. |
|  | **Can you say a few words about your family environment, what kind of family did you grew up in?** |
|  | I was born in a György middle class family, my father was a lawyer and my mother was a homemaker. To my best knowledge nobody in my family ever had any interest in science. I was fortunate enough that I got a fairly schooling, education – much was said about the schools of Budapest and the number of people came out, not only in the sciences but in other areas – musicians, conductors and so on. There was a wonderful music school in Budapest, founded by Franz Liszt, but there was a number of these gymnasien – combined middle and high schools – so I have gone to one of them, not to the one where all my well-known physicist compatriots have gone, but this was run by a Catholic order called the Piarist Brothers. It was really a gear to a general education, heavily in the humanities and so on.  I never heard the name ever mentioned, and I am quite sure about it during my eight years in this school, of [George de Hevesy](https://www.nobelprize.org/prizes/chemistry/1943/hevesy/facts/) who was a student of the same school who won the Nobel Prize in Chemistry I guess in 1944 or 1945 – maybe there was some tradition, but if it was it was hidden. I guess it served me very well, that it was a very well-balanced general education. I am sorry to say I can’t remember my chemistry teacher’s name, but I remember my physics teacher whose name was Joseph /- – -/, who was a very inspiring teacher. Later I understand he became well known because he became a university professor and introduced on television popular science, in Hungary, but he certainly had a substantial influence as young boys. Also, I never considered to go into physics. |
|  | **Could you say a little bit about how you came to do the work that you eventually were awarded the Nobel Prize for? Did you have any inspiration, a special idea, was there some new development of apparatus or how did it come about?** |
|  | I am a chemist who is mostly interested in compounds of the element carbon which is a fairly central element on Earth. Table salt, sodium chloride, is composed of a sodium ketone positive ion and a chloride ion, but carbon compounds are supposed to be different and their ability to form ionic compounds was doubted for a long while. I was studying reactions which involved chemistry, which could have involved ionic carbon compounds – nobody really knew – it was suspected. For long years I had an interest to pursue this chemistry but also with an eye to try to find out how this really goes. It’s not a question of intuition overnight, that you wake up and you have a tremendous idea – some people may have it – it wasn’t with me, but through a fairly long struggle eventually I was lucky to find systems in which these long elusive positive ions of carbons which they call carbocations – carbon is the element and cations is the positive ions – were observable and as the Nobel Committee stated I give supposedly long life to these ions.  There were many things I don’t want to bore with chemistry, but in order to do this it was necessary to use very acidic systems which now are called superacids. When I say very acidic, say your car battery has sulphuric acid in it, and when I grew up, maybe even now in most schools, school children are taught that sulphuric acid is a strong acid. The acid in which my chemistry was possible has acidities which are say a trillion times stronger than sulphuric acid. These are very big numbers, very little meaning, I don’t know whether Sweden has any national debt to hide this, but the US has a national debt I guess of about 5 trillion dollars, so my acids are in this range. |
|  | **Receiving the Nobel Prize – did that mean for you any big change in your daily life, in your direction of study or was it just another one of those prizes that you have received earlier?** |
|  | Obviously, anybody who receives this prize tells you that it has an effect. Look, one thing is I wouldn’t be sitting here interviewed by you if I wouldn’t have received the Nobel Prize. On the other hand, I was quite well established in my doubt that it basically … I am still a working scientist and I still love to do this, and I’m also blessed with a wonderful wife who keeps me very down to Earth. I haven’t changed, I hope, as an individual. I work harder than ever because with the Nobel Prize are coming new responsibilities which I try to perform, but I still, my primary life hasn’t changed. |
|  | **Professor Olah, would you say that your work that you received the Nobel Prize for has a special meaning, that it has a special importance for applications, for chemistry or for society?** |
|  | Of course all scientists believe what they are doing is significant, but I mentioned that my work still center the chemistry of carbon compound. There was a well-known famous German chemist in the middle of the last century called Kekulé, and one of Kekulé’s major contributions to chemistry was his concept, which still is guiding, chemistry which is generally called organic chemistry, the chemistry of carbon and /- – -/ compound. The carbon can attach itself simultaneously to not more than a maximum of four other atoms or groups. In studying these positively charged species of carbon we realised through a series of investigation that with these systems carbon can attach five atoms or groups, sometimes six, and recently we showed even seven.  This doesn’t violate the fundamental rule of what in chemistry is called the octet rule, so you can’t have more than eight electrons surrounding carbon at any time, but on the other hand, if I mention the simplest carbon hydrating compound, hydrocarbon, is methane, CH4. With our very strong acids we can attach a proton to methane and CH5+ is not a fictional species anymore, it’s a very realistic and quite intriguing species which has substantial bearing on fundamental chemistry in general, but in a practical way. This chemistry opened up possibilities to activate and react hydrocarbons, natural gas, and all are really just mixtures of hydrocarbons. The chemistry we developed and are still developing, to take say methane, a natural gas, and transform it into all kinds of useful products through this new type of chemistry. |
|  | **What did you think about when you first received the news of your Nobel Prize?** |
|  | You know it’s a time difference – this was early in the morning – we are early risers so we were up and having breakfast and you get this proverbial phone call. Obviously, it takes a while to sink in. You are obviously gratified and elated – you really don’t know what this all means – and then all hell broke loose. Then we had this wonderful week in Stockholm when we were floating on adrenaline – it takes a while that it settles in. I already told you that it is a wonderful gratifying thing, on the other hand you should keep your proportion. The fact that you get a prize really isn’t making you overnight some type of a different person. |
|  | **Do you think you could say three words to describe yourself?** |
|  | Three, it may be five. I am a human being interested in science. In the sequence. The second one is that I learned one thing very useful, it is that I gladly admit how little I know. The third one is that whereas that you don’t plan that your work has practical uses it’s a great pleasure when you can apply some of your knowledge to do something which may be useful for the future. |
|  | **What do you do when you relax?** |
|  | My wife sits here so I must be very careful answering you. I am blessed with a wonderful family and we have two wonderful grandchildren, but I must tell you that I am still a very hard worker. My life is around science, although one thing the prize did to me was that my wife and some friends convinced me to write something which was originally supposed to be about my life experience, but it ended up to a great degree about reflections. I told you I was much interested, growing up, in many things, so I spent five years really reading very hard, filling in lots of gaps – not the chemistry but science, philosophy, history and so on – and I greatly enjoyed doing this. |
|  | **Have you written it? Is it published?** |
|  | It should come out this year. As it happened the great Greek thinker /- – -/ they were able to cover everything, the physical world and the spiritual world. Then things became very complicated. By the end of the 19th century many philosophers really gave up because I guess they tried to avoid to be embarrassed about the limited knowledge of the physical world. Then, it looks like that physicists – please forgive me mentioning this – particularly practical physicists took over and they believe they have all the answers. I personally believe we probably never will have all the answers, but maybe a few chemists, [Eigen](https://www.nobelprize.org/prizes/chemistry/1967/eigen/facts/) and [Prigogine](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/facts/) and others express some views, not too many, so I try to put in my /- – -/, maybe a big failure but I enjoyed doing it. |
|  | **Do you teach at all, and if you teach – do you think that teaching gives you something back?** |
|  | I still teach, and I love teaching. It may surprise you, but I am teaching presently in this fall again, undergraduates, on a topic which is concerned with the relationship between science, arts and even economics. Whereas it is a very informal course, the text we are using is Goethe’s Faustas something which was written by a great poet. The story, at least the first part, is a chemist story, or an alchemist story, and the second part is that whereas the alchemist couldn’t make gold, but paper money was about invented at the time and he puts paper money instead of gold saying that from nothing some value is created. I enjoyed it very much and I learned more than my students. |
|  | **Thank you very much. The goal of conversation for your life as a scientist – do you think conversation with others is an important factor for your scientific work?** |
|  | It is absolutely important. I was never fortunate enough to have been in a milieu /- – -/ than in an institution which was one of the leading outstanding institutions in the world. Scientifically, I am a grandson of [Emil Fischer](https://www.nobelprize.org/prizes/chemistry/1902/fischer/facts/) who was one of the great organic chemists of our time, and my professor brought back to little Hungary some of Fischer’s concepts. That is for any scientist essential to have contact, free exchange and as we said ‘kicking around ideas’. You do this all the time with your students. I consider the greatest blessing to be a university professor. It keeps you young, your students are your wider scientific family and in this given /- – -/ I hope that I am inspiring them a little. At the same time they keep me active and going. |
|  | **Can you give an example of a creative milieu – an example, you don’t have to define it. Can you think of some place?** |
|  | Obviously there are wonderful creative milieus because there are these wonderful schools and centers of culture and so on. As I told you I never was lucky enough, so I needed to create my own milieu. Maybe I am strong, /- – -/ and lucky but I was able to do it and I have my students today, in a small way, I can too provide. I think my role as a professor is a catalyst and somebody who tries to provide them a milieu where they can pursue their work and study relatively shielded from what they will later be exposed into. |
|  | **It is said that mathematics is beautiful. Is there a beauty also in chemistry?** |
|  | The beauty is in the eye of the beholder. For me, certainly there is. Mathematics I guess is a universal language of all the sciences. For mathematics I think you must have some born talent. I am not so sure for chemistry you need to have a born talent. Probably it’s helpful that you need to have an enquiring mind. Creativity I think, what we discussed, is to me very difficult to define, because if somebody can define creativity, he or she is probably isn’t very creative. I think an artist or a great painter, sculptor can define for you or you never think the word creativity. But to me chemistry certainly has a great degree of beauty. There are people who find symmetry and write books about it, but to me the beauty is that if you find out some underlined principles then you can build on it. It’s a wonderful experience. |
|  | **Why are so many Nobel Laureates of Hungarian origin?** |
|  | I can’t answer you. I don’t think there are special Hungarian genes for science or music or whatever. Probably, and this is only … I never knew the rest of Hungary, only Budapest. Probably in part, it was due to the fact that there were some good schools, there was a basis for it. Many of the Hungarian scientists /- – -/ got recognition did their work outside of Hungary. [Albert Szent-Györgyi](https://www.nobelprize.org/prizes/medicine/1937/szent-gyorgyi/facts/) worked there and I did at least my initial work in Hungary. Being in a small country maybe there is this extra initiative that you try to prove that even in a small poor country you can do something. But I really don’t think that Hungarians are very different from anybody else, we are all human beings. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0378 |
| **Biographical** | My father Cecil Banks Mullis and mother, formerly Bernice Alberta Barker grew up in rural North Carolina in the foothills of the Blue Ridge Mountains. My dad’s family had a general store, which I never saw. My grandparents on his side had already died before I started noticing things. My mother’s parents were close to me all during my childhood, and her father Albert stopped by to see me in a non-substantial form on his way out of this world in 1986. I was living in California. “Pop” died at 92 and wondering what was happening to me out in California, stopped by Kensington for a couple days. My house afforded a view of San Francisco and the Golden Gate Bridge. His visit was an odd experience. Not at all frightening. I have cultivated the curious things in life and found this one pleasant. “Pop” and I sat in the evenings in my kitchen and I told him about the contemporary California world while we drank beer. I drank his for him as it appeared that although he was very much there for me, he was not there at all for the beer. Many of my friends when I told them of this thought it fanciful. (I think it more likely than much of our math today and at least half of our physics, both of which I like).  Until I was five my immediate family lived near my grandfather’s farm where my mother had grown up, and with the exception of a few modern conveniences, had not changed a lot over the years.  My grandfather milked several cows twice a day and supplied the neighbours with dairy products. He liked to go visiting around the county on Saturdays and he also enjoyed the neighbours when they came by once a week with their empty milk jars. He walked them out to their cars and hung over the driver’s side window until they drove off. The road was two tire tracks on well mown grass between barbed wire fences, cows off to the right, alfalfa or sometimes corn to the left.  I remember mostly the summers. My mother and aunts presided out on the big screened back porch shelling peas, stringing beans, peeling apples, pears, and peaches. The peaches were peeled with a special machine that had a hand crank and left a spiraling groove on what was left of the peach. The peels went to the pigs. Everything else went into steaming Mason jars which would go down into the earthen floored cellar. Down there in the dark, and it was always a little moist, were spiders in abundance and magnificent biodiversity. My brothers, and my cousins, and I ventured into the cellar once in a while to inspect the sweet potatoes and the hibernating jars. No one wanted to stay there alone ever, and mostly we played in the woods, the swamp, the orchards, the barn, the granary, which had wasps, and the woodshed, which also had wasps and, like the barn, allegedly, snakes.  We tortured the cows. We sliced apples and slipped them onto the electric fence that contained them in the newer parts of the pasture. Cows like apples and they kept trying. We watched the chickens pecking at the black mud around their chicken house. We heard the squeal of young pigs being castrated by my grandfather and the veterinarian, but we weren’t allowed to watch. We heard stories from our moms about balls of fire during thunder storms streaming up the drain pipe that led down to the chicken yard and dancing out of the sink onto the grey floor of the back porch. All the scorched marks had been sanded and painted over by the time we heard about it, and sadly it never happened while we were there. But there were thunderstorms. Rain would come down from a cloudburst in the summer afternoons and the woods would explode with thunder. Our moms would keep us inside and out of the draft from any windows. We… wanted to see those fireballs.  We could play in the attic. Even in the day there was not enough light to keep us calm in the attic, and there were animal-skin coats and unfamiliar garments that lurked in the closets. There was a horrible picture of [Teddy Roosevelt](https://www.nobelprize.org/nobel_prizes/peace/laureates/1906/index.html) killing a bear. Very bloody. And there were black widow spiders waiting for us always, down in their funnel shaped webs in all the dark corners. It was a thrilling place during a thunderstorm and, like the hay loft of the barn, a place where my pre-adolescent sexuality concerning my cousin Judy, who was one month my senior, would come a little more sharply into focus. We were only nine or ten, but it was there already with it’s pressing curiosity. We sometimes kissed. My techniques have improved, but not the thrill.  When my great-grandmother died she was almost a hundred and we were glad to see her go because every time she would come over to my grandmother’s house, she would try to kiss all of us. She looked almost a hundred and, heartless, cruel, mindless little children that we were, she repulsed us. She grabbed us anyway and kissed us until she was through. They put her body in a metal casket with gauzy curtains and left it in the living room near the grandfather’s clock, which announced the hours with a number of resonant bongs and marked the half-hours with a single chilling tone. Her body was there for three days until the service on Sunday at Mt Zion Baptist Church. We dared each other to go in and look at her. The adults were unaffected and took their regular meals right in the next room. We found it difficult to sleep. The clock seemed more alive than usual.  My great-grandmother, as I learned from Judy much later, when we were adults, had been an unusual woman in Saw Mills, North Carolina. She lived just a bit on the wild side. She gave birth to my grandmother out of wedlock following an affair with a railroad man named Stowe. We never heard much about him. “Nanny”, as we called our great-grandmother, was tolerated by the community because she was the only person for miles around who knew the rudiments of medicine. She provided medical care to livestock, for which she had been trained, but also to people for whom she was the only alternative on her side of the Catawba River. She also ran the post office in Granite Falls. She was the first postmistress anyone had heard about, and rural North Carolinians at the time were not in the mood for new customs, but they accepted what they couldn’t avoid. And granite does fall.  When my grandfather, “Pop”, James Albert Barker, son of Cary Barker from Cary, N.C. decided to marry Nanny’s illegitimate daughter, Princess Escoe Miller, his father gave him a piece of land to farm and tolerated his choice of bride. My given name derives from Cary with a slight change of spelling that my mother thought practical so as to keep my initialed name from being the same as my Dad’s, C.B. Mullis. She probably never imagined that I would be living far away before it ever mattered.  The rest of my life has passed quite suddenly. Around ten or twelve I fell into the inevitable logarithms of time. It seems to go faster and faster. I wonder now why we have to have Christmas so often.  I went to high school in Columbia. I met my first wife, Richards, whom I married while I was working on a B.S. in chemistry at Georgia Tech. She bore Louise and I studied. I learned most of the useful technical things, math, physics, chemistry, that I now use, during those four years. I did little else, except to play with Louise and change her diapers at night. We moved to Berkeley, California in 1966. I did my Ph.D. in biochemistry under J.B. Neilands and there I learned the rest, the non-technical things. After that, it happened so quickly that it’s hard to really talk about in the wake of my grandparents’ farm.  Except for Cynthia and our boys.  I met Cynthia while I was in Kansas for three years. She’s the very special daughter of an old grain trading family and a pathologist, David Gibson. Cynthia encouraged me to write and brought Christopher and Jeremy into the world. I left her, some say foolishly, when we were living in California in about 1981.  I was working for Cetus, making oligonucleotides. They were heady times. Biotechnology was in flower and one spring night while the California buckeyes were also in flower I came across the polymerase chain reaction. I was driving with Jennifer Barnett to a cabin I had been building in northern California. She and I had worked and lived together for two years. She was an inspiration to me during that time as only a woman with brains, in the bloom of her womanhood, can be. That morning she had no idea what had just happened. I had an inkling. It was the first day of the rest of my life.  From there it’s a single sentence. I worked as a consultant, got the Nobel Prize, and have now turned to writing. It is 1994.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1993*, Editor Tore Frängsmyr, [Nobel Foundation], Stockholm, 1994  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 1993 **Addendum, August 1999** And then early in the spring of 1997 there was Nancy and my whole heart began to unfold and everything else before seemed like a long dream from which I had awakened at last. Married: Nancy Lier Cosgrove, San Francisco, CA March 21, 1998.  Copyright © The Nobel Foundation 1999 **Addendum, April 2005** One of the nice things about being honored with a Nobel Prize is that it is a ticket into any office once. There’s almost no one you might want to see who refuses to see you just once on the basis of your Stockholm credentials alone. After that you’re on your own. People also invite you to visit their campus or meeting, and talk. I warm to a microphone and a crowd. I like to travel, so Nancy and I have been able to see the world, and there’s always someone there to meet us at the airport and take care of us in their hometown. What a deal. I get tired of talking about the polymerase chain reaction, but I read a lot, and think a lot, and I can talk about almost anything. Being a Nobel laureate is a license to be an expert in lots of things as long as you do your homework.  In the last two years, my long travel holiday has fallen partial victim to an idea I started thinking about several years ago and lately started working on for real. It is a method using specific synthetic chemical linkers to divert an immune response from its nominal target to something completely different which you would right now like to be temporarily immune to. Let’s say you just got exposed to a new strain of the flu. You’re already immune to alpha-1,3-galactosyl-galactose bonds. All humans are. Why not divert a fraction of those antibodies to the influenza strain you just picked up. A chemical linker synthesized with an alpha-1,3-gal-gal bond on one end and a DNA aptamer devised to bind specifically to the strain of influenza you have on the other end, will link anti-alpha-Gal antibodies to the influenza virus and presto, you have fooled your immune system into attacking the new virus.  DARPA officials let me into their offices one time with this idea and before long I was a practicing immunologist. It’s not too far from being a synthetic DNA chemist if you don’t mind reading a strange new language for awhile. The concept is actually working now with rodents and their diseases. Hopefully it’s going to work in humans. I’ve started a little company called Altermune to coordinate the work, which is happening in several research labs, whose directors made the fatal mistake of letting me into their offices once. It is an interdisciplinary project, requiring chemists, immunologists and infective disease people. It also gives me something exciting and new to talk about when I take time off and indulge my old habit of traveling and talking.  For more biographical information, see: Mullis, Kary, *Dancing Naked in the Mind Field*. Pantheon Books, New York, 1998.  *Kary B. Mullis died on 7 August 2019.* |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** | 378 |
| Q2 | The question is, of course, when you were out driving the car and you realised that you’d cracked it, you know that was it, you know you’d got it, I mean, what was it that made it and what was it that led you to that? |
|  | Kary B. Mullis: Well, it was, you know, it was typical of a really interesting development in that I was looking for something else, and PCR was the possible outcome of a solution to a hypothetical problem that didn’t really exist. I mean, I was working on trying to sequence the single base pair, you know, I think they’re called SNPs today, single nucleotide polymorphisms, because those were medically important and I was trying to do it with all nuclear tides because I ran a lab that made them and we had really improved the efficiency with which we made them over the three or four years prior, so that we could make them a lot faster than the company, Cetus, that I worked for could use them.  I had seven people working for me and I was thinking, well I’m either going to have to cut the staff down to like about three, because now we’ve got these little automated devices that will do that, or I’m going to have to increase the demand for oligonucleotides and so I started thinking of what else can you do with them. And I thought it’s possible that you can make a rapid clinical essay for single base polymorphisms like sickle cell anaemia was a good example. That took, back then, maybe going to a clinic and take a sample and maybe three weeks later you’d know, which is a lot of agonising, kind of, and I thought, you know, it would be nice to have that in one shift in a hospital, kind of go in and they’d let you know, and I thought oligonucleotides maybe were the answer there, but I was a chemist, I still am a chemist, and I really didn’t have an appreciation for the hugeness of the human genome compared to, say, a 5,000 based plasma that I was using as a sort of a model system. And I was thinking of this method that would require a couple of oligonucleotides and one of them would sort of be a control, but they would be pointed, like, toward each other just like in a PCR reaction and I was thinking of how am I going to get rid of the excess of the axinucleotide triphosphates or something.  There was a couple of technical problems I was trying to solve and one of them, the way I decided to solve it, was to like run a sort of sham reaction first with the oligonucleotides in place and the sample and then I add my radioactive traces and stuff and run it again and this was going to use up the … it was just not for the idea at first, duplicating the signal, but then I realised the side effect of that was going to be that I was going to double the signal if there were enough, like, the axinucleotides available. I was trying to get rid of those if there were any coming in with the sample. |
| Q1 | Do you think, a danger for many young students and scientists today? They have brilliant idea, brilliant minds and then, you know, particularly today when so much is driven by commercial interest, that if they’re not careful, they might lose out? |
|  | Kary B. Mullis: Well, you know, if you really are in it for the money then science is not the place, because generally speaking, you’re not going to get filthy rich being a scientist. I mean, once in a while some people do but it’s more likely that you’ll end up being a professor in a university, it’s not a bad job. But, you know, industrial scientist isn’t all that bad even though they won’t grant you the rights to some, maybe, you know, most people are not going to advance something that’s going to make a lot of money anyway, you know, so these days I would say if I was a student, I’d probably do just about the same things. I like the industrial kind of, like at Cetus they did have some good things. One of the good things was that they said 10% of your time you should do whatever you want to do.  That was a sort of, most people didn’t pay much attention to that but I said, you know, it’s hard for them to measure what 10% of my time is so I can use as much of my time as I want to for my own sort of just curiosity and that was a nice thing and then if you wanted some piece of equipment or something, you didn’t have to wait two years, like, for a government grant to come through and get funding and all, you know, there’s a lot of red tape involved in government for your … and stuff, which I’m dealing with now because the nice thing about a government grant is you keep everything yourself. I mean, if you come up with something interesting you patent it personally and so I’m doing that now and I’m going to probably be looking for, actually I’m not looking, I’m sort of screening investors now because I’ve decided to take some private money. |
| Q14 | You’re working with research now in an institute with regard to children’s health, am I correct? |
|  | Kary B. Mullis: I have a position at the Children’s Hospital Oakland Research Institute, but that’s one of the places where research is going on on this idea that I had some time ago about how to design drugs that would engage an already present immune response that a person had. Like, you’re immune to a lot of things already and when you get a new disease, it would be nice to just tap into a present immunity with a pharmaceutical that would take that immune response that you’d already made for one thing and turn it against the pathogen that you’d just been infected with. That sort of like would be the ideal way rather than having to say new pathogen, it’s going to probably take 11 or 12 days for me to get immune to it, my immune system has to figure out if this thing is actually bad or not and then it’s got to figure out how to deal with it and all of that takes a long time and sometimes the pathogen wins, like a really serious case of an influenza, like the kind of thing that happened in 1918. That killed about 30 million people in 18 months. |
| Q14 | May I ask how far you’ve got into this research? |
|  | Kary B. Mullis: Well, we’ve cured a couple of diseases in mice and rats and we’re now going to try the approach in, we’ve got these kind of mice that are minus the enzyme that makes this thing called alphagalapatose, they’re kind of like people in that regard. We don’t make a particular kind of chemical and we’re immune to it. It’s probably something that happened maybe 25 million years ago because chimps and orangutans are the same way. We’re allergic to just anything that makes up a one-three alphagalbon which is probably real common in pests that we started eating somewhere 25 million years ago so we started being immune to it and I’ve got some mice now from somebody in Australia made that have that same thing, so they can be immune to it, so they’re kind of like, otherwise there’d be these experiments on chimps. So we do them in mice and we’re going to try to protect mice against the form of the flu that was prevalent like in 1957, the Asian flu they call it or something like that, or the Hong Kong flu and if it works there, then we’ll go to rather a deadly strain of the flu that’s now circulating in southern China, in Vietnam and Thailand. |
| Q14 | May I ask if your research would have any implication on the race that we have now for finding some kind of vaccine for the HIV and AIDS epidemic? |
|  | Kary B. Mullis: It would be perfectly useful. It’s not like a vaccine in the sense that it doesn’t make a new response. It takes one that’s already there and using … |
| Q9 | I would like to come back to the issue of the award, the Nobel award. Was that a kind of, for you, having done the discovery you made and then when you got the prize, was that some kind of feeling of justification but also that your knowledge and your idea was finally acknowledged and that you got some kind of, you know, payback after having …? |
|  | Kary B. Mullis: There is nothing like getting the Nobel Prize. I mean, there’s just nothing quite like that as far as I’m concerned. By 1993, I didn’t need any confirmation that this was a useful thing because every time I’d pick up *Science* or *Nature* or *Cell* or any scientific kind of magazines, there’s not only articles that are using the technique but there’s full colour, full page ads for the equipment to do it with. It’s pretty hard these days even to find an issue, in fact you couldn’t find an issue with *Science* or *Nature* or *Cell* or, you know, PNAS or anything that didn’t have lots of people using PCR. |
| Q9 | But did you wait for it? Did you think, ok, one day I will get it, or did you think, ok, no? |
|  | Kary B. Mullis: No, I really didn’t think that much about the Nobel Prize except for the fact on the night, then I thought ah yes. I told my girlfriend. I said, you know, this is the kind of thing you get the Nobel Prize for. This will change DNA chemistry, you know, for a long time and she didn’t take it very seriously but I didn’t think of it too much after that until years later somebody, my mentor when I was in graduate school, Joe Neilands called me one day and he said something like if you’ll start talking to the press about all kinds of issues and stuff, and I was starting to talk about AIDS … because they were using PCR to detect it, the HIV molecule, and I was going to a lot of those meetings and I was thinking these guys are on the wrong track and they’ve got blinders on them in a sense, he said if you just stop that, you know, you’re probably going to get the Nobel Prize but they don’t have to give it to you until you’re dead; I mean, they don’t have to give it to you until right before you’re dead, so make it easier on yourself and just, I said you wouldn’t stop talking about something that you thought was important, would you Joe? And I knew he wouldn’t because he had been a real activist against Vietnam and that kind of stuff and that was the one thing I learned from him, was that scientists had a responsibility. |
| Q15 | Do you feel that after you got the Prize, that you have had to, I mean, is it more responsibility, not just as a scientist but also as a Laureate, to bring up issues of importance, things that you, I mean, environment issues or other issues that you feel very strongly about? |
|  | Kary B. Mullis: I don’t think I’ve felt any more responsibility. I’d already felt some, you know, but I don’t feel, for instance, like it’s necessary for me to get involved in things that I don’t know anything about and have an opinion on those things. Like, I noticed in the symposium the other day here where every single member of this channel talking about climate change and stuff, introduced himself as saying, well I don’t know much about this, I’m not an expert in this field. I would feel kind of funny standing up and saying, I don’t really know anything at all about AIDS, I haven’t read any of the papers and stuff, but here’s my ideas. Like, who cares? You know, on that issue, I said I’ve read all the literature, I’ve read more of the literature than most of the people that I’ve listened to at this meeting and most of the people who consider themselves to be experts haven’t really spent the time reading. I love to read, so there are some things I have opinions about. |
| Q1 | What do you, when [young people] come up to you and ask you what field to get into, what would you say? What would you advise them? |
|  | Kary B. Mullis: What interests you now, you know. If you don’t really like it, there’s no real reason to go into it and there are a lot of people that like it. If you like, you know, more chemical kind of things, get into the biochemistry kind of stuff. If you like dealing with more biological stuff, that’s, you know, I mean, this sounds stupid but, you know, you have to follow your own interests. If I really was doing what I’m doing now because I felt like I should or it was helpful for the world or anything like that, that would not be sufficient reason to sit there and every time that, you know, there’s four or five journals that come to my house, some of them once a week, some of them once a month, but I read them all the way through and that takes a lot of effort and then I try to digest that and then I write research plans and stuff that needs to have absorbed that kind of stuff. It’s a lot of effort and I do it for fun now. I don’t do it for, it would take a lot of … I don’t know, if you don’t really like science, it would be a dumb idea to go into it. There’s a lot of other things out there to do. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0379 |
| **Biographical** | I was born on April 26th, 1932 at 65 St. Heliers Road, South Shore, Blackpool, England in the house of my maternal grandmother, Mary Martha Armstead, having been delivered by the District Nurse, Ms. Parkinson, a lady who I can remember from my infant and juvenile days in her uniform and navy blue raincoat on her bicycle doing her rounds and visiting schools for health inspections. My parents, Mary Agnes Smith and Rowland Smith, both had to work since their early teens, she in the holiday boarding house of her mother and he in his father’s market garden in Marton Moss, a village on the south side of Blackpool, just north of Saint Anne’s-on-Sea. I went to the local school, Marton Moss Church of England School for 6 years from the age of 5. My mother attended the local church, Saint Nicolas, and consequently I attended that church and its Sunday School. My only prizes from the Sunday School were “for attendance”, so I presume my atheism, which developed when I left home to attend university, although latent, was discernible.  During my last year at elementary school, 1943, I sat for the “Elevenplus” examination which was used in the English schools in those days. In principle, of course, it was an invidious system designed to identify the approximately 20% of the school population that would be offered an academic education and the 80% who would be obligated to take a secondary education that terminated with no further academic options at age 15 (of course, there was the alternative of private schooling, but that was not an option if you were the child of poor parents, as was I). I was lucky enough to obtain a scholarship to the local private school, Arnold School. I did not, at the time, consider this to be luck. I did not want to go to Arnold School because the pupils were considered to be snobs and I thought that I would be ostracized by my friends in Marton Moss. Luckily, my mother insisted, and I went to Arnold School. I cannot say it was the happiest time in my life (I was no good at sports, and proficiency in sport is important in private school life. And I hated the war-time meals that were provided at lunch, as well as the prefect who insisted that I eat the awful food). But the schooling was first-rate, and in this I flourished, although not equally well in all subjects. Clearly, science was my metier, and I was lucky to have a chemistry teacher, Sidney Law, who stimulated my interest in chemistry and who took a personal interest in me (he told me I should read a better newspaper than the one to which my parents subscribed and, as a consequence, I became a life-long reader of the Manchester Guardian. That, in turn, stimulated me to become a reader of the New Yorker as soon as I came to North America, another life-long addiction).  The seven years from 1943 to 1950 were also a time when I became a boy scout. That was a piece of luck. The headmaster at Arnold School, Mr. Holdgate, at the end of my first term, sent me to a dentist, Mr. Paterson, in the hope that he could correct my protruding front teeth, about which I had been teased by my schoolmates. Mr. Paterson did not correct the problem with my teeth but he did introduce me to a wonderful scoutmaster, Mr. Barnes, who inducted me into the happy world of camping and outdoorsmanship which provided me with enjoyment and vacations throughout my secondary school years and right up to the present. An enjoyment that explains why I have a particular delight in living amid the rugged outdoors and beauty of British Columbia.  The second World War impinged on the lives of many of us who were alive at the time. Blackpool, as it turned out, was a very safe place, being in the northwest of England and distant from the targets for German bombing. The large number of hotels and boarding houses in this seaside resort were used to house military trainees, mainly for the airforce. And my father, working his father’s market garden, grew primarily food crops rather than his preference, chrysanthemums. Occasionally, bombers, presumably diverted from their primary targets of Manchester and Liverpool, would try to bomb the new factory behind our house that produced Wellington bombers. Usually, they hit the market gardeners’ greenhouses which showed up better at night. And I remember one night, alone in the house with my baby brother Robin, when a stick of bombs fell on either side of the house.  I was not proficient in Latin and so was not able to go to Oxford or Cambridge. However, I did enter the first-rate chemistry honours program at the University of Manchester in 1950, where the professors were E.R.H. Jones and M.G. Evans, and graduated in 1953, with the financial support of a Blackpool Education Committee Scholarship. I had hoped to get a firstclass degree, but only got a 2(i)! I was very disappointed. However, I still was able to obtain a State Scholarship which supported me throughout my graduate studies until I finished my Ph.D. degree in 1956. My supervisor was H.B. Henbest. He was an outstanding young organic chemist, and I was glad to have him as a supervisor of my work on cyclohexane diols. However, we did not have a particularly warm relationship. I was socially shy and moody and was probably quite hard to understand.  The last year of our graduate studies saw me and my classmates writing to various American professors seeking post-doctoral fellowships. I had no luck in obtaining my desire of a fellowship on the west coast of the United States, but I heard, in the summer of 1956, that a young scientist in Vancouver, Canada, [Gobind Khorana](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1968/index.html), might have a fellowship to work on the synthesis of biologically important organo-phosphates. While I knew this kind of chemistry was much more difficult than the cyclohexane stereochemistry in which I was trained, I wrote to him and was awarded a fellowship after an interview in London with the Director of the British Columbia Research Council, Dr. G.M. Shrum.  I arrived in Vancouver in September 1956. My first project was to develop a general, efficient procedure for the chemical synthesis of nucleoside-5′ triphosphates based on the synthesis of ATP by Khorana in 1954. This study led to more extensive investigations of the reactions of carbodumides with acids, including phosphoric acid esters and to a general procedure for the preparation of nucleoside-3′,5′ cyclic phosphates, a class of compounds whose existence and great biological significance had only recently been discovered. One particular pleasure of that period was the development of the methoxyl-trityl family of protecting groups for nucleoside-5′-hydroxyl groups (one synthesis of trimethoxytritanol erupted and left a large orange stain on the laboratory ceiling); this class of protecting group is still in use in modern automated syntheses of DNA and RNA fragments.  In 1960, the Khorana group, including myself, newly married (I have three children, Tom, Ian and Wendy. My wife Helen and I separated in early 1983), moved to the Institute for Enzyme Research at the University of Wisconsin. There I worked on the synthesis of ribo-oligonucleotides, that most challenging of chemical problems for a nucleic acid chemist. Early in 1961, I began to realize that it was time to move on. Helen and I wanted to return to the West Coast of North America, and I accepted a position with the Fisheries Research Board of Canada Laboratory in Vancouver. I enjoyed my time there because of the opportunity it presented to learn about marine biology and I was able to sustain my interest in nucleic acid chemistry because of the award of a U.S. National Institutes of Health Grant, which led to a new synthetic method for nucleoside-3′,5′ cyclic phosphates. However, the atmosphere of the laboratory, although based on the campus of the University of British Columbia, was not really conducive to, or supportive of, academic research. Hence, in 1966, I was very glad that Dr. Marvin Darrach, then Head of the Department of Biochemistry, offered to nominate me for the position of Medical Research Associate of the Medical Research Council of Canada. This award, which provided salary support, allowed me to become a faculty member of the Department, my academic home ever since, except for sabbaticals at Rockefeller University, the Laboratory of Molecular Biology of the Medical Research Council in Cambridge and Yale University. The Council also has provided research grant support throughout my academic career.  In 1981, Ben Hall and Earl Davie, of the University of Washington, invited me to be a scientific cofounder of a new biotechnology company, Zymos, which was funded by the Seattle venture capital group, Cable and Howse. One of the first contractors was the Danish pharmaceutical company, Novo, who asked Zymos to develop a process for producing human insulin in yeast. After a considerable cooperative effort by Zymos and Novo researchers a successful process was developed. In 1988, the pharmaccutical company, now Novo-Nordisk, purchased outright the biotechnology company, now named ZymoGenetics. I am pleased that, although I no longer have any involvement, ZymoGenetics has subsequently expanded and has continued research on a wide variety of potential protein pharmaceuticals.  In 1986, I was asked by the then Dean of Science at the University of British Columbia, Dr. R.C. Miller, Jr., to establish a new interdisciplinary institute, the Biotechnology Laboratory. I decided that it was time for me to start paying back for the thirty years of fun that I had been able to have in research. I have very much enjoyed recruiting and helping to get established the group of young faculty members that constitute the core of the Biotechnology Laboratory. I also have enjoyed being Scientific Leader of the National Network of Centres of Excellence in Protein Engineering that was funded in 1990. It has been very satisfying, in this case, to see established scientists, working in the various subdisciplines of biochemistry, come together in nation-wide collaborations to solve important problems in protein structure-function analysis and to work with Canadian industry in improving technology transfer which has been less than optimal in the past.  One difflcult chore was presented to me in 1991 when I became Acting Director of the Biomedical Research Centre, a privately funded research institute on the Campus of the University of British Columbia. Its source of funding disappeared; therefore I had the responsibilities of managing the Centre on a tight budget, negotiating future funding from the Provincial Government, and helping to ensure the transfer of ownership to the University. This task was made difficult because many of the staff had been led to believe that I was trying to take over and subvert the activities of the Centre. This misguided belief helped the problems of the Centre to become a public political football in an election year. However, funding was negotiated, the University took over the ownership and I was able to step down after 12 months with the Centre and its mission intact.  I look forward to shedding all my administrative responsibilities in another couple of years and returning to my first scientific love, working at the bench and having more time for sailing and for skiing. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0380 |
| **Biographical** | My first encounters with McGill University came when I was still in a baby carriage. My mother used to wheel me about the campus when we lived in that neighborhood and, as she recounted years later, she would tell me that I would go to McGill. There was some precedent for my going there, since two of my father’s brothers received their M.D.’s at McGill.  I have always loved going to school. Since neither of my parents had a higher education, my academic “idols” were these two paternal uncles and one of their uncles, my great-uncle, Henrik Steen (né Markus). My admiration for him, living in faraway Sweden, was not because of a teol.dr. (which he received from the University of Uppsala in 1915) nor because of the many books he wrote – I knew nothing of that – but rather because he was reputed to speak 13 languages. I learned decades later that the number was only 9! Growing up, mostly in Montreal, I was an only child of loving parents. I admired my father’s athletic prowess – he excelled in several sports – and my mother’s expressive singing and piano playing.  My interest in the sciences started with mathematics in the very beginning, and later with chemistry in early high school and the proverbial home chemistry set. My education at Baron Byng High School was excellent, with dedicated masters (boys and girls were separate). I spent the next years at McGill University, for both undergraduate and, as was the custom of the time, graduate study. Our graduate supervisor, Carl A. Winkler, specialized in rates of chemical reactions. He himself had received his Ph.D. as a student of [Cyril Hinshelwood](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1956/hinshelwood-facts.html) at Oxford. Hinshelwood was later the recipient of the Nobel Prize for his work on chemical kinetics. Winkler brought to his laboratory an enthusiastic joyousness in research and was much loved by his students.  During my McGill years, I took a number of math courses, more than other students in chemistry. Upon receiving a Ph.D. from McGill University in 1946, I joined the new post-doctoral program at the National Research Council of Canada in Ottawa. This program at NRC later became famous, but at the time it was still in its infancy and our titles were Junior Research Officers. The photochemistry group was headed by E.W.R. Steacie, an international figure in the study of free-radical reactions and a major force in the development of the basic research program at NRC. I benefitted from the quality of his research on gas phase reaction rates. Like my research on chemical reaction rates in solution at McGill (kinetics of nitration), it was experimental in nature. There were no theoretical chemists in Canada at the time, and as students I don’t think we ever considered how or where theories were conceived.  About 1948 a fellow post-doctoral at NRC, Walter Trost, and I formed a two-man seminar to study theoretical papers related to our experimental work. This adventure led me to explore the possibility of going on a second post-doctoral, but in theoretical work, which seemed like a radical step at the time. I had a tendency to break the glass vacuum apparatus, due to a still present impetuous haste, with time-consuming consequences. Nevertheless, the realization that breaking a pencil point would have far less disastrous consequences played little or no role, I believe, in this decision to explore theory!  I applied in 1948 to six well-known theoreticians in the U.S. for a postdoctoral research fellowship. The possibility that one of them might take on an untested applicant, an applicant hardly qualified for theoretical research, was probably too much to hope for. Oscar K. Rice at the University of North Carolina alone responded favorably, subject to the success of an application he would make to the Office of Naval Research for this purpose. It was, and in February 1949 I took the train south, heading for the University of North Carolina in Chapel Hill. I was impressed on arrival there by the red clay, the sandy walks, and the graciousness of the people.  After that, I never looked back. Being exposed to theory, stimulated by a basic love of concepts and mathematics, was a marvelous experience. During the first three months I read everything I could lay my hands on regarding reaction rate theory, including Marcelin’s classic 1915 theory which came within one small step of the Transition State Theory of 1935. I read numerous theoretical papers in German, a primary language for the “chemical dynamics” field in the 1920s and 1930s, attended my first formal course in quantum mechanics, given by Nathan Rosen in the Physics Department, and was guided by Oscar in a two-man weekly seminar in which I described a paper I had read and he pointed out assumptions in it that I had overlooked. My life as a working theorist began three months after this preliminary study and background reading, when Oscar gently nudged me toward working on a particular problem.  Fortunately for me, Oscar’s gamble paid off. Some three months later, I had formulated a particular case of what was later entitled by B. Seymour Rabinovitch, RRKM theory (“Rice-Ramsperger-Kassel-Marcus”). In it, I blended statistical ideas from the RRK theory of the 1920s with those of the transition state theory of the mid-1930s. The work was published in 1951. In 1952 I wrote the generalization of it for other reactions. In addition, six months after arrival in Chapel Hill, I was also blessed by marriage to Laura Hearne, an attractive graduate student in sociology at UNC. She is here with me at this ceremony. Our three sons, Alan, Kenneth and Raymond, and two daughters-in-law are also present today.  In 1951, I attempted to secure a faculty position. This effort met with little success (35 letters did not yield 35 no’s, since not everyone replied!). Very fortunately, that spring I met Dean Raymond Kirk of the Polytechnic Institute of Brooklyn at an American Chemical Society meeting in Cleveland, which I was attending primarily to seek a faculty position. This meeting with Dean Kirk, so vital for my subsequent career, was arranged by Seymour Yolles, a graduate student at UNC in a course I taught during Rice’s illness. Seymour had been a student at Brooklyn Poly and learned, upon accidentally encountering Dr. Kirk, that Kirk was seeking new faculty. After a subsequent interview at Brooklyn Poly, I was hired, and life as a fully independent researcher began.  I undertook an experimental research program on both gas phase and solution reaction rates, wrote the 1952 RRKM papers, and wondered what to do next in theoretical research. I felt at the time that it was pointless to continue with RRKM since few experimental data were available. Some of our experiments were intended to produce more.  After some minor pieces of theoretical study that I worked on, a student in my statistical mechanics class brought to my attention a problem in polyelectrolytes. Reading everything I could about electrostatics, I wrote two papers on that topic in 1954/55. This electrostatics background made me fully ready in 1955 to treat a problem I had just read about on electron transfers. I comment on this next period on electron transfer research in my Nobel Lecture. About 1960, it became clear that it was best for me to bring the experimental part of my research program to a close – there was too much to do on the theoretical aspects – and I began the process of winding down the experiments. I spent a year and a half during 1960-61 at the Courant Mathematical Institute at New York University, auditing many courses which were, in part, beyond me, but which were, nevertheless, highly instructive.  In 1964, I joined the faculty of the University of Illinois in Urbana-Champaign and I never undertook any further experiments there. At Illinois, my interests in electron transfer continued, together with interests in other aspects of reaction dynamics, including designing “natural collision coordinates”, learning about action-angle variables, introducing the latter into molecular collisions, reaction dynamics, and later into semiclassical theories of collisions and of bound states, and spending much of my free time in the astronomy library learning more about classical mechanics, celestial mechanics, quasiperiodic motion, and chaos. I spent the academic year of 1975-76 in Europe, first as Visiting Professor at the University of Oxford and later as a Humboldt Awardee at the Technical University of Munich, where I was first exposed to the problem of electron transfer in photosynthesis.  In 1978, I accepted an offer from the California Institute of Technology to come there as the Arthur Amos Noyes Professor of Chemistry. My semiclassical interlude of 1970-80 was intellectually a very stimulating one, but it involved for me less interaction with experiments than had my earlier work on unimolecular reaction rates or on electron transfers. Accordingly, prompted by the extensive experimental work of my colleagues at Caltech in these fields of unimolecular reactions, intramolecular dynamics and of electron transfer processes, as well as by the rapidly growing experimental work in both broad areas world-wide, I turned once again to those particular topics and to the many new types of studies that were being made. Their scope and challenge continues to grow to this day in both fields. Life would be indeed easier if the experimentalists would only pause for a little while!  There was a time when I had wondered about how much time and energy had been lost doing experiments during most of my stay at Brooklyn Poly- experiments on gas phase reactions, flash photolysis, isotopic exchange electron transfer, bipolar electrolytes, nitration, and photoelectrochemistry, among others-and during all of my stay at NRC and at McGill. In retrospect, I realized that this experimental background heavily flavored my attitude and interests in theoretical research. In the latter I drew, in most but not all cases, upon experimental findings or puzzles for theoretical problems to study. The growth of experiments in these fields has served as a continually rejuvenating influence. This interaction of experiment and theory, each stimulating the other, has been and continues to be one of the joys of my experience.  Honors received for the theoretical work include the Irving Langmair and the Peter Debye Awards of the American Chemical Society (1978, 1988), the Willard Gibbs, Theodore William Richards, and Pauling Medals, and the Remsen and Edgar Fahs Smith Awards, from various sections of the ACS, (1988, 1990, 1991, 1991, 1991), the Robinson and the Centenary Medals of the Faraday Division of the Royal Society of Chemistry (1982, 1988), Columbia University’s Chandler Medal (1983) and Ohio State’s William Lloyd Evans Award (1990), a Professorial Fellowship at University College, Oxford (1975 to 1976) and a Visiting Professorship in Theoretical Chemistry at Oxford during that period, the Wolf Prize in Chemistry (1985), the National Medal of Science (1989), the Hirschfelder Prize in Chemistry (1993), election to the National Academy of Sciences (1970), the American Academy of Arts and Sciences (1973), the American Philosophical Society (1990), honorary membership in the Royal Society of Chemistry (1991), and foreign membership in the Royal Society (London) (1987) and in the Royal Society of Canada (1993). Honorary degrees were conferred by the University of Chicago and by Goteborg, Polytechnic, McGill, and Queen’s Universities and by the University of New Brunswick (1983, 1986, 1987, 1988, 1993, 1993). A commemorative issue of the Journal of Physical Chemistry was published in 1986. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** | 380 |
| Q12 | Many Laureates actually are sons and daughters of immigrants. Did they encourage you to go into higher studies? |
|  | Rudolph A. Marcus: I think that was the sort of atmosphere in my family. Now, I’m not sure if you’d regard the following as encouragement, but when I was a baby in a carriage, we lived near McGill University and my mother used to wheel me around McGill University and tell me that I would be going to McGill University. I suppose that’s a form of encouragement. |
| Q2 | After McGill, what happened next? |
|  | Rudolph A. Marcus: After McGill, there was a chance of applying for a fellowship to go to France and of course that would give one a chance to see Europe. It’s true, I’d been to Europe before, because when I was six months old my mother took me to see her parents and her family but I didn’t remember much from that time. So this would’ve been a chance to see Europe but somehow I felt I was kind of frivolous, you see, and so when somebody came down from the National Research Council of Canada, which was just beginning a post-doctoral programme, the NRC, as you know, in Ottawa, just beginning the post-doctoral programme, came down to seek people to apply for fellowships.  It was before the fellowship programme so we were called junior research officers you see, but we were doing the same thing that later on fellows did and so that was such a natural thing, so I sort of just drifted. In fact, I think all along, up to my first few years there, it had really been just sort of flowing, drifting and not sort of thinking great things or what have you, just flowing naturally. I mean, for example, there was no question I was going to be in science earlier on, but there was a question was it going to be math and that terrible statics in physics? Or whether it was going to be chemistry, so it ended up being chemistry but of course eventually it was the kind of chemistry where I was able to draw on physics, and that played a big role, androns, a little mathematics. |
| Q14 | There is one special thing about today because today, the 26th of June, the year 2000, there’s been a joint announcement by groups in the United States and in Britain and elsewhere about the first published, I would say almost draft, not completed, but the human genome is now sort of put officially on paper, so that everyone can study it and read it and look at it. And there has been even a joint discussion between Bill Clinton and Tony Blair about this event and so I can say that this is a major event in the history of mankind, actually, not just science. Would you like to comment about this? |
|  | Rudolph A. Marcus: Yes, of course everything in that field is far from my own, but one thing has been clear over the years, is how much biology is drawing upon chemistry and physics so that in a sense biology has become so much more molecular and now with the human genome project and the business of understanding the genes and understanding disease on a molecular basis, you know, I think the future for young people is just immense. I mean, I know a little bit about certain narrow corners of biology and I’m excited by those corners, so imagine with a broad area.  One of the wonderful things, and of course the Nobel committee has recognised this, one of the wonderful things is how some of these complex structures had become known, determined and of course to understand some of these biological processes, or understand any chemical reaction, you have to know something about structure as a starting point. Now in chemistry, for simple molecules, that’s well understood. I mean, automatically we assume we know what the starting structures are and the final; we don’t know the intermediates, that we learn about but the starting we don’t. Well before this, in the biological area, as you know, not much was known in detail about structure but now so much is being known. Like, for example, the prize on photosynthesis and how that fitted into the detailed studies of reaction mechanisms involving photosynthesis, or the transfer of energy that’s picked up from the sunlight to this photosynthetic reaction centre, the structural information there, or very recently with the prize for the ATP synthase, how nature’s smallest rotary engine, I mean what a wonderful device, what an exciting device to have in one little sort of unit. An ion current going across, an electric field being applied, something rotating and in the process forming ATP, which is such an important agent. And, of course, these rotary motors and linear motors are sort of ubiquitous so I mean there’s surely plenty of excitement.  I know there are already quite a few experiments, of course, but I think that things are just opening up. What we have learned so far at the basic level for basic understandings has so many potentialities for applying to these more complex systems. You know, are we going to be able to use the same rules or is there going to be a kind of simplification, for example, of protein folding? Is somebody going to come out with something which says, ah, you don’t need everything at this complete level of detail, but this is the broad outline and if you understand that, you have understood a … I mean, I have no idea, it’s not my area, I don’t know, but I think there’s so many possibilities. Of course, that’s one area, ok? And certainly the genome is going to expand it into molecular medicine, which is already very active, but of course there are other frontiers where physics and chemistry and mathematics meet, you know, and construct all sorts of the nanostructures, molecular computers eventually, goodness knows what. I think it’s exciting for young people to have these opportunities, but of course there’s been excitement for a long time. Many of us were excited by it. Yes. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0381 |
| **Biographical** | I was born 1933 in Winterthur, Switzerland, where our ancestors resided at least since the 15th century. We lived in a home built in 1898 by my grandfather, a merchant. My father, Robert Ernst, was teaching as an architect at the technical high school of our city. I had the great luck to grow up, together with two sisters, in a town that combined in a unique way artistic and industrious activities. Invaluable art collections and a small but first rank symphony orchestra carry the fame of Winterthur far across the borders of Switzerland. On the other hand, industries producing heavy machinery, like Diesel motors and railway engines, provided the commercial basis of prosperity.  I soon became interested in both sides. Playing the violoncello brought me into numerous chamber and church music ensembles, and stimulated my interest in musical composition that I tried extensively while in high school. At the age of 13, I found in the attic a case filled with chemicals, remainders of an uncle who died in 1923 and was, as a metallurgical engineer, interested in chemistry and photography. I became almost immediately fascinated by the possibilities of trying out all conceivable reactions with them, some leading to explosions, others to unbearable poisoning of the air in our house, frightening my parents. However, I survived and started to read all chemistry books that I could get a hand on, first some 19th century books from our home library that did not provide much reliable information, and then I emptied the rather extensive city library. Soon, I knew that I would become a chemist, rather than a composer. I wanted to understand the secrets behind my chemical experiments and behind the processes in nature.  Thus, after finishing high school, I started with high expectations and enthusiasm to study chemistry at the famous Swiss Federal Institute of Technology in Zurich (ETH-Z). I was rapidly disappointed by the state of chemistry in the early fifties as it was taught at ETH-Z; we students had to memorize incountable facts that even the professors did not understand. A good memory not impeccable logic was on demand. The physical chemistry lectures did not reveal much insight either, they were limited just to classical thermodynamics. Thus, I had to continue, similar as in high school, to gain some decent chemical knowledge by reading. A book from which I learned a lot at that time was “Theoretical Chemistry” by S. Glasstone. It revealed to me the fundamentals of quantum mechanics, spectroscopy, statistical mechanics, and statistical thermodynamics, subjects that were never even mentioned in lectures, except in a voluntary and very excellent lecture course given by the young enthusiastic Professor Hans H. Günthard who had studied chemistry and physics in parallel.  It was clear to me, after my diploma as a “Diplomierter Ingenieur Chemiker” and some extensive military service, I had to start a PhD thesis in the laboratory of Professor Günthard. Fortunately, he accepted me and associated me with a young most brilliant scientist Hans Primas, who never went through any formal studies but nevertheless acquired rapidly whatever he needed for his work that was then concerned with high resolution nuclear magnetic resonance (NMR), a field in its infancy at that time. Much of his and also my time was spent on designing and building advanced electronic equipment for improved NMR spectrometers. In parallel, we developed the theoretical background for the experiments we had in mind as well as for the optimum performance of the instruments. Signal-to-noise ratio calculations and optimizations were daily routine as NMR suffers from a disappointingly low sensitivity that severely limits its applications. Hans Primas developed and analyzed field modulation techniques, constructed a field frequency lock system, and contributed a new design of shaped pole caps for the electromagnet that was supposed to deliver an extremely homogeneous magnetic field. These developments led to two types of spectrometers that were adopted by Trüb-Täuber, a Swiss electronics company, and sold all over Europe. Later in 1965, Trüb-Täuber was dissolved, and the NMR spectroscopy section led to the foundation of Spectrospin AG that is, together with Bruker Analytische Messtechnik, nowadays the world leading producer of NMR spectrometers.  My own work dealt with the construction of high sensitivity radio frequency preamplifiers and in particular high sensitivity probe assemblies, initially for a 25 MHz, later for a 75 MHz proton resonance spectrometer. On the theoretical side, I was concerned with stochastic resonance. The goal set by Hans Primas was the usage of random noise for the excitation of nuclear magnetic resonance, following the famous concepts of Norbert Wiener for the stochastic testing of non-linear systems. The theoretical treatment was based on a Volterra functional expansion using orthogonal stochastic polynomials. I tried in particular to design a scheme of homonuclear broadband decoupling to simplify proton resonance spectra. By applying a stochastic sequence with a shaped power spectral density that has a hole at the observation frequency, all extraneous protons should be decoupled without perturbing the observed proton spin. The theoretical difficulties were mainly concerned with the computation of the response to nonwhite noise. Experiments were not attempted at that time, we did not believe in the usefulness of the concept anyway, and I finished my thesis in 1962 with a feeling like an artist balancing on a high rope without any interested spectators.  I thus decided to leave the university forever and tried to find an industrial job in the United States. Among numerous offers, I decided for Varian Associates in Palo Alto where famous scientists, like Weston A. Anderson, Ray Freeman, Jim Hyde, Martin Packard, and Harry Weaver, were working along similar lines as we in Zürich but with a clear commercial goal in mind. This attracted my interest, hoping to find some motivation for my own work. And indeed, I was extremely lucky. Weston Anderson was on his way to invent Fourier transform spectroscopy in order to improve the sensitivity of NMR by parallel data acquisition. After his involvement in the development of a cute mechanical device, the “wheel of fortune”, to generate and detect several frequencies in parallel, he proposed to me in 1964 to try a pulse excitation experiment that indeed led to Fourier transform (FT) NMR as we know it today. The first successful experiments were done in summer 1964 while Weston Anderson was abroad on an extensive business trip. In this work I could take advantage in an optimum way of my knowledge in system theory gained during my studies with Primas and Günthard. The response to our invention was however meager. The paper that described our achievements was rejected twice by the Journal of Chemical Physics to be finally accepted and published in the Review of Scientific Instruments. Varian also resisted to build a spectrometer that incorporated the novel Fourier transform concept. It took many years before in the competitive company Bruker Analytische Messtechnik Tony Keller and his coworkers demonstrated in 1969 for the first time a commercial FT NMR spectrometer to the great amazement of Varian that had the patent rights on the invention.  Still at Varian, I was further extending my earlier work on stochastic resonance with the introduction of heteronuclear broadband decoupling by noise irradiation, the “noise decoupling” that led to a rapid development in carbon-13 spectroscopy. It has been replaced later by the much more efficient multiple pulse schemes of Malcolm H. Levitt and Ray Freeman using composite pulses.  Of major importance for the success of more advanced experiments and measurement techniques in NMR was the availability of small laboratory computers that could be hooked up directly to the spectrometer. During my last years at Varian (1966-68), we developed numerous computer applications in spectroscopy for automated experiments and improved data processing.  In 1968 I returned, after an extensive trip through Asia, to Switzerland. A brief visit to Nepal started my insatiable love for Asian art. My main interest is directed towards Tibetan scroll paintings, the so-called thangkas, a unique and most exciting form of religious art with its own strict rules and nevertheless incorporating an incredible exuberance of creativity.  Back in Switzerland, I had a chance to take over the lead of the NMR research group at the Laboratorium für Physikalische Chemie of ETH-Z after Professor Primas turned his interests more towards theoretical chemistry. Despite an initial lack of suitable instrumentation, I continued to work on methodological improvements of time-domain NMR with repetitive pulse experiments and Fourier double resonance. In addition, we performed the first pulsed time-domain chemically-induced dynamic nuclear polarization (CIDNP) experiments. We developed at that time also stochastic resonance as an alternative to pulse FT spectroscopy employing binary pseudo-random noise sequences for broadband excitation, correlating input and output noise. Similar work was done simultaneously by Prof. Reinhold Kaiser at the University of New Brunswick.  The next fortunate event occurred in 1971 when my first graduate student, Thomas Baumann, visited the Ampere Summer School in Basko Polje, Yugoslavia, where Professor Jean Jeener proposed a simple two-pulse sequence that produces, after two-dimensional Fourier transformation, a two-dimensional (2D) spectrum. In the course of time, we recognized the importance and universality of his proposal. In my group, Enrico Bartholdi performed at first some analytical calculations to explore the features of 2D experiments. Finally in the summer of 1974, we tried our first experiments in desperate need of results to be presented at the VIth International Conference on Magnetic Resonance in Biological Systems, Kandersteg, 1974.  At the same time, it occurred to me that the 2D spectroscopy principle could also be applied to NMR imaging, previously proposed by [Paul Lauterbur](https://www.nobelprize.org/nobel_prizes/medicine/laureates/2003/index.html). This led then to the invention of Fourier imaging on which the at present most frequently used spin-warp imaging technique relies. First experiments were done by Anil Kumar and Dieter Welti.  From then on, the development of multi-dimensional spectroscopy went very fast, inside and outside of our research group. Prof. John S. Waugh extended it for applications to solid state resonance, and the research group of Prof. Ray Freeman, particularly Geoffrey Bodenhausen, contributed some of the first heteronuclear experiments. We started 1976 an intense collaboration, lasting for 10 years, with Professor Kurt Wüthrich of ETH-Z to develop applications of 2D spectroscopy in molecular biology. He and his research group have been responsible for most essential innovations that enabled the determination of the three-dimensional structure of biomolecules in solution.  During the following years, a large number of ingenious coworkers, in particular Geoffrey Bodenhausen, Lukas Braunschweiler, Christian Griesinger, Anil Kumar, Malcolm H. Levitt, Slobodan Macura, Luciano Müller, Ole W. Sørensen, and Alexander Wokaun, contributed numerous modifications of the basic 2D spectroscopy concept, such as relay-type coherence transfer, multiple quantum filtering, multiple quantum spectroscopy, total correlation spectroscopy, exclusive correlation spectroscopy, accordion spectroscopy, spy experiments, three-dimensional spectroscopy, and many more. In parallel, numerous other research groups contributed an even larger number of innovative methods.  Besides these activities in high resolution NMR, we always had a research program in solid state NMR going aiming at methodological developments, such as improved 2D spectroscopy techniques and spin diffusion, and applications to particular systems such as one-dimensional organic conductors, polymer blends, and dynamics in hydrogen-bonded carboxylic acids in collaboration with Thomas Baumann, Pablo Caravatti, Federico Graf, Max Linder, Beat H. Meier, Rolf Meyer, Thierry Schaffhauser, Armin Stöckli, and Dieter Suter.  More recently, I had also the pleasure to closely collaborate with Prof. Arthur Schweiger, an extremely innovative EPR spectroscopist, in the development of pulsed EPR and ENDOR techniques. This turned out to be a specially challenging field due to the inherent experimental difficulties and the many ways to overcome the problems.  In recent years, more and more of my time has become absorbed by administrative work for the research council of ETH-Z of which I am presently the president. I recognized that teaching and research institutions vitally depend on the involvement of active scientists also in management functions.  Looking back, I realize that I have been favored extraordinarily by external circumstances, the proper place at the proper time in terms of my PhD thesis, my first employment in the USA, hearing about Jean Jeener’s idea, and in particular having had incredibly brilliant coworkers. At last, I am extremely grateful for the encouragement and for the occasional readjustment of my standards of value by my wife Magdalena who stayed with me so far for more than 28 years despite all the problems of being married to a selfish work-addict with an unpredictable temper. Magdalena has, without much input from my side, educated our three children: Anna Magdalena (kindergarten teacher), Katharina Elisabeth (elementary school teacher), and Hans-Martin Walter (still in high school). I am not surprised that they show no intention to follow in my footsteps, although if I had a second chance myself, I would certainly try to repeat my present career. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** | 381 |
| Q10 | Dr Richard Ernst, welcome to this Nobel interview. You come from Zurich and from the very famous school there, University ETH where also [Albert Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/einstein-facts.html) have studied for about 100 years ago. But today you are very critical to the way the university function in the society. What are the major drawbacks in the university system? |
|  | Richard Ernst: I was just two days ago giving a lecture in Luleå in the north of Sweden and I was showing a transparency of the gentleman you just mentioned, Einstein, and he said it’s one of the greatest miracles of mankind that after 20 years sitting in a school bench one still can maintain some creativity, and I think that’s indeed one of the problems that we try just to listen and to accumulate knowledge without doing anything and some of the students, and also the professors, have lost contact with reality. I mean I might express it in a little bit too extreme way, but I think it’s a tendency and we should try to get more into contact with reality. |
| Q10 | What do you mean by contact with reality? |
|  | Richard Ernst: There are different aspects. First of all, I mean there’s the necessities of life so that we all end our research projects to what really mankind is needing the relation between North and South between the super-rich and the super-poor. That’s one aspect and the other aspect is that we also try to develop more responsibility for what is going on in the world. In the moment the world is so to say governed by politicians and by industry and both they have urgent problems to solve.  Today to survive for tomorrow and I think we need somebody who is looking more into the future on long term and I mean we have been granted freedom to do essentially what we want but whoever has acquired freedom he also has to accept responsibility. So we have essentially to do what is needed to be done and this is long range planning. Long range planning on a global scale that we really try to develop new concepts. How can we safely survive also in the future? |
| Q10 | So you mean this would be a task for a community of scientists all over the world? |
|  | Richard Ernst: Yes, exactly, that’s true. Not a single individual can do that. We don’t need specialists for that. Political science of course it’s important of futurology or whatever you’d like to call it that’s fine to have a few specialists but I think everybody should be concerned and one needs to collaborate and work together in order to develop new concepts and it means essentially that we have to work as a scientist on two levels. First of all we have to do our detailed work, we have to concentrate on our details and go as deep as we can into the ground, deep and deep to the foundations so that we have solid foundations to build our future on. But on the other side we should also work on an upper level and develop bits and foresight and have global views for what is going on in the world. So these two levels they should essentially work side by side. |
| Q18 | Why do you think that scientists can be, what do I say, better moral leaders in the world than other people? |
|  | Richard Ernst: I mean they are not … but again what I said before, politicians for example they are so much in their business and they can’t afford, I mean they have to survive personally in the next election for example and business people they have profits and the shareholder value is so important that they can’t have this long term vision so I mean universities are essentially left for taking such a job. Of course we are not particularly well suited for it perhaps because we are so specialists and have been trained as specialists but still I think we are those who should develop these ideas and especially because we are actually training the leaders of the future. |
| Q18 | What do other people, your colleague scientists, say when you tell them that you have to take more responsibility for the long term thinking in the world? |
|  | Richard Ernst: Yes, yes I mean they say it’s nice talking it’s easier to talk about it than to do it but that’s, I ‘m aware of that and they don’t completely disagree, but they try to ignore it. |
| Q5 | What about students? Are they broad minded? |
|  | Richard Ernst: That’s true, the students, if I talk like that in front of an audience than the students that would come to me and say “Oh that’s great and we thank you for these ideas” I mean that’s exactly how we feel. So the students they are very motivated for that. The professors they just see their own limits. But the students they would like to get more involved in that. You see it’s so difficult today to motivate young students to go into the sciences. Chemistry in particular. We don’t have enough chemistry students but I mean if one would combine the detailed science with the broader view I think we could also motivate bright young students with the broad mind to study natural sciences again. |
| Q3 | Why do you think that young students and people don’t go into sciences today? |
|  | Richard Ernst: I think one reason is that it’s become so specialised and so detached from the human interests. I mean if you are married it’s so difficult even to tell your wife what you’re doing and she might not care and might not understand and doesn’t want to understand. So I mean you need something in common with other people as well to be part of a community otherwise if you are just a group of specialists, of course I have friends everywhere in the world in different cities who understand me and who understand my language. But I mean you need also people like that in your immediate environment. |
| Q4 | Did you have any idea that this apparatus would work in this way? |
|  | Richard Ernst: Not really. At the beginning I didn’t see the power of it but I mean it was always my goal. It was my goal to develop something which could be used afterwards and in essence I’m not really what one would imagine to be a scientist who wants to understand the world. To understand how the basic principles of a human being, I was more interested in really designing tools. So I’m a toolmaker and not really a scientist in this sense and I wanted to provide other people these capabilities of solving problems. |
| Q3 | Was this the motivation for you to go into the university and study chemistry to be useful for the society? |
|  | Richard Ernst: Not really. I mean initially I once discovered in our old house a box of chemicals when I was 13 and I started to play with this box of chemicals and I survived and our house survived and so I became very interested in this phenomena, this strange phenomena of chemistry and at that time I really wanted to gain understanding. So at that time I had more scientific motivation. And for this reason I went into chemistry but later on I developed more in that direction of a toolmaker. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0382 |
| **Biographical** | My birth in July 1928 in Methuen, Massachusetts was followed just eighteen months later by the death of my father, Elias, a successful business man in that community 30 miles north of Boston. My mother, Fatina (née Hasham), changed my name from William to Elias shortly after my father’s passing. I do not remember my father, but all his friends and associates made it clear that he was a remarkably gifted and much admired person. I have always been guided by a desire to be a worthy son to the father I cannot remember and to the loving, courageous mother who raised me, my brother, and two sisters through the trials of the Depression and World War II. My grandparents on both sides, who emigrated from Lebanon to the United States, also knew how to cope with adversity, as Christians in a tragically torn country, under the grip of the Ottoman empire.  In 1931, our family grew to include my mother’s sister, Naciby, and her husband, John Saba, who had no children of their own. We all lived together in a spacious house in Methuen, still a gathering place for family reunions. My uncle and aunt were like second parents to us. As a youngster I was rather independent, preferring such sports as football, baseball and hiking to work. However, when my aunt, who was much stricter than my mother, assigned a household chore, it had to be taken seriously. From her I learned to be efficient and to take pleasure in a job well done, no matter how mundane. We were a very close, happy and hardworking family with everything that we needed, despite the loss of my father and the hard economic times. Uncle John died in 1957, and too soon afterwards, in 1960, my aunt passed away. My mother died in 1970 at the age of seventy. They all lived to see each of the four children attain a measure of success.  From the ages of five to twelve I attended the Saint Laurence O’Toole elementary school in Lawrence, a city next to Methuen, and was taught by sisters of the Catholic order of Notre Dame de Namour. I enjoyed all my subjects there. I do not remember ever learning any science, except for mathematics. I graduated from Lawrence Public High School at the age of sixteen and entered the Massachusetts Institute of Technology, just a few weeks later, in July, 1945, with excellent preparation, since most of my high school teachers had been dedicated and able. Although my favorite subject was mathematics, I had no plan for a career, except the notion that electronic engineering might be attractive, since it utilized mathematics at an interesting technological frontier. My first courses at M.I.T. were in the basic sciences: mathematics, physics and chemistry, all of which were wonderful. I became a convert to chemistry before even taking an engineering course because of the excellence and enthusiasm of my teachers, the central position of chemistry in the sciences and the joy of solving problems in the laboratory. Organic chemistry was especially fascinating with its intrinsic beauty and its great relevance to human health. I had many superb teachers at M.I.T., including Arthur C. Cope, John C. Sheehan, John D. Roberts and Charles Gardner Swain. I graduated from M.I.T. after three years and, at the suggestion of Professor Sheehan, continued there as a graduate member of his pioneering program on synthetic penicillins. My doctoral work was completed by the end of 1950 and, at the age of twenty-two, I joined the University of Illinois at Urbana-Champaign as an Instructor in Chemistry under the distinguished chemists Roger Adams and Carl S. Marvel. I am forever grateful to them for giving me such a splendid opportunity, as well as for their help and friendship over many years.  Because my interests in chemistry ranged from the theoretical and quantitative side to the biological end of the spectrum, I decided to maintain a broad program of teaching and research and to approach chemistry as a discipline without internal boundaries. My research in the first three years, which had to be done with my own hands and a few undergraduate students, was in physical organic chemistry. It had to do with the application of molecular orbital theory to the understanding of the transition states for various reactions in three dimensional (i.e. stereochemical) detail. The stereoelectronic ideas which emerged from this work are still widely used in chemistry and mechanistic enzymology. By 1954, as an Assistant Professor with a group of three graduate students, I was able to initiate more complex experimental projects, dealing with the structure, stereochemistry and synthesis of natural products. As a result of the success of this research, I was appointed in 1956, at age twenty-seven, as Professor of Chemistry. My research group grew and the scope of our work broadened to include other topics: enantioselective synthesis, metal complexes, new reactions for synthesis and enzyme chemistry. The pace of discovery accelerated.  In the fall of 1957, I received a Guggenheim fellowship and my first sabbatical leave. It was divided between Harvard, to which I had been invited by the late Prof. Robert B. Woodward, and Europe. The last four months of 1957 would prove eventful. In September, shortly after the beginning of my stay at Harvard, my uncle John passed away. At least I had been lucky enough to have seen him just two days before. I was deeply affected by the loss of this fine and generous man whom I loved as a real father. In solitude and sadness I returned to my work and a very deep immersion in studies which proved to be pivotal to my future research. In early October several of the key ideas for a logical and general way of thinking about chemical synthesis came to me. The application of these insights led to rapid and unusual solutions to several specific synthetic problems of interest to me at the time. I showed one such plan (for the molecule longifolene) to R. B. Woodward and was pleased by his enthusiastic response. Later in 1957 I visited Switzerland, London and Lund, the last as a guest of [Prof. Karl Sune Bergström](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1982/index.html). It was at Lund, in Bergström’s Department, that I became intrigued by the prostaglandins. Our research in the mid 1960’s led to the first chemical syntheses of prostaglandins and to involvement in the burgeoning field of eicosanoids ever since.  In the spring of 1959 I received an offer of a Professorship at Harvard, which I accepted with alacrity since I wanted to be near my family and since the Chemistry Department at Harvard was unsurpassed. The Harvard faculty in 1959 included Paul D. Bartlett, [Konrad Bloch](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1964/index.html), Louis F. Fieser, George B. Kistiakowski, E. G. Rochow, Frank H. Westheimer, E. B. Wilson and [R. B. Woodward](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1965/index.html), all giants in the field of Chemistry. Roger Adams, who was always very kind and encouraging to me, gave his blessing even though years before he had declined a professorial appointment at Harvard. I have always regarded the offer of a Professorship at Harvard as the most gratifying of my professional honors.  At Harvard my research group grew in size and quality, and developed a spirit and dynamism which has been a continuing delight to me. I was able to start many new scientific projects and to teach an advanced graduate course on chemical synthesis. Using the concepts of retrosynthetic analysis under guidance of broad strategies, first-year graduate students could be taught in just three months to design sophisticated chemical syntheses. My research interests soon evolved to include the following areas: synthesis of complex, bioactive molecules; the logic of chemical synthesis; new methods of synthesis; molecular catalysts and robots; theoretical organic chemistry and reaction mechanisms; organometallic chemistry; bioorganic and enzyme chemistry; prostaglandins and other eicosanoids and their relevance to medicine; application of computers to organic chemical problems, especially to retrosynthetic analysis. My personal scientific aspirations can be similarly summarized: to be creative over a broad range of the chemical sciences; to sustain that creativity over many years; to raise the power of research in chemistry to a qualitatively higher level; and to develop new generations of outstanding chemists.  In September, 1961, I married Claire Higham, a graduate of the University of Illinois. We have three children. David Reid is a graduate of Harvard (A.B. 1985) and the University of California, Berkeley (Ph.D., 1990), who is currently a Postdoctoral Fellow in Chemistry/Molecular Biology at the University of California Medical School at San Francisco. Our second son, John, graduated from Harvard (A.B. 1987) and the Paris Conservatory of Music (1990) and is now carrying out advanced studies in classical music composition at the latter institution. Our daughter, Susan, graduated from Harvard with a major in anthropology (A.B. 1990) and plans graduate work in Education. Claire and I live near the Harvard Campus in Cambridge, as we have for nearly thirty years. My leisure interests include outdoor activities and music.  I am very proud of the many graduate students and postdoctoral fellows from all over the world who have worked in my research group. Their discoveries in my laboratory and their subsequent achievements in science have been a source of enormous satisfaction. The Corey research family now includes about one hundred fifty university professors and an even larger number of research scientists in the pharmaceutical and chemical industry. It has been my good fortune to have been involved in the education of scholars and leaders in every area of chemical research, and especially, to have contributed to the scientific development of many different countries. My research family has been an extraordinarily important part of my life. Much of the credit for what I have achieved belongs to that professional family, my wonderful teachers and faculty colleagues, and not least, to my own dear personal family. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0383 |
| **Biographical** | I was born in Montreal in 1939, the second son of poor immigrants. My mother worked in a textile mill and my father in a grocery store before they met and married. It was from them that I learned that hard work in stable surroundings could yield rewards, even if only in infinitesimally small increments.  For our immediate family and relatives, Canada was a land of opportunity. However, it was made clear to the first generation of Canadian-born children that the path to opportunity was through education. No sacrifice was too great to forward our education and, fortunately, books and the tradition of study were not unknown in our family.  I am conscious of two events that sparked my early interest in science, the first being the appearance of the A-bomb. The mystique associated with the bomb, the role that scientists played in it, and its general importance could not fail to impress even a six-year old. About seven years later I was given a book about the periodic table of the elements. For the first time I saw the elegance of scientific theory and its predictive power. I should mention that while I was growing up, [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/index.html) was presented as a worthy role model for a young boy who was good at his studies. I added various writers of fiction and stars of ice hockey and baseball to my pantheon.  By the time I reached high school my father’s grocery store had made our life adequately comfortable and I was able to choose, without any practical encumbrances, the subjects that I wanted to pursue in college. My intention was to enroll at McGill University but an unexpected series of events led me to study physics at the Massachusetts Institute of Technology. There I experienced four years of over-stimulation among brilliant, arrogant and zany peers and outstanding teachers. Lee Grodzins supervised my senior thesis in nuclear physics and provided me with a wonderful research experience and with his friendship. During my final semester at MIT, I took a short introductory course in molecular biology to find out what all the excitement was about. That course, taught by Cyrus Levinthal, familiarized me with nucleic acids and molecular genetics and prepared me for future encounters with these topics.  I spent eighteen months as a graduate student in physics at Columbia University, waiting unhappily for an opportunity to work in a laboratory and wondering if I should continue in physics. Eight months later, having left Columbia, I was studying physics in a summer program and working in Colorado when I decided to enroll as a graduate student in biophysics. George Gamow, the physicist, had steered me to Leonard Lerman, then working on the intercalation of acridines into DNA at the University of Colorado Medical Center. In the excellent department chaired by Theodore T. Puck, Lerman provided the guidance, friendship and critical analysis that enabled me to enjoy molecular biology in a productive manner. After working on the effects of acridines on the replication of bacteriophage T4 DNA, I joined Mathew Meselson’s laboratory at Harvard University to study a DNA endonuclease involved in the replication and recombination of T4 DNA. Two years later I was privileged to become a member of the group led by Sydney Brenner and [Francis Crick](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1962/index.html) at the Medical Research Council Laboratory of Molecular Biology in Cambridge, England. As an ex-physicist, I felt as if I was joining the equivalent of [Bohr’s](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html) group in Copenhagen in the 1920’s. It turned out to be scientific heaven.  At the MRC laboratory I started the work that led to the discovery of RNase P and the enzymatic properties of the RNA subunit of that enzyme. John D. Smith, as well as several post-doctoral colleagues, provided me with much good advice that enabled me to test my ideas. The discovery of the first radiochemically pure precursor to a tRNA molecule enabled me to get a job as an assistant professor at Yale University in 1971, a difficult time to get any job at all.  My career at Yale followed a standard academic pattern with promotion through the ranks until I became Professor in 1980. I was Chairman of my department from 1983 – 1985 and in 1985 became the Dean of Yale College for four years, an experience that not only provided me with the opportunity to make many new friends, mostly outside the sciences, but also revealed to me the full panorama of human and academic problems that exist in a university community. On July 1, 1989 I returned to the post of Professor on a fulltime basis.  I have been blessed with outstanding mentors, people who became personal friends and who have illuminated so many aspects of human creativity for me with their intellectual power, expertise as scientists and qualities as human beings. In particular, they are Leonard Lerman, Mathew Meselson, Sydney Brenner and Lee Grodzins. There are, of course, many others whose names I cannot list here. My life has been enormously enriched by my marriage to Ann Korner in 1972. My wife is my colleague, mentor and friend in every respect. She and our two wonderful children, Daniel, born in 1974 and Leah, born in 1977, have contributed immeasurably to whatever success I have achieved. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** | 383 |
| Q12 | Let me ask you first of all, you are the son of an immigrant. Did your family encourage you to go into education? |
|  | Sidney Altman: Yes, my family felt that the pass to advancement in the new society that my parents had come to was through education. That education was the means to understand new things, to understand the new world they were in and to enable their children to do better than they had done. And some of the older relatives in my family were teachers of one kind or another so it was not so out of the question to think so highly of education and the value of education. |
| Q12 | And they didn’t push you in any special direction? |
|  | Sidney Altman: No, I feel very fortunate about that. That they didn’t impose what their views of what might be the best things to do. For example many kids who were in the same situation I was in were encouraged to go into medicine or law because they were very secure professions. And I never had that kind of constraint put on me by my parents and I felt free to choose whatever I enjoyed and whatever I was interested in. |
| Q2 | How did you become interested in science by the way? |
|  | Sidney Altman: I have of course been asked that question frequently and I try to remember it correctly but it’s always hard to know if you remember the past correctly, let’s put it that way. I recall a few incidents. And one of them may not be so exciting in terms of the rest of what the world may perceive as something that interested me, but I somehow knew about Einstein when I was growing up as a kid. I think I had been given a book, short biographies of great men for children, and I was six years old when the atomic bomb was dropped to end the Second World War in Japan. Naturally the whole world was full of the news. The newspapers were full of pictures. The story of the bomb and how it was built was written up everywhere. And the role of scientists and pure research in generating the ideas and then the success of the bomb were clearly laid out at that time and a direct connection to Einstein, although he might not have wanted it to be so direct, was there.  So as a kid I was impressionable, being young, and this made me feel that science was very important. At the same time I was interested in natural phenomena. Obviously if I didn’t have both sides of these things going I might not have pursued it. But I would say in terms of the specifics of science, when I was about 12 years old somebody gave me a book to read, and I can’t remember who it was. It was written for I would say kids or the layman and it was called *Explaining the Atom*. But the significant thing I remember in it was the periodic table. A very lucid explanation of the periodic table and how Mendeleyev formulated his ideas and even more important the notion that Mendeleyev was able to predict the properties of elements that had not yet been found. And to me that was absolutely amazing when I was 12 years old. It was so elegant. So beautiful, that I think it was at that time I decided really to try and study physics which I went on to do. And so that was the beginning, I think, of my real interest in science.  I switched to molecular biology later on, from physics. And I do remember actually reading one paper when I already had my Bachelors degree in physics. I just came across it one day in the journal *Nature*. I wasn’t looking for it because I wasn’t knowledgeable in the field of molecular biology. It was a paper on the nature of the genetic code. It said that the genetic code is read in groups of three letters from DNA essentially. And it was a paper from Cambridge England and [Francis Crick](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1962/crick-facts.html) and [Sydney Brenner](https://www.nobelprize.org/nobel_prizes/medicine/laureates/2002/brenner-facts.html) were the senior authors. And I remember thinking to myself, not merely this is again very elegant, beautiful, but thinking to myself how could they possibly have understood this. How did they find this out? It seems magical that you would be able to say something about the way we are encoded in a sense in nature. And that stimulated greatly my interest in molecular biology as a discipline also. I thought it was something I’d never heard of before. Something I wasn’t really in touch with. I’d heard about DNA. I knew a little bit about it but to be able to do that kind of experiment I thought was amazing. |
| Q1 | It’s striking when you read the autobiographies of the Nobel Laureates that many of them started out in a field very different from where they ended up. I sometimes sense that youngsters of the day are a little worried that they start out in the wrong field and they will get trapped there. Would you like to say something about that? |
|  | Sidney Altman: Well, I think that I’m not sure that I would include myself in this statement, but a lot of the people who for example are at this meeting are extremely imaginative and curious people. And I think people like that will always be looking for new challenges and new ideas that are not necessarily established ideas in their own field or derivative ideas from things in their own traditional disciplines. They will always be looking at the borders of knowledge. And one can’t predict exactly where that will take you. I think that’s part of it. I don’t really want to get into a sociological analysis of today’s students but in my particular field molecular biology, I will say that it was not really a field *per se* for example when I went to university. So that everybody who came into it before that time and about the same time as I did had to come from another field. There was no training in molecular biology.  Today, because of the success of modern molecular biology, every university department has a department of molecular biology or called something else maybe. But similar. And you have a lot of people going into what is now an established field. And a lot of these students, as you say, do it because they know there are jobs available at the other end. And that’s what they want. And there’s nothing wrong with that. A lot of people go to university for security and a profession and that’s fine. But one hopes one still gets the occasional student who has that restlessness of intellect and curiosity to want to go on and do research. |
| Q15 | Today, the 26th June 2000, is a very special day in a way that a joint group of scientists from many countries have announced that they have completed and also published at least the first version of the human genome. This is of course is a major step in the history of mankind and history of science. Would you like to comment on that? |
|  | Sidney Altman: I think there are several important things to say about the actual announcement which I just happened to see when I was taking a little rest before. And it was very interesting for a number of reasons. First of all let’s talk about the science. Of course if you are in molecular biology you knew this was coming. So it’s not striking or surprising that this stage has been reached in our knowledge of the genome. I thought that during the talks given by various people today it was clear that they are trying to be very careful about the practical implications and the timeline for fulfilling some of the practical promise of this knowledge. We have to understand exactly what all this information means. We have a huge amount of information in front of us. We don’t know how much of it is relevant to the everyday function of the human body. We don’t know at the moment. But I am perfectly confident as are the scientists who have been working on this project that within five to ten years, perhaps even sooner, we will know a tremendous amount because of this information. About how the human body works. That is to say in comparison to what we know now. We’ll know much, much more. And it will have a very major bearing on our understanding of human disease and probably give us new insights into treatment of disease.  Now I’ve made some great generalisations just now and I’m probably not the person to become more specific about it, except to say that I think I agree with you it is a milestone event and it is important but it will be a few years before we derive the benefits. But that’s not surprising. It happens with every major scientific advance. I also want to comment on some other thing that were said and how the announcements took place because I think they’re almost equally important. There were two factors in addition to the science that I thought were important. One was the fact that you had the political chiefs of two of the leading countries of the world. Leading in terms of advanced technology, financial comforts, standard of living and things like that. The Prime Minister of Great Britain and the President of the US participated in a joint transatlantic television conference which in itself is interesting. And both these heads of state were clearly knowledgeable about the advance. They seemed to feel at ease talking about what it meant.  But what it meant to me was that the political establishment in both countries and all the other participating countries in which there are several, therefore the people the societies themselves, were behind this whole enterprise. This is one of the few times I think in history where so much has been devoted by so many countries in a truly international effort with the political heads of all the countries behind them to participate in this project which will benefit mankind in a great way. And I have to also say that both heads of state and the other scientists who spoke did speak about minimising the potential harmful effects that might arise from this new knowledge. The use of the information for example. As an aside I will say that Mr Blair used that word minimise. He didn’t say you could make zero the potential negative effects. He said minimise which I think is the correct way to put it.  The other aspect of this announcement which I thought was very interesting and very significant in many ways, in the States you had accompanying the President the head of a large commercial enterprise which was involved in producing this information as well as the head of a government lab. Actually the head of the international human genome project. And they clearly had reached before this announcement an agreement about how the information was going to be used. How the commercial enterprise would be able to benefit from it but still work together with the public enterprise to make the information available to everybody. Now we don’t know the details of that agreement yet but I think it is significant that after a few years of rather bitter wrangling supposedly this conflict between funding of private research and public research has either come together or been ironed out with some kind of compromise which will allow everybody to benefit from it. I think that’s also very important because governments alone cannot do everything. They cannot support all research. And this is one case where the private sector has been very energetic and ambitious, and so it’s significant that, in the end, it’s worked out its differences and terms of intellectual property sharing with the government. So that’s my rough reaction to today’s announcements. |
| Q4 | On a more lighter side, I noticed that in science there has now been a kind of betting scheme on how many genes there are in the human genome... Would you like to make a bet on how many genes there are in the human genome? |
|  | Sidney Altman: Well, the low numbers are very interesting, because the other genomes that have been sequenced recently, for example the fruit fly, *Drosophila*, and some other bacteria, the nematode *C. elegans*, have all turned out to have fewer genes than we thought that they would have. However, a mammal is different to some extent, and so I’m not willing to make a wager, but I would say fewer than 100,000, and I also repeat a comment that we heard earlier today, made by one of our colleagues saying that predictions are very hard to make, especially those about the future. I would hedge my bets there. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0384 |
| **Biographical** | Grandfather Josef, a shoemaker, immigrated to the U.S. from Bohemia in 1913. My other grandparents, also of Czech origin, were first-generation Americans. My father was and is a physician, my mother the homemaker. I was born in Chicago on December 8, 1947.  The safe streets and good schools of Iowa City, Iowa provided the backdrop for the childhood years of my sister Barbara, my brother Richard and myself. My father, who loved physics as much as medicine, interjected a scientific approach and point of view into most every family discussion. I discovered science for myself in fourth grade, collecting rocks and minerals and worrying about how they were formed. By the time I was in junior high school, I would knock on Geology professors’ doors at the University of Iowa, asking to see models of crystal structures and to discuss meteorites and fossils.  In 1966 I entered Grinnell College, where I was to derive as much enjoyment studying Homer’s Odyssey, Dante’s Inferno, and Constitutional History as Chemistry. I met Carol over the melting point apparatus in a make-up Organic Chemistry lab, starting the partnership of our lives that is now more than 20 years old.  The Chemistry I appreciated the most from textbooks was physical chemistry. However, undergraduate research experiences at Argonne National Laboratory and at Lawrence Berkeley Laboratory taught me that I didn’t have a long enough attention span for the elaborate plumbing and electronics of gas-phase chemical physics. I was later attracted to biological chemistry because of the almost daily interplay of experimental design, observation, and interpretation.  Berkeley, 1970. Carol and I chose the University of California as much for the excitement of life there as for the excellence of its Chemistry Department. My thesis advisor, John Hearst, had an enthusiasm for chromosome structure and function that proved infectious; I have not yet recovered, nor do I wish to. Long days in the laboratory were punctuated by occasional backpacking trips in the alpine splendor of the Sierra Nevada.  In 1975 we obtained our Ph.D.’s and moved to postdoctoral positions in Cambridge, Massachusetts, Carol at Harvard and I at M.I.T. I strengthened my knowledge of biology in Mary Lou Pardue’s laboratory, and enjoyed being part of the interactive scientific scene at M.I.T.  We began our first faculty positions at the University of Colorado, Boulder in 1978. I was initially attracted by the enthusiasm and energy of the faculty; I have stayed because in my field the intellectual environment here would be very hard to equal. We have benefitted from very fine colleagues, with whom we have shared many great dinners and ski trips to the nearby Rocky Mountains. More recently, life has been transformed by the addition to our family of two energetic daughters, Allison (born 1982) end Jennifer (1986). It promises to return to normal sometime in the next century.  Because of my research group’s discoveries, more than a dozen national and international awards preceded the Nobel Prize for Chemistry in 1989. Among them were the Pfizer Award in Enzyme Chemistry (American Chemical Society), the Award in Molecular Biology (U.S. National Academy of Sciences), the Heineken Prize (Royal Netherlands Academy of Arts and Sciences), and the Lasker Award. I received an honorary D.Sc. degree from Grinnell College in 1987 and from the University of Chicago in 1991. I have been elected to the U. S. National Academy of Sciences (1987) and to the American Academy of Arts and Sciences (1988). In 1987 I was awarded a lifetime Professorship by the American Cancer Society, and in 1988 became Investigator of the Howard Hughes Medical Institute. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** | 384 |
| Q2 | How on Earth did you feel and what did you think about when you sat there in the lab and saw all these results coming out that could only be explained in mechanism that no-one would believe in? |
|  | Thomas R. Cech: We ourselves did not believe it for more than one year and I was working very hard and encouraging the people in the laboratory to work very hard to find a protein that we were sure must be responsible for the RNA splicing and …  Thomas R. Cech: It wasn’t there and we kept getting evidence really that it wasn’t there. For example, we would take the RNA preparation and we would subject it to detergent, which is very bad for proteins, and the splicing reaction continued unabated and we would boil the solution, which normally denatures and unravels protein chains and again, the splicing reaction continued after that treatment. So the system kept trying to tell us that there was not a protein but for a long time I refused to listen and kept urging people to look harder for that evidence for the protein. |
| Q2 | But you must have thought of the risk of being ridiculed when you came out on the stage, in the open air, with all those results. Were you, by the way? |
|  | Thomas R. Cech: Not immediately. The world was sort of waiting for this result because Francis Crick and Leslie Orgel and others had speculated about such a possibility in the mid-1960s. I’m embarrassed to say I had not read those, I was too young, I had not read those papers and I was unaware of this, but others, who were more senior, were still thinking that maybe RNA could tell us this would be perhaps important for the origins of life and that the fact that the ribosome was composed of partially RNA seemed to many people to be a hint that RNA had a very primordial role in catalysis.  Now, since there were no examples, that had sort of faded away in the meantime but once we provided this evidence, there was really great excitement. I think the fact that we did the experiments in this obscure organism, this pond animal *Tetrahymena thermophile* allowed many people to think of this as a curiosity rather than something that would be central to biology, so that perhaps also prevented us from being ridiculed. We said Oh this is a weird organism, it does weird things, it’s interesting but we don’t have to incorporate it into the centre of our thoughts. |
| Q2 | But there is a very exciting mechanism that I want to know something about inside now. I mean, how do you really break down a paradigm? Because that was what you did. I mean, how do you function as a scientist to be able to do that in the first place? |
|  | Thomas R. Cech: You have to, of course, have an open mind and you have to have very rigorous experiments and you have to be your own worst critic. You have to be so demanding to get very sharp proof yourself, in your own laboratory, that by the time you announce it to others; they have to accept it because you’ve done things so carefully. |
| Q17 | Yes, but how come that your mind is starting to think of something that no-one else thought of? |
|  | Thomas R. Cech: Well, as I said, we were a bit slow to come to that realisation. It took a full year and you just … As a scientist, you get hooked on these questions and you’re thinking about it all the time, you think about it in the shower in the morning; you think about it in the middle of the night, you think about it when you’re riding your bicycle and you just keep turning over the possibilities and the important thing is to recognise that there could be other explanations. |
| Q4 | So now this is 15 years ago, so there must have been happening a lot since then. What is going on in the field now? |
|  | Thomas R. Cech: The important thing for what we called ribozymes which means ribonucleic acid that has catalytic activity, and I think this was key to the Nobel Prize, was other people finding more examples because if it had only been the one example in this pond animal, I’m not sure the interest would have been so sustained. But within, well the next year, there was another example and then the year after that several more examples and then literally hundreds of examples of RNA catalysis in nature and that helped not only sustain the interest but increase the interest so that there actually started to be conferences on catalytic RNA. There was enough of a community that hundreds of people around the world were working on this and they would get together and talk and compare their results, so that was one development that we had no way of predicting but that was of course very exciting, for us and for the field. |
| Q14 | This must mean that all this talk about junk DNA and DNA that is not working or RNA that you don’t know any use of, it will totally change in the coming five or six years? There will be no junk left? |
|  | Thomas R. Cech: I don’t know. I think there still could be some junk. Much of the junk appears to be almost parasitic transposable elements which inhabit chromosomes and then have a strategy for copying themselves without damaging the ability of the host organism to function properly, so there could still be some junk but I think buried, you’re correct, that buried within what now appears to be junk may be some RNA transcripts which have activities that are not yet imagined. |
| Q18 | Is there an equivalent perspective on the biosciences and the 21st century? |
|  | Thomas R. Cech: Absolutely yes. I think it’s already starting to happen. The completion of the human genome project was a wonderful event, but it was not an end in itself. It provides a tool which will allow the medical sciences to understand both healthy human life and also disease in a much different way than they were able to do before and we, as we learn more and more, we will learn that particular variations of genes that each of us, one person to the next has thousands of differences, some of which may be inconsequential but some of which may give us a resistance to a virus or a susceptibility to heart disease and as these are, the meaning of these differences is unravelled, then medical science will be able to use these to better diagnose and treat disease. |
| Q15 | But how will it affect our lives? Will there be eternal life or will agriculture be something totally different? |
|  | Thomas R. Cech: I hope not in terms of the eternal life because if you remember the gods on Mount Olympus from Ancient Greek mythology, who had eternal life, they were wasting time and drinking and partying all the time. They did nothing useful with their lives because there was no time pressure, so I think that having, I think the fact that life is finite is what makes it so valuable and what gives us incentive to make the best use of every day. |
| Q15 | But surely it will affect the way on our route to the final end? I mean, what will the journey look like when bioscience is ripe? |
|  | Thomas R. Cech: Of course, the dream is that people will live a healthy productive life, as many people are now, but even more people will be able to look forward to having a life that lasts for perhaps 100 years in a productive healthy manner. |
| Q4 | What about the greatest riddle of them all, how life originated? |
|  | Thomas R. Cech: Yes. That one is fascinating to think about and one wonders if it will ever be solved because if you think about it, it’s not so much just as scientific question, it’s for historians to talk about, but how do historians normally ask questions about things that are that ancient? That are almost, you know, over three billion, perhaps close to four billion, years ago? You look for fossil evidence if you are a paleontologist and single molecules, such as RNA, do not leave a fossil imprint, so it’s very difficult to ask the historical question. Instead, as scientists, we try to recapitulate steps in what might have been a plausible scenario for the origin of life. |
| Q10 | But to get these answers, if they are impossible to get, you need people coming into science. Are there enough people coming into science in United States and also in Europe? |
|  | Thomas R. Cech: Well, certainly in the developed countries, we see a discouraging decrease in interest in sciences. In the United States, the only thing that is sustaining our biomedical sciences is an influx of very talented students from Eastern Europe, from Russia, from Asia, especially China, some from South America, from countries where people are hungry for opportunity and I’m afraid many of the American students have an easy life and this seems very difficult, to have to think about the scientific concepts, so we see a declining interest. It worries us a great deal. |
| Q18 | But don’t you think that also the scientists are a little to blame because the idea of putting scientific knowledge in peer reviews or in scientific papers is to rip off all the emotions, to limit it to the pure knowledge and nothing of the excitement should be left in the paper when it’s in nature or science, so how can you ask people to walk along a street that lies in darkness when not even the people are walking there are illuminated? |
|  | Thomas R. Cech: Of course there are some museums which now do quite a good job of, I think, communicating the excitement of science and also allowing the public to play around with various experiments and have an interplay with an experimental system. There are some television shows and some web based activities. Now you can do virtual laboratories on the web where you’re actually mixing things together in a test tube and seeing what happens, so I think that there are quite a number of points of light around the world where people are doing some innovative things, but the overall picture is still that we’re not doing enough and that only a small part of the general public really gets exposed to these ideas. |
| Q10 | What do you hope to get out of that? |
|  | Thomas R. Cech: We recognise that biology has changed over the last decade and that technology, both computer technology but also instruments for peering into the function of cells, what we call imaging, to be able to see how molecules move and combine with each other and perform reactions in a living cell, that it’s very difficult to explore those areas in the best way in a university setting, because in universities, the biologists are in a separate building away from the physicists, who are separate from the engineers and the chemists are yet somewhere else.  And so technology and biology really need to be working more shoulder to shoulder and the university puts people in separate places who need to really talk to each other, so we decided to build a place where there was a single building with great horizontal spaces so that physicists, chemists, engineers, computer scientists could be working shoulder to shoulder with biologists and learning about what kind of technology was really required to drive biomedicine to the next step. |
| Q2 | So small groups, a lot of money and people coming from all different sort of disciplines, putting them together in one place and make them talk, that is the recipe? |
|  | Thomas R. Cech: That’s part of the recipe. The other part is that we are very close to Dulles International Airport in the Washington DC area, and so we hope to have visitors from all over the world who come, because we want to share the things that we create. We don’t want to just hold them to ourselves. We want to make them accessible to the world, so we have built a hotel for 100 people and we have built 48 apartments so that people can come and stay for a month or six months or a year, and then the proximity to the airport should make it very easy for scientists to come from all over the world to join us there, to bring their ideas or maybe bring their problems, bring their samples to put in a microscope or to learn a new bioinformatics algorithm, so we want to have a very active visitors programme as well. |
| Q14 | So what will this laboratory have explained to us in 10 years from now? The secrets of consciousness? |
|  | Thomas R. Cech: Maybe the secrets of consciousness will be more 100 year goal, but one step towards that will be to unravel the network of neurons that are responsible for behaviour and we don’t want to, we’re ultimately interested in human behaviour but the human central nervous system, the human brain, is too complicated, so we will work with simpler organisms such as the fruit fly, the zebra fish and the mouse, that are genetically tractable organisms and also ones that are smaller so that we can apply this imaging technology, this advanced microscopy to visualise these neurons in action. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0385 |
| **Biographical** | I was born on September 30, 1943 in Zusamaltheim, Bavaria, now Federal Republic of Germany, as the first son of Thekla and Johann Deisenhofer. After my father’s return from military service my parents ran the family farm. In 1948, our family grew to its final size with the birth of my only sister, Antonie.  My early youth was influenced by the environment provided by a little village that, after World War II, tried to find its way back to some kind of a normal life. Nevertheless, it was a most enjoyable place for a little boy. In 1949, I entered elementary school at Zusamaltheim, and continued to attend until 1956. According to the local custom, the oldest son was designated to take over the family’s farm. However, to their great disappointment, my parents early noticed my lack of interest in farming, and made the difficult decision to send me away to school. My way to higher education started in 1956 at the “Knabenmittelschule HI. Kreuz”, Donanwoerth, and continued 1957 to 1959 at the “Staatliche Realschule Wertingen” and 1959 to 1963 at the “Holbein Gymnasium”, Augsburg. There, in 1963, I underwent the “Abitur” examination that allowed me to go to a university. I was awarded the “Stipendium für besonders Begabte” of the “Bayerische Staatsministerium für Unterricht und Kultus” which helped to lower the financial burden on my parents for my education.  In the fall of 1965, after 18 months of military service in the German Bundeswehr, where I did not exceed the rank of private, I began to study physics at the Technische Universität München. A major reason for choosing physics was an interest in physical, especially astronomical problems, aroused by popular books on this subject. The book I most clearly remember was a popular review of the state of astronomy by Fred Hoyle, describing the impact of modern physics on astronomy, and the recent achievements and open questions in that field. The Technische Universität München (TUM) was the obvious choice because Rudolf L. Moessbauer had just accepted a professorship at the TUM; moreover Munich is only about 100 km from Zusamaltheim.  During my time at the TUM, I learned that physics was quite different from what I expected; also, my interest slowly shifted to solid state physics. Together with a couple of colleagues I started my Diplomarbeit in this field in the laboratory of Klaus Dransfeld. As it turned out, Klaus Dransfeld was a person almost as shy as myself, so that we could not establish a good personal contact at the time. Nevertheless, the experimental work I did under the supervision of Karl-Friedrich Renk in Dransfeld’s lab was very successful, and led to a publication in Physical Review Letters in 1971; this was my first scientific publication. During my time in his lab, Klaus Dransfeld transmitted his interest in biophysical problems to many students. This had direct consequences for my career because it made me look for a suitable institution to get a Ph. D. in this field. From a friend I heard about a new group at the Max-Planck-Institut für Eiweiss- und Lederforschung whose head, [Robert Huber](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1988/index.html), was looking for students. After a brief interview with Robert Huber, it was agreed that I could start my work in June 1971, following the final examination for my physics diploma at the TUM. In 1972, the institute moved a few kilometers from Munich to Martinsried, and became the “Max-Planck-Institut für Biochemie”. The work I did together with Wolfgang Steigemann (also one of Huber’s Ph.D. students at that time) on the crystallographic refinement of the structure of bovine pancreatic trypsin inhibitor was a success, and our 1975 paper in Acta Crystallographica has been cited ever since.  At the end of 1974, when I had obtained my Ph.D. degree, Robert Huber offered me a postdoctoral position for two years which I accepted. This position was converted into a permanent position in 1976. I joined Peter M. Colman, then a postdoctoral fellow in Huber’s lab, and Walter Palm from the University of Graz, Austria, in their work on the human myeloma protein Kol. After the solution of this interesting structure, I continued, together with Robert Huber, Peter Colman’s work on the human Fc-fragment, and its complex with an Fc-binding fragment from protein A from *Staphylococcus aureus*. The refinement of these structures was finished in 1980. In the following two years I joined several projects in Robert Huber’s lab: human C3a, citrate synthase, and alpha-1 -proteinase inhibitor. During all my time in Martinsried I enjoyed working with computers, and developing and maintaining crystallographic software.  In 1982, Hartmut Michel, who had come to Martinsried together with Dieter Oesterhelt, reported in one of Huber’s group seminars about his spectacular success with the crystallization of the photosynthetic reaction center from *Rhodopseudomonas viridis*. After discussions between Hartmut and myself, and after Robert Huber had given his agreement, I joined the reaction center project in order to determine the three-dimensional structure of this molecule. Shortly afterwards Kunio Miki, a post-doctoral fellow from Osaka University, arrived in Martinsried, and helped us until September 1983. Later Otto Epp, a colleague and friend since I joined the Max Planck-Institute, made most valuable contributions to the project.  In a surprisingly short time, at the end of 1983, we came to a point where the success of the project was at the horizon. It still took almost two years until we had worked out the complete structure, and two more years to refine the model at 2.3Å resolution.  The work on the photosynthetic reaction center changed my life in many ways. It was a special privilege to belong to the very small group of people who saw the structural model of this molecule grow on the screen of a computer workstation, and it is hard to describe the excitement I felt during this period of the work. Soon after the news of our success spread through the interested scientific community, we received many invitations to report our results during scientific meetings, in seminars, and even in TV shows. The wide recognition of our work also opened the possibility for me to move to a new place, and to build a research group of my own. The best of several opportunities was an offer from the University of Texas Southwestern Medical Center at Dallas which I joined in March 1988 to become Professor of Biochemistry, and Investigator in the Howard Hughes Medical Institute. Almost immediately after my arrival I fell in love with Kirsten Fischer Lindahl, Professor of Microbiology and Biochemistry and Investigator in the Howard Hughes Medical Institute; we got married in 1989.  For the determination of the three-dimensional structure of the reaction center Hartmut Michel and I received the 1986 Biological Physics Prize of the American Physical Society, and the 1988 Otto Bayer Prize. The 1988 Nobel Prize in Chemistry was followed by several non-scientific honors such as honorary citizenships of my home town Zusamaltheim and of my current residence Dallas, and a high order of the Federal Republic of Germany. I am a member of the Academia Europaea, and a Fellow of the American Association for the Advancement of Science. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q4 | Along the path from physics to at least biophysics and biochemistry had you also had brushes with other branches of science? Had chemistry figured for instance at that stage? |
|  | Johann Deisenhofer: Not very much. I knew very, very little about chemistry and that was in part because among physicists at that time chemistry was considered a sort of a second-class science. You could hear people say Well, I mean, quantum mechanics describes everything and chemistry is the detail. |
| Q4 | Where does that leave biology? |
|  | Johann Deisenhofer: Pretty stupid point of view, but they were present, and so in the curriculum for budding physicists chemistry played sort of a very minor role so we had to take one course, I think, in the first two semesters and a practical course, that was it. Compared to let’s say, the math that we had to take at the same time, chemistry was indeed, or at that level was quite easy. |
| Q4 | Do you think things are still the same when you study physics? Is chemistry still seen as a second-class subject? |
|  | Johann Deisenhofer: I don’t know. I hope not because it is not justified as I learned the hard way later. |
| Q5 | And you found yourself very much at home with Robert Huber, what was it about his style of leadership that particularly appealed to you? |
|  | Johann Deisenhofer: He was pretty young, only a few years older than I was, I think six years older and was very liberal, modern thinking. He had many ideas to do things differently than his mentor as always and he, for example, introduced the custom to address him as Robert and not as Professor Huber. |
| Q10 | And the resources were tremendous, I imagine, at the Max Planck? |
|  | Johann Deisenhofer: Yes. I mean I didn’t pay attention to what things cost at that time. They had the modern machinery, the x-ray generators, even a computer controlled data collection, machines and so on and there was the Max Planck Institute for Plasma Physics in another part of Munich which had I think one of the fastest computers in Germany at that time an IBM 370 something or 360 91 … I’ve forgotten the details but this was for anybody who had the calculations it was almost ideal conditions.  So I became a computer freak for a while at least and indeed at that time computing was one of the limiting resources in crystallography especially protein crystallography. If you look at the pictures of the beginning days of that field [Max Perutz](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1962/perutz-facts.html) and his students. When they were collecting paper tapes … I mean this was really pioneering work with a very, very limited availability of computing resources. And luckily this is all changed now, our laptops can do a lot more than the IBM machine at that time could do. |
| Q12 | Was that the norm for the environment you were growing up in? |
|  | Johann Deisenhofer: No not really. It had to do with my personal history. I was not meant to become a scientist. I was meant to become a farmer and when it became clear that I would never be a good farmer I had lost one or two, three years at school and I had to catch up and for some reason the German army desperately wanted me so I had to lose another two years in the Army or one and a half years but in practice this amounted to two years and so that explains why I was relatively old when I got my PhD. |
| Q17 | Just referring to your childhood and the expectation that you would become a farmer. Are there, despite the fact that you didn’t follow the family path, are there the nevertheless familiarly aspects to the way you work? |
|  | Johann Deisenhofer: I enjoyed being on my own, being by myself. I certainly played with other kids and interacted but I had no problems with being on my own and that is still so. I think I enjoyed the occasions most when I had a well-defined project and I could solve the problems by myself. |
| Q6 | Having got your PhD and then embarked on post doctoral projects looking at the structures of a number of different proteins, you then had a momentous meeting with Hartmut Michel in 1982 when he revealed that he had crystallised a membrane bound protein. Did lights flash in your head at that moment and you thought this is the project for me? |
|  | Johann Deisenhofer: It took me a while because it was, at that time the larger protein the structure of each would be solved and it was not clear at all, there was no precedent whether the standard methods of structure determination would work and so I think everybody hesitated for a while but not very long. I like Hartmut because I knew him, I knew that he tried to crystallise membrane protein in a different group at the Max Planck led by Dieter Oesterhelt so we occasionally talked and then at some point it was clear that we would collaborate. |
| Q4 | It all moved quite rapidly at that point and you’ve written before about the fact that you were deeply excited to be involved with it and so excited that you couldn’t even sleep. Can you describe how it felt to reveal this structure? |
|  | Johann Deisenhofer: There’s one point in every structure determination when you can see the general idea of the protein. In this case the biggest question was how are the chromofores arranged in this protein? It has four chlorophylls, two chlorophyll derivatives and heme groups and iron and quinones and it has lots of those and the task of the protein is to keep these in the right geometrical arrangement so that the electronic properties follow from this. That was the most exciting point when I recognised in the electron distribution the chlorophylls and I immediately got hold of some models of, I mean atomic coordinates of chlorophyll molecules, and fit them in and it was very clear that that was correct. |
| Q4 | What was the biggest surprise about the structure for you? |
|  | Johann Deisenhofer: Its symmetry. It was known from spectroscopic experiments that there was a, known is perhaps not the right way to say it, but some people proposed that there was a pair of chlorophylls in close contact and then there was a so called pheo-phytin that was the acceptor of the electron.  But only one and then a quinone and then a second quinone and there were all kinds of models out there in the literature that accounted for the pair and the pheo-phytin and the quinone and the second quinone but when we looked at the structure it was clear that there were two branches of chlorophylls and pheo-phytins that looked to the naked eye almost identical. They’re not identical they’re subtle differences in distance and in the protein environment but everybody was speechless at first about the symmetry. |
| Q2 | And you trusted your structure determination above any of the previous evidence yes? |
|  | Johann Deisenhofer: Yes. The electron density is obtained without any previous hypothesis. I mean you cannot manipulate the /- – -/ information of 50,000 reflections such that they wrongly produce a picture that come from your own preconception, that is impossible, luckily, so there was not always easy to convince more senior scientists that, I mean it’s not our fault that this protein has a symmetry. They all thought we had made it up to make it more, or may in their hearts they thought then it was never really said but it took quite a while for the whole field to realise that they have to live with the symmetric reaction centre. |
| Q2 | Those didn’t exist when you were doing these experiments and therefore that wasn’t a potential problem? |
|  | Johann Deisenhofer: The example you mentioned indeed came from a piece of software the apparently was never really carefully checked and it caused us a severe kink in a bright young scientist’s career yes. But it could have happened to us too but it didn’t. |
| Q9 | End of story yes. And having determined the structure of the reaction centre then you and your group moved centre stage. Was that hard to deal with? |
|  | ohann Deisenhofer: Yes it was. It meant a difference in my way of working and in my lifestyle, because before that I went perhaps to two meetings per year and after the structure came out and it became known that we had a structure, we were inundated with invitations. So there was one year when I travelled to the States six times and it was really amazing how much interest that structure created.  And the most peculiar meeting was in a hotel near the Pentagon which apparently was organised by the Department of Defence and they were at that time trying to think about molecular electronics, and they thought Well, this is indeed a prime example of molecular electronics and there were these four star generals. It was a little strange yes and especially the selection of talks was very, very unusual. I mean there were some talks that were about things that maybe 200 years in the future or something that sort. It was more science fiction than science but I mention this only as an example. |
| Q10 | So this period of activity culminated in your decision to move away from Germany and come to Dallas which was quite a major move. What brought you to Dallas? |
|  | Johann Deisenhofer: After my PhD I thought I might, and because at that time the situation of protein crystallography was not particularly good because this was pre recombinant DNA technology and the proteins were hard to get. The whole field stagnated for a while and the opportunities for young people. So I stayed at the Max Planck and I … for quite a while I was prepared to spend my whole career there and then came this success story and I was asked many times Would you look at the position at this institution, that institution. I seriously looked at two places in Germany and one of them was the EMBL had as a consequence that somebody at UT Southwestern got the news that I was looking for a job.  And so, out of the blue sky came a letter would you like to visit and look at the possibility to join the faculty and I did some research because I had no clue, it was called UT Health Science Centre at Dallas at the time. I had no clue but I got very good recommendations from people, especially a colleague who did his PhD in Houston and he knew quite a lot about UT Southwestern, so I decided to visit and there was a very stark contrast between the way recruiting is done here and how it is done in Germany. It’s essentially going from a seller’s market to a buyer’s market and that impressed me a lot. So I decided to come for a second visit and then got an offer and still to my surprise one evening I pick up the phone and I call the chairman of biochemistry saying Okay I accept. |
| Q7 | You mentioned the question of learning how to recruit people and building a team. From your experiences of the people who recruited you and your own working methods, what were you looking for and what do you look for in people you bring in to your lab? |
|  | Johann Deisenhofer: People who have a clear idea of the general direction they want to go. People who, now I’m talking about postdocs or in some respect also students, people who are willing to use their own head, who are not waiting for me to tell them what to do and who are hard working I mean that’s almost … |
| Q2 | It’s the same problem on a different scale? |
|  | Johann Deisenhofer: It’s a different scale yes. I mean the main part of the Howard Hughes Medical Institute is essentially supporting investigators all over the United States and some scientists are in other countries but what they’re doing now, their research centre in Janelia Farm is something of that sort. I mean they must find people who can work together and the whole idea of this new institute is to promote collaboration between small groups of scientists and multi-disciplinary approach and all the buzz words that you can hear all the time but essentially it goes down to hiring the right people and that’s a big project. |
| Q2 | So it could have thrown people on the wrong track for a while? |
|  | Johann Deisenhofer: Yes. It would have created a sort of not quite correct picture of membrane proteins so these were the competitors we knew about, but our project could hardly have gone any faster than it did. Perhaps nowadays of course it would go much faster because we would have a different technology that collect data and we would synchrotron radiation. We had that at that time also but it was still a wilderness trip to go to synchrotron. |
| Q4 | So staying with the theme of structural biology for a while, do you think that the discipline itself and indeed the way that structural biologists are contributing, is changing? |
|  | Johann Deisenhofer: Yes I think so. When I was young it was a very young discipline still and you could still fit all the protein crystallographers into the ballroom of a small Austrian village which happened actually, yes. But nowadays the field has become very, very large. We talked about the structural genomics and I have the feeling that the job description of structural biologists changes, that institution who hire new faculty, new researchers look for people with a lot broader perspective than just, let’s say, crystallography. On the other hand crystallography the methods have been streamlined to an extent that one doesn’t need to know much anymore. So I’m talking about computer programmes, I’m talking about data collection facilities at the synchrotrons.  The number of synchrotrons has increased and essentially we will soon be at that point where we send the crystals by mail and get data back through the Internet. This opens of course a certain risk that, I mean, crystallography’s still a complicated subject and on the other hand, if you can’t follow, you can go through the motion and get a structure. There will be cases in which people who are not experts go through the motions and think they get a structure, or the computer offers them a structure but it may not be the right one. So at South-Western we have tried to start an initiative with a core facility where well trained expert crystallographers offer their collaboration to anybody who’s interested. They provide training and in the essence they provide expertise and a reasonable assurance that all the necessary precautions have been followed to exclude the possibility of wrong structures and so on. So I think that could be a very popular model actually. Of course you have to find the right people. As far as people who are doing their individual research projects, I think, as I said before, there will be more and more requirements for much broader approach to biological problems then just one technique. |
| Q2 | So the problem with providing structures as a kind of service industry in the same way as you might provide DNA sequencing or something is that once you have the structure that isn’t really it. You need to understand more about the derivation and, so it has to be a collaboration, it can’t be a provision? And these structural biologists gathered in the ballroom in an Austrian village which is a beautifully European conception, they were crystallographers pure and simple? |
|  | Johann Deisenhofer: Yes. I mean they were, at that time crystallography was the most difficult part of protein structure determination. I mean you had to be a computer programmer, you had to be skilful with your hands, and you had to be very, very careful to keep track of what measurements you made and so on. I mean, as I said before, the computers were a huge bottleneck so you really had to know a lot about crystallography to do that job and the people I mentioned in the ballroom many of them were physicists, some chemists Max Perutz was a chemist and you could clearly see from where they came from. I mean do you know Michael Rossmann. Michael Rossmann was obviously a physicist and he was into developing computation methods. |
| Q14 | Compared with what size for the crystals …? |
|  | Johann Deisenhofer: When I joined Hoover’s group in 1971 I was very disappointed that the crystal was slightly under a millimetre and that means a factor of 1,000 in volume. Because in my physics time I was working with ruby crystals which you could buy and you could specify the size and so we often used 10 by 10 by 20 millimetres or so just to make them easy to handle. One can say through my career I have used smaller and smaller crystals and at some point the whole thing must end at a single molecule but that of course is the dream of the people who want to build the free electron lasers and the, I mean, extremely powerful next generation of synchrotrons and I don’t know whether I will live long enough to see that. |
| Q9 | It’s a nice dream to hold though. The last thing I think that I’d like to ask you about is to return to the question of what the Nobel Prize meant for your work and indeed your life outside of work. You’ve mentioned that there was a slight hiccup in research productivity. In terms of benefit, what do you see as the most tangible benefits of the Prize? |
|  | Johann Deisenhofer: There were two let’s say categories of benefit. One funding and the other one on my internal attitude towards work and towards science and if I start with the latter I really could, I was much more relaxed. After the whole ballyhoo had died down I was much more relaxed than I was as an obscure researcher because I could tell myself, well if I accomplish any more the rest of my life, at least I’ve done something quite /- – -/ so that was good. And funding was of course easier to come by. It has a definite affect. People don’t like to say no to Nobel Laureates so easily. I mean they do but … |
| Q10 | Yes because you mentioned that there were no financial pressures when you were working at the MPI that, for instance, nobody was going to turn around and say, you can’t crystallise membrane proteins, so you can’t do that project. And that free funding environment must be very liberating? |
|  | Johann Deisenhofer: Yes it is but of course, in principle there could have been the Hartmut Michel’s boss or my boss and we could not have done much about it. And I would say it’s not necessarily unlimited the funding in Max Planck. It looks great but on the other hand the groups are much larger and I think less streamlined than the research groups in America and so the groups in Max Planck they tend to become, or at least some of them in Martinsried tended to become worlds of their own, I mean independent, and they became reluctant to interact with others and so on and here we don’t have the money to maintain a group of 50 and it would be an enormous nightmare for me if I had, I mean so I’m perfectly fine with a much smaller group and more interactions with colleagues. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0386 |
| **Biographical** | I was born February 20, 1937 in München as the first child of Sebastian and Helene Huber. My father was cashier at a bank and my mother kept the house and brought up the children, me and my younger sister, a difficult task during the war, a continuous struggle for some milk and bread and search for air-raid shelters. There was no Grammar school in 1945 and 1946 and I entered the Humanistische Karls-Gymnasium in München 1947 with intense teaching of Latin and Greek, some natural science and a few optional monthly hours of chemistry. I learned easily and had time to follow my inclination for sports (light athletics and skiing) and chemistry, which I taught myself by reading all textbooks I could get.  I left the Gymnasium with the Abitur in 1956 and began to study chemistry at the Technische Hochschule (later Technische Universität) in München, where I also made the Diploma in Chemistry in 1960. A stipend of the Bayerisches Ministerium für Erziehung und Kultur and later of the Studienstiftung des Deutschen Volkes helped to relieve financial problems of my family and allowed me to study without delay. The most impressive teachers I remember were W. Hieber and the logical flow and impressive diction of his lectures in inorganic chemistry; [E.O. Fischer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1973/index.html), the young star in metalloorganic chemistry; F. Weygand and his deep knowledge of organic chemistry; and G. Joos and G. Scheibe, the physicist and physicochemist, respectively. I joined the crystallographer W. Hoppe’s laboratory for my diploma work on crystallographic studies of the insect metamorphosis hormone ecdysone. Part of these studies were made in Karlson’s laboratory at the Physiologisch-Chemisches Institut der Universität München, where I found by a simple crystallographic experiment the molecular weight and probable steroid nature of ecdysone which Hoppe and I later elucidated in atomic detail after my thesis work which was on the crystal structure of a diazo compound (1963). This discovery convinced me of the power of crystallography and led me to continue in this field.  After a number of structure determinations of organic compounds and methodical development of Patterson search techniques I began in 1967, with Hoppe’s and Braunitzer’s support, crystallographic work on the insect protein erythrocruorin (with Formanek). The elucidation of this structure and its resemblance to the mammalian globins as determined by Perutz and Kendrew in their classical studies suggested for the first time a universal globin fold. In 1971 the University of Basel offered me a chair of structural biology at the Biozentrum and the Max-Planck-Gesellschaft the position of a director at the Max-Planck-Institut für Biochemie, which I accepted. I remained associated with the Technische Universität München, where I became Professor in 1976.  In 1970, I had begun work on the basic pancreatic trypsin inhibitor which has later become the model compound for the development of protein NMR, molecular dynamics, and experimental folding studies in other laboratories. Work in the field of proteolytic enzymes and their natural inhibitors has been continued and extended to many different inhibitor classes, proteases, their proenzymes, and complexes between them (with Bode, Bartels, Chen, Fehlhammer, Deisenhofer, Loebermann, Kukla, Papamokos, Ruhlmann, Steigemann, Toknoka, Wang, Walter, Weber, Wei) including recently inhibitors of cysteine proteases (with Musil, Bode, Engh) and other hydrolytic enzymes like a-amylase (with Pflugrath, Wiegand) and creatine hydrolase (with Hoeffken). The potential of these systems for drug and protein design has spurred our interest until today.  Early in the seventies I initiated work on immunoglobulins and their fragments, which culminated in the elucidation of several fragments, an intact antibody and its Fc fragment, the first glycoprotein to be analysed in atomic detail (with Colman, Deisenhofer, Epp, Marquart, Matsushima). Work was extended to proteins interacting with immunoglobulins and to complement proteins (with Paques, Jones, Deisenhofer). We also studied a variety of enzymes leading to the elucidation of the structure and the chemical nature of the selenium moiety in glutathione peroxidase (with Ladenstein, Epp). We determined the structures of citrate synthase in different states of ligation (with Remington, Wiegand) and recently of a very large multienzyme complex, heavy riboflavin synthase (with Ladenstein).  Early in the 1 980s we began with studies of proteins involved in excitation energy and electron transfer, light-harvesting proteins (with Schirmer, Bode), later bilin-binding protein, the reaction centre (with Deisenhofer, Epp, Miki in collaboration with Michel) and ascorbate oxidase (with Messerschmidt, Ladenstein) which are described in my lecture.  Most of these structural studies were collaborative undertakings with other laboratories, many of them from foreign countries.  We had discovered that some of the proteins analysed showed large-scale flexibility which was functionally significant. The trypsinogen system was investigated (with Bode) in great detail by low temperature crystallography, gamma-ray spectroscopy, chemical modification, and molecular dynamics calculations. However, it required some years before the scientific community in general accepted that flexibility and disorder are very relevant molecular properties also in other systems.  The development of methods of protein crystallography has been in the focus of my laboratory’s work from the beginning and led to the development of refinement in protein crystallography (with Steigemann, Deisenhofer, Remington), to the development of Patterson search methods (with Bartels and Fehlhammer), to methods and suites of computer programmes for intensity data evaluation and absorption correction (FILME, with Bartels, Bennett, Schwager), for protein crystallographic computing (PROTEIN, with Steigemann), for computer graphics and electron density interpretation and refinement (FRODO, Jones), and for area detector data collection (MADNES, Pflugrath, Messerschmidt). These methods and programmes are in use in many laboratories in the world today.  I married Christa Essig in 1960. We have four children. The eldest daughter (1961) and the two sons (1963, 1966) have been or are studying economics. The youngest daughter (1976) shows some interest in biology, a last hope. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q1 | Today and during the Lindau Meeting there are many opportunities for young students to meet great scientists. What does it give you? |
|  | Robert Huber: I like to interact, to speak with students, and I just like to encourage them to approach me and the other Laureates. We have the forum sitting on the stage. We Laureates are lecturing and I very much hope that the students afterwards come and question us about what we have said and other things. In meeting us I can see the ease for the students, very much so. They do come – 500 this year from all over the world – that’s very good. |
| Q5 | Is it important for young students and scientific researchers to have role models and do you have a responsibility, do you think, as a Nobel Prize winner? |
|  | Robert Huber: I have a responsibility, as an academic teacher I would say, that is not much different from what I do at home at my university or as a visitor to other universities, to speak about my field of science and tell the students that this is interesting, it’s worthwhile to pursue, and to convince them that they work in my field. This is what I do here too. Perhaps it’s a more mixed, more international audience than what I usually have at home or at some universities I visit. So I like it, I must say. |
| Q2 | Were there times when you were a student or back when you were working where there were failures in your work and you maybe felt despair and you had to have a role model? How did you then go about it when there were times which were difficult maybe in your work? |
|  | Robert Huber: You always have that, clearly. And I had excellent teachers. I studied in Munich, chemistry, and I had wonderful teachers in physics and in chemistry, inorganic and organic chemistry, so they were my heroes. The students right now are perhaps a little more to the earth. |
| Q2 | Can you remember a specific time when you faced some difficulties and it was just sheer hard work? |
|  | Robert Huber: I do remember that. It was when /- – -/ I considered giving up my study of chemistry. There was some practical work in technical chemistry which I didn’t like at all and I did not succeed with some of the experiments and had to repeat them and repeat them a second time. I was an excellent student with the best marks and there was a failure. And I felt very miserable concerning myself because it was a failure in a field that I didn’t like. |
| Q15 | When you were given the award, people said that if there was a possibility of recreating this artificially, one can solve the world’s energy problem. Where are we standing at that point? |
|  | Robert Huber: No, I never thought that. It’s certainly not the case. We learned a lot about biology. We learned about physics as well – the physical principles behind biological reactions. But it had no consequences in technical application. So we learned a lot about the method and instrumentations and techniques that we had to use to solve this problem, which was a crystallographic problem. And these methods are then applied to proteins of very different sources and very different biological function. And there is a very clear applied aspect – all those proteins that play a role in disease processes. And there is, as I see it, the future of pharma research and drug development. |
| Q1 | What would your message be to a young student? What is your opinion? How can one encourage people to go into this field? |
|  | Robert Huber: The young people certainly want challenges. They would like to work in the forefront of science and we, the Laureates, try to convince them that work in biology is in fact in the forefront and it produces, even with simple experiments, often unexpected results opening new avenues for research. So to bring that forward there is so much to be discovered, in particular in biology, where we just have scratched the surface of an understanding of the biological phenomena. And on the other hand there are the methods available, an enormous development in methods that we have now available to study biological phenomena. So I would encourage them to study biology and work in biology. There’s so much to be discovered. |
| Q18 | Would you like to get the general public and companies and others to support the scientific world and the young students in a different way? |
|  | Robert Huber: It very much depends also on the media, you see, to bring forward that message that science is fascinating, it is rewarding. It is necessary for countries like ours, like Sweden and Germany – there are no natural resources except our brains – for these nations. So to bring that forward also the media can help. |
| Q6 | What is your greatest memory from your time as a scientist? |
|  | Robert Huber: I can tell you what was very important for me as a student who had just finished his first exams and started to work on a scientific problem – which looking back right now was a very minor problem, but at that time it was important – it was a small molecule a Nobel Laureate was working on and a famous professor associated with him. I joined them as a diploma student, aged 22 or so. And I did a simple experiment and found that they were wrong, they made an error, an essential error, suggesting the molecular shape of this molecule incorrectly. And I could correct them, you see. This was great – I, a nothing. All of this was then very friendly and they liked what I had done. But, for me, it convinced me that I could achieve something and it was very decisive for me to stay in science. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0387 |
| **Biographical** | I was born in Ludwigsburg, Württemberg, in the southwestern part of the Federal Republic of Germany on July 18, 1948, as the elder son of Karl and Frieda Michel. My ancestors lived in that area for generations, mainly as farmers. There the inherited land is equally divided among sisters and brothers, and not enough land was left for one family’s living during my grandparents’ generation. During the day my father worked in a factory as a joiner, my mother at home as a dressmaker, in the evenings and on Saturdays care had to be taken of the huge gardens.  As a child I liked to play outside, to stroll through the fields, and I was an active member of the local children’s gang, frequently being chased by field guards and building supervisors. Nevertheless, my performance at school was very good, and mainly due to the influence of my mother I was allowed to attend high school. At age eleven I became a member of the circulating library of my home town. From there on I was rarely seen outside, but was reading two to four books per week, the subjects ranging from archaeology over ethnology and geography to zoology. Needless to say that I did not do much homework. At school my favorite subjects were history, biology, chemistry and physics. Especially the teaching in physics was excellent. Most of my understanding of it I got at high school, not at the university.  In parallel, my interest in molecular biology rose. In 1969 – after the obligatory military service – I applied to study biochemistry at the University of Tübingen. At that time Tübingen was the only place in Germany, where one could study biochemistry from the first year, and I was happy to be accepted. Studying biochemistry meant that one had to take part in nearly the same amount of lectures and courses as chemistry students in addition to numerous lectures and courses in biology. The atmosphere between senior teachers and students was impersonal, and the only time I talked to the full professor of biochemistry was during the final examination. However, the possibility existed to work for one year in the various biochemistry labs at the University of Munich and the Max-Planck-Institut für Biochemie instead of attending lab courses in Tübingen. I took that chance in 1972/1973, and at the end I was convinced that academic research was what I wanted to do.  After the examination in Tübingen in 1974 I did the experimental part of my biochemistry diploma in Dieter Oesterhelt’s lab at the Friedrich Miescher-Laboratorium of the Max-Planck-Gesellschaft in Tübingen. In cooperation with Walter Stockenius, Dieter Oesterhelt had discovered bacteriorhodopsin in halobacteria and later proposed that it acts as a lightdriven proton pump in the framework of Peter Mitchell’s chemiosmotic theory. During my diploma work I characterized the ATPase-activity of halobacteria. In 1975, Dieter Oesterhelt moved to Würzburg. I joined him, and as a thesis I correlated the intracellular levels of adenosine di- and triphosphate with the electrochemical proton gradient across the halobacterial cell membrane. Having received the doctorate in June 1977 I tried to fuse delipidated bacteriorhodopsin with bacterial vesicles in order to achieve light-driven amino acid uptake. Upon storage in the freezer the delipidated bacteriorhodopsin yielded solid, glass-like aggregates. On the basis of this observation I was convinced that it should be possible to crystallize membrane proteins like bacteriorhodopsin, which was considered to be impossible at that time. With Oesterhelt’s help I started the experiments, and already four weeks later we obtained a new two-dimensional membrane crystal of bacteriorhodopsin. It was not the three-dimensional crystal we wanted, but allowed me to travel to the MRC at Cambridge, England, and to do electron microscopical studies together with Richard Henderson. Back in Würzburg, we observed the first real three-dimensional crystals of bacteriorhodopsin in April 1979. The success led me to cancel my plans to do post-doctoral studies with Susumu Ohno, Duarte, California, on sexual differentiation in mammals. Instead of this, I moved with Dieter Oesterhelt again, this time to the Max-Planck-Institut für Biochemie at Martinsried near Munich, where he became a department head and director. Before moving to Munich, Ilona Leger became my wife. Her understanding and patience helped me a lot.  A promising aspect of the move to Martinsried was the possibility of a cooperation with Robert Huber and colleagues, who at the Max-Planck-Institut had established a very productive department for X-ray crystallographic protein structure analysis. Our bacteriorhodopsin crystals were found to diffract X-rays, but to be too small and too disordered for a structural analysis. We tried to improve size and quality of the crystals. Since all the X-ray crystallographers had beautifully diffracting crystals of soluble proteins, I, understandably, had very limited access to the X-ray equipment at Martinsried. As a consequence, I spent four months at the MRC in Cambridge, England, together with Richard Henderson in 1980, in order to perform X-ray experiments. This period was essential for improving the crystallization method. After my return Dieter Oesterhelt decided to buy an X-ray generator for the ongoing work with bacteriorhodopsin. The generator was installed in Robert Huber’s department and guaranteed us continued access to the equipment, and the know how, of the X-ray crystallographers. Later on, I used this generator for the work with the reaction centres.  Frustrated from the lack of the final success with bacteriorhodopsin, I tried to crystallize several other membrane proteins, mainly photosynthetic ones. After developing a new isolation procedure I obtained the first crystals of the photosynthetic reaction centre from the purple bacterium Rhodopseudomonas viridis at the end of July 1981. One week later our daughter Andrea was born. During September 1981 the first reaction centre crystal was X-rayed by Wolfram Bode and myself, and turned out to be of excellent quality. Therefore 1981 was the happiest and most successful year of my life.  Dieter Oesterhelt immediately agreed that the reaction centre should be a project of the young people. In February 1982, I started the data collection for the X-ray structure analysis. In April or May I gave a seminar in Robert Huber’s department and asked officially for collaboration. After some internal discussions Robert Huber agreed that Johann (“Hans”) Deisenhofer, who was the partner of my choice, should take part in the reaction centre project. During the work Hans and I became the best friends. In August 1982, Hans and Kunio Miki, a Japanese post-doctoral research associate in Robert Huber’s department, started to evaluate the pile of X-ray films. I continued with the experimental work, occasionally helped by Robert Huber, who showed me how the diffraction pattern of a promising derivative should look like. Not only the X-ray work, but also the entire biochemical characterization and sequence determination had to be done. After the preliminary tracing of the peptide chains by Johann Deisenhofer, the sequence determination, which was performed by Karl A. Weyer, Heidi Gruenberg and myself with Dieter Oesterhelt’s support and help, turned out to be the bottle neck for our progress. During that period of heavy work our son Robert Joachim was born in 1984.  As one of the results of the success I received many offers. I accepted the one to become a department head and director at the Max-Planck-Institut für Biophysik in Frankfurt/Main, West Germany, where I am since October 1987.  For the success with the crystallization of membrane proteins and the elucidation of the three-dimensional structure of the photosynthetic reaction centre from the purple bacterium *Rhodopseudomonas viridis* I received various prizes and awards. Among these are the Biophysics Prize of the American Physical Society (together with d. Deisenhofer), the “Chemiedozentenstipendium” of the “Fonds der Chemischen Industrie”, the “Otto Klung-Preis” for chemistry, the Leibniz-Preis of the Deutsche Forschungsgemeinschaft, the “Otto-Bayer-Preis” (together with J. Deisenhofer) and now the Nobel Prize (together with J. Deisenhofer and R. Huber). |
| **Autobiographical** |  |
| **Podcast** | No scripts |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q9 | You’ll have a short talk with us here today. It’s a few years now since you received the prize, in chemistry. What is it today that you have use for in what you discovered at that time or what are you using, on a daily basis, in that discovery, so to speak? |
|  | Hartmut Michel: From the discovery mainly it broke down barriers that now we and other people, more and more other people, try to get structural information about membrane proteins. I work mainly all the time on photosynthesis for materials here, for the synthesis here occurs in this plant, but it also occurs in some bacteria in a more simple version and at the time I worked on that simple version it was good pleasure, because these proteins were nicely coloured and it makes fun to work with nicely coloured material instead of this usual whitish protein stuff. |
| Q18 | Was it then also easy in a sort of a way to explain what you were doing to make this structure and is it important to be able to explain what you’re doing in a sort of visual way? |
|  | Hartmut Michel: I wouldn’t say it’s easy. It depends also to whom you talk, but in general the prize had two major points. One point was that it was the first membrane protein, as I mentioned already, and to crystallise membrane proteins, when I started, was considered to be impossible. There was even a text book statement that it is impossible to crystallise membrane proteins and I thought this statement cannot be true and I worked hard in order to prove the opposite and at the end I was right and it was a lucky coincidence that I worked and I decided to work in photosynthesis, on the heart of photosynthesis, which is in the centre, of course, of the energy conversion which you get from the sun. So all that energy we have, apart from nuclear energy and maybe also a little bit of energy from the deep sea, is derived at the end from the solar energy so this is a very central process on Earth. |
| Q14 | And today, you’re continuing to work on the membrane proteins? |
|  | Hartmut Michel: I gave up mainly photosynthesis and actually I thought at that time that photosystem 2 is still pretty good to go. Photosystem 2 is the system which converts water into oxygen. And the oxygen is released as a waste product and all the oxygen which you have on Earth is derived from the photosynthesis and this kind of photosynthesis in evolution was established about maybe two billion years ago and I still think from the chemistry, to split water, to take away the electron centre and the proton centre, to take away the oxygen molecule, is a highly interesting reaction but I thought it so complicated that it will not make fast progress and I always told people that I will re-start with this difficult project after my official retirement but for the moment, other people made big progress … |
| Q15 | You have mentioned that about 80% of the different drugs that are available on the market today for treating different medical disorders or diseases are working on these membrane proteins. What implication does that have on your scientific works and what is it that you want to achieve there in terms of new discoveries? |
|  | Hartmut Michel: Oh, actually my personal aim is to understand how these membrane proteins perform their function. What is their mechanism of action? What do they, how do they that? And when you know that, you also have some side, beside, projects and this can be used in uplight research and when you know the structure, say, of a hormone interacting with a receptor and when you know how the receptor converts the message and transduces the method to the other side of the membrane, then you can interact with the message and most of our diseases are simply caused by the fact that the messages aren’t getting from, either there’s no message or there’s a permanent message and when you know how the messages are converted, then you will be able to find compounds which stop the wrong message or which correct the message and this is what drugs in the end do and when we have this drugs knowledge, we should be able to develop new drugs against many diseases which, for the moment, cannot be cured or that the drugs are not specific enough or have harmful side effects. |
| Q15 | Could you give us an example where you can see it would be very, very useful for humankind? |
|  | Hartmut Michel: There are many, many diseases in the area but we work mainly now is practically each area of application. Asthma, inflammation, obesity, neural diseases … these all defects in the same kind of class of receptors and this class of receptors, it’s about, I said 58% of all drugs act on membrane proteins and about 60% act on one specific … membrane protein, so called G protein carbon receptors and we have not much structural knowledge. The rhodopsin in the eye which converts the light energy which detects the light energy and you use for vision is the same class of molecule. There we have structural knowledge since two years, it’s a related molecule but of course light is a substrate there, light is used there to trigger the reaction but normally it’s chemicals, it’s hormones, it’s neural transmitter for that. Mainly neural transmitters in the brains are used for the communication between the cells so many psychoses and Parkinson, schizophrenia, are caused by disequilibrium in these receptors in the brain and we would like to understand these better. |
| Q4 | So you can take a drug and then you eat something and it will have a different effect on you than it would have on another person, for example? |
|  | Hartmut Michel:  That is one example and the other example is that many of these enzymes involved in drug metabolism are induced, they are not there if there is nothing to convert, and one example is grapefruit juice and grapefruit juice has many flavonoids and this compound induce enzymes which work for drug metabolism and when you have drunk grapefruit juice, you get these enzymes active. They are not there before, only a very little amount, so they are made and once you have consumed grapefruit juice, then your drug metabolism’s very different then from the day before. This is not only personalised medicine, it’s also medicine from day to day basis and this is difficult to… |
| Q18 | That leads me to the kind of responsibility, you have mentioned, for example, that drug companies develop drugs to make money, it’s profit involved, therefore there are places on Earth where people do not receive certain drugs – we can talk about malaria, for example, it’s changing at the moment. In which way could people like you contribute to that discussion or would you like to contribute to that discussion to make it more clear, you know, if there are any responsibilities on that level? |
|  | Hartmut Michel: I think for the moment the big drug companies, they are clearly aware of their responsibility and they invest also in the tropical diseases, also malaria and I’m very optimistic that with our current methodology we will be soon able to fight malaria and I think one approach which looks for me very promising which was actually discovered in India is that malaria has the synthesised fatty acids themselves by a mechanism which is very different from the mechanism which your body, my body uses, so malaria has to synthesise fatty acids and when you block fatty acids, as it’s in its synthesis, the malaria causing organism is dead so I’m very optimistic for that. We know all now the enzymes which are responsible, most of the enzymes now and for that, we find out which are essential and I think the chance that we really can fight off malaria is very, very good. |
| Q7 | What are the necessary qualities in a team building like that? |
|  | Hartmut Michel: Everybody has a separate project or a certain task within the project and when you are assigned a task, you have to think, you have to consider the education of the person, the knowledge of the person and also the behaviour, how the person would fit into a team. |
| Q17 | And your role? |
|  | Hartmut Michel: My role, I would say mainly I compare it with soccer playing and I’m now the coach, I’m no longer the player. |
| Q18 | How do you view the difference between applied and basic science and what role does an institution like Max Planck Institute, where you work, have? |
|  | Hartmut Michel: If you look back, all of the discoveries which changed our life, improved our life, come out of basic research, not of applied research and that’s the reason why we have to go on with basic research, especially if you go to biomedicine and you look for single cascades in the body and for using and … molecules, then immediately when you discover and you’re into leukine or something like that or xylocaine which has a function in the body, stimulating the growth of a certain type of cell, then this immediately could become very important in medicine so the gap between basic research and applied research in biomedicine in basic, is very, very small. I shouldn’t even say it’s a gap, it’s a bridge. |
| Q3 | And to be working at the institute like this, what is the driving force, so to speak? |
|  | Hartmut Michel: The driving force is the best is if people are really interested to get results and to get new knowledge and of course some people try to just to finish a PhD in order to have the title PhD but these are not the type of research students we would have like to have. Some people like to have the PhD chance in order to further their careers in business or somewhere else and we would like really the people who do the research to create new knowledge. They must have a very high motivation and on the other hand I should say in India the conditions are such that they have many, many students but only very few are allowed to go on to PhD thesis so the competition is very, very hard so their motivation to work hard is extreme, I have to say. |
| Q6 | Is there a moment working in the lab, working with your wish of discovering new things, which was of major importance to you, that you can’t forget? |
|  | Hartmut Michel: In our field it was getting the crystal and especially I clearly remember the summer in 1981, July 1981, when I first discovered the crystal of the reaction centres in my numerous crystallisation attempts. This is number one. The second gap, then, is you have to show what the quality of these crystals is by X-ray crystallography, so when I did the first X-ray photograph and the crystal reflecting beautifully, it was quite clear that I had made the breakthrough and the rest was working it out. So these were the two moments – getting the crystal and showing that they reflect well. |
| Q9 | One last question – in which way did the Nobel Prize change your life, if it did, and would you like another one? |
|  | Hartmut Michel: How did it change my life? One point is that I got it at the age of 40 and this is of course pretty early and as a result of that, your lifestyle changes, not your personal life, like how you live at home. Say how you do, because you are now a person of public interest and this means that you are very often invited and people ask you for advice in society and politics, so you end up on many, many advisory boards. Advisory boards, supervisory boards, and you get many, many more requests and as a result of that, for quite some time I only could react, I could no longer act on myself and I would think if I would have gotten the Nobel Prize much later, I would have been a more successful scientist. I would’ve had much more time which I could have devoted to my research, to my own projects. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0388 |
| **Biographical** | The beginning is distant, and was a time when we as a people were without much of the fruits of science that now refine our lives. But it was also a good time, when family and town were the domain of our existence. My father of Scottish, and mother of German extraction, migrated with their three children from Ontario, Canada, to rural Chester, Vermont, USA, where I was born in the spring of 1919, as the Cram’s fourth and only male child. Two years later, the family moved to Brattleboro, Vermont.  My mother, Joanna, was high-spirited throughout her 94 years, starting with a girlhood rebellion against the strict Mennonite faith in which she was raised. My father, William, was a romantic, a cavalry officer, later working alternately as a successful lawyer and unsuccessful farmer. He died of pneumonia at 53, leaving my mother with a set of Victorian upper-class English values, and the task of providing for and raising my sisters and me, then aged four.  According to my oldest sister, Elizabeth, I was as a child precocious, curious, and constantly in, or causing, trouble. This character trait started at birth; I weighed over ten pounds, and had an unusually large head! Determined to walk at seven months, I pulled a pan of fresh eggs down from a table onto that head. At three years, I broke my first window. My father took me directly to our neighbor, Mr. Mason, to apologize. Reputedly, I said to him, “Sorry, you nasty Mason”.  By the time I was four and a half, I was reading children’s books. My mother, steeped in English literature, cultivated incentive by reading to me only the *beginnings* of tales that involved heroes, heroines, hypocrites, and villains. When we reached the exciting part, she left me with the story to finish by myself. My childhood was adventuresome and idyllic in the things that mattered.  Elementary schooling for me was series of multiclass single-room buildings, where the young and very young witnessed each other being taught. On report card days, I faced searching questions and criticism during which “character grades” were stressed over academic accomplishments. I usually was marked “A” in attitude and accomplishment, “B” in effort, and “C” in obedience.  What I call my real *education* occurred principally outside of the classroom, in a private world of books and brooks. All of Dickens, Kipling, Scott, Shaw, and much of Shakespeare were read. I learned how to run up spring-fed brooks, jumping from one glacier-polished stone to another. I carried firewood, emptied ashes, and shoveled snow for music lessons. I picked apples to be paid in apples, strawberries to be paid in strawberries; and I moved the lawns of a large estate belonging to the dentist who filled my cavities and pulled my teeth.  The rate of exchange of my time for his was fifty to one. During the Christmas rush, I sold ties, shirts, shoes, gloves, and jackets in return for the same. By the time I was fourteen, my mother’s barter arrangements were replaced by a flat fee of 15 cents an hour working for neighbors, raking leaves, hoeing corn, digging potatoes, pitching hay, and delivering newspapers. The last job taught me about debts, dogs, and bicycle bags.  At sixteen, I left my home in Brattleboro, Vermont, a handsome old town on the Connecticut River. By then I had learned how to adapt to eighteen different employers, and had played high school varsity tennis, football, and ice hockey. I also had my full growth of 195 pounds, and was 6 feet tall. My family dispersed at that time, and I drove two elderly ladies to Florida in a Model-A Ford, without benefit of a license, and in return for transportation. Stopping at Lake Worth, Florida, I worked in an ice-cream shop and weeded lawns in return for room and board. There, I continued my secondary schooling while suffering an acute case of homesickness for New England. Nine months later, I hitchhiked north again to Massachusetts, and passed the summer as a house painter and roofer. My twelfth grade was spent at Winwood, a little private school on Long Island, New York, working as a factotum for my tuition and board. While there, I did three things significant to my future. I took a course in chemistry, taught myself solid geometry from a book, and won a $6,000 four-year National Rollins College Honor Scholarship.  While at Rollins, in the resort town of Winter Park, Florida, I worked at chemistry and played at philosophy (four courses!). I read Dostoevski, Spengler, and Tolstoy, and sang in the choir and in a barbershop quartet. In four halcyon years, I obtained an airplane pilot’s license, acted in plays, produced-announced a minor radio program, and, while my fellow students complained, dined on the best food I had yet encountered.  During the summers of 1938 to 1941, I worked for the National Biscuit Company in New York City, at first as a salesman covering an area from 144th Street to 78th Street on the tough East Side. The largest city in the country was also its best teacher. This was my first exposure to ghetto slums, youth gang warfare, drugs, prostitution, and petty thievery. The northern end of my territory was dominated by Jewish delicatessens, out of which at first (but not later) I was urged to leave. I got to know everybody, and everybody’s business. In the Irish-run grocery stores, we would conduct “wakes” for the last fig bar in the bin. Harlem was a place where each street was a playground, and each store a small fort. The Puerto Rican district was full of street vendors of all sorts. I started that summer weighing 195 pounds, and ended it at 155. My $ 15 per week provided me with cash, and a crash course in ethnic groups and big city street-life. The other summers involved analyzing cheeses for moisture and fat content in the National Biscuit Laboratories.  From these routine jobs, I extracted pleasure by making them into games. They taught me self-discipline, and illustrated how I did not want to spend my life. But this period provided me with a keen interest in the differences between people, and an overwhelming dislike of repetitive activities. When the word “research” entered my vocabulary, it had a magic ring, suggesting the search for new phenomena. Chemical research became my god, and the conducting of it, my act of prayer, from 1938 to the present. When told by my first college chemistry professor, Dr. Guy Waddington, that he thought I would make a good industrial investigator – but probably not a good academic one – I determined upon an academic research career in chemistry.  Out of 17 applications for teaching assistantships to go to graduate school, three offers came. I accepted the University of Nebraska’s, where an MS was granted in 1942. My thesis research there was done under the supervision of Dr. Norman O. Cromwell. By then, World War II was upon us, so I went to work for Merck & Co., ultimately on their penicillin project, where my search for excellence was symbolized by Dr. Max Tishler. Immediately after the war ended in 1945, Max arranged for me to attend Harvard University, working for Professor L.F. Fieser. The work for my Ph.D. degree on a National Research Council Fellowship was in hand in eighteen months.  At Harvard, scientific excellence was personified for me by Professors Paul D. Bartlett and Robert B. Woodward. After three months at M.I.T., working for Professor John D. Roberts, I set out for the University of California at Los Angeles on August 1, 1947, and have taught and researched there ever since, after 1985 as the S. Winstein Professor of Chemistry.  In retrospect, I judge that my father’s death early in my life forced me to construct a model for my character composed of pieces taken from many different individuals, some being people I studied, and others lifted from books. The late Professor Saul Winstein, my colleague, friend, and competitor, contributed much to this model. It was almost complete by the time I was 35 years of age. Thus, through the early death of my father, I had an opportunity – indeed the necessity – to animate that father image that was slowly maturing in my mind’s eye. And, at last, I realized who in fact this figure was. It was I.  The times and environment have been very good to me during my forty-six years of chemical research. I entered the profession at a period when physical, organic, and biochemistry were being integrated, when new spectroscopic windows on chemical structures were being opened, and when UCLA, a fine new university campus, was growing from a provincial to a world-class institution. My over 200 co-workers have shared with me the miseries of many failures and the pleasures of some triumphs. Their careers are my finest monument. My countrymen have supported our research without mandating its character. Jean Turner Cram, as my first wife, sacrificed for my career from 1940 to 1968. Dr. Jane Maxwell Cram, my second wife, acted as foil, unsparing but inspiring critic and research strategist in ways beyond mention.  My fellow scientists have generously honored my research program with three American Chemical Society awards: for Creative Work in Synthetic Organic Chemistry; the Arthur C. Cope Award for Distinguished Achievement in Organic Chemistry; and the Roger Adams Award in Organic Chemistry. Local sections of the same society awarded me the Willard Gibbs and Tolman Medals. I was elected to membership in the National Academy of Science (1961), to become the 1974 California Scientist of the Year, and the 1976 Chemistry Lecturer and Medalist of the Royal Institute of Chemistry (UK). In 1977, I was given an Honorary Doctor’s degree from Sweden’s Uppsala University, and in 1983 a similar one from the University of Southern California.  I have contributed directly to the teaching of organic chemistry-about 12,000 undergraduate students-and, indirectly, by writing three textbooks: Organic Chemistry (with G.S. Hammond; translated into twelve languages), Elements of Organic Chemistry (with D.H. Richards and G. S. Hammond; three translations), and Essence of Organic Chemistry (with J.M. Cram; one translation), plus the monograph, “Fundamentals of Carbanion Chemistry” (one translation). I enjoy skiing and surfboarding, playing tennis, and playing the guitar as an accompaniment to my singing folk songs. The award of a Nobel Prize at the age of 68 years was ideally timed to enhance rather than divert my research career.  In the four years that have elapsed since I shared a Nobel Prize in Chemistry (1987), the effect of receiving this honor on my life has been profound. Most importantly, the Prize has extended my career by enough years to allow me to obtain the most exciting results of my 50 years of carrying out research. The Prize has also broadened the range of my experiences, most of which have been both interesting and educational. Finally, the research field of molecular recognition in organic chemistry gained much impetus by being recognized by the Nobel Prize. I am grateful that our research results were chosen as a vehicle for honoring those who know the joys of carrying out organic chemical research. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0389 |
| **Biographical** | I was born on September 30 1939 in Rosheim, a small medieval city of Alsace in France. My father, Pierre Lehn, then a baker, was very interested in music, played the piano and the organ and became later, having given up the bakery, the organist of the city. My mother Marie kept the house and the shop. I was the eldest of four sons and helped out in the shop with my first brother. I grew up in Rosheim during the years of the second world war, went to primary school after the war and, at age eleven, I entered high school, the Collège Freppel, located in Obernai, a small city about five kilometers from Rosheim. During these years I began to play the piano and the organ, and with time music has become my major interest outside science. My high school studies from 1950 to 1957 were in classics, with Latin, Greek, German, and English languages, French literature and, during the last year, philosophy, on which I was especially keen. However, I also became interested in sciences, especially chemistry, so that I obtained the baccalauréat in Philosophy in July 1957 and in Experimental Sciences in September of the same year.  I envisaged to study philosophy at the University of Strasbourg, but being still undecided, I began with first year courses in physical, chemical and natural sciences (SPCN). During this year 1957/58, I was impressed by the coherent and rigorous structure of organic chemistry. I was particularly receptive to the experimental power of organic chemistry, which was able to convert at will, it seemed, complicated substances into one another following well defined rules and routes. I bought myself compounds and glassware and began performing laboratory practice experiments at my parents home. The seed was sown, so that when, the next year, I followed the stimulating lectures of a newly appointed young professor, Guy Ourisson, it became clear to me that I wanted to do research in organic chemistry.  After having obtained the degree of Licencié-ès-Sciences (Bachelor), I entered Ourisson’s laboratory in October of 1960, as a junior member of the Centre National de la Recherche Scientifique in order to work towards a Ph.D. degree. This was the first decisive stage of my training. My work was concerned with conformational and physico-chemical properties of triterpenes. Being in charge of our first NMR spectrometer, I was led to penetrate more deeply into the arcanes of this very powerful physical method; this was to be of much importance for later studies. My first scientific paper in 1961 reported an additivity rule for substituent induced shifts of proton NMR signals in steroid derivatives.  Having obtained my degree of Docteur ès Sciences (Ph.D.) in June of 1963, I spent a year in the laboratory of [Robert Burns Woodward](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1965/index.html) at Harvard University, where I took part in the immense enterprise of the total synthesis of Vitamin B12. This was the second decisive stage of my life as a researcher. I also followed a course in quantum mechanics and performed my first computations with [Roald Hoffmann](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1981/index.html). I had the chance to witness in 1964 the initial stages of what was to become the Woodward-Hoffmann rules.  After my return to Strasbourg, I began to work in the area of physical organic chemistry, where I could combine the knowledge acquired in organic chemistry, in quantum theory and on physical methods. It was clear that, in order to be able to better analyze physical properties of molecules, a powerful means was to synthesize compounds that would be especially well suited for revealing a given property and its relationships to structure. This orientation characterized the years 1965- 1970 of my activities and of my young laboratory, newly established after my appointment in 1966 as maître de conférences (assistant professor) at the Chemistry Department of the University of Strasbourg. Our main research topics were concerned with NMR studies of conformational rate processes, nitrogen inversion, quadrupolar relaxation, molecular motions and liquid structure, as well as ab initio quantum chemical computations of inversion barriers, of electronic structures and later on, of stereoelectronic effects.  While pursuing these projects, my interest for the processes occurring in the nervous system (stemming diffusely from the first year courses in biology as well as from my earlier inclination towards philosophy), led me to wonder how a chemist might contribute to their study. The electrical phenomena in nerve cells depend on sodium and potassium ion distributions across membranes. A possible entry into the field was to try to affect the processes which allow ion transport and gradients to be established. I related this to the then very recent observations that natural antibiotics were able to make membranes permeable to cations. It thus appeared possible to devise chemical substances that would display similar properties. The search for such compounds led to the design of cation cryptates, on which work was started in October 1967. This area of research expanded rapidly, taking up eventually the major part of my group and developing into what I later on termed “supramolecular chemistry”. Organic, inorganic and biological aspects of this field were explored and investigations are continuing. In 1976 another line of research was started in the area of artificial photosynthesis and the storage and chemical conversion of solar energy; it was first concerned with the photoly is of water and later with the photoreduction of carbon dioxide.  I was promoted associate professor in early 1970 and full professor in October of the same year. I spent the two spring semesters of 1972 and 1974 as visiting professor at Harvard University giving lectures and directing a research project. This relationship extended on a loose basis to 1980. In 1979, I was elected to the chair of “Chimie des Interactions Moléculaires” at the Collège de France in Paris. I took over the chemistry laboratory of the Collège de France when Alain Horeau retired in 1980 and thereafter divided my time between the two laboratories in Strasbourg and in Paris, a situation continuing up to the present. New lines of research developed, in particular on combining the recognition, transport and catalytic properties displayed by supramolecular species with the features of organized phases, the long range goal being to design and realize “molecular devices”, molecular components that would eventually be able to perform signal and information processing at the molecular level. A major research effort is presently also devoted to supramolecular self-organisation, the design and properties of “programmed” supramolecular systems.  The scientific work, performed over twenty years with about 150 collaborators from over twenty countries, has been described in about 400 publications and review papers. Over the years I was visiting professor at other institutions, the E.T.H. in Zürich, the Universities of Cambridge, Barcelona, Frankfurt.  In 1965 I married Sylvie Lederer and we have two sons, David (born 1966) and Mathias (born 1969).  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1987*, Editor Wilhelm Odelberg, [Nobel Foundation], Stockholm, 1988  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 1987 **Addendum, January 2006** Research Activities – Update Over the years, the studies in supramolecular chemistry in my laboratory at the Université Louis Pasteur in Strasbourg extended into a broad new area at the interface of chemistry with biology: *self-organization processes*, making use of molecular recognition to control and direct the spontaneous formation of functional architectures of high complexity.  Realizing that recognition implies information, led to the concepts of molecular programming and of *programmed chemical systems*, undergoing self-organization on the basis of the molecular storage of information and its processing at the supramolecular level through algorithms defined by the specific features of the intermolecular interaction patterns involved in the system considered.  These investigations provide steps towards a progressive understanding of the passage from condensed matter to organized matter, of which living organisms represent the highest expression. They seek to lay the chemical, molecular and supramolecular foundations on which the highly complex events of biological self-organization are built and to provide means for analyzing their mechanism as well as for acting on them.  A variety of chemical self-organizing systems of either organic or inorganic nature were designed and studied, leading to the generation of various organic and inorganic supramolecular architectures from molecular components assembled respectively through hydrogen-bonding and ligand-metal ion recognition processes, such as – the “*helicates*“, double, triple, and circular inorganic helices, – multicompartmental *nanocylinders*, –”*grid-type*” entities, ordered polymetallic arrays presenting a range of intriguing physico-chemical properties (multiple redox states, spin crossover magnetism, etc.).  The combination of molecular “softwares” specific for the assembly of different architectures into a single programme gave rise to the concept of *multiple expression of molecular information*, whereby the supramolecular processing of the information by different recognition algorithms generates different outputs, in a one code/several products mode.  Introducing the concepts and results of supramolecular chemistry into materials science, led to the emergence and the development of *supramolecular polymer chemistry*, as a new area in polymer chemistry.  On the other hand, the elaboration of approaches towards the generation of architectures for nanostructured materials stresses the broad impact that self-organization may have in *nanoscience and nanotechnology*, by allowing the potential replacement of tedious and expensive fabrication and addressing procedures by powerful self-fabrication and self-addressing processes.  Starting in the early 1990s, a novel line of research was initiated. It developed from the implementation of a basic feature of supramolecular chemistry, the fact that it is by essence a *dynamic chemistry* with respect to the constitution of its entities. Indeed, a supramolecular entity may continuously exchange, incorporate/decorporate and rearrange its molecular components, and thus continuously modify its constitution as a consequence of the inherent reversibility of the non-covalent interactions that connect these components. Importing such dynamic features into molecular chemistry requires the intentional introduction of reversible covalent bonds into molecules, so as to confer upon them a plasticity in constitution, characteristic of supramolecular chemistry.  These considerations led to the definition of a general concept, covering molecular as well as supramolecular chemistry, that of *constitutional dynamic chemistry (CDC)*.  Thus, dynamic chemistry, that is usually considered to concern either reaction dynamics or motional dynamics, is extended to the very *constitution* of chemical entities itself. CDC may have a profound impact on numerous areas of investigation from drug discovery, to materials science and to nanotechnology.  On the molecular/covalent level, CDC covers *dynamic combinatorial chemistry* (DCC), an approach that, in contrast to classical “static” combinatorial chemistry based on vast collections of prefabricated molecules, implements dynamic libraries whose constituents undergo continuous interconversion by recombination of their building blocks through reversible chemical reactions. Addition of a target molecule to the dynamic set creates a driving force that favours the formation of the best-binding constituent – a self-screening process operating on the basis of molecular recognition between the partners. The application of this methodology to biological systems has allowed the generation of biologically active substances, in particular enzyme inhibitors (carbonic anhydrase, acetylcholine esterase). It is capable, in principle, of accelerating the identification of lead compounds for drug discovery.  In the area of materials science, CDC has been implemented in the development of dynamic polymers, *dynamers*, reversible polymers of molecular as well as supramolecular nature, such as dynamic polyamides. The potential uses of dynamers resulting from their reversibility have been explored towards applications in areas such as degradable materials and controlled release of active substances through collaborations with companies, leading to a number of patents.  CDC introduces a paradigm shift with respect to constitutionally static chemistry. The latter relies on design for the generation of a target entity, whereas CDC takes advantage of dynamic diversity to allow variation and selection. The implementation of selection in chemistry introduces a fundamental change in outlook. Whereas *self-organization by design* strives to achieve full control over the output molecular or supramolecular entity by explicit programming, *self-organization by selection* operates on dynamic constitutional diversity in response to either internal or external factors to achieve adaptation in a darwinistic fashion.  By extending its characteristic features information/programmability, dynamics/reversibility, constitution/structural diversity, supramolecular chemistry is thus impacting molecular chemistry, leading towards the emergence of *adaptive* and *evolutive chemistry*.  The research activities of my laboratory at the Collège de France in Paris covered several topics different from but related to those pursued in Strasbourg: – molecular recognition of nucleic acid features by macrocycles containing intercaland groups; – extension of transport processes towards gene transfer through the design of efficient cationic vectors; – implementation of molecular recognition in lipid vesicles, leading to selective aggregation and fusion processes between vesicles doped with complementary recognition groups (recosomes) interacting through hydrogen bonding or metal coordination. Reversible photochemical reactions were used for introducing switching capability into molecular wires and for developing write/read/erase processes for information storage.  In 1998, I set up and directed a research group at the Nanotechnology Institute newly created in the Research Center of Karlsruhe. This allowed to offer to former post-doctoral coworkers the opportunity to develop and to progressively set up independent research activities in nanoscience and nanotechnology.  In the years since 1987, I also had the occasion to engage in activities of general interest. They were as diverse as being Founding Chairman of a new chemical journal “Chemistry, a European Journal”, created in 1995 and truly European as it is now co-owned by 14 European chemical societies. It also gave the starting impetus to a range of European journals (European Journal of Organic Chemistry, European Journal of Inorganic Chemistry, ChemBioChem, ChemPhysChem) that resulted from the termination of national journals of long tradition (such as Berichte der Deutschen Chemischen Gesellschaft, Liebigs Annalen der Chemie, Bulletin de la Société Chimique de France, Gazzetta Chimica Italiana, Recueil des Travaux Chimiques des Pays Bas, Bulletin des Sociétés Chimiques Belges), an all too rare manifestation of European spirit and supranationality bridging historical divides!  A notable activity was the scientific planning of a novel institute ISIS (Institut de Science et d’Ingénierie Supramoléculaires) inaugurated in December 2002, housed in an attractive new building generously financed by the local authorities and provided in equipment and positions thanks to the strong support of the French Ministery of Research. It was possible to assemble a number of high level senior scientists from various countries, promising junior scientists as well as research laboratories from companies in a very stimulating atmosphere.  I have over the years been involved in a number of public and private boards and committees as well as participated in several start-up companies.  Finally, as president of the non-governmental organization IOCD (International Organization for Chemical Sciences in Development), I have tried, with a group of highly dedicated colleagues, to contribute to helping chemists in developing countries.  The scientific work performed now over forty years with about 300 collaborators from over twenty countries has been described in about 800 publications and review papers as well as two books. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** | 389 |
| Q9 | Welcome to the Nobel interview, Professor Jean-Marie Lehn. You are one of a few French Nobel Prize winners I would say. How did the prize change your life or academic work? |
|  | Jean-Marie Lehn: It didn’t change my work because when you are a scientist you have your own way of looking at things and the prize doesn’t change that so much. It may make you think about starting to change fields. That is possible. On the other hand, it changed life quite a bit. First of all one has more to speak about science and chemistry because of this resonance the Nobel Prize now has in the society, in newspapers, in media. You are supposed to be interpreter of science in some ways. To speak in more public occasions which we must do. One may or may not like it but one must do it because it’s a way to talk about your field. To talk about your colleagues. To talk about what’s going on in the area. Maybe more generally even to talk about what science is doing in society. And this is the most important I would say. I like to talk about what chemistry has been doing, which often is underestimated and very much neglected. But more generally I think just trying to tell people that science is part of culture. Not only literature and arts. Science is also part of our culture. It penetrates our life in practical terms even more than any other forms of human culture. |
| Q9 | After the Nobel Prize, did you consider changing fields? |
|  | Jean-Marie Lehn: Yes, you consider it but you know, on the other hand I’m of the type where I like to deduce things from one another. So the field has evolved enormously. And in fact, what we are doing now has absolutely nothing to do with what I got the Nobel Prize for. But it is deduced from it. It came out rationally by expanding it and by going to other areas. It’s a total change in subject in the type of the things we are doing in the laboratory. But it is not a change in the direction. It’s an evolution which when you look back at it looks quite logical. |
| Q4 | What kind of molecules do you study? All super molecules? |
|  | Jean-Marie Lehn: Life is short. And the day is only 24 hours. That is the problem. We would like to have days with many many more hours and life’s much longer. So you have to select things. So we started with trying to understand simple processes. The idea is the following. The way I started is sort of an inductive way. It starts from something small and you see it’s wider than you initially thought. And then you go ahead with it. |
| Q2 | When did you start? |
|  | Jean-Marie Lehn: 1967. 66/67. But it started in what you might call an awkward way. It didn’t lead to that. I didn’t call it super molecular chemistry at the start because you don’t realise when field is very broad you get the broad ideas only when you see what you are doing is more general than what you thought it was. So I was in fact interested in neuro chemistry which seems to have nothing to do with it. In fact I was interested in philosophy first. I wanted to study philosophy first. |
| Q2 | Philosophy of science or philosophy? |
|  | Jean-Marie Lehn: Philosophy in general. Philosophy of knowledge. That’s what I was interested in. I was extremely interested when I was a youngster in the way we think. What is in there? Of course a fantastic problem. But you soon realise that I don’t think we have an answer to that. So let’s be more modest and study something where progressively we build up this area. At least approach it. And so I thought neuro chemistry, neuro system, has something to do with thinking so why not start there?  And then if you are a chemist you realise that the neuro system is extremely complicated and you cannot approach it like that. And one simple way of approaching it is to try to see if there is a process inter neural system which is simple and which one might have access to and might study. This is the following which I became interested in. In the nerve membrane the propagation of the electric influx, the neural influx occurs by pumping or by exchanging across the membrane of the nerves of two types of ions. Entities called ions. Potassium and sodium. That’s exactly how our nerves function. These run along like this. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0390 |
| **Biographical** | I was born in Pusan, Korea, on October 3, 1904. My father Brede Pedersen, was a Norwegian marine engineer who left home as a young man and shipped out as an engineer on a steam freighter to the Far East. He eventually arrived in Korea and joined the fleet of the Korean customs service, which was administered by the British. Later, he abandoned seafaring and became a mechanical engineer at the Unsan Mines in what is now the northwestern section of present-day North Korea.  My mother, Takino Yasui, was born in 1874 in Japan. She had accompanied her family to Korea when they decided to enter large-scale trade in soybeans and silkworms. They established headquarters not far from the Unsan Mines, where she met my father. I had a sister, Astrid, five years my senior, and an elder brother who died in childhood prior to my birth.  The Unsan Mines were an American gold and lumber concession, 500 square miles in area. Because the mines were administered by Americans, there was an effort to make life there as American as possible. English was the spoken language, and it was the language I learned as a child. Foreign language schools did not exist in Korea at that time and so at the age of 8 years I was sent to Japan to attend a convent school in Nagasaki. When I was 10 years old my mother took me to Yokohama, and I began my studies at St. Joseph College. St. Joseph’s was a preparatory school run by a Roman Catholic religious order of priests and brothers called the Society of Mary, also known as the Marianists. There I received a general secondary education and took my first course in chemistry.  When it came time for university, I chose, with my father’s encouragement, to study in America. I selected the University of Dayton because it was in Ohio were we had family and friends and because it too was run by the Society of Mary. After taking a bachelor’s degree in chemical engineering at the University of Dayton, I went to the Massachusetts Institute of Technology where I obtained a master’s degree in organic chemistry. I did not remain at MIT to take a Ph.D.; I was still being supported by my father, and I was anxious to begin working. In 1927, I obtained employment at the Du Pont Company in Wilmington, Delaware, through the good offices of Professor James F. Norris, a very prominent professor and my research advisor. At Du Pont, I was fortunate enough to be directed to research at Jackson Laboratory by William S. Calcott. I remained at Du Pont for my entire 42-year career as a chemist.  As a new scientist I was initially set to work on a series of typical problems, which I solved successfully. After a while, I began to search for oil-solvable precipitants for copper, and I found the first good metal deactivator for petroleum products. As a result of this work, I developed a great interest in the affects of various ligands on the catalytic properties of copper and the transition elements generally and worked in the field for several years.  I next expanded my interests in the oxidative degradation of the substrates I was working on, namely petroleum products and rubber. By the mid-1940s I was in full career, having established myself in the field of antioxidants and independent in terms of the problems I might choose. In 1947, I was appointed research associate, then the highest title that a Du Pont Company researcher could attain. Also at that time, I married Susan Ault and settled in the town of Salem, New Jersey, where I have lived ever since.  During the late ’40s and ’50s my scientific interests became more varied. I became interested in the photochemistry of some new phthalocyanine adducts and of quinoneimine dioxides. I developed polymerization initiators and even made some novel polymers. In 1960 I returned to investigations in coordination chemistry, and decided to study the effects of bi- and multidentate phenolic ligands on the catalytic properties of the vanadyl group, VO. In the course of these investigations one of my experiments yielded an unexpected small quantity of unknown white crystals which I eventually identified as dibenzo18-crown-6, first crown ether. The last nine years of my career were spent in the further study of crown ethers. I retired from Du Pont in 1969. During my retirement, I have pursued interests in fishing, gardening, bird study and poetry. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0391 |
| **Biographical** | I was born in San Jose, California on June 18, 1932, the first of six children of Robert and Dorothy Herschbach. My father was then a building contractor and later a rabbit breeder. His family had lived in this part of California for three generations; although our surname comes from a pair of villages in the Rhine Valley, most of his immediate ancestors were of English or Irish origin. My mother’s family had moved to San Jose from Illinois when she was a young girl; most of her known ancestors were of German, Dutch, or French origin.  In my boyhood we lived in what was then a rural area of fruit orchards, only a few miles outside San Jose. For years I milked a cow, fed the pigs and chickens, and during summers picked prunes, apricots, and walnuts. From an early age I loved to read but was also very involved in outdoor activities, scouting, and sports. My interest in science was excited at age nine by an article on astronomy in *National Geographic;* the author was Donald Menzel of the Harvard Observatory. For the next few years, I regularly made star maps and snuck out at night to make observations from a locust tree in our back yard.  When I attended Campbell High School, I took all the science and mathematics courses offered. Chemistry I found at first puzzling and then most intriguing, thanks to John Meischke, a superb teacher. At the time, I was at least as interested in football and other sports; perhaps that presaged my later pursuit of molecular collisions. Like most of my classmates, I did not expect to attend college; none of my known relatives had graduated from a university. However, my teachers and coaches presumed I would go. Indeed, I received offers of football scholarships from some universities to which I had not even applied for admission.  I entered Stanford University in 1950 and found a new world with vastly broader intellectual horizons than I’d imagined. Although I gladly played freshman football, I had turned down an athletic scholarship in favor of an academic one. This permitted me to give up varsity football after spring practice, in reaction to a dictum by the head coach that we not take any lab courses during the season. By then the lab and library already were for me much the more exciting playground. My chief mentor at Stanford was Harold Johnston, who imbued me with his passion for chemical kinetics. Many other subjects and professors were also compelling and I took up to ten courses a term. Mathematics was especially appealing; I so admired the teaching of Harold Bacon, George Polya, Gabor Szego, and Bob Weinstock that I simply took all the courses they gave. I received the B.S. in mathematics in 1954 and the M.S. in chemistry in 1955. My Master’s thesis, done under the direction of Harold Johnston, was titled: “Theoretical Pre-exponential Factors for Bimolecular Reactions.” It employed the transition-state theory of Henry Eyring and Michael Polanyi and treated the proportionality factor in the most venerable formula of chemical kinetics, the Arrhenius equation.  My graduate study continued at Harvard, where again I found an exhilarating academic environment. I received the A.M. in Physics in 1956 and the Ph.D. in Chemical Physics in 1958. My Doctoral Thesis, done under E. Bright Wilson, Jr., was titled: “Internal Rotation and Microwave Spectroscopy”. This presented theoretical calculations and experiments dealing with hindered internal rotation of methyl groups. The height of the hindering barrier could be accurately determined because the observed spectra were very sensitive to tunneling between equivalent potential minima. Much that shaped my later research I learned from Bright Wilson and other faculty, especially George Kistiakowsky and Bill Klemperer, or from fellow students, especially Jerry Swalen, Victor Laurie and Larry Krisher. My thesis work also benefited from visits of several months to take spectra at the National Research Council in Ottawa and to compute Mathieu functions at Los Alamos National Laboratory. During 1957-1959, while a Junior Fellow in the Society of Fellows at Harvard, I developed plans for molecular beam studies of elementary chemical reactions.  This work was launched at the University of California at Berkeley, where I was appointed an Assistant Professor of Chemistry in 1959 and became an Associate Professor in 1961. The chief experiments dealt with reactions of alkali atoms with alkyl iodides, systems studied forty years before by Michael Polanyi. Rather simple apparatus sufficed to attain single-collision conditions and revealed that the product molecules emerged with a preferred range of recoil angle and translational energy. The possibility of resolving such features of reaction dynamics encouraged other workers pursuing kindred experiments and fostered an outburst of new theory. My early work thus interacted particularly with that of Richard Bernstein, Sheldon Datz, Ned Greene, John Polanyi, John Ross, and Peter Toennies.  This new field developed rapidly after I returned to Harvard in 1963 as Professor of Chemistry. We studied a wide range of alkali reactions and found several prototype modes of reaction dynamics which could be correlated with the electronic structure of the target molecule. Processes involving abrupt, *impulsive* bond exchange or formation of a *persistent* complex comprise the two major categories. In 1967 Yuan Lee joined our group as a postdoctoral fellow and led the construction of a “supermachine”. This employed greatly augmented differential pumping, sophisticated mass spectroscopy using ion counting techniques adapted from nuclear physics, and supersonic beam sources advocated by enterprising chemical engineers, especially John Fenn and Jim Anderson. The new machine greatly extended the scope of crossed-beam experiments, taking us “beyond the alkali age”. In particular, we were then able to study the same reactions elucidated by John Polanyi with his complementary method of infrared chemiluminescence. This much enhanced the interpretation of reaction dynamics in terms of electronic structure.  The most representative descriptions of the work recognized by the Nobel Prize probably appeared in:  Adv. Chem. Phys. 10, 319-393 (1966). Disc. Faraday Soc. 44, 108-122 (1967). J. Chem. Phys. 56, 769-788 (1972). Faraday Disc. Chem. Soc. 55, 233-251 (1973). Pure and Applied Chem. 47, 61-73 (1976). Mol. Phys. 35, 541-573 (1978). J. Phys. Chem. 87, 2781-2786 (1983).  In current research we are developing a method for simultaneous measurement of three or four vector properties of reactive collisions, such as reactant or product relative velocities or rotational angular momenta. Theory has shown that data on correlations among these vectors can undo much of the averaging over initial molecular orientations and impact parameters and thereby reveal more incisive information about reaction dynamics. Other studies deal with processes akin to liquid-phase reactions by solvating reactant molecules during a supersonic expansion. We are also examining bulk liquid interactions by means of vibrational frequency shifts induced by high pressure; this offers a way to determine solute-solvent intermolecular forces. In addition to theoretical studies related to these experiments, we are pursuing a new approach to electronic structure calculations which exploits exact solutions obtainable in the limit of one- and infintie-dimension. For two-electron systems this has given high accuracy for the electron correlation energy with far less effort than conventional methods.  Other biographical items pertaining to Harvard include my appointment in 1976 as Frank B. Baird, Jr. Professor of Science; service as Chairman of the Chemical Physics program (1964-1977) and the Chemistry Department (1977-1980), as a member of the Faculty Council (1980-1983), and as Co Master with my wife of Currier House (1981-1986). At Currier we were in effect reincarnated as undergraduates to preside over an extremely lively community of 400 students and tutors. Typical of many memorable episodes was the night we were summoned to a student’s room to meet a seal in the bathtub. My teaching includes graduate courses in quantum mechanics, chemical kinetics, molecular spectroscopy, and collision theory. In recent years I have given undergraduate courses in physical chemistry and especially general chemistry for freshmen, my most challenging assignment.  Away from Harvard, I have been a Visiting Professor at Göttingen University in 1963, a Guggenheim Fellow at Freiburg University in 1968, a Visiting Fellow of the Joins Institute of Laboratory Astrophysics in 1969, and a Sherman Fairchild Scholar at the California Institute of Technology in 1976. I also serve as a consultant to Aerodyne Corporation, the Fluorocarbon Research Panel, and Los Alamos National Laboratory. I was appointed an Exxon Faculty Fellow in 1981 and visit regularly the Corporate Research Laboratory in New Jersey to participate in projects there. I have also served since 1980 as an Associate Editor of the Journal of Physical Chemistry.  Other honors include election to the American Academy of Arts and Sciences in 1964 and to the National Academy of Sciences in 1967; the Pure Chemistry Prize of the American Chemical Society in 1965, the [Linus Pauling](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1954/index.html) Medal in 1978, the Michael Polanyi Medal in 1981, and the Irving Langmuir Prize in Chemical Physics in 1983. The University of Toronto bestowed in 1977 the D. Sc., *honoris causa.*  Chemistry also brought my wife Georgene Botyos to Harvard as an organic graduate student. We were married in 1964 and our daughters Lisa and Brenda arrived as harbingers before she received her Ph.D. in 1968. Georgene is now Assistant Dean of Harvard College, a multifaceted position that often requires delicate personal chemistry. Lisa is now a junior in humanities at Stanford, this year enjoying the overseas option at Oxford. Brenda is a junior in chemistry at Harvard, already pursuing research. Our home is in Lincoln, Massachusetts. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** | 391 |
| Q2 | That was the backdrop to you somehow developing an interest in science as a child. Where did that come from? |
|  | Dudley Herschbach: Actually, I can tell you very specifically. I just had my 11th birthday and we were visiting my grandmother who lived, like we did, near the edge of San Jose. I always enjoyed going there because she had a *National Geographic* magazine, which of course was filled with beautiful photographs and all. This one had an article on the heavens above by Donald H. Menzel, Harvard college observatory ‒ this is way out in San Jose, California. It was certainly the first time I heard of Harvard and I wasn’t even aware of hearing it, but then of course … but had these gorgeous star maps with the mythological figures for the consolation super bowls, I fell in love with them. My grandmother gave me that issue because she saw how much I was taken with it, and I started making my own star maps and reading about astronomy and climbing up a tree we had in the backyard at night and I had a flashlight, it just clicked briefly, I didn’t want to light adapt my eyes, picking out the pattern I made and print pick the paper with patterns of the constellations. It just fascinated me. |
| Q10 | Was there an appropriate sort of structure to foster this interest, this burgeoning interest in science then? |
|  | Dudley Herschbach: Well, it was fostered. I was then in a little four room school, two grades per school. My graduating class from that grammar school was 11 kids, but there was a bookcase about this wide, about this high, and it had a couple of books, as my teacher pointed out, about the planets and all. Those were the first scientific things I read. Then of course our town had a little library and I began reading things there and I just got more and more interested in science. By the time I went to high school, of course I was interested in taking science courses and all, but even then, I wasn’t even thinking of going to college yet for people of my vintage and that social category it was not something you expected to do. But I was a good athlete and my coaches would say, “Of course you’re going to college”. Then my teachers began saying the same thing because I was a very good student, so then I began to realize, oh, I guess maybe I’ll try college. |
| Q6 | But the original impetus to come to go to college was coming from the sports side, a sports scholarship? |
|  | Dudley Herschbach: It was, yes. I was quite a good football player, American football, and in fact, when it came to applying to college, I got many telegrams, congratulating me on being admitted to colleges and universities I hadn’t applied to at all. I had not applied to them, but the football coaches would get their admissions committee to accept a student who hasn’t applied. |
| Q6 | You were a hot property at that point? |
|  | Dudley Herschbach: Well, any rate, I was offered several athletic scholarships, but back then, at least at Stanford where I went, the academic scholarship I was offered was more valuable than the athletic one, so naturally I took that. Now I doubt it’s true anymore, sad to say. |
| Q6 | What’s a punter? |
|  | Dudley Herschbach: A punter is on fourth down. You have to kick the ball down the field or you risk … If you don’t make your first down of enough yardage, the other team takes over, so usually they punt. I didn’t like that because suddenly in spring practice they had all these other coaches telling us what to do. Whereas before the game really belonged to the players, that was a whole philosophy, coaches weren’t allowed to signal in plays from the sidelines, anything, it’s completely the opposite of what it is today, and so I said, “Well, hell, I don’t want to play that way”. Today it would be different if you have some talent to do well in a sport like football, you’d have to do it at least because as a decent player you get millions of dollars a year as a rookie, even in the pros. |
| Q3 | You were taken very early on by the idea of reaction kinetics and the field grabbed you. What was it that you found so appealing? |
|  | Dudley Herschbach: My freshman advisor was a young chemist that really I owe to the fact that in high school, the most outstanding teacher I encountered was in chemistry, even though chemistry, when I first encountered it in high school, mystified me. Anything else I started, it seemed obvious right away with the general idea the subject was and all, but chemistry looked to me a hopeless hotchpotch of odds and ends. It took a while before I began to get a feeling for it. I now, after years and years, have a way of explaining why I think a similar reaction with many people when they meet it in high school or college, you can come to that later if you like … |
| Q5 | That’s an interesting word to choose as a description for Bright Wilson ‒ an architect of understanding. Do you mean by that, that he was able to pull all the pieces together to create a framework for understanding? |
|  | Dudley Herschbach: I’ve often said that people think science is essentially a technical thing. I think the kind of science we’re talking about is better described as architectural. An architect has to understand quite a few technical things, but not in the same way that the engineer or the building contractor needs to. Those technical things are not actually the essence of architecture. The way I think of an architect is someone who recognizes that at a particular time, the building materials and methods available allows them to create space in a way that’s shaped to not only be the practical sense, effective, structure to carry out its purpose, but it conveys something beyond that, that inspires people to realize that it opens their eyes to new possibilities that you didn’t anticipate would be there. That’s what really frontier science is like, it’s a human enterprise.  Often the exact content, I think, is not very important compared with how it inspires other people. I’ve often said that it’s the spiritual content of an experiment or theory that has the greatest impact, because science depends on how other people respond to it, just as in art. I think you value a great piece of art or a performance because in some way, having witnessed it, it changes how you think, how you react or what you do next. It is exactly the same in a fine work of science that the people who come in contact with it, they are after thinking in a different way. They probably do something different in their own work often and so on. Most people think, Oh, it’s just some technical thing and you do this or that, but there’s much more to it than that. |
| Q1 | Why would one do anything else in life if you’ve got the chance to be wrong all the time? |
|  | Dudley Herschbach: Maybe it’s a temperamental thing. I often say to students that, what you want to find out as a student is some domain which appeals to you temperamentally as well as in terms of what seemed to be your special talents. That’s part of what the student needs to discover, by exploring different areas and all. What you want to try to achieve in your education is competence in some area and confidence. You need both those things and you want them in appropriate measures. If your level of confidence is much higher than your level of competence, that doesn’t work out well and vice versa. |
| Q10 | Do you think it would be hard to get support for the risky? |
|  | Dudley Herschbach: I think there’s no chance. If I wrote a research proposal in today’s climate that was akin or similar to what I was doing back then, that wouldn’t get funded. The whole attitude is such, you don’t find that anymore. |
| Q18 | Why has that change taken place? |
|  | Dudley Herschbach: Partly because there are many more scientists and the whole business of proposals has changed character. Now it’s almost a commercial enterprise, what will come of this and so on. That wasn’t true way back in the late fifties, when we were talking about, when I got started as a young faculty member at Berkeley, it was okay to do something that was pretty far out blue skies. |
| Q5 | What was so special about him? What is this about him? |
|  | Dudley Herschbach: I met you Yuan Lee when he first came to this country from Taiwan as a graduate student at Berkeley and his English still wasn’t too good. It was wonderful to see his transformation, but, as it turned out, I moved to Harvard only a year after he arrived at Berkeley as a graduate student. He really did, as he explained to me later, want to work with me, but he misunderstood when I was telling him, because my general policy was always to accept any student who wanted to work with me. I felt I had a responsibility to do that, but I wanted to be sure since it’s a very important decision for them that you talk with other faculty and think of other possibilities. I always stressed this and then if they came back another time or two, then okay, I would be willing to let them do this reckless thing and work with me. But I think because of the language thing, he didn’t quite understand any … |
| Q5 | He thought you were sending him away? |
|  | Dudley Herschbach: Yes, well I can appreciate it now, but when he came here to post doc with me, it was now at Harvard, he actually wanted to do theory because he had as a graduate student at Berkeley, done a beautiful off scale experimental work. I knew that Yuan was something super special as an experimentalist, but he really wanted to do theory because I was known for doing both theory and experiment, but he wanted to fill out his background. But he was too shy to tell me this. When I was ranting about, Oh, Yuan, the time has come to make this jump and build us new apparatus that will allow us to go beyond this limited family of reactions we had been able to study up till that point. Of course, he went ahead and did, that was great success. There’re about a hundred machines there in the world now that are essentially like the one that Yuan led construction of when he was a postdoc here. |
| Q2 | Indeed. On being a nice person, it’s not the image of the single scientist working alone, isn’t it at all? It’s community? |
|  | Dudley Herschbach: Yes, that’s right. Even the people like [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) who are the iconic model for the lonely genius, it’s completely misleading. I’ve written a paper on Einstein as a student, who was for one of these celebrations in 2005 of his miracle year, and I looked into that pretty thoroughly and Einstein himself would say how much you owe to many other people. It’s very clear when you study his early years. |
| Q1 | Could you summarize the approach to science that you’re trying to impart to your students? What do you tell them to do? How do you encourage them to approach the questions? |
|  | Dudley Herschbach:  My basic philosophy is that science should be thought of as part of a venture of our species just as artistic things are. I try to teach science as a liberal art. To me that means the highest aim, as I understand it, of liberal art is to bring students to the habit of self-questioning, critical thinking, not just accepting what they’re told and all the rest. Science should be a very important component of this, and it’s not generally the case for most students who go through college in the United States, at least. Even those who are studying science don’t think of it as part of liberal art education for the most part, they tend to think of mastering technical stuff. The technical stuff certainly you need, no doubt about that, and it’s very satisfying to master some part of mathematics, particularly because it’s like a language, you’re empowered. You can do things, you can compose things, you can work out things and all you could do otherwise. It’s a very satisfying thing. |
| Q4 | Turning just for a little while to the collisions themselves, they allow you to study the making and breaking of individual bonds, which is sort of the basis of everything in chemistry. Maybe it’s too broad a question, but has that ability changed the way one views chemical reactions? |
|  | Dudley Herschbach: Yes, I think it’s had quite an influence in the way people think, which is in the end for a long time. Chemists, as I alluded to earlier, wanted to map out these individual molecular steps, but they had no way of looking at individual steps and checking if that’s really what happened because, often you have what are called, intermediates ‒ they are so reactive. There’s a special, not very stable, combination of atoms, they’re so reactive they don’t stay around long enough to be observed by ordinary means. They’re postulated to be there maybe, and when you made deductions from what you thought the influence of that intermediate might be, you’d try to test it by varying the temperature or the concentration of reagents and seeing if things changed in a way that seemed consistent with a postulated presence of this intermediate species.  Often it didn’t agree with experiments, so you’d postulate something else. That was the way the game was played for a very long time. Of course, in addition to our ability to observe a single collision so we could see whether the step that was postulated took place more and more, you had other means to probe in particular laser techniques. [Ahmed Zewail](https://www.nobelprize.org/prizes/chemistry/1999/zewail/facts/) for example is a spectacular example in that he could follow reactions in time and trace them. Whereas the beam experiment is a different thing. Fundamentally, you prepare things that are on the molecular scale very far apart. They come in and interact very close together. They fly apart and you observe them and they’re far apart again. But what has happened in between is influencing the speed with which they come apart, how rapidly the molecules are tumbling, how they’re oriented in space, how fast the atoms vibrate, all those things you could observe was they’ve come apart.  But /- – -/ you didn’t have a way to follow them all the way through in time, that was a very satisfying thing. More and more techniques have come along that augmented this ability to study the individual steps and detect in particular whether there are very short-lived intermediate species reacting intermediate in all the rest, probed what’s called a transition state when they’re really close together. Chemists also now have much better ability to compute from first principles from quantum mechanics what the forces are like. We have both better ways to test that and compute it but is not yet fully under control. Chemistry is charged difficult enough and complicated enough to keep you humble. We don’t understand all that we’d like to, new things keep being discovered, even in this domain we’re talking about, but of course, many other domains and frontiers of chemistry. It’s a very rich and ongoing enterprise. |
| Q4 | If one’s able to start defining the intermediate states, can you start to reclassify chemical reactions in terms of the intermediate states through which they go? |
|  | Dudley Herschbach: Yes, I always felt that what we needed to do and needed to aim at doing in this business of single collision, chemical reaction dynamics, was in a way to emulate what Pauling ‒ and he’s the label for this whole field of molecular structure ‒ understood in terms of electronic structure, because electrons are the key part of governing what goes on in interactions between atoms and molecule. We needed in these experimental tools to develop ways that we could connect the dynamics to the electronic structure. That’s what we were able to do in an observational way. Now theory allows you to connect it with quantum mechanics more directly, so that overall aim is being more and more approached and fulfilled along the way. It’s very satisfying to see how that works out, and every day we are getting a little bit further along, |
| Q5 | Just a last quick question. How do you solve the problem at school of people not being able to get the correct distance from chemistry to really get the picture early on? |
|  | Dudley Herschbach: My short answer is you teach a liberal art. You realize it is that kind of thinking and you help them understand that our ordinary kind of conversations about many things have this character that is the level of approximation or abstraction in chemistry, that you use changes as you talk about bonding. One moment about terminal overall properties of bulk substances, another speed of reaction. They all have a different level of projection and approximation and vocabulary and all that. But the chemist in ordinary discussion goes back and forth, back and forth. fluidly. We do too in ordinary discussions of many topics. When you stop to analyze it, that’s the case. But when you are accustomed to a language, say like English, it’s a holy mess from a linguistic point of view, it’s a sloppy mixture of several other languages, and all the rest. Well, so is chemistry, and once you understand that you see, Gee.  I’ve been talking like a chemist all along, except I use different words and I don’t realize talking about people instead of molecules or institutions instead of molecular processes. But the chemist sees all these analogies and when you bring them out in teaching, I think it helps people to see. It’s so much fun to make connections. For example, one thing I love to do when I talk about the gas laws, a standard topic, you always have to discuss in the chemistry course. Suppose Hercules was confronted with a 13th labor to weigh the Earth’s atmosphere. Put it that way, because that’s a consequence, and a so-called gas law. Then I say, let’s figure out what the atmosphere weighs and it’s easy to do. Then they say, But that’s a big, big number.  How to make it meaningful? To make it meaningful, you compare it with something else. What else? Everyone knows that one atmosphere will hold up about 29 or 30 inches of mercury, that’s equivalent to 34 feet of water. It’s significant. How much pressure does that exert? It turns out that the pressure the atmosphere is exerting right now can be compared with how much you exert when you stand on the floor. We take your weight. We take the area of your feet and pressure is force pre unit area. It’s a gravitational force your weight, and it works out. You’re about 80 kilograms, more or less, your feet occupy about 400 square centimeters, and the atmosphere is one kilogram per square centimeter turns out. So if you take 80 over 400 that’s one fifth. You only exert one fifth of the pressure. When you stand up that the air does, doesn’t that make you feel lighter right away, you can dance practically. This is the way you say it to the class, it makes them see it in a different way. There are countless opportunities like this to teach chemistry as a liberal art. That’s what I mean by it. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0392 |
| **Biographical** | Yuan Tseh Lee was born on November 19, 1936 in Hsinchu, Taiwan. His father is an accomplished artist and his mother a school teacher.  He started his early education while Taiwan was under Japanese occupation – a result of a war between China and Japan in 1894. His elementary education was disrupted soon after it started during World War II while the city populace was relocated to the mountains to avoid the daily bombing by the Allies. It was not until after the war when Taiwan was returned to China that he was able to attend school normally as a third year student in grade school.  His elementary and secondary education in Hsinchu was rather colorful and full of fun. In elementary school, he was the second baseman on the school’s baseball team as well as a member of the ping-pong team which won the little league championship in Taiwan. In high school he played on the tennis team besides playing trombone in the marching band.  Besides his interest in sports during this time, he was also an avid and serious reader of a wide variety of books covering science, literature, and social science. The biography of [Madame Curie](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1911/index.html) made a strong impact on him at a young age. It was Madame Curie’s beautiful life as a wonderful human being, her dedication toward science, her selflessness, idealism that made him decide to be a scientist.  In 1955, with his excellent academic performance in high school, Lee was admitted to the National Taiwan University without having to take the entrance examination, a practice the Universities took to admit the best students. By the end of his freshman year he had decided chemistry was to be his chosen field. Although the facilities in the Taiwan University were less than ideal, the free and exciting atmosphere, the dedication of some professors, and the camaraderie among fellow students in a way made up for it. He worked under Professor Hua-sheng Cheng on his B.S. thesis which was on the separation of Sr and Ba using the paper electrophoresis method.  After graduation in 1959, he went on to the National Tsinghua University to do his graduate work. He received his Master’s degree on the studies of the natural radioisotopes contained in Hukutolite, a mineral of hot spring sediment under Professor H. Hamaguchi’s guidance. After receiving his M.S. he stayed on at Tsinghua University as a research assistant of Professor C.H. Wong and carried out the x-ray structure determination of tricyclopentadienyl samarium.  He entered the University of California at Berkeley as a graduate student in 1962. He worked under the late Professor Bruce Mahan for his thesis research on chemi-ionization processes of electronically excited alkali atoms. During his graduate student years, he developed an interest in ion-molecule reactions and the dynamics of molecular scattering, especially the crossed molecular beam studies of reaction dynamics.  Upon receiving his Ph.D. degree in 1965, he stayed on in Mahan’s group and started to work on ion molecule reactive scattering experiments with Ron Gentry using ion beam techniques measuring energy and angular distributions. In a period of about a year he learned the art of designing and constructing a very powerful scattering apparatus and carried out successful experiments on N2+ + H2 –> N2H+ + H and obtained a complete product distribution contour map, a remarkable accomplishment at that time.  In February 1967, he joined [Professor Dudley Herschbach](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1986/index.html) at Harvard University as a post-doctoral fellow. He spent half his time working with Robert Gordon on the reactions of hydrogen atoms and diatomic alkali molecules and the other half of his time on the construction of a universal crossed molecular beams apparatus with Doug McDonald and Pierre LeBreton. Time was certainly ripe to move the crossed molecular beams method beyond the alkali age. With tremendous effort and valuable assistance from the machine shop foreman, George Pisiello, the machine was completed in ten months and the first successful non alkali neutral beam experiment on Cl + Br2 –> BrCl + Br was carried out in late 1967.  He accepted the position as an assistant professor in the Department of Chemistry and the James Franck Institute of the University of Chicago in October 1968. There he started an illustrious academic career. His further development as a creative scientist and his construction of a new generation state-of-the-art crossed molecular beams apparatus enabled him to carry out numerous exciting and pioneering experiments with his students. He was promoted to associate professor in October 1971 and professor in January 1973.  In 1974, he returned to Berkeley as professor of chemistry and principal investigator at the Lawrence Berkeley Laboratory of the University of California. He became an American citizen the same year.  In the ensuing years, his scientific efforts blossomed and the scope expanded. His world leading laboratory now contains seven very sophisticated molecular beams apparati which were specially designed to pursue problems associated with reaction dynamics, photochemical processes, and molecular spectroscopy. His laboratory has always attracted bright scientists from all over the world and they always seem to enjoy working together. He takes great pride in the fact that more than fifteen of his former associates are serving as professors in major universities, and many others are making great contributions at the national laboratories and in the private sector.  Lee and his wife, Bernice Wu, whom he first met in elementary school have two sons, Ted (born in 1963), Sidney (born in 1966) and a daughter, Charlotte (born in 1969).   |  | | --- | | *Among some of the awards and recognitions he has received over the years include:* | | Alfred P. Sloan Fellow, 1969-1971 | | Camille and Henry Dreyfus Foundation Teacher Scholar Grant, Recipient 1971-1974. | | Fellow, American Academy of Arts and Science, 1975. | | Fellow, American Physical Society, 1976. | | John Simon Guggenheim Fellow, 1976-1977. | | Member, National Academy of Sciences, 1979. | | Member, Academia Sinica, Taiwan, China, 1980. | | Honorary Professor, Institute of Chemistry, Chinese Academy of Science, Beijing, China, 1980. | | Honorary Professor, Fudan University, Shanghai, China, 1980. | | Miller Professorship, University of California, Berkeley, California, 1981-1982. | | Ernest O. Lawrence Award, U.S. Department of Energy, 1981. | | Sherman Fairchild Distinguished Scholar, California Institute of Technology, 1983. | | Harrison Howe Award, Rochester Section, American Chemical Society, 1983. | | Peter Debye Award of Physical Chemistry, American Chemical Society, 1986. | | National Medal of Science, 1986. | | Honorary Professor, Chinese University of Science and Technology, Hofei, Anhuei, China, 1986. | | Honorary Doctor of Science Degree, University of Waterloo, 1986. |   From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1986*, Editor Wilhelm Odelberg, [Nobel Foundation], Stockholm, 1987  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 1986 **Addendum, March 2006** After receiving the Nobel Prize in 1986, Yuan Tseh Lee continued his research in chemical dynamics. Aside from research on reactive scatterings, his research group has made major contributions in the elucidation of various photochemical processes as well as in the determination of the structure of various protonated molecular clusters by obtaining infrared spectra. Many new instruments were developed for these purposes. He also directed much of his attention to the advancement of international scientific developments and to the promotion of general public affairs. As a professor of chemistry at the University of California at Berkeley from 1986 to 1993, Lee on different occasions served as Co-Chair of the Chancellor’s Asian-American Affairs Committee at UC Berkeley, Member of the California Council on Science and Technology, and Member of the California Institute of Technology Board of Trustees. At the national level, he served on the Secretary of Energy Advisory Board and the Welch Foundation Science Advisory Board.  In January 1994, after 32 years of research and teaching in the U.S., he took the important step of returning to his home country, Taiwan, to serve as President of Academia Sinica. Originally founded on the Chinese mainland in 1928, Academia Sinica has long been the most prominent research institution in Taiwan; at present, it has over 30 research institutes, covering the humanities, social sciences as well as the physical and biological sciences. During his tenure as President of Academia Sinica, Lee has worked hard to improve the quality of research in that institution. He believes the research conducted at Academia Sinica in several fields, including his own, now rivals the best works done in other parts of the world.  Lee has also taken an active role in promoting scientific and cultural developments in Taiwan. From 1994 to 1996, he was the chair of the national committee for educational reform. From 1996 to 2000, he led a national organization for community empowerment in Taiwan. From 2000 to 2002, he chaired a nonpartisan group that gave advice on matters concerning cross-strait relations (i.e. relations between Taiwan and China) to President Chen Shui-bian, whose electoral victory in 2000 marked the first change in the ruling party since World War II. Since his return to Taiwan, Lee has established several new foundations and aided existing organizations that support educational and research activities. He has also traveled extensively around the world to attend scientific conferences and hold lectures.  Lee is scheduled to retire from his position as President of Academia Sinica in October 2006. Thereafter, he plans to work at the Institute of Atomic and Molecular Sciences and the Genomics Research Center, both at Academia Sinica. So far he has received 32 honorary doctoral degrees from universities around the world. **Addendum, January 2018** Yuan Tseh Lee is President Emeritus at the Institute of Atomic and Molecular Sciences Academia Sinica, Taiwan.  Born in Taiwan in 1936, he received his B.S. degree from Taiwan University in 1959 and Doctorate from University of California, Berkeley in 1965. He joined Dudley Herschbach at Harvard University as a postdoctoral fellow in 1967 and has had faculty appointments at University of Chicago and University of California, Berkeley. He was University Professor and Principal Investigator at University of California, Berkeley and the Lawrence Berkeley Laboratory before he became President of Academia Sinica (1994-2006). From 2011 to 2014 he served as President of the International Council for Science (ICSU).  He has received numerous awards and honours, including the 1986 Nobel Prize in Chemistry for his contributions concerning the dynamics of chemical elementary processes. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** | 392 |
| Q3 | Welcome to this interview Professor. I want to start off by asking you a little bit about your childhood and what made you want to take the way to becoming a scientist. Was there something particular in your childhood? |
|  | Yuan T. Lee: I was born in Taiwan before the end of the Second World War, so when I was young, airplanes from America started bombing Taiwan every day. That was when I started my elementary school. So war certainly influenced everybody’s lives. And the bombing and airplane flying – all those scientific things came to our minds very early. But after the end of the Second World War Taiwan returned to China. At that time China was very chaotic. Very weak. Everybody believed there was only democracy in science would save China. I’m also interested in using my bare hands. And also the creations. So when I went to High School and read a book on [Madam Curie](https://www.nobelprize.org/nobel_prizes/physics/laureates/1903/marie-curie-facts.html) I thought that it would be a good thing to become a scientist so that’s why I decided to become a scientist. |
| Q10 | That is interesting. I want to come back to that, but we could just briefly say that you did return. You went to America, you returned, and you’ve been back in Taiwan for 11 years now. Can you see these changes? Can you feel that you have influenced? |
|  | Yuan T. Lee: When I went to America in 1962 I didn’t plan to stay in America for so long. But after I got my PhD people gave me a professorship and it went on and on and before I knew it I found myself to be 57 years old and I had spent 32 years in America. I decided to go back to help because Taiwan needed my help more than California. So I did go there. Taiwan is a small place with 23 million people – a small island. According to physical law, if you push the acceleration is invert in proportion to the mass so Taiwan is relatively small. With many people returning to Taiwan from America, tried to push science and such a transformation, and it seems to be moving. |
| Q10 | Which is the greatest challenge do you think for Taiwan in the field that you are working? |
|  | Yuan T. Lee: I think history goes through the democratisation. In 1996 Taiwan allowed people to elect a president for the first time in the history of China. So -96 was an important change. But by the year 2000 it was the first time the ruling party, which has been in power for 50 years, has been overthrown by popular vote. So that really marked another phase of democratisation. So when I went back, what I was really happy to see was the society become more democratic. So human power would be liberated because of the democratic process. |
| Q5 | You came back with a Nobel Prize and of course you have many students today and I would imagine that some of them are also going to America to do some of their research and then come back. How do you feel that somebody is going away for a while? Is it a loss for Asia? |
|  | Yuan T. Lee: In the 1960s and 70s it certainly was a big brain drain. Many people went to America and stayed there. But now, during the last ten years, I asked many established scientists including the member of National Academy of Sciences, several of them returned during the last years to Taiwan. And so I would like to look at it this way: it’s the brain circulation. Young people go out and they learn something and then they come back.  On the other hand, the situation in Taiwan improves more and more students like to stay at home rather than go abroad. So now we are sitting looking at scholarships, who is supported, and sending them out to foreign countries. We really need them to see the world before settling down in Taiwan. |
| Q9 | What did it mean to you that you were given the Nobel Prize back in 1986, the Nobel Prize in Chemistry? |
|  | Yuan T. Lee: I was really surprised, first of all. I have been enjoying science with all my students and actually I’ve had some kind of record in America. If you look at the faculty member produced in major universities, famous research universities like MIT, Caltech, Berkeley or the Chicago or Cornell, I have former students in all those universities among the chemistry faculty. I learned that I produced more professors for American universities than anybody else. So I really enjoy doing science and by the 1980s we know that we are doing the best because we see many chemical researchers and many things nobody else in the world can do. So we are very happy that we are leading the world and people from Europe and Japan came to do post doctoral research. And as a scientist knowing that you are doing well is quite satisfactory.  But in 1986 I received an award from chemical society, /- – -/ White House. At the time I was a US citizen. They gave me the National Medal of Sciences. And then the Nobel Foundation gave me the Nobel Prize. At the same time Wolf Foundation in Israel also wanted to give me a prize and I was really surprised. Life suddenly changed. I was a very shy person. Even if I see a lady my face become red. I was very shy. But after the prize I had to be in the limelight so often to give after dinner speeches and then gradually I got used to it. But I remember during the first three months I suffered so much so my wife said ‘Yuan, you enjoy so much working in the laboratory with students, why do you have to become an after dinner speaker?’. And she suggested maybe we should return the prize to the Nobel Foundation and then you can go back to the laboratory.  Well, that was my wife’s suggestion but I’m glad I didn’t do it since winning the Nobel Prize you have the different opportunities to serve different segments of the society. I went back to Taiwan and became the President of the Academy of Sinica in Taiwan. We run 30 different institutes in an assembly of academicians that belong to the office of the president. So I am certainly quite influential in Taiwan. I can talk to the President anytime I want. So I have been able to do quite a bit in Taiwan. That’s a good opportunity given by the Nobel Foundation. |
| Q9 | Is there any special memories that you have from the time you went to Stockholm that you would like to share with us? |
|  | Yuan T. Lee: When I received the Nobel Prize people said ‘Yuan, you have to be a spokesperson. You have to speak for science. And people will ask you all sorts of questions that you don’t know the answer to but you still have to give the answer.’ That has been a big challenge for me so I studied very hard. Especially when I went back to Taiwan and took the position of the President of the Academia Sinica because academy cover humanity, social sciences, and physical sciences and biological sciences. As the President, as a spokesperson, I did learn quite a bit. So that was an exciting thing. Actually, Taiwan is a member of APEC, Asian Pacific Economical Corporation. Our president was not allowed to participate in APEC because men in China would not allow us to go. So I have been representing our president to attend APEC for the last three years. It was interesting.  First time I went there all the presidents of the countries of the pacific region including George Bush and all the people they are quite curious to see that a scientist, a Nobel Laureate, came to this political meeting and talking about economical development. It was quite interesting that they look at me in a very different way. As if I’m a different animal. Of course, for politicians scientists are very different. We all look at the things in the longer term. What is the truth? How can we do better? The politicians tend to think in short term. All they worry about is next year, maybe another election. They want to keep their position and so on and so forth. So that was interesting. |
| Q10 | Definitely. That leads me to the next question which is about the environmental situation that we’re facing in the world today. You knowing so much about chemical reactions and so on, what do you think we are facing at the moment with global warming and ozone layer getting thinner? What is the reason for this and how can we as human being change it? Is it necessary for us to change the way we are living our lives? |
|  | Yuan T. Lee: Yes certainly. Let me say this. Human beings developing on the surface of the earth in a biosphere for quite a long time. The earth is infinity for human beings. But 250 years ago after the industrial revolution we invented so many machines and used so much energy and people become so comfortable with the science advances. Population went up during the last century by /- – -/ now we have six billion people living on earth. So if you look a the development of the history of mankind last century we went through a critical point. It means earth used to be infinity. Now you become finite. It’s finite because of all the people. It means human activity is starting to damage the ecosystem and that was not happening 200 years ago.  But now it’s happening. But people are now waking up. People didn’t realise that we are going through the transition almost like water become ice. There is a fresh transition taking place on earth. So if you look at the development of China, India, or Taiwan, Thailand. All follow the pattern of western development but that was the time that earth was infinity. Now earth is finite. So we really have to change. Change the use of the energy. Fossil energy is going to dry out and we have to depend on the solar energy or nuclear. And that we really have to make our mind to do a lot of research.  So I do see in the next 20 years because the petroleum will peak, demand will go up. There will be a gap between the demand and supply. And there will be an energy crisis on the one hand. On the other hand earth has become finite so we have to worry about how our earth consume the pollutions. So those two conditions will make it extremely difficult for human society. The only way out I can see is to learn to use energy efficiently. For example lighting in the room. We can use about 25 percent of the electricity to get the same light by using light emitting diode. You can use the combined hybrid automobile even at the present time will save you energy more than factor two.  We do have enough technology to save energy. To use energy more efficiently. So that will go a long way. On the one hand, we have to learn to use the solar energy more efficiently but for the next 50 years saving energy, making energy more efficient, will be a very important way. As a scientist, I do believe that in spite of the fact that science and technology will help humanities but we have to learn to walk together as one community. Globalisation has gone halfway. So you do see economies is globalised but nation based competition still fiercely going /- – -/ so you see war, friction. Before the end of the century if we do not learn to operate as one community for the entire world I think the chances of humanity to survive on earth will be very, very small. |
| Q18 | Do the politicians listen to you when you tell them this message? |
|  | Yuan T. Lee: They certainly listen and they seem to understand. But they look at the short-term effect. So nobody dare to change. I saw the President of Academy of Sciences of China, former academy of science president Dr Cho. I know him for 25 years now so when we met a couple of weeks ago I said we have been talking about China should develop a different way. Not following the pattern of western style. And keep on using the automobile and you should have the public transportation. But then he said ‘Yes we know but everybody believe that unless the automobile industry becomes successful it will not bring up the entire economy.’ So the automobile industry in Japan, Germany, you know you name it, China. It is producing millions and millions of automobile now. The question is where are you going to get the petroleum in the next 20 years. |
| Q14 | When you talk to your young students and ask them to look into the future for the future kind of research that is necessary, what fields would you suggest? Where will the big breakthroughs come in the next 20 years? |
|  | Yuan T. Lee: For quite a long time we have been interested in the origin of life. Creation of the universe. Structure of matters and the forces operating in the universe. Last century because we understand the motion of microscopic particles atomic physics developed and that influenced the chemistry. So my field of study doing molecular sketching certainly is based on the advancement made earlier in the 20th century. But now you look at the biology because of the advancement of chemistry, the advancement with the tools, it is now possible to understand many phenomena related to life. So I do believe that in the 21st century life sciences will be bring lots of excitement.  So if I were to start again probably I will pay more attention to biological sciences even as a chemist. But it was interesting when I started out as a chemist many of my friends told me ‘Yuan, if you want to be a chemist you have to learn physics better otherwise you will not become a good chemist.’ So I studied lots of physics. And I entered areas called chemical physics. It’s an inter discipline area. That certainly helped me develop other things. Now if the young people ask me I will see biology certainly given you excitement but you have to study chemistry better or physics better, otherwise you will not be able to go too far. |
| Q6 | Just one last question I believe that you’re a good sportsman as well. Do you get time for sport still? |
|  | Yuan T. Lee: Yes. Unless you do some exercise you will be melted away. I’m a tennis player. I play tennis. So probably you know this week is Wimbledon. It is going on. Last night I saw Davenport was playing with Cluster. Salapovo was playing with somebody else. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0393 |
| **Biographical** | John Charles Polanyi was born in 1929 in Berlin, Germany, of Hungarian parents, Michael and Magda Elizabeth Polanyi. The family moved to England in 1933 where he received his education.  His University training was at Manchester University, where he obtained his B.Sc. in 1949, and his Ph.D. in 1952.  From 1952-1954, he was a Postdoctoral Fellow at the National Research Council Laboratories in Ottawa, Canada, and from 1954-1956 Research Associate at Princeton University.  In 1956, John Polanyi was appointed as a Lecturer at the University of Toronto where he was successively Assistant Professor (1957-1960), Associate Professor (1960-1962) and Professor (1962- present). He was given the (honorific) title University Professor in January 1974.  In 1958, he married Anne (Sue) Ferrar Davidson. They have two children, Margaret Alexandra (born 1961), and Michael Ferrar (born 1963).  He serves on the Board of the Ontario Laser and Lightwave Research Centre, Canada (1988-present), is a Member of the Board of the Steacie Institute for Molecular Sciences, Canada (1991-present), and Member of the Science Advisory Board, Max Planck Institute for Quantum Optics, Germany (1982-present), and is Honorary Consultant to the Institute for Molecular Science, Okazaki, Japan (1989-1992). He was a Founding Member and is currently President of the Canadian Committee of Scientists and Scholars, and also was a Founding Member of The Royal Society of Canada Committee on Scholarly Freedom, a Member of the American Academy of Arts and Science Committee on International Security Studies, and a Member of the Board of the Canadian Centre for Arms Control and Disarmament to which he is currently an Advisor.  He was awarded the Marlow Medal of the Faraday Society 1962, Centenary Medal of the British Chemical Society 1965, the Steacie Prize for Natural Sciences (shared with N. Bartlett) 1965, the Noranda Award of the Chemical Institute of Canada 1967, the Henry Marshall Tory Medal of the Royal Society of Canada 1977, the Wolf Prize in Chemistry (shared with G. Pimentel) 1982, the Izaak Walton Killam Memorial Prize 1988, the Royal Medal of the Royal Society of London 1989, and the John C. Polanyi Lecture Award of the Canadian Society for Chemistry 1992.  He is a Fellow of the Royal Society of Canada (1966), and the Royal Society of London (1971), a Member of the American Academy of Arts and Sciences, (1976), the U.S. National Academy of Sciences (1978), the Pontifical Academy of Rome (1986), a Fellow of the Royal Society of Edinburgh (1988), an Honorary Fellow of the Royal Society of Chemistry of the United Kingdom (1991), and of the Chemical Institute of Canada (1991).  He has been the recipient of honorary degrees from the Universities of Waterloo 1970; Memorial 1976; McMaster 1977; Trent 1977; Carleton 1981; Harvard 1982; Dalhousie 1983; Rensselaer 1984; Brock 1984; St. Francis Xavier 1984; Lethbridge 1987; Victoria 1987; Ottawa 1987; Sherbrooke 1987; Laval 1987; York 1988; Manchester, England 1988; Montreal 1989, Acadia 1989; Weizmann Institute, Israel 1989; Bari, Italy 1990; British Columbia 1990; Concordia 1990, McGill 1990 and Queen’s 1992.  He was made an Officer of the Order of Canada in 1974, and a Companion of the Order of Canada in 1979.  In addition to his scientific papers he has published approximately one hundred articles on science policy, on the control of armaments and the impact of science on society. He has produced a film ‘Concepts in Reaction Dynamics’ (1970), and has co-edited a book, ‘The Dangers of Nuclear War’ (1979) |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** | 393 |
| Q10 | Today we have had a session, a round table session on the creative environment and creativity in science. There were many views expressed there on how to best accomplish a creative environment. Would you like to comment, and how would you like to …, what is your, the elements of a creative environment? |
|  | John Polanyi: Gosh. I mean that was a very free ranging discussion today which covered all the things that we tend to chat about in the evening as scientists when we’re exhausted. And I wouldn’t dare to try to summarise the elements in it. I think probably the most stressed thing at the round table, and rightly so, was the need to have freedom in order that you can be opportunistic. Because you cannot make plans in science and then doggedly follow them. If you do that’s really a prescription for disaster.  Actually I once sat on a committee which was required to examine the progress of a group of scientists who formed the team, and we were supposed to see that they had followed the plan that they presented in order to get the money to do the research. And I said well, all right, I’ll do it, but only with one stipulation, that if they follow their plan, then we’ll not give them any more funds because clearly, they didn’t discover anything. And so, people were rather shocked. But this had a really rather serious aspect to it as far as I was concerned. I think that the freedom of scientists to pursue discoveries where nature gives an opportunity for learning something, that freedom is being more and more restricted.  And it’s understandable that in well-functioning democracies people want to see accountability. But then they go and introduce a form of accountancy which is inappropriate and damaging and they say, well the sort of thing I just said in that story. They say tell us what you’re going to discover, tell us what those discoveries will, how they will benefit mankind, how they will create prosperity. And this involves scientists in either saying well, that’s inappropriate which is dangerous because you don’t get the funds. And so we start telling these stories and they’re not really true stories, they’re sophisticated sort of science fiction. And when you start to lie about the way in which science is done and the justification for doing it, eventually these things come back to haunt you and people say well, where is the application that you said would come, and in fact it will come. If you put intellectual power into people’s minds that can be used for good and for ill, it’s partly our job to see that it’s used for good. But it can certainly be used.  The irony of course is that as a scientist one has to face two different groups of people, one of whom are very concerned that you are taking public money, you’re disappearing into your laboratory and then you are amusing yourself by learning something about nature perhaps. And then there’s another group of people who see you taking public money, disappearing into your laboratory and they say each time you go into the laboratory you come out with something terrible. And you produce opportunities which we don’t know how to handle, you’re making the world a very dangerous place. Well, you can’t have it both ways, that we are spending our time futilely indulging our curiosity and at the same time we are revolutionising practice to a degree that nobody can handle it.  So which is true? I would say the second is true. So long as you are willing, this was a term used around the round table this morning, as long as you are willing to trust the scientists, and you don’t have to trust them very far, you have to trust them for three years, four years, five years, and then you have to say what did you do? And that’s legitimate. And you have to be tough about it.  But judge them on the basis of their ability to add to the store of human knowledge in a way that’s going to make a difference to people’s thinking. If you do that, you’ll get new ideas and you’ll get them in a cost-effective way. And the problem really lies not with good scientists frittering their time away and wasting people’s money, it comes later with the fact that indeed the discoveries will be made but then we have to somehow decide how to use them. We, not just we scientists, but we human beings. |
| Q18 | Let’s turn for a while to university education. I find, at least in my country, that it’s still very sort of conservative in the sense that there is chemistry, there is physics and there’s biology, and to me it seems that the borderlines between the subjects are so fussy today that we might perhaps need to reconsider some of our education systems. Have you had any thoughts about that? |
|  | John Polanyi: I’m not sure I caught your thought and I’m not sure I’m going to respond in an interesting way to it. You’re talking about the division between subject areas? |
| Q18 | Would we as scientists have been too complacent about the changes that we have seen, about the requirements from governments that we should be more applied? |
|  | John Polanyi: I think that’s a very good question. I think the scientists are much too complacent and there are a whole lot of reasons for that. I mean one being that scientists want to be left alone to do science and if you start fighting battles of science policy you of course take large amounts of your time away from research. But there’s another reason and that is that biting the hand that feeds you is not encouraged, and it may be punished. And so people are loath to do that, not just for themselves, for their colleagues. You know, if I say this scheme which is actually helping your research and helping mine, but it’s a badly funded scheme because it just doesn’t take account of the fact that university research should be free. Anyway, so I argue against it and they cancel it, well not just I suffer but you suffer too. So a lot of people have a vested interest in scientists not complaining too loudly and they are not complaining sufficiently. |
| Q18 | You mentioned your involvement in the Pugwash movement. How much of your time did you spend on these issues? |
|  | John Polanyi: That’s very difficult to say but certainly not enough of my time. But I was involved in getting Pugwash moving in Canada but this was in the late 1950s and in that forum and in other forums I have been part of a sub group of the scientific community which has tried to, in the first place, tried to damp down the arms race by saying there are ways in which we could have arms control. Politicians tend to say it’s not been done, people will cheat, we’re safer just building a bigger weapon or a better weapon than our opponent. And so, I think that scientists have a role in saying the world can change, we should not rely always on winning the arms race with every other country because that way lies disaster. And scientists have been saying this but not a large enough group of scientists. That’s what Pugwash existed for.  So there were a succession of arms control issues in which scientists were providing technical advice, but they were doing much more than that. They were saying this is a rotten direction to go in, it’s dangerous, we should be going in that direction. It happened, in my country it happened with the question of should Canada get nuclear weapons, the United States actually wanted us to get them because we could help defend the US, this was late 1950s. Next question: should Canada build nuclear fallout shelters? I and other scientists said look, this is ridiculous, they aren’t going to work. But really what we were saying was we don’t want to live in a world in which we dig holes and all disappear in them. There must be a more happy, dignified outcome to the forward march of technology than that we all become underground creatures.  And so it went on. And most recently it has been the question of sophisticated missile defences. And it’s certainly true that under favourable conditions you can hit a bullet with a bullet and that you can hope to defend yourself poorly in that way. But under real circumstances where somebody does something surprising, there is a conflict, those circumstances you will never succeed in hitting a bullet with a bullet and it’s really part of every scientist’s experience that it is easier to make something malfunction, in this case we’re talking about a sophisticated defence system, than make it function. It is easier therefore for me to go into the lab where my student is and mess up his experiment than it is for him to make it work. And that being the case this anti-missile defence screen which has now been re-born as the US National Missile Defence which shortly will become the US and Canadian Missile Defence unless we argue against it, and thereafter will become the US Canada and Europe Missile Defence, and it won’t work. And what it will do is spend hundreds of billions of dollars on a direction which really offers no hope.  The direction that offers hope is to build up democracies, that requires spending money, give people in those countries the opportunity to reach levels of wealth which are satisfying so that they have countries which are open and to a degree contented. And at the same time establish an international rule of law which we’re trying to do, a new world is being born in which when the nationals of a certain country are victimised, other countries say this is intolerable, we’ve got to do something. That doesn’t mean they actually do something, but they say it and they mean it. Increasingly they will do something if we clamour for it. And I think that scientists, having gone so far along the path of saying what sort of world they think is a decent and civilised one, could help further that process and that’s what scientists in Pugwash and other similar groupings are trying to do. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0394 |
| **Biographical** | I was born in New York City on February 14, 1917, the oldest child of Israel Hauptman and Leah Rosenfeld. I have two brothers, Manuel and Robert.  I married Edith Citrynell on November 10, 1940. We have two daughters, Barbara (1947) and Carol (1950).  My interest in most areas of science and mathematics began at an early age, as soon as I had learned to read, and continues to this day. I obtained the B.S. degree in mathematics from the City College of New York (1937) and the M.A. degree in mathematics from Columbia University (1939).  After the war I made the decision to obtain an advanced degree and pursue a career in basic scientific research. In furtherance of these goals I commenced a collaboration with Jerome Karle at the Naval Research Laboratory in Washington, D.C. (1947) and at the same time enrolled in the Ph.D. program at the University of Maryland. The collaboration with Dr. Karle proved to be fruitful because his background in physical chemistry and mine in mathematics complemented each other nicely. Not only did this combination enable us to tackle head-on the phase problem of X-ray crystallography, but this work suggested also the topic of my doctoral dissertation, “An N-Dimensional Euclidean Algorithm”. By 1954 I had received my Ph.D. degree and Dr. Karle and I had laid the foundations of the direct methods in X-ray crystallography. Our 1953 monograph, “Solution of the Phase Problem I. The Centrosymmetric Crystal”, contains the main ideas, the most important of which was the introduction of probabilistic methods, in particular the joint probability distributions of several structure factors, as the essential tool for phase determination. In this monograph we introduced also the concepts of the structure invariants and seminvariants, special linear combinations of the phases, and used them to devise recipes for origin specification in all the centrosymmetric space groups. The extension to the non-centrosymmetric space groups was made some years later. The notion of the structure invariants and seminvariants proved to be of particular importance because they also serve to link the observed diffraction intensities with the needed phases of the structure factors.  In 1970 I joined the crystallographic group of the Medical Foundation of Buffalo[\*](https://www.nobelprize.org/prizes/chemistry/1985/hauptman/biographical/#footnote) of which I was Research Director in 1972, replacing Dr. Dorita Norton. My work on the phase problem continues to this day. During the early years of this period I formulated the neighborhood principle and extension concept, the latter independently proposed by Giacovazzo under the term “representation theory”. These ideas laid the groundwork for the probabilistic theories of the higher order structure invariants and seminvariants which were further developed during the late seventies by myself and others. During the eighties I initiated work on the problem of combining the traditional techniques of direct methods with isomorphous replacement and anomalous dispersion in the attempt to facilitate the solution of macromolecular crystal structures. This work continues to the present time. More recently I have formulated the phase problem of X-ray crystallography as a minimal principle in the attempt to strengthen the existing direct methods techniques. Together with colleagues Charles Weeks, George DeTitta and others, we have made the initial applications with encouraging results. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0395 |
| **Biographical** | I was born in New York City in 1918 into a family that had a number of artistic people among its members. My father’s brother and a sister’s husband were probably the best known. The latter, Ivan Olinsky, taught for many years at the Art Students’ League in New York City. I have been told that my paternal grandfather professionally made artistic decorations in peoples’ homes. The propensity for artistic endeavors extended to my generation and beyond.  My mother was an excellent pianist and organist and it was one of her hopes that I would become a professional pianist. As a youth I was entered into “Music Week” competitions in New York City. I had some modest success, but found at an early age that I had no taste for public performance. On the other hand, I was strongly attracted to science as a lifelong career at an early age.  I had the privilege of attending schools in the New York City public school system. Their standards of education, character building and discipline were very high and I, most certainly, benefited from them. They separated out the more advanced students and permitted them to progress at their own pace. In my case, this occasionally led to some curious circumstances. In my senior year in high school (Abraham Lincoln), the girls would join the boys to practice dancing. I was 14 years old at the time and the girls were the usual 17-18 years old. The physical discrepancy between this 14 year old boy and 17-18 year old girls was considerable. Their first reaction was incredulity but after a while they got used to my presence and even danced with me. I took the chemistry and physics courses that were available, both taught by the same man. He recognized my interests and was very encouraging to me.  I enjoyed a number of sports that I participated in at every opportunity, swimming in the ocean nearby, a game called single-wall handball, played with a little hard black ball and well-known mainly in some metropolitan areas, touch football whose rules eliminate the bruises from tackling and ice-skating that was facilitated by the flooding of a huge parking lot by the local fire department.  I entered the City College of New York in 1933 and, at first, found it to be a bit of a struggle. Their academic standards were very high and they had a concentration of the best students in New York City. In addition, I spent three hours a day traveling on the subway system to and from home. This marked the end of piano practicing. City College had no tuition fee. The only financial requirement was one dollar per year for a library card. At the College, there were broad course requirements for all students that ranged through mathematics, the physical sciences, the social sciences and literature. There were even two years of compulsory public speaking courses. I studied, in addition to the requirements, some additional mathematics, some physics, and much chemistry and biology. The year after graduation from City College was spent at Harvard University in the study of biology, for which I received a master’s degree, M.A., in 1938.  After a brief hiatus, I went to work with the New York State Health Department in Albany. While there, I had the opportunity to spend some time again at the piano. At the time I was in Albany, the fluoridation of drinking water was getting underway. I developed a procedure for determining the amount of fluorine in water supplies that became a standard method. This was my first modest contribution to science.  It was my intention to save enough money while at the Health Department to return to graduate school. This I did, and I entered the Chemistry Department of the University of Michigan in 1940 where I met my wife, Isabella Lugoski, whom I married in 1942, at an adjoining laboratory desk the first day that I went to physical chemistry class. We were both attracted to physical chemistry and took our degrees with Professor Lawrence O. Brockway whose speciality was the investigation of gas-phase molecular structure by means of electron diffraction. Although my Ph.D. degree was awarded in 1944, I had completed all my work for it during the summer of 1943 and went off to work on the Manhattan Project at the University of Chicago. Isabella joined me on this project a few months later.  In 1944, we returned to the University of Michigan, I went to work on a project of the Naval Research Laboratory and Isabella as an instructor in the Chemistry Department. While at the University of Michigan, I performed some experiments on the structure of monolayers of long-chain hydrocarbon films involved in the boundary lubrication of metallic surfaces. I also derived a theory that explained the electron diffraction patterns obtained from the oriented monolayers.  In 1946 we both went to work permanently in Washington for the Naval Research Laboratory. Our interest continued in developing the quantitative aspects of gas electron diffraction analysis. The solution of a key problem that arose in such analyses had evident implications for crystal structure analysis and, in fact, other areas of structure determination. At about the time that these matters were developing, [Herbert Hauptman](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1985/index.html) joined us at the Naval Research Laboratory and we decided to pursue the implications for crystal structures. This eventually led to the development of the direct methods for crystal structure analysis with the major part of the mathematical foundations and procedural insights established in the early 1950’s.  While all this was going on and with hardly missing a step from her research activities, Isabella mothered three children, Louise in 1946, Jean in 1950, and Madeleine in 1955. Louise is a theoretical chemist, Jean an organic chemist and Madeleine is a museum specialist trained in geology.  The initial applications of the procedure for structure determination for centrosymmetric crystals involving probability measures and formulas derived from the joint probability distribution were performed in the middle 1950’s in collaboration with colleagues at the U.S. Geological Survey. Then, in the second half of the 1950’s, through the efforts of Isabella Karle, an experimental X-ray diffraction facility was established in our own laboratory.  During the 1960’s, there was an intensive program in my laboratory to develop a procedure for crystal structure determination of broad applicability that would encompass noncentrosymmetric as well as centrosymmetric crystals. Largely through the efforts of Isabella Karle, such a procedure was developed and called the symbolic addition procedure. This procedure had its origins in the theoretical work and the experience in practical application of the 1950’s, but it also required some new procedural insights and some additional theoretical work to make it efficient and broadly applicable and avoid the pitfalls that easily arise when optimal pathways through a procedure must be chosen on the basis of probability measures. The first application of the symbolic addition procedure was published in 1963 and the first essentially equal atom noncentrosymmetric crystal structure to be solved by direct phase determination was published in 1964. This was followed by a number of exciting applications and toward the end of the 1960’s many laboratories started to become interested in the potential of the direct method for structure determination.  During the 1960’s, I collaborated with Isabella in some of her investigations and derived with her a variance formula that was the basis for applying probability measures to procedures for analyzing noncentrosymmetric crystals. In addition, I also carried out a number of theoretical investigations. Perhaps, the most useful one concerned a procedure for developing a fragment of a structure into a complete one by use of the so-called tangent formula for phase determination.  During the 1950’s and 1960’s, I maintained an interest in gas electron diffraction and made some experimental and theoretical studies of internal rotation and coherent diffraction associated with excitation processes. The latter was especially interesting, but required extensive experimental development that the resources available to me did not permit.  In the 1970’s, I continued theoretical work in crystal structure analysis that included the derivation of a “tangent formula” for phase determination that was based on the more restrictive higher and higher order determinants from the determinantal inequalities. I showed how joint probability distributions relevant to crystallographic quantities could be put into an exponential form and thereby decrease considerably problems with asymptotic convergence. I also derived heuristic joint probability distributions based on the determinants involved in the determinantal inequalities and obtained from them formulas for evaluating triplet phase invariants and, later on, formulas for the expected values of phase invariants and embedded semi-invariants of any order, triplet, quartet, quintet, etc. The utility of phase invariants of high order in phase determination has so far been rather limited, except perhaps collectively in the high order determinants where they have been useful for refining the values of approximately determined phase values.  I participated with Wayne Hendrickson of my laboratory in some refinements of macromolecular structure with the use of the tangent formula and also had some early participation with John Konnert and Wayne Hendrickson in the constrained refinement technique for macromolecules. In collaboration with John Konnert and Peter D’Antonio, procedures were developed for determining atomic arrangements in amorphous materials based on criteria similar to those applied to molecular vapors. Collaborations on structural problems also included Judith Flippen-Anderson, Clifford George, Richard Gilardi and Alfred Lowrey.  At the end of the 1970’s Wayne Hendrickson made some valuable advances in the application of anomalous dispersion to the determination of macromolecular structure that rekindled an interest that I formerly had in this subject. I developed an exact, linear algebraic theory that includes any number and type of anomalous scatterer and any number of wavelengths. It can also incorporate information from isomorphous replacement measurements. Exact data give exact values for the unknown quantities that include phase differences. I have also been investigating the evaluation of triplet phase invariants to see what their potential usefulness may be. This activity continues to the present and is greatly facilitated by Stephen Brenner who has performed my programming and computing for me since the early 1960’s.  In addition to participating in the development of new analytical methods and their applications, I have taught from time to time, mathematics and physics in the University College of the University of Maryland, I have taken an active role in the affairs of crystallography over the years as, for example, President of the International Union of Crystallography (1981-1984) and have enjoyed having a laboratory that investigates a broad variety of subjects ranging over gaseous molecules, amorphous solids, fibers, crystals and crystalline macromolecules.  During my entire married life I have had the strong support of my wife, both technical and spiritual. I also deeply appreciate the supportive atmosphere provided by the Naval Research Laboratory. This was especially helpful during the early 1950’s when a large number of fellow-scientists did not believe a word we said.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1985*, Editor Wilhelm Odelberg, [Nobel Foundation], Stockholm, 1986  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 1985 **Addendum, 1992** Since this biography was written in 1985, advances have been made in macromolecular structure analysis by applications of the linear algebraic theory for the multiwavelength anomalous dispersion technique that I published in 1980. A number of such applications have been made by Wayne Hendrickson and colleagues who, along with the applications, developed suitable techniques for the use of synchrotron radiation and relatively weak anomalous scatterers. In recent years, I have been concerned with additional developments in the anomalous dispersion technique and have become interested in some aspects of the solution of nonlinear simultaneous equations, the determination of electron densities in crystals and some new approaches to phase determination in crystal structure problems.  Receipt of the Nobel Prize has given me the opportunity to have contact to an unprecedented degree with young people who look forward to careers in science and other intellectual and artistic pursuits. I have also had many contacts with organizations whose purpose is to improve the quality of life on this planet in a variety of contexts. These contacts have not changed my earlier views, but, in many instances, have perhaps given some of them a sharper focus. I would like to share a few.  Societies must provide a framework of encouragement in which its children can develop their skills fully and an educational system open to all in which this can be achieved. In many societies with which I am familiar, this would require a major change of priorities. Encouragement within the family structure is also very important.  This world has enormous social, economic and political problems, not the least of which concern the environment and natural resources. The degradation of the environment must be brought under control if there is to be a worthwhile and sustainable quality of life for most people. This too will require a reordering of priorities. It is very likely that continued population growth will defeat any attempts to halt environmental degradation and the unconscionable destruction of resources. Everyone has a responsibility in this regard.  Respect for the dignity of all human beings, if widespread, would go very far toward relieving numerous social stresses that much too often lead to societal deterioration and violence.  Our world has a long way to go.  Peace.  Copyright © The Nobel Foundation 1992 **Addendum, April 2006** Since the time of my receipt of the Nobel Prize in 1985, as you undoubtedly know, my life has changed considerably. I have written about it up to 1992. I have now prepared an additional addendum that reaches to the present (March, 2006).  My wife, Isabella, and I still go to work regularly at the Naval Research Laboratory in Washington, D.C. Although we collaborated several times in the past, I have been mainly concerned with the use of quantum mechanics to facilitate the solution of problems in chemistry that heretofore were quite limited because of their complexity. There are several publications which have included myself, a member of my research group, Dr. Lulu Huang, and Prof. Lou Massa of the City University of New York.  Some of the most attractive features of obtaining a Nobel Prize in science is the opportunities that express themselves in invitations to participate in important issues and activities which arise in the form of meetings of distinguished persons who also take the time to encourage young people to commit themselves to a life in science. A perfect example of the latter is the yearly meetings of Nobel Laureates in science and students in Lindau, Germany. I have had the very pleasant privilege of engaging in both types of activities.  Poland is another country in which, for some years, my wife and I participated in one of their yearly lecture programs. My wife’s parents came from Poland, and although she was born in Detroit, Michigan, she spoke only Polish until she started elementary school at the age of seven. One year, when we were in Poland we made a visit to a mining town for unusual minerals, Przebenia, where my father was born. The lady who was the mayor put together for us a variety of items describing the history of the town and its many features, a most kindly effort on our behalf.  I should mention that my research program has changed very much over the years. The main objective now is to develop a broad and detailed understanding of how nature operates in carrying out the processes that are known to occur in the human body. Of course, many such processes are understood to a certain extent but there are many processes that are only partially known or not at all well understood. This is not a project that is generally easily or rapidly solved. Some progress has been made in developing methods for discovering the properties of important substances such as proteins which may lead, in time, to a better understanding of the processes in which such substances play a major role.  *Jerome Karle died on 6 June 2013.* |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** | 395 |
| Q18 | Thank you for coming to this interview, professor. I just wanted to ask you first of all, I believe this year, which is the year 2005, it’s the year of [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/einstein-facts.html) and people are trying to commemorate and trying to make the general public understand more about science. Do you think that it’s important that we highlight our genius, the geniuses that we have, who have been with us and those we have around us still? |
|  | Jerome Karle: Yes, I believe that it’s an excellent idea. Einstein was very special. He saw and understood things that really revolutionised the understanding of what the scientific world was all about 100 years ago. He changed things. It took something of the order of 40 years of various people, good scientists, trying to find out whether he was right or not, and trying to find out what they could do with it. And they finally ended up with some very important practical applications. Now the thing about Einstein was that he did things like that all his life. And when scientific developments come into a person’s head which are then passed on to others to think about, to work on, to develop, and if that comes or occurs time after time, then you have an example of true genius. |
| Q18 | Is it important for the scientific world to be able to have people that one could relate to the general public, to make us understand how important science is for the development? |
|  | Jerome Karle: I think that that’s very important because, in effect, the public generally, through their governments, need to support. I think that life would be very difficult for scientists if they irritated the public. No, but I think that the public understands. For example, with respect to medical questions, they don’t relate generally to esoteric science, but they can certainly understand when there are new developments and understandings that will be able to make life safer and better for them. |
| Q9 | In your personal life, in which way did it change after you received the Nobel Prize? |
|  | Jerome Karle: I don’t readily change very much. There was a fun part. For example, since my institution is a governmental institution, the President of the United States called me up and we had a nice chat for a while. The fun part was not only the telephone call but he invited me and my wife to come to some of the big dinners that were held at the White House and that sort of thing. So it was an opportunity to see things that generally speaking you’d never see. |
| Q5 | That must have been great. I believe that you have worked with your wife for many years and she’s been very much part of your research and studies. Can you tell us a bit about the partnership that you two have? |
|  | Jerome Karle: What we did was that we continued a research problem that we had worked on when we were getting our PHD degrees. And we worked very closely on that. She is extremely bright and has all kinds of honours – in fact probably if one went down the list and so forth, she would have more honorary degrees and prizes and so forth – but she didn’t receive the Nobel Prize, which astonished me, because we co-worked on that together. |
| Q5 | Do you think she should have had it as well, together with you? |
|  | Jerome Karle: Absolutely. And in fact one of the professors in Sweden who was responsible for the Nobel Prize was very outspoken about it and he did something which is never done and that is, after you receive the Nobel Prize you’re expected to visit universities in Norway and Sweden and give lectures. And he fixed it up so that she would accompany me and give her own lectures. |
| Q18 | In general, afterwards, when you thought about that work, do you think you as a scientist have a responsibility for what you are creating, so to speak, if you look at the long term, the way the politicians and so on use the creations? |
|  | Jerome Karle: Yes. I have a story about that. I worked at the University of Chicago. They had a special set up there. There were – oh, I don’t know – six or eight major places around the United States working on this problem. And I can tell you that at one time, after it seemed as if we were succeeding in making what we needed to make, we wrote a letter to the President and asked him to please not be the first one to use it. And we thought that was settled. But, as you know, that didn’t happen. And that was always a source of discomfort to me. |
| Q18 | Do you think today that too much research …? I mean you’re still working and I know that a lot of scientific research is being brought in to the military machine, not just in the United States, all over the world. Is it OK that so much money still is being spent on military research? Whether one could maybe do it more on other issues – or do they go hand in hand? Does it have to be like this? |
|  | Jerome Karle: I can only speak for myself. For myself, I certainly wish that it would have stopped a long time ago. But there’s another issue that’s close to it and that is the issue of trying to get everybody to agree to stop. I don’t know how to feel about it. Certainly let’s not build it up any more. But I’m not so sure about destroying it at this point. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0396 |
| **Biographical** | Bruce Merrifield was born in Fort Worth, Texas, July 15, 1921, the only son of George E. and Lorene (Lucas) Merrifield. In the spring of 1923 they drove across the southwest desert to settle in California where they lived in several cities throughout the state. He attended nine grade schools and two high schools before graduating from Montebello High School in 1939. His interest in chemistry began there and he also enjoyed the astronomy club where he ground a mirror and built a small reflecting telescope. As a senior he managed to be runner up in the annual science contest and in the process learned a valuable lesson in the scientific method.  College began at Pasadena Junior College and at the end of two years he transferred to the University of California at Los Angeles (UCLA). After graduation in chemistry he worked for a year at the Philip R. Park Research Foundation taking care of an animal colony and assisting with growth experiments on synthetic amino acid diets. One of these was the experiment by Geiger that first demonstrated that the essential amino acids must be present simultaneously for growth to occur.  It soon became clear that more education was necessary and he returned to graduate school at the UCLA chemistry department with professor of biochemistry M.S. Dunn to develop microbiological methods for the quantitation of the pyrimidines. Graduation was on June 19, 1949, on June 20 he and Elizabeth Furlong were married, and on June 21 they left California for New York City and the Rockefeller Institute for Medical Research.  At the Institute, later Rockefeller University, he worked as an Assistant for Dr. D.W. Woolley who was to have a profound influence on his career. They worked on a dinucleotide growth factor he discovered in graduate school and on peptide growth factors that Woolley had discovered earlier. These studies led to the need for peptide synthesis and, eventually, to the idea for solid phase peptide synthesis in 1959. The development and application of the technique have occupied him and his laboratory up to the present date. He is very proud of the fact that his office was once occupied by the great pioneer peptide chemist, Max Bergmann, and has been inspired by the knowledge that his laboratories were once filled with names like Leonidas Zervas, Joseph Fruton, Klaus Hoffmann, Emil Smith, [William Stein, and Stanford Moore](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1972/index.html).  In the meantime his wife, Libby, who was a biologist by training, stayed home in Cresskill, New Jersey, and raised their six children who now range in age from 19 to 32 years. They have been the great joy in the life of their parents; and now Jim has a daughter, Kelly, who is the pride of the whole family. Three years ago Libby joined the Merrifield laboratory at Rockefeller University.  He was a Nobel Guest Professor at Uppsala University in 1968 and was elected a member of the U.S. National Academy of Sciences in 1972. He has received several awards for his work on peptide chemistry including the Lasker Award for Basic Medical Research (1969), the Gairdner Award (1970), the Intra-Science Award (1970), the American Chemical Society Award for Creative Work in Synthetic Organic Chemistry (1972), the Nichols Medal (1973), the Instrument Specialties Company Award of the University of Nebraska (1977), and the 2nd Alan E. Pierce Award of the American Peptide Symposium (1979).  He has received honorary degrees from the University of Colorado (1969), Uppsala University (1970), Yale University (1971), Newark College of Engineering (1972), the Medical College of Ohio (1972), Colgate University (1977), and Boston College (1984). In 1984 he was appointed the John D. Rockefeller Jr. Professor of the Rockefeller University. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0397 |
| **Biographical** | Professional Experience  Instructor, University of California, Berkeley, 1940-41  Instructor and Assistant Professor, Cornell University, 1941-46  Assistant Professor, Associate Professor, Professor, University of Chicago, 1946-61  Professor, Stanford University, 1962-86  Professor Emeritus, 1986  Chairman, Department of Chemistry, University of Chicago, 1956-59  Chairman, Department of Chemistry, Stanford University, 1972-74 & 1978-79    Honors and Awards  Guggenheim Fellow, 1949 and 1955  American Chemical Society Award for Nuclear Applications in Chemistry, 1955  Harrison Howe Award, Rochester Section, ACS, 1960  Chandler Medal, Columbia University, 1964  John Gamble Kirkwood Award, New Haven Section, ACS, 1966  ACS Award for Distinguished Service in the Advancement of Inorganic Chemistry, 1967  Nichols Medal, New York, ACS, 1971  Willard Gibbs Medal, Chicago Section, ACS, 1971  F.P. Dwyer Medal, University of New South Wales, Australia, 1973  Honorary Doctorate (L.L.D.) University of Saskatchewan, 1973  Marguerite Blake Wilbur Endowed Professorship, 1976  National Medal of Science, Washington, D.C., 1977  Allied Chemical Award for Excellence in Graduate Teaching & Innovative Science, 1979  Degree of Ph. D. Honoris Causa of the Hebrew University of Jerusalem, 1979  T.W. Richards Medal of the Northeastern Section, ACS, 1980  ACS Award in Inorganic Chemistry of the Monsanto Company, 1981  The Linus Pauling Award, Puget Sound Section, ACS, 1981  National Academy of Sciences Award in Chemical Sciences, 1983  Bailar Medal, University of Illinois, 1983  Doctor of Science, University of Chicago, 1983  Robert A. Welch Foundation Award in Chemistry, 1983  Nobel Prize in Chemistry, 1983  Doctor of Science, Polytechnic Institute, New York, 1984  Honorary Member, College of Chemists of Catalonia and Beleares, 1984  Priestley Medal, ACS, 1985  Doctor of Science, State University of New York, 1985  Corresponding Member, Academy of Arts and Science of Puerto Rico, 1985  Honorary Member, Canadian Society for Chemistry, 1986  Distinguished Achievement Award, International Precious Metals Institute, 1986  The Oesper Award, The Cincinnati Section of the American Chemical Society, 1986  Doctor of Science, University of Guelph, 1987  Honorary Member, Hungarian Academy of Sciences, 1988  Doctor of Science, honoris causa, Seton Hall University, 1988  Doctor of Science, Lajos Kossuth University of Debrecen, Hungary, 1988  Honorary Fellowship, Royal Society of Chemistry, 1989  Honorary Fellowship, Indian Chemical Society 1989  G. M. Kosolapoff Award, Auburn Section, ACS, 1990  Doctor of Science, Northwestern University, 1990    Membership in Societies  American Chemical Society  National Academy of Sciences  American Academy of Arts & Sciences  Phi Beta Kappa  Sigma Xi  Phi Lamda Upsilon (honorary member)  Royal Physiographical Society of Lund  American Philosophical Society  Royal Danish Academy of Sciences & Letters  Foreign Member of the Finnish Academy of Science & Letters  Foreign Member, Royal Society  Corresponding Member, Brazilian Academy of Sciences  Foreign Associate, Engineering Academy of Japan  Corresponding Member, Australian Academy of Science    Consultantship  Catalytica Associates, Inc., Mountain View, California    Research Interests  Current research interests include: charge transfer as affecting properties including the reactivity of ligands; mixed valence molecules; mechanisms of “atom” and electron transfer reactions; basic chemistry of osmium and ruthenium, effects arising from back-bonding.    Publications  Over 350 scientific articles and a book have been published as a result of this research. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0398 |
| **Biographical** | I was born in 1926 to Lazar and Bella (née Silin) Klug in Zelvas, Lithuania, but remember nothing of the place, because I was brought to South Africa as a child of two and grew up there. My father was trained as a saddler, but in fact as a young man worked in his father’s business of rearing and selling cattle, so he grew up in the countryside. He had a traditional Jewish education and secular schooling, and though not a conventionally well educated man, he had some gift for writing, and had a number of articles published in the newspapers of the capital, for which he acted as what would now be called a stringer. Shortly after I was born he emigrated to Durban, where members of my mother’s family had settled at the turn of the century, and the rest of the family followed soon thereafter.  Durban was then a relatively sleepy town in subtropical surroundings. It was a fine place for a boy – there was the beach and the bush and school was not too taxing. I went to a good school, Durban High School, which was run on traditional English lines, with a curriculum somewhat adapted to South African circumstances. We had some good masters particularly in History and English. However, by the standards of to-day, there were few challenges other than Advanced Latin Prose Composition in the 6th Form. The philosophy of the school was quite simple – the bright boys specialised in Latin, the not so bright in science and the rest managed with geography or the like. There was a good library but it was the playing fields that kept one out of mischief. I did not feel a particularly strong call to any one subject, but read voraciously and widely and began to find science interesting. It was the book called *Microbe Hunters* by Paul de Kruif, well known in its time, which influenced me to begin medicine at university as a way into microbiology.  At the University of Witwatersrand in Johannesburg, I took the pre-medical course and, in my second year, I took, among other subjects, biochemistry, or physiological chemistry as it was then called, which stood me in good stead in later years when I came to face biological material. However, I felt the lack of a deeper foundation, and moved to chemistry and this in turn led me to physics and mathematics. So finally I took a science degree.  I had by then decided that I wanted to do research in physics and I went to the University of Cape Town which was then offering scholarships which enabled one to do an M.Sc. degree, in return for demonstrating in laboratory classes. The University lay in a beautiful site on the slopes of Table Mountain, which one climbed at week-ends. I was lucky to find as Professor there, R.W. James, the X-ray crystallographer, who had brought to Cape Town the traditions of the Bragg school at Manchester. He was an excellent teacher and I used to attend his undergraduate lectures as well as those in the M.Sc. course. From him I acquired a feeling for optics, and a knowledge of Fourier theory, and I remember particularly certain optical experiments on rather abstruse phenomena such as external and internal conical refraction which fascinated me. After taking my M.Sc. degree, I stayed on and worked on the X-ray analysis of some small organic compounds, in the course of which I developed a method of using molecular structure factors for solving crystal structures, and taught myself some quantum chemistry to calculate bond lengths and so on. During this time, I developed a strong interest, broadly speaking, in the structure of matter, and how it was organised. I had now acquired a good knowledge of X-ray diffraction, not only through my own work, but through having helped James check the proofs of his fine book – *The Optical Principles of the Diffraction of X-rays* – still a standard work. James wrote beautifully and fully and took great pains to make everything clear.  Supported by an 1851 Exhibition Scholarship and also by a research studentship to Trinity College, I went to Cambridge in 1949. Cambridge was the place for someone from the Colonies or the Dominions to go on to, and it was to the Cavendish Laboratory that one went to do physics. I wanted to work on some form of “unorthodox” X-ray crystallography, for example protein structure, but the MRC Unit where [Perutz and Kendrew](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1962/index.html) were working was full, and Bragg, then the Cavendish Professor, had closed down a project on order – disorder phenomena in alloys, which interested me. I finally found myself a research student of D.R. Hartree, who had been a colleague of both Bragg and James at Manchester. He suggested to me a theoretical problem left over from his work during the war on the cooling of steel through the austenite-pearlite transition, and I learned a fair amount of metallurgy in order to understand the physical basis of the phenomenon. It turned out however in the end that it was not special crystallographic insight that was called for – the course of the transition was in practice governed by the diffusion of the latent heat and I ended up using numerical methods to solve the partial differential equations for heat flow in the presence of a phase transition. I learned a good deal during this time, particularly in computing and solid state physics, and the idea of nucleation and growth in a phase change had its echo when I came later to think about the assembly of tobacco mosaic virus.  After taking my Ph.D., I spent a year in the Colloid Science department in Cambridge, working with F.J.W. Roughton, who had asked Hartree for someone to help him tackle the problem of simultaneous diffusion and chemical reaction, such as occurs when oxygen enters a red blood cell. The methods I had developed for the problem in steel were applicable here, and I was glad to put them to use on an interesting new problem. The quantitative data came from experiments in which thin layers of blood were exposed to oxygen or carbon monoxide. In the course of my stay there, I also showed how one could analyse the experimental kinetic curves for the reaction of haemoglobin with carbon dioxide or oxygen by simulations in the computer, and so fit the rate constants.  This work made me more and more interested in biological matter, and I decided that I really wanted to work on the X-ray analysis of biological molecules. I obtained a Nuffield Fellowship to work in J.D. Bernal’s department in Birkbeck College in London and I moved there at the end of 1953. I joined a project on the protein ribonuclease, but shortly afterwards met Rosalind Franklin, who had moved to Birkbeck earlier and had begun working on tobacco mosaic virus. Her beautiful X-ray photographs fascinated me and I was also able to interpret some pictures which had apparently anomalous curved layer lines in terms of the splitting which occurs when the helical parameters are non-rational. From then on my fate was sealed. I took up the study of tobacco mosaic virus, and in four short years, together with Kenneth Holmes and John Finch, who had joined us as research students, we were able to map out the general outline of the structure of tobacco mosaic virus. This work was done partly in parallel with that of Donald Caspar, then at Yale, but he spent 1955 – 56 in Cambridge, and I formed an association with him which continued across the Atlantic for many years. It was during this time that I met [Francis Crick](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1962/index.html) and we published a paper together on diffraction by helical structures. I was fortunate to work with him again later, and so be able to learn, as he once wrote of Bragg, from watching the way he went about a problem.  Rosalind Franklin died in 1958 and, supported by an N.I.H. grant, Finch, Holmes and I continued the work on viruses, now extended to spherical viruses. We were joined soon after by Reuben Leberman, a biochemist. In 1962 we moved to the newly built MRC Laboratory of Molecular Biology in Cambridge which, under the leadership of Perutz, was to house the original unit from the Cavendish Laboratory (Perutz, Kendrew, Crick and, later, Brenner), enlarged by Sanger’s group from the Biochemistry Department and Hugh Huxley from University College London. I was thus privileged to join the laboratory at this stage in its expansion and so be able to take advantage of, and to help build up, its unique environment of intellectual and technological sophistication. The rest of my scientific career is largely a matter of record and much of this is dealt with in the lecture that follows.  However, I should perhaps add that during the 20 years I have been back in Cambridge, I have been actively involved in the teaching of undergraduates, as well as of course supervising research students. I am still a Director of Studies in Natural Science at my College, Peterhouse, and under the tutorial or – as it is called in Cambridge – supervision, system, I teach undergraduates myself. I like teaching and the contact with young minds keeps one on one’s toes, but increasing responsibilities have forced me to shed much of it in recent years.  Before I came to Cambridge I married Liebe Bobrow whom I had met in Cape Town. She trained in modern dance at the Jooss-Leeder School in London and later became a choreographer and coordinator for the Cambridge Contemporary Dance Group. More recently she has directed and acted in the theatre. We have two sons, Adam and David, born in 1954 and 1963. Adam, after studying History and Economics at Oxford and the London School of Economics, is now doing research in Econometrics. David is a second year student of Physics. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q6 | Can you tell us about your first years there? |
|  | Aaron Klug: Well, I came to Cambridge as a graduate student from South Africa.  Aaron Klug: In 1949. And I thought originally I would do a Part II, which is the last year of the undergraduate, but my tutor, supervisor it was called, said I knew enough and I might as well start research, which I did. I should explain there was a fair amount of freedom which was rather important. And actually I had to find a supervisor. And indeed it wasn’t a very good choice of supervisor in some sense because my PhD was on a problem in solid state physics – my PhD is in physics – which didn’t lead anywhere directly, but it gave me the chance of learning a great deal of things which later turned out to be very useful, although I didn’t know it. In fact it was a preparation for things to come, without my knowing it; it wasn’t planned in any way. |
| Q6 | When you came to London? |
|  | Aaron Klug: Yes, and that determined really my scientific career, because up to then I hadn’t had a firm idea of what I wanted to do. I wanted to work on the structure of matter by logic or inorganic. In fact I wanted to work with the problems in solid state physics, which I found quite fascinating. I’m interested in how things are made and constructed.  But I did know some biology because I’d been a medical student in South Africa before – well pre-medical – so I’d done physiology, biochemistry, things of that sort. So I wasn’t totally green when I came to work on viruses. And I wasn’t scared. Because most physicists can’t bear to touch wet, messy things, you see, but I didn’t mind mincing up a plant when I worked on plant viruses and doing a bit of chemistry and things like that. So in a sense it was quite a long preparation, but it turned out to be very good in the end – but totally unplanned. |
| Q5 | I see. So you moved to London and then worked together with Rosalind Franklin? |
|  | Aaron Klug: Yes I did. She died while I was there. I met her in 1954 and she died in 1958. But during a few short years together with two or her researchers we worked out the general structure of tobacco mosaic virus and after she died I went on with the three dimensional structure – because it’s not an ordinary crystal; you have to develop new analytical methods for dealing with it. And I can do some mathematics. But I have always kept close to experimental work. |
| Q5 | We’ll come back to viruses, but I know that you have written an article on Rosalind Franklin and she is quite known for her role, or the denial of her role, in the discovery of the double helix of DNA. Can you say some words about that? And Wilkins …? |
|  | Aaron Klug: She did the basic experimental work which supplied [Watson](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1962/watson-facts.html) and [Crick](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1962/crick-facts.html) with key information, some known to her and others unknown to her. And she was a very single minded person, but she had no collaborator. She didn’t get on well with [Wilkins](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1962/wilkins-facts.html).  Aaron Klug: Wilkins was the man who got the Nobel Prize with Watson and Crick. He had started the work on DNA, so in that sense he was justified, but later he gave up the problem and began to work on complexes of DNA in sperm, you see, because nobody knew at that time that the DNA would carry the secret of its action with structure which was very unusual then in science. But she, being a very systematic and very good physical chemist, which is what she was, she was able to sort out two different forms of DNA.  Up to then everybody had been working on mixtures and that had made it possible to get very good x-ray diffraction photographs of the two forms. And she developed methods – improved the x-ray method, again as another crystal in the ordinary sense. So if she had continued to live and if Watson and Crick hadn’t intervened she would have got the structure out but it would have come out in a less dramatic way. It would have come out in stages. I have no question about it and that’s what I’ve written about it in two articles – one in ’68 and one in ’74. But I did learn from her. I think the main thing I learnt was to be single minded. I tended to do different things because I was interested in a curious amount then rather than being dedicated to a particular target. |
| Q2 | Why was it so interesting to get this structure? |
|  | Aaron Klug: It was there. Also, it became a test object, in the sense that some of the methods of working out large assemblies, which is what we pioneered – because you must remember that at the time, when I started on it, nobody had even solved the structure of a single protein; Perutz and [Kendrew](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1962/kendrew-facts.html) only did that in … 1962 they got the Nobel Prize. It came out in 1958 whereas I started working on viruses in 1954. And it was a time when people didn’t believe that Perutz and Kendrew could solve the structure of a protein, let alone a virus. We took risks in some sense, although I didn’t think of it at the time. |
| Q4 | During your career, what do you consider is the most significant discovery in molecular biology? |
|  | Aaron Klug: I think the structure of chromatin which is a material of which chromosomes were made. And I began that after work on transfer RNA, which also are quite a major achievement. That was also done by Alex Rich. We competed with him. But the chromatin – and I would think another major achievement has been work I’ve done since my Nobel Prize when I discovered a new class of proteins which are involved in switching genes on and off. They’re called zinc fingers and they work in a modular fashion. Each of the fingers, which includes zinc, recognises three base pairs of DNA so it’s a modular system, a combinatorial system and after we began to understand how it worked and the fingers are flexibly connected so they can attach themselves and recognise the sequence of DNA.  And we’ve now used this to engineer artificial transcription factors which can switch genes on and off and there are now a couple of companies in the world developing it. I think that’s quite a major achievement and I’m still working on it. I’m trying to improve the specificity, because you have to be sure when you target DNA that you have to hit the right target and not hit something closely similar. In a human genome of 3,000,000,000 bases, if you have a run of DNA, a certain sequence, you might well have something very similar elsewhere and you want to be sure you get the right one. So you spend a lot of time trying to develop specific recognition. |
| Q14 | My last question then – where are you going to jump over to now? |
|  | Aaron Klug: Well I’m not going to live forever. Some years ago I started a group on Alzheimer’s disease. I started out in a managerial capacity but I got involved in the science and I cover quite a wide range in these methods and so we showed that there was a molecule called tau which aggregated in an abnormal form and killed the cells and that wasn’t known before. And so that has been quite useful. And I still keep a foot in that, together with my colleagues. I don’t work in it myself but I’ve done a few useful things there. I can’t say, but mostly I’m thinking about gene regulation, how to regulate gene expression, which is an immediate problem in molecular biology. Because if you can intervene in gene expression you can actually follow a biological pathway and develop a central pathway, or in the case of a disease. We have used zinc fingers to reduce the herpes simplex virus by a factor of ten, using zinc fingers to attack the gene of the virus. That’s done not so much to cure the disease but to be able to see what you can do. This is what I’m doing now. That’s my main interest. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0399 |
| **Biographical** | I was born the eldest of three sons of Ryokichi Fukui, a foreign trade merchant and factory manager, and Chie Fukui, in Nara, Japan, on October 4, 1918. In my high school years, chemistry was not my favourite subject, but the most decisive occurrence in my educational career came when my father asked the advice of Professor Gen-itsu Kita of Kyoto Imperial University concerning the course I should take. Prof. Kita suggested that Ryokichi, one of his juniors from the same native province, should send me to the Department of Industrial Chemistry with which he was then affiliated.  For a few years after my graduation from Kyoto Imperial University in 1941, I was engaged in experimental research on synthetic fuel chemistry in the Army Fuel Laboratory. The result brought me a prize in 1944. I became lecturer in the Fuel Chemistry Department of Kyoto Imperial University in 1943, assistant professor in 1945, and professor in 1951. In 1947 I married Tomoe Horie. I have two children, Tetsuya (son) and Miyako (daughter).  While I started originally as an experimentalist, I had built up a subgroup of theoreticians in my group before 1956. My work on experimental organic chemistry continued along with this, and the results were mostly published in Japanese papers, the number of which amounted to 137 during the period 1944 – 1972, together with my papers on reaction engineering and catalytic engineering.  But the nature of my main work in chemistry can be better represented by more than 280 English publications, of which roughly 200 concern the theory of chemical reactions and related subjects. Other English papers relate to statistical theory of gellation, organic synthesis by inorganic salts, and polymerization kinetics and catalysts.  My first scientific delight came in 1952 when I found a correlation between the frontier electron density and the chemical reactivity in aromatic hydrocarbons. This success led my theoretical group to the chemical reactivity theory, extending more and more widely the range of compound and reactions that were discussed.  The year in which my 1952 paper was published was the same as that of Professor [Mulliken](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1966/index.html)‘s publication of the important paper on the chargetransfer force in donor-acceptor complexes. Influenced by this paper, I gave a theoretical foundation for the findings mentioned above. The basic idea was essentially the consideration of the importance of the electron delocalization between the frontier orbitals of reactant species. The frontier orbital approach was further developed in various directions by my own group and many other scientists, both theoretical and experimental.  I was also interested in formulating the path of chemical reactions. The first paper appeared in 1970. This simple idea served to provide information on the geometrical shape of reacting molecules, and I was able to make the role of the frontier orbitals in chemical reactions more distinct through visualization, by drawing their diagrams.  I must confess that, when I was writing the 1952 paper, I never imagined I would be coming to Stockholm to receive the Nobel Prize 30 years later. But I have to add that already at that time Professor Gen-itsu Kita encouraged me by suggesting the possibility of the growth of my theory leading me one day to this supreme prize. The possibility became a reality through the good circumstances in which I found myself: with my teachers, my colleagues and students, and, of course, my parents and family. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0400 |
| **Biographical** | I came to a happy Jewish family in dark days in Europe. On July 18, 1937 I was born to Clara (née Rosen) and Hillel Safran in Zloczow, Poland. This town, typical of the Pale of the Settlement, was part of Austria-Hungary when my parents were born. It was Poland in my time and is part of the Soviet Union now. I was named after Roald Amundsen, my first Scandinavian connection. My father was a civil engineer, educated at the Lvov (Lemberg) Polytechnic, my mother by training a school teacher.  In 1939 the war began. Our part of Poland was under Russian occupation from 1939-1941. Then in 1941 darkness descended, and the annihilation of Polish Jewry began. We went to a ghetto, then a labor camp. My father smuggled my mother and me out of the camp in early 1943, and for the remainder of the war we were hidden by a good Ukrainian in the attic of a school house in a nearby village. My father remained behind in the camp. He organized a breakout attempt which was discovered. Hillel Safran was killed by the Nazis and their helpers in June 1943. Most of the rest of my family suffered a similar fate. My mother and I, and a handful of relatives, survived. We were freed by the Red Army in June 1944. At the end of 1944 we moved to Przemysl and then to Krakow, where I finally went to school. My mother remarried, and Paul Hoffmann was a kind and gentle father to me until his death, two months prior to the Nobel Prize announcement.  In 1946 we left Poland for Czechoslovakia. From there we moved to a displaced persons’ camp, Bindermichl, near Linz, in Austria. In 1947 we went on to another camp in Wasseralfingen bei Aalen in Germany, then to München. On Washington’s Birthday 1949 we came to the United States.  I learned English, my sixth language at this point, quite quickly. After P.S. 93 and P.S. 16, Brooklyn, I went on to the great Stuyvesant High School, one of New York’s selective science schools. Among my classmates were not only future scientists but lawyers, historians, writers – a remarkable group of boys. In the summers I went to Camp Juvenile in the Catskills, a formative experience. Elinor, my younger sister, was born in 1954.  In 1955 I began at Columbia College as a premedical student. That summer and the next I worked at the National Bureau of Standards in Washington with E.S. Newman and R.E. Ferguson. The summer after I worked at Brookhaven National Laboratory, with J.P. Cumming. These summers were important because they introduced me to the joys of research, and kept me going through some routine courses at Columbia. I did have some good chemistry teachers, G.K. Fracnkel and R.S. Halford, and a superb teaching assistant, R. Schneider. But I must say that the world that opened up before me in my non science courses is what I remember best from my Columbia days. I almost switched to art history.  In 1958 I began graduate work at Harvard. I intended to work with W.E. Moffitt, a remarkable young theoretician, but he died in my first year there. A young instructor, M.P. Gouterman, was one of the few faculty members at Harvard who at that time was interested in doing theoretical work, and I began research with him. In the summer of 1959 I got a scholarship from P.O. Lowdin’s Quantum Chemistry Group at Uppsala to attend a Summer School. The school was held on Lidingö, an island outside of Stockholm. I met Eva Börjesson who had a summer job as a receptionist at the school, and we were married the following year.  I came back to Harvard, began some abortive (and explosive) experimental work, and Eva and I took off for a year to the Soviet Union. It was the second year of the U.S.-U.S.S.R graduate student exchange. I worked for 9 months at Moscow University with A.S. Davydov on excitor theory. Eva and I lived in one of the wings, Zona E, of that great central building of Moscow University. My proficiency in Russian and interest in Russian culture date from that time.  On returning to the U.S. I switched research advisors and started to work with W.N. Lipscomb, who had just come to Harvard. Computers were just coming into use. With Lipscomb’s encouragement and ebullient guidance, L.L. Lohr and I programmed what was eventually called the extended Hückel method. I applied it to boron hydrides and polyhedral molecules in general. One day I discovered that one could get the barrier to internal rotation in ethane approximately right using this method. This was the beginning of my work on organic molecules.  In 1962 I received my doctorate, as the first Harvard Ph.D. of both Lipscomb and Gouterman. Several academic jobs were available, and I was also offered a Junior Fellowship in the Society of Fellows at Harvard. I chose the Junior Fellowship. The three ensuing years in the Society (1962 – 65), gave me the time to switch my interests from theory to applied theory, specifically to organic chemistry. It was EJ. Corey who taught me, by example, what was exciting in organic chemistry. I began to look at all kinds of organic transformations, and so I was prepared when in the Spring of 1964 R.B. Woodward asked me some questions about what subsequently came to be called electrocyclic reactions. That last year at Harvard was exciting. I was learning organic chemistry at a great pace, and I had gained access to a superior mind. [R.B. Woodward](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1965/index.html) possessed clarity of thought, powers of concentration, encyclopedic knowledge of chemistry, and an aesthetic sense unparalleled in modern chemistry. He taught me, and I have taught others.  The 1962 – 65 period was creative in other ways as well: Our two children, Hillel Jan and Ingrid Helena, were born to Eva and me.  In 1965 I came to Cornell where I have been ever since. A collegial department, a great university and a lovely community have kept me happy. I am now the John A. Newman Professor of Physical Science. I have received many of the honors of my profession. I am especially proud that in addition to the American Chemical Society’s A.C. Cope Award in Organic Chemistry, which I received jointly with R.B. Woodward in 1973, I have just been selected for the Society’s Award in Inorganic Chemistry in 1982, the only person to receive these two awards in different subfields of our science.  I have been asked to summarize my contributions to science.  My research interests are in the electronic structure of stable and unstable molecules, and of transition states in reactions. I apply a variety of computational methods, semiempirical and nonempirical, as well as qualitative arguments, to problems of structure and reactivity of both organic and inorganic molecules of medium size. My first major contribution was the development of the extended Huckel method, a molecular orbital scheme which allowed the calculation of the approximate sigma- and pie- electronic structure of molecules, and which gave reasonable predictions of molecular conformations and simple potential surfaces. These calculations were instrumental in a renaissance of interest in sigma electrons and their properties. My second major contribution was a two-pronged exploration of the electronic structure of transition states and intermediates in organic reactions. In a fruitful collaboration R.B. Woodward and I applied simple but powerful arguments of symmetry and bonding to the analysis of concerted reactions. These considerations have been of remarkable predictive value and have stimulated much productive experimental work. In the second approach I have analyzed, with the aid of various semiempirical methods, the molecular orbitals of most types of reactive intermediates in organic chemistry-carbonium ions, diradicals, methylenes, benzynes, etc.  Recently I and my collaborators have been exploring the structure and reactivity of inorganic and organometallic molecules. Approximate molecular orbital calculations and symmetry-based arguments have been applied by my research group to explore the basic structural features of every kind of inorganic molecule, from complexes of small diatomics to clusters containing several transition metal atoms. A particularly useful theoretical device, the conceptual construction of complex molecules from MLn fragments, has been used by my research group to analyze cluster bonding and the equilibrium geometries and conformational preferences of olefin and polyene metal carbonyl complexes. A satisfactory understanding of the mode of binding of essentially every ligand to a metal is now available, and a beginning has been made toward understanding organometallic reactivity with the exploration of potential energy surfaces for ethylene insertion, reductive elimination and alkyl migrative insertion reactions. Several new structural types, such as the triple-decker and porphyrin sandwiches, have been predicted, and recently synthesized by others. On the more inorganic side, we have systematically explored the geometries, polytopal rearrangement and substitution site preferences of five, six, seven and eight coordination, the factors that influence whether certain ligands will bridge or not, the constraints of metal-metal bonding, and the geometry of uranyl and other actinide complexes. I and my coworkers are beginning work on extended solid state structures and the design of novel conducting systems.  The technical description above does not communicate what I think is my major contribution. I am a teacher, and I am proud of it. At Cornell University I have taught primarily undergraduates, and indeed almost every year since 1966 have taught first-year general chemistry. I have also taught chemistry courses to non-scientists and graduate courses in bonding theory and quantum mechanics. To the chemistry community at large, to my fellow scientists, I have tried to teach “applied theoretical chemistry”: a special blend of computations stimulated by experiment and coupled to the construction of general models – frameworks for understanding.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1981*, Editor Wilhelm Odelberg, [Nobel Foundation], Stockholm, 1982  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.  Added in 1992  In the last decade I and my coworkers have begun to look at the electronic structure of extended systems in one-, two-, and three dimensions. Frontier orbital arguments find an analogue in this work, in densities of states and their partitioning. We have introduced an especially useful tool, the COOP curve. This is the solid state analogue of an overlap population, showing the way the bond strength depends on electron count. My group has studied molecules as diverse as the platinocyanides, Chevrel phases, transition metal carbides, displacive transitions in NiAs, MnP and NiP, new metallic forms of carbon, the making and breaking of bonds in the solid state and many other systems. One focus of the solid state work has been on surfaces, especially on the interaction of CH4 , acetylene and CO with specific metal faces. The group has been able to carry through unique comparisons of inorganic and surface reactions. And in a book “Solids and Surfaces. A Chemist’s View of Bonding in Extended Structures,” I’ve tried to teach the chemical community just how simple the concepts of solid state physics are. And, a much harder task, to convince physicists that there is value in chemical ways of thinking.  In 1986-88 I participated in the production of a television course in introductory chemistry. “The World of Chemistry” is a series of 26 half-hour episodes developed at the University of Maryland and produced by Richard Thomas. The project has been funded by Annenberg/the Corporation for Public Broadcasting. I am the Presenter for the series which began to be aired on PBS in 1990, and will also be seen in many other countries.  My first real introduction to poetry came at Columbia from Mark Van Doren, the great teacher and critic whose influence was at its height in the 1950’s. Through the years I maintained an interest in literature, particularly German and Russian literature. I began to write poetry in the mid-seventies, but it was only in 1984 that a poem was first published. I own much to a poetry group at Cornell that includes A.R. Ammons, Phyllis Janowitz and David Burak, as well as to Maxine Kumin. My poems have appeared in many magazines and have been translated into French, Portuguese, Russian and Swedish. My first collection, “The Metamict State”, was published by the University of Central Florida Press in 1987, and is now in a second printing. A second collection, “Gaps and Verges”, was also published by the University of Central Florida Press, in 1990. Articles on my poetry have appeared in *Literaturnaya Gazeta* and *Studies in American Jewish Literature*. I received the 1988 Pergamon Press Fellowship in Literature at the Djerassi Foundation, Woodside, California, where I was in residence for three years.  It seems obvious to me to use words as best as I can in teaching myself and my coworkers. Some call that research. Or to instruct others in what I’ve learned myself, in ever-widening circles of audience. Some call that teaching. The words are important in science, as much as we might deny it, as much as we might claim that they just represent some underlying material reality.  It seems equally obvious to me that I should marshal words to try to write poetry. I write poetry to penetrate the world around me, and to comprehend my reactions to it.  Some of the poems are about science, some not. I don’t stress the science poems over the others because science is only one part of my life. Yet there are several reasons to welcome more poetry that deals with science.  Around the time of the Industrial Revolution – perhaps in reaction to it, perhaps for other reasons – science and its language left poetry. Nature and the personal became the main playground of the poet. That’s too bad for both scientists and poets, but it leaves lots of open ground for those of us who can move between the two. If one can write poetry about being a lumberjack, why not about being a scientist? It’s experience, a way of life. It’s exciting.  The language of science is a language under stress. Words are being made to describe things that seem indescribable in words – equations, chemical structures and so forth. Words do not, cannot mean all that they stand for, yet they are all we have to describe experience. By being a natural language under tension, the language of science is inherently poetic. There is metaphor aplenty in science. Emotions emerge shaped as states of matter and more interestingly, matter acts out what goes on in the soul.  One thing is certainly not true: that scientists have some greater insight into the workings of nature than poets. Interestingly, I find that many humanists deep down feel that scientists have such inner knowledge that is barred to them. Perhaps we scientists do, but in such carefully circumscribed pieces of the universe! Poetry soars, all around the tangible, in deep dark, through a world we reveal and make.  It should be said that building a career in poetry is much harder than in science. In the *best* chemical journal in the world the acceptance rate for full articles is 65%, for communications 35%. In a *routine* literary journal, far from the best, the acceptance rate for poems is below 5%.  Writing, “the message that abandons”, has become increasingly important to me. I expect to publish four books for a general or literary audience in the next few years. Science will figure in these, but only as a part, a vital part, of the risky enterprise of being human. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q18 | What is science and what is science in relation to other ways for humans to deal with the wonders of life, like literature and poetry or music or arts or religion? |
|  | Roald Hoffman: First I think science is a social system of western European invention, not an American one, for gaining reliable knowledge. I would say not truth, and we can talk about why there is a difference. And it has several components to it. It has curious people, some of whom are interested in mathematics or good at it. It has people who are willing to get their hands dirty, experimenters, not just philosophers. It has a system for communicating that knowledge and in fact a compulsion if not an addiction for doing so, exchanging that knowledge. And it is a system which makes use of pretty normal people to get interesting things to learn about the world around us and for us.  It has another aspect and which sometimes has been lost, it was over romanticised in the 19th century and that is that it should improve the human condition in some way. And that is implicit in Nobel’s will, it is interesting that it is there because it is a 19th century document. I would say like [Peter Medawar](https://www.nobelprize.org/prizes/medicine/1960/medawar/facts/) does that at least we should ameliorate the human condition, to make it a little bit better. It is a little bit lost when science has become so professional as it is but I think it is important to revive it. |
| Q18 | Are there any similarities between the scientific way of searching for answers and for example religious or for artistic? |
|  | Roald Hoffman: Well, let’s talk about religion and science a little bit. I think religion and science come out initially from some of the same roots and that is a desire to understand the world. The mysteries of the world. The lightning, the lights, the stars, the fine patterns in it. Now they start out also for many religions with a feeling that the world is real. Some religions have added on that the real world does not matter and what matters is the afterlife. But a lot of religion begins with the idea that the world is real and that the actions of human beings matter, whether you are good in some way. I think that is actually a fairly common starting point. After that I think that there are differences on deciding who is in charge of the order, let’s say. But I think they start off from some similar things. What was the rest of the question? |
| Q18 | Are there some dogmas in science that you just not mention? |
|  | Roald Hoffman: One can look for dogmas in science and I think they are there. At the same time the edible complex is very strong in science of young people making careers by slaying their fathers, by drawing out ideas and that is an accepted way of doing so. A much less respect for authority in the end. But there are dogmas. So why are there dogmas? Because there are human beings at work.  But somehow the social system of science that there are many people doing things assures that in the long run those dogmas do not stay dogmas. Whereas the social system of religion perpetuates the authority often. And makes it more difficult to introduce change. Interesting balance between change and stasis in science. You do invoke authority that is in part what those footnotes are about in papers, but at the same time you want to do something new. An interesting system. |
| Q2 | Do you think there is such a relation between truth, beauty and simplicity? |
|  | Roald Hoffman: No. I think this is a falling into the weakness of the human mind to think that the world is simple. And it is an interesting tripartite relationship. I guess I’m fighting in part what I would say is, pernicious ideas of physicists coming into chemistry who think the moment you can write up Sherman’s equation that all of chemistry is solved. And behind that are statements by the Iraq great physicist, theoretical physicist about this equation that an equation, if it is beautiful, must be true.  Yes, some are in the old world but not in this world that we have. The equations are simple in part and yet complex. Symmetry is there, symmetry gets broken. Things happen in a body especially when they are subject to evolution things in incredibly complicated ways. The way a message is sent from one nerve to another with an intermediate of a molecule, dopamine on serotonin and there is one thing that sets it loose and another one that detects it, but it is not done in one way. After a while this thing looks like what we would call in the United States, a Rube Goldberg machine, one of those cartoons where there is something that does something else that does something else and terribly complicated.  So those things are I think those are natural, that things are complicated. I think it is the dreams of the simplicity of our mind which cannot sometimes be over complexity and that’s true of personal relationships as well as it is with politics or with science. What is interesting to me is that scientists should fall for simplicity. Given that they are faced with the realities of complexity all the time. And they will come back and tell you, physicists, well if you take it apart underneath there are some simple equations. Yes, but the taking apart has destroyed the essence and the reality of anything that is real, and they never put it together. They only take the watch apart like children. |
| Q2 | You mentioned this is also a voice of a poet, maybe we can listen to one of your poems? |
|  | Roald Hoffman: Sure. Let me read a poem but let me preface it by some comments about it. In it you will see many voices of different ways of trying to understand the world around something simple which is something happening in Provence in France, […]. And the poem is called ‘Enough Already’ and behind it, initially it was called ‘Dayenu’ which is what one a prayer, a song in the Passover say there, which means that would have been enough if God had only taken us out of Israel that would have been enough, but he had done other things. And there is a little bit of that. And there is in this also … so there is something about my Jewish background in this, in the phraseology here. Let me read it anyway. |
| Q2 | Do you find science is an inspiration for your writing? In science and in poetry? |
|  | Roald Hoffman: Yes, it is. It is limited because some of the sort of poetry and sciences depend on them being part of the cognitive framework. Knowing what went before, what it is about. But part of it is what I find inspiring is the language, the kind of natural language that scientists speak, and they don’t the language is important. They think equations and chemical formulas are important. But language is all we’ve got, and this language is made to serve and express things so words like energy and force mean special things well defined and we try to make them fit. And then there are words invented and words used, let me give you an example.  What often happens to me is it is given to me to go to dull seminars and sometimes after I fall asleep I wake up and it is still going on and so I listen sometimes to the words even if they are not so interesting. And someone came at a seminar and was talking about some mathematical equation, he said “Let us assume free boundary conditions”. He was talking about some technical words, boundary conditions are what you do on a mathematical equation to make it fit at the ends but sort of the idea, free boundaries was very interesting because it was a typical Zen idea of something being free and something being constrained. So I worked that into a poem. So the language is interesting. Then there is a lot of metaphor, let me give you an example.  Roald Hoffman: Yes, in science which I can use in poetry. Someone was talking about once at another seminar about the metamict state. It is a funny word, metamict. It is not used very much. Someone had invented it, so what was it? It was a state of matter of, for instance, radioactive minerals. Minerals in which there was a radioactive atom like thorium. The minerals when formed gave beautiful transparent crystals, perfect crystals, but after a while the atoms which were sitting at the sites in the crystal began decomposing in nuclear reactions. And as they emitted some ray they rebounded and the ions that were left behind bounced into other atoms and created this order in the lattice and with time the crystal grew whitish yellow and amorphous dull looking and was destroyed. And that was the metamict state. It’s a poem by itself, I mean the ending is within that. And all that order has been destroyed without any plan to do so. So I took that and wrote a poem about that. So there are metaphors like that. |
| Q10 | How do your colleagues or the scientific community react to that? |
|  | Roald Hoffman: Well, I think or how do they react to the poetry in general to the excursions out of science. On one hand they react positively, and they are glad someone is doing it. They have a lot of respect for spiritual things. It shows up in interesting ways not necessarily directly, but it shows up in little things like when somebody writes a book that they use a poem as an epigraph or something like that. I think some of them think that he can afford to do it and we can’t and to some extent they are right.  The young assistant professor up for promotion, should he or she put up front their activity outside the science. I think they should put in something about their teaching and their attempts to communicate to the public. But very often they are afraid to do so and they make some judgement and it is correct that it is not the prime thing they are evalued on but it certainly adds little on the edges and adds some colour, could make a difference in a decision. Some people are just, some of my colleagues might think that this is something one does when one no longer does science, goes into administration, does history, sociology of science. Well, so to them I have to prove to them that I still do science. I have just finished writing two proposals. Maybe I feel I have to prove but I enjoy doing it also.  You know what hurts the most is when it comes through that somehow people think that this activity of writing poetry or running this café or something, when they think this is easy. That hurts. Because on a poem I go through typically 15, 20 drafts, I don’t do that on a scientific paper. No, it is true. I have written 500 scientific papers, so it becomes easier but still it was always easier to do the science and I am actually trying something harder. And they just have to try it, I would say. So I get angry at that if that happens. But by and large there is a lot of respect in the community for this. |
| Q2 | The poetry writing, does this make you a better scientist do you think? |
|  | Roald Hoffman: I think the poetry writing of course makes me feel better about myself for having tried to do that. I think that is true about anyone, that they need some other thing to do. I think the poetry writing has given me a bit of an appreciation and more for the power of concise statement but then I have that in science too. Once again comes back maybe simplicity, but it is not quite simplicity because it is a complex statement of word but it says something economically. Something intensely. And I have learned a little how to do that through the poetry.  I think much more than either the science helping me to do the poetry or the poetry helping the science. I think I am working out something that is in me that wants to see the relationships, to watch to see relationships, to communicate that to people. And I am doing that in the science and I am doing that in the poetry in other ways. And occasionally because I don’t believe in separating my worlds I will separate them. I’ll not write that article or the poetry because I am not going to get it published because I have got to get by the gate keepers. But occasionally and the kind of thing I have done here in Stockholm, this visit, someone gives me a chance to talk about both worlds and not to separate them and I love it. |
| Q6 | So my final question would be how do you find time to do all of this? |
|  | Roald Hoffman: Oh, that is not a good question because it is, I’ll answer it honestly but there is no time to do all this. First of all, I should probably give up the science and do the writing, so why should I? If I had some rational reasoning behind it but I’m not rational. The rational reasoning is that there is simply lots of good people doing the science, there are fewer people and therefore I think it is of more value to society to be able to work on this area in-between, of popularising science, of building bridges between the scientists and especially the shapers of the spirit and the arts and humanities. There are less people doing that. But I still find the science fun, first of all.  But now how do I do all this? So I haven’t given up the science and the teaching. I am 67 and I could retire but I don’t want to, I don’t have to, so I am still doing the science. As I said I have just written two major research proposals to the National Science Foundation for two different aspects of my work, each with three years of research. I have ideas that I want to work them out. So I have added on the writing to the science and I would say that if I were to analyse my days are very complex and interesting. But if I were to analyse it more than half the time I spend probably on writing or societal activities somehow related to it more than on the science. I’ve added it on and it has not been done without personal problems in my life. It is not easy to live with someone like me who does all these things because I need to be in control of my time, I need to be able to, if I want to, to sit down and write and there have been stresses in my personal life as a result. So, it has not been without some difficulties. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0401 |
| **Biographical** | I was born in New York on June 30, 1926 and my formative years were spent in a small, gated community named Sea Gate, at the southernmost tip of Brooklyn. By the time I reached Jr. high school I had already formed a strong ambition to be a scientist, in part stimulated by my readings about medical scientists: “Arrowsmith” by [Sinclair Lewis](https://www.nobelprize.org/nobel_prizes/literature/laureates/1930/index.html) and “Microbe Hunters” by Paul DeKruif. An inspiring high school “teacher”, Sophie Wolfe, whose job was to supervise the stockroom that supplied the classes in chemistry, physics and biology, nurtured that ambition. Her love of young people and interest in science led her to start an after school program of science clubs. Rather than answering questions we asked, she encouraged us to seek solutions for ourselves, which most often turned into mini research projects. Sometimes that involved experiments in the small lab she kept but sometimes it meant going to the library to find the answers. The satisfaction derived from solving a problem with an experiment was a very heady experience, almost addicting. Looking back, I realize that nurturing curiosity and the instinct to seek solutions are perhaps the most important contributions education can make. With time, many of the facts I learned were forgotten but I never lost the excitement of discovery.  I graduated from high school (1943) just a year after the bombing of Pearl Harbor and enrolled at New York’s City College to study chemical engineering. But the prospects of designing chemical plants for industrial scale chemical processes seemed far less interesting than the chemical events that occur in biological systems. But I felt it necessary to be part of the war effort and I enlisted in the Navy to be a flyer. While waiting to be called to pre-flight training, I enrolled at the Pennsylvania State University to study biochemistry. After a few months I was called to active duty but allowed to remain at Penn State for pre-flight training. After a year at Penn State and six months training at sea I served on a submarine chaser through the end of the war. I returned to the university in the fall of 1946 and completed my undergraduate degree in biochemistry two years later.  As a project during the senior year, I studied a group of papers that dealt with the application of newly available radioisotopes as tracers for the study of intermediary metabolism. Particularly fascinating were a succession of papers from Western Reserve University that showed how compounds labeled with isotopic carbon and/or heavy nitrogen atoms could be tracked during their conversion from foodstuffs to cellular materials. That seemed exciting and although I had never heard of Western Reserve University it was my next destination.  That was a fortunate choice; in fact, it changed the course of my career. Because of its pioneering work in this new field, the department had by the late 1940’s become one of the important biochemistry centers in the country. Harland Wood, the department head, was an inspiring scientist and teacher but he was also devoted to his students and colleagues. My doctoral thesis concerned what is today referred to as C1 metabolism, more specifically the conversion of formic acid, formaldehyde and methanol to the fully reduced state of methyl groups in methionine. I was among the first to demonstrate, in vitro, that folic acid and vitamin B12 cofactors participated in these processes. (1952). By that time I was hooked on a career in academic research instead of one in the pharmaceutical industry that I had originally considered in deciding to get a PhD.  Seeking more experience with enzymes, I studied for a year in Copenhagen with Herman Kalckar at the Institute of Cytophysiology and for a second year with [Arthur Kornberg](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1959/index.html) at Washington University in St. Louis. Both were very productive and enjoyable experiences. In Copenhagen, Wolfgang Joklik and I discovered a new enzyme that created the nucleoside triphospates for nucleic acid assembly and the next year in Kornberg’s lab, I discovered a previously unknown class of biological compounds – acyl adenylates – intermediates in the formation of fatty acyl-CoAs from fatty acids, ATP and CoA. This type of reaction, I discovered, was also central to the way amino acids are activated as amino-acyl adenylates prior to being linked to tRNAs. By then, I was making the slow transition from classical biochemistry to molecular biology and becoming increasingly preoccupied with how genes act and how proteins are made.  After 6 years in St. Louis, I moved to Stanford University’s Medical Center (1959) to help Kornberg set up the new department of biochemistry. In time, my interests shifted from studies with microorganisms to mammalian cells and I spent a year experimenting with Polyoma and SV40 tumor viruses in mammalian cell culture with Renato Dulbecco at the Salk Institute. Soon after I returned to Stanford, I conceived of using SV40 as a means for introducing new genes into mammalian cells much in the way that bacteriophage transduce cellular DNA among infected cells. My colleagues and I succeeded in developing a general way to join two DNAs together in vitro; in this case, a set of three genes responsible for metabolizing galactose in the bacterium *E. coli* was inserted into the SV40 DNA genome. That work led to the emergence of the recombinant DNA technology thereby providing a major tool for analyzing mammalian gene structure and function and formed the basis for me receiving the 1980 Nobel Prize in Chemistry. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q4 | Are you happy with the way that genetic engineering has developed since then? |
|  | Paul Berg: Well, like most scientific advances, they open the door to new kinds of researches and I don’t carry the title of father of genetic engineering very well, because in fact we made only the first step and everybody else seized immediately on this new technology, new capability and began to use it for their own purposes, which is what it was intended to do. But there are many, many very smart people in this world and they all figured out new ways to be able to use recombinant DNA so that today recombinant DNA is the most important tool and governs almost every question that anybody could ask in biology and even outside biology, which is really interesting because it has affected virtually every way in which people want to investigate living organisms – plants or insects or whatever. Recombinant DNA is the basic tool and so one has to feel very good about the impact of one’s early work, even though we didn’t foresee everything that would happen. |
| Q9 | You could only imagine the impact? |
|  | Paul Berg: No, many people have asked me did you foresee all the new kinds of things that would develop in the whole industry and the answer is very clearly no. We had in mind to do the experiment that we set out to do and, in the early stages, we increased or widened the range of what we did but it was like an avalanche behind us. Many, many people were developing new approaches and particularly industry. |
| Q18 | So what is your opinion about the commercial pressure on research? |
|  | Paul Berg: I think it’s, well it’s positive in the sense that it brings money to the academic institutions; it provides jobs for the people who are trained in our laboratories, but the universities have themselves become commercialised and I think my university, Stanford University, is probably one of the leaders in large part because it made a lot of money very early from the patents on recombinant DNA. I mean, Stanford probably over 17 years of the patent acquired something like, probably more than $100 million, so other universities see that as a possibility of a way of earning money so they become much more aggressive about seeking patents so now, whereas before when I was growing up in the university, nobody ever asked me to patent anything and I never patented anything in my life. I gave away everything we did.  I gave away everything we ever did because what we did it for was to benefit research, but in fact today universities come into the labs and try to teach people how to look for things to patent, help them to start companies, so we now have a very different kind of mix and I think the openness with which scientists usually talk about their research is compromised to some extent, but it has changed the whole climate about the thing. People think in terms of what they’re doing, will I be able to start a business? Will I be able to patent it? Instead of thinking, getting it published and being excited by the fact that people want to use it. |
| Q18 | Can you tell us some words about the Asilomar conferences? |
|  | Paul Berg: Yes. Well, the concerns about the recombinant DNA or genetic engineering came from the scientists, so that was a very crucial fact which I think historically people now identify as a very major and important stage in the development of science, and that is scientists who developed the new technology themselves began to worry about whether there were any dangers and therefore took a major step to make a public announcement saying that this research is very, very important, very powerful but it also has certain risks and we need to examine these risks and determine whether the research should go forward or whether we have to develop new ways to do the research and protect the public and I think we gained an enormous amount of public admiration, if you will, and tolerance and so we were allowed to actually begin to deal with the question of how can we prevent any dangers, things coming out of our work? And so this led to the Asilomar conference. The Asilomar conference was called to bring scientists who wanted to work using recombinant DNA … |
| Q18 | Do you think it should be time now for Asilomar 3? |
|  | Paul Berg: Well, we had an Asilomar 3 and it was to discuss whether an Asilomar model, whether that way of approaching problems could be applied to today’s problems and different people had different views. |
| Q18 | Do you think that some parts of research or of science should be forbidden? |
|  | Paul Berg: I think when there is clear evidence of inflicting damage or danger, then I think there’s a strong argument that things should be forbidden but I think they should only be forbidden for specific time intervals, so for example if there’s a moratorium on a particular line of research, or even if you pass legislation to forbid certain kinds of research, it should only last, say, five years and in five years to re-examine the issue again. Maybe the agreement is it is still something that shouldn’t be done and so you go forward this way; but in many cases in our country, if the government passes a Bill and makes it law, sometimes it’s exceedingly difficult to change it and so you’re locked into a situation which, five years later, doesn’t look the right decision. Just if you have a minute, I just want to mention one interesting anecdote.  This year, the Alaska Foundation, which was a major award giving foundation in the US, gave the award to a man named [Robert Edwards](https://www.nobelprize.org/nobel_prizes/medicine/laureates/2010/edwards-facts.html). Robert Edwards was the man who invented in vitro fertilisation. 25 years ago, when he was doing the preliminary experiments, I can tell you that the huge protests were the same as they are today for stem cells. People thought he was going to make monsters, that the was going to produce children who were going to be deformed and very incapacitated. So today we have a million, more than a million live births, many people who have had children they could not have had before, so 25 years later we recognise that this was a major health and medical benefit and we give him a big prize. Alaska Award often comes just before the Nobel Prize, and yet if you think back at that time, if we would’ve passed the law which says IVF is forbidden, against the law, we would’ve shut down a whole very important advancement and I think we can point to recombinant DNA as another.  Recombinant DNA came very close in our country and I know in Sweden also, to being forbidden. We would’ve lost the entire bio-technology revolution, so we have to think very clearly before you pass laws that forbid scientific work and so the question you asked is are there some lines of science that should be forbidden? Temporarily perhaps, but certainly not forever. |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0402 |
| **Biographical** | I was born on March 21, 1932 in Boston, Massachusetts. My father, Richard V. Gilbert, an economist, was at that time at Harvard University. He worked for the Office of Price Administration during the second World War and later headed up a planning group advising the Pakistani government. My mother, Emma Cohen, was a child psychologist, who practiced giving intelligence tests to me and my younger sister. She educated us at home for the first few years, to keep my sister and me amused. We loved reading and raided the adult section of the public library. In 1939 my family moved to Washington D.C.; I was educated there in public schools, later at the Sidwell Friends high school.  I always had an interest in science, in those years minerology and astronomy (I was a member of a minerological society and an astronomical society as a child). I became interested in inorganic chemistry at high school. In my last year in high school, 1949, I was fascinated by nuclear physics and would skip school for long periods to go down to the Library of Congress to read about Van de Graaf generators and simple atom smashers. I went to Harvard and majored in chemistry and physics. I became interested in theoretical physics and, as a graduate student, worked in the theory of elementary particles, the quantum theory of fields. I spent my first graduate year at Harvard, then went to the University of Cambridge for two years, where I received my doctorate degree in 1957. My thesis supervisor was Abdus Salam; I worked on dispersion relations for elementary particle scattering: an effort to use a notion of causality, formulated as a mathematical property of analyticity of the scattering amplitude, to predict some aspects of the interaction of elementary particles. I met Jim Watson during this period. I returned to Harvard and, after a postdoctoral year and a year as Julian Schwinger’s assistant, became an assistant professor of Physics. During the late fifties and early sixties, I taught a wide range of courses in theoretical physics and worked with graduate students on problems in theory. However, after a few years my interests shifted from the mathematical formulations of theoretical physics to an experimental field.  In the summer of 1960, Jim Watson told me about an experiment that he and Francois Gros and his students were working on. I found the ideas exciting and joined in for the summer. We were trying to identify messenger RNA, a short-lived RNA copy of a DNA gene, which serves as a carrier of information from the genome to the ribosomes, the factories that make proteins. After each messenger is used a few times to dictate the structure of a protein, it is broken down and recycled to make other messenger RNA molecules. The experiments sought a fleeting new component that we finally managed to pin down. I found the experimental work exciting and have continued research in molecular biology ever since.  After a year of work on messenger RNA, I returned briefly to physics then came back to biology to study how proteins are synthesized. I showed that a single messenger molecule can service many ribosomes at once and that the growing polypeptide chain always remains attached to a transfer RNA molecule. This last discovery illuminated the mechanism of protein synthesis: the protein chain is transferred in turn from one amino-acid-bearing transfer RNA to another as it grows, their order dictated by messenger RNA and ultimately by the genetic code on the DNA. In the middle sixties, Benno Müller-Hill and I isolated the lactose repressor: the first example of a genetic control element. A repressor is a protein product made by one gene in the bacterium in order to control a second gene by turning it off when its product is not wanted. This control function had been defined genetically by the work of [Jacob and Monod](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1965/index.html), but a repressor is made in such small amounts that it was an extraordinarily elusive biochemical entity. We identified, characterized, and purified one. We developed bacterial strains which made several thousand fold more protein, and showed how that repressor functioned. In the late sixties, David Dressler and I invented the rolling circle model, which describes one of the two ways DNA molecules duplicate themselves. In the early seventies I isolated the DNA fragment to which the *lac* repressor bound and studied the interaction of the bacterial RNA polymerase and the lac repressor with DNA. In the middle seventies, Allan Maxam and I developed the rapid chemical DNA sequencing. At this time, I also became interested in and developed some of the recombinant DNA techniques, specifically showing that blunt end ligation was efficient in putting DNA fragments together. In the late seventies with Lydia Villa Komaroff and Argiris Efstratiadis I worked on bacterial strains that expressed a mammalian gene product, insulin. Currently I am interested in, on one hand, the making of useful proteins in bacteria and, on the other hand, the structure of genes and the evolution of DNA sequences.  After my change from physics to molecular biology, I was promoted at Harvard in Biophysics and later in Biochemistry and Molecular Biology. Since 1974 I have been an American Cancer Society Professor of Molecular Biology.  I am married to Celia Gilbert, a poet, and have two children, John Richard and Kate. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |

|  |  |
| --- | --- |
| **Chemistry\_1999** | |
| **ID** | 0403 |
| **Biographical** | I was born on 13th August 1918 in the village of Rendcombe in Gloucestershire, where my father, also Frederick Sanger, was a medical practitioner. Influenced by him, and probably even more so by my brother Theodore (a year older than me), I soon became interested in biology and developed a respect for the importance of science and the scientific method. At Bryanston School and St John’s College, Cambridge, I was probably above average but not an outstanding scholar. Initially I had intended to study medicine, but before going to University I had decided that I would be better suited to a career in which I could concentrate my activities and interests more on a single goal than appeared to be possible in my father’s profession. So I decided to study science and, on arrival at Cambridge, became extremely excited and interested in biochemistry when I first heard about it, principally through Ernest Baldwin and also other members of the relatively young and enthusiastic Biochemistry Department that had been founded by [F.G. Hopkins](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1929/index.html). It seemed to me that here was a way to really understand living matter and to develop a more scientific basis to many medical problems.  After taking my B.A. degree in 1939 I remained at the University for a further year to take an advanced course in Biochemistry, and surprised myself and my teachers by obtaining a first class examination result. I was a conscientious objector during the war and was allowed to study for a Ph.D. degree, which I did in the Biochemistry Department with A. Neuberger, on lysine metabolism and a more practical problem concerning the nitrogen of potatoes. It was Neuberger who first taught me how to do research, both technically and as a way of life, and I owe much to him. In 1943 A.C. Chibnall succeeded F.G. Hopkins as Professor of Biochemistry at Cambridge and I joined his research group working on proteins and, in particular, insulin. This was an especially exciting time in protein chemistry. New fractionation techniques had been developed, particularly by [A.J.P. Martin](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1952/index.html) and his colleagues, and there seemed to be a real possibility of determining the exact chemical structure of these fundamental components of living matter. I succeeded in developing new methods for amino acid sequencing and used them to deduce the complete sequence of insulin, for which I was awarded the [Nobel Prize for Chemistry in 1958](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1958/index.html). This award had an important and stimulating effect on my subsequent career. I had remained in Cambridge concentrating only on basic research and avoiding as far as possible teaching or administrative responsibilities. This recognition of my work gave me renewed confidence and enthusiasm to continue in this way of life, which I enjoyed. It also enabled me to obtain better research facilities and, even more important, to attract excellent colleagues.  Until 1943 I received no stipend. I was able to support myself as my mother was the daughter of a relatively wealthy cotton manufacturer. From 1944 to 1951 I held a Beit Memorial Fellowship for Medical Research, and since 1951 I have been on the staff of the Medical Research Council. In 1962 I moved to their newly built Laboratory of Molecular Biology in Cambridge, together with [M.F. Perutz](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1962/index.html)‘s unit from the Cavendish Laboratory which included [F.H.C. Crick](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1962/index.html), [J.C. Kendrew](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1962/index.html), H.E. Huxley and [A. Klug](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1982/index.html). In this atmosphere I soon became interested in nucleic acids. Although at the time it seemed to be a major change from proteins to nucleic acids, the concern with the basic problem of “sequencing” remained the same. And indeed this theme has been at the centre of all my research since 1943, both because of its intrinsic fascination and my conviction that a knowledge of sequences could contribute much to our understanding of living matter. My work on nucleic acids is summarised in my Nobel lecture. This work has not been done single-handed and it owes much to the excellent collaborators I have had. Most of these have been students and postdoctoral fellows spending a few years in the laboratory and bringing their experience and ideas with them, but I feel particularly indebted to my more permanent colleagues, B.G. Barrell, A.R. Coulson and G.G. Brownlee, who have contributed so much to the methods we have developed.  I was married to Margaret Joan Howe in 1940. Although not a scientist herself she has contributed more to my work than anyone else by providing a peaceful and happy home. We have two sons, Robin and Peter, born in 1943 and 1946, and a daughter, Sally Joan, born in 1960. Apart from my work my main interests are gardening and what can best be described as “messing about in boats”.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 1980*, Editor Wilhelm Odelberg, [Nobel Foundation], Stockholm, 1981  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 1980 **Addendum, May 2005** Following the work on insulin I developed further methods for studying proteins and particularly the active centres of some enzymes. Around 1960 I turned my attention to the nucleic acids, RNA and DNA. I developed methods for determining small sequences in RNA. The work culminated in the development of the “dideoxy” technique for DNA sequencing around 1975. This was a relatively rapid method and was used to determine the DNA sequence of the bacteriophage f*x* 174 of 5375 nucleotides in 1977, of human mitochrondrial DNA (16,338 nucleotides) and of bacteriophage l (48,500 nucleotides). The method has been improved and automated in other human genome (3 billion nucleotides).  I retired in 1983.  *Frederick Sanger received his first* [*Nobel Prize in Chemistry in 1958*](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1980/index.html)*.*  *Frederick Sanger died on 19 November 2013.* |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q9 | Can you share your secret with us (on winning the Nobel Prize twice)? |
|  | Frederick Sanger: Well, I don’t think there are any secrets. First thing, you know, people ask me this sometimes and I tell them the first thing is to get a prize. And that’s much more difficult to get the first prize than to get the second one. Because if you’ve already got a prize then you can get facilities for work and you can get collaborators and everything is much easier. And I think I was lucky in that respect that I got my first prize when I was rather young, about 40, and most people when they get a prize they get a good job, they become professors or directors of a large group. And I didn’t because I wouldn’t be any good at that.  So really you’ve got to be bad at something, but I was quite good at doing research and I could go on doing research for another 20 years and I didn’t have to apply for grants and that sort of thing. Nowadays there’s a lot of people that when they’re doing science they have to apply for grants, spend a lot of time doing paperwork which I never had to do after I had won the first prize. So I had 20 years when I could just do what I wanted. And I started on a rather difficult project and spent all my time at it and I was very lucky at that time, succeeded in certain parts of the work. |
| Q2 | But you switched your fields after the first prize? |
|  | Frederick Sanger: It wasn’t such a serious switch. My first work was on proteins, you know, things we’re actually made of. And the problem was to do what we call sequencing, actually measure the structure of the proteins, and I worked on proteins. And after that I attacked a much more difficult problem which was the nucleic acids and particularly the DNA. And I eventually came up with a method for sequencing DNA and learning about its structure and that’s what I got the second prize for, together with Wally Gilbert who was also working on the same thing. |
| Q4 | How do you view this development in modern biology? |
|  | Frederick Sanger: It’s very exciting I think. I think there’s going to be a lot of use for medicine if we can really understand DNA and you know it is very complicated. There’s 3,000 million letters, as it were, in the DNA and you’ve got to read all that. And of course there are a lot of people working on that now to try and decipher the means and how it works. And I think that’s going to be of use to medicine. But we don’t know exactly what the applications will be. There’s a lot of talk of course in newspapers and wild talk about we’re going to cure all sorts of diseases and things. |
| Q18 | Do you understand the concerns or fears that people feel when reading about modern biology? |
|  | Frederick Sanger: Not really, no. I mean there are certain things of course you’ve got to think about. But as regards to the human genome the only thing that I’ve heard about is that some people may have to pay more for their insurance policies. And I don’t think that’s a very serious thing. I mean that’s a question for society to decide whether or what things should be used for. But surely it’s much more significant that one should consider what might happen, what good things might happen, that people might be more healthy and more happy. That’s what science is about, to try and improve conditions for humanity on this earth. And I think we should think about that ultimately. And against that you can worry a bit about what insurance you’re going to have to pay. And I think that’s for the insurance industry and not a very serious thing is it? |
| Q18 | What do you think about the commercial success and pressures in molecular biology? |
|  | Frederick Sanger: Of course it’s I think essential thing that happens. I mean that is a question for society to decide, isn’t it? I mean that’s the way capitalism works, isn’t it, that people want to make money. And if they’re working on some good, useful work then that should be very agreeable. But it would be nice to know everything that’s going on and there is a lot of work going on now using the human genome work, which is kept secret now until it’s used. |
| Q18 | What is the place of religion within your scientific philosophy? |
|  | Frederick Sanger: Well, that’s rather difficult to say I think. I was brought up as a Quaker. My father was a Quaker and I think I was never actually a sort of Quaker, I was never a member of the Society of Friends. And when I got to university and stuff and started studying science I gradually lost interest really in religion and Quakerism. But it has influenced I think my philosophy in two ways really. The Quakers are pacifists. They will not take human life. And I still go along with that, and I believe that.  And the Quakers are also very keen on truth and they believe in fundamental truth is one of the virtues. And I was brought up with that philosophy, that I must not tell lies when I was a little boy. And I think this is very important for scientists, because a scientist is really studying truth and you’ve got to be very certain about what you say is truthful. And I think this is a philosophy which is worthwhile because a lot of people I think nowadays are quite happy to say something that’s wrong and it can get them into difficulty and it can get other people into difficulty. I actually have a sort of fixation about it rather, even when somebody asks me, you know they say to me how are you Fred? And I say oh I’m terrible. I should say I’m fine thanks. It’s a standard thing. |
| Q6 | You have developed some other interests such as gardening and ‘messing about in boats’. What do you do when you’re messing about? |
|  | Frederick Sanger: Well, I used to do a bit of sailing and messing about really. I had a boat, a sailing boat, and I had a motor boat. Now I have given up sailing, I don’t do any sailing, but I still have a motor boat and I go on the river and mess about, just being on the river and cruising away from the roads is very pleasant. But gardening is what I do mostly and just living it. |
| Q6 | Are you a successful gardener? |
|  | Frederick Sanger: No, I find it’s … no I’m actually much busier, I think, than I was when I was working because I’ve got time to do all sorts of things. I was going to do lots of different things like painting and building. And I used to do a lot of carpentry at one time, used to do a lot of carpentry. I find that there’s too many things to do. And I’ve got rather a large garden and that takes most of my time now. |
| Q6 | So what is your biggest success in the garden? |
|  | Frederick Sanger: Biggest success? I don’t know really. I think just enjoying myself. And it’s nice for my family. |
| **ID** | 0404 |
| **Biographical** | My parents, Charles Brovarnik and Pearl Gorinstein, were born in Zhitomir in the Ukraine and came to London in 1908 as part of the vast Jewish immigration in the early part of this century. They were married in London. In 1909 my sister, Ann, was born. I arrived on May 22, 1912. In June 1914 my father decided to join his mother and father and other members of his family in Chicago, much to the dismay of my mother, whose own family largely remained in England. My grandfather’s name had been anglicized to Brown, and that became our name. In the United States, my two sisters, Sophie and Riva, were born in 1916 and 1918.  My father had been trained as a cabinet maker, doing delicate inlaid work. However, he found little market for his skills in the U.S. and turned to carpentry.  The Depression of 1920 persuaded him to go into business and he opened a small hardware store in Chicago at 18th and State Street, largely a black neighborhood. We lived in an apartment above the store and I attended the Haven School at Wabash and 16th Street with predominantly black classmates.  I did well in school and was advanced several times, graduating at 12. Indeed, I was offered, but refused, further advancement since I did not want to be in the same class with my sister, Ann.  On graduation, I went to Englewood High School on the South Side of Chicago. Unfortunately, my father became ill of some sort of infection and died in 1926. I left school to work in our store. I am afraid that I was not really interested in the business and spent most of my time reading. My mother finally decided that she would attend the store and I should go back to school. Accordingly, I reentered Englewood in February 1929 and graduated in 1930.  At Englewood I ran the humor column of the school paper and won a national prize. I never recovered.  We sold the store at that time. I had no hope of going on to college. However, this was the beginning of the Depression and I could find no permanent job. Studying appealed to me much more than the odd jobs I could find. I decided to go to college. I entered college intending to major in electrical engineering. I had heard that one could make a good living in that area. However, I took chemistry and became fascinated with that subject, and remained with chemistry thereafter. I had just completed one semester at Crane Junior College when it was announced in 1933 that the school was to be closed for lack of funds. I then went to night school at the Lewis Institute, taking one or two courses, financing myself by working as a part-time shoe clerk.  I then heard that one of the instructors at Crane, Dr. Nicholas Cheronis, had opened his laboratory to several students, so that they could continue their studies on their own. I went there and grew to know and love a fellow student, Sarah Baylen. Sarah had been the brightest student in chemistry at Crane prior to my arrival. She has described (“Remembering HCB”) how she initially “hated my guts.” But since she could not beat me, she later decided to join me, to my everlasting delight.  In 1934 Wright Junior College opened its doors. We went there and nine of us graduated in 1935 as the first graduating class. In my yearbook Sarah predicted that I would be a Nobel Laureate!  I had been advised to take the competitive examination for a scholarship at the University of Chicago. I did so. To my astonishment, little of the examination was devoted to the chemistry, physics, and mathematics that had constituted the major portion of my studies. Instead, the examination emphasized general subjects: history, art, music, literature, etc. – subjects I had never studied formally. I did the best I could and was pleasantly surprised when I received a half scholarship.  I entered the University of Chicago in the Fall of 1935, accompanied by my girlfriend, Sarah. This was the time when the President of the University, Robert Maynard Hutchins, was arguing for the principle that students should be permitted to proceed as rapidly as possible. Indeed, at that time it cost no more to take ten courses than it did the usual three. I did so, and completed my junior and senior year in three quarters, receiving the B.S. in 1936. . I did not apply for graduate work. I wanted to find a job and marry my girlfriend. However, a famous organic chemist, Julius Stieglitz – then Emeritus, but still teaching – called me into his office and urged me to reconsider my decision. He predicted a favorable future as a research chemist. I discussed the matter with Sarah and she agreed that marriage could wait. Accordingly, I began graduate work.  On my graduation, Sarah presented me with a gift – a copy of Alfred Stock’s book, *The Hydrides of Boron and Silicon.* This book interested me in the hydrides of boron and I undertook to study with Professor H.I. Schlesinger, then active in that area of research.  Sarah and I were married “secretly” on February 6, 1937. We were such innocents that we did not realize that marriages are published in the daily newspapers. Consequently, our marriage was a secret for the weekend!  Once the news got out, I had to begin supporting her. But my income as a graduate assistant was only $400 per year, out of which had to come $300 for tuition. But Sarah obtained a position at Billings Hospital in Medical Chemistry and kept us solvent.  I received my Ph.D. in 1938. Unfortunately (perhaps fortunately), I could not find an industrial position. Professor M.S. Kharasch then offered me a position as a postdoctorate at a stipend of $1600 and my academic career was initiated. The following year Professor Schlesinger invited me to become his research assistant with the rank of Instructor, replacing Anton B. Burg, who was moving on to the University of Southern California. Consequently, I am an unusual example of a chemist who ended up in academic work because he could not find an industrial position.  At that time one did not achieve tenure until after ten years. I had seen a number of individuals who had remained at Chicago as Instructors for nine years without tenure and then had to find another position under severe pressure. I decided to avoid this situation. Accordingly, after four years I asked Professor Schlesinger for a decision as to my future in the Department. When he came back with the word that there was no future, I undertook to find another position.  Fortunately, Morris Kharasch had a good friend, Neil Gordon, who had just gone as Department Head to Wayne University in Detroit. (Neil Gordon, the originator of the Gordon Research Conferences, had given Morris Kharasch his first position at the University of Maryland back in 1920.) Neil Gordon was persuaded to give me a position at Wayne as Assistant Professor, preserving my academic career. I became Associate Professor in 1946, and was invited to Purdue in 1947 by the Head of the Chemistry Department, Henry B. Hass, as Professor of Inorganic Chemistry. In 1959 I became Wetherill Distinguished Professor and in 1960 Wetherill Research Professor. I became Emeritus in 1978, but continue to work with a large group of postdoctorates.  Originally my research covered physical, organic and inorganic chemistry and I took students in all three areas. However, as the Department became more organized into divisions, it became necessary to make a choice, and I elected to work primarily with coworkers in organic chemistry.  In addition to my research program in the borane-organoborane area, described in my Nobel Lecture, my research program has involved the study of steric effects, the development of quantitative methods to determine steric strains, the examination of the chemical effects of steric strains, the non-classical ion problem, the basic properties of aromatic hydrocarbons, a quantitative theory of aromatic substitution, and the development of a set of electrophilic substitution constants, s+, which correlate aromatic substitution data and a wide variety of electrophilic reactions.  Recognitions Professor Brown was the Harrison Howe Lecturer in 1953, the Centenary Lecturer of The Chemical Society (London) in 1955, and the Baker Lecturer in 1968. He was elected to the National Academy of Sciences in 1957, the America n Academy of Arts and Sciences in 1966, received an honorary Doctorate of Science degree from the University of Chicago in 1968 and was elected Honorary Fellow of The Chemical Society and Foreign Member of the Indian National Academy of Sciences in 1978. Finally, he is the recipient of the Nichols Medal for 1959, the ACS Award for Creative Research in Synthetic Organic Chemistry for 1960, the Linus Pauling Medal for 1968, the National Medal of Science for 1969, the Roger Adams Medal for 1971, the Charles Frederick Chandler Medal for 1973, the Madison Marshall Award for 1975, the CCNY Scientific Achievement Award Medal for 1976, the Allied Award for 1978, the Ingold Memorial Lecturer and Medal for 1978, the Elliott Cresson Medal for 1978, and the Nobel Prize for 1979. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0404 |
| **Interview** |  |
|  |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0405** |
| **Biographical** | Born in Berlin in 1897. Doctorate and University Teaching Thesis in Marburg/Lahn from the Faculty of Chemistry. Head of Department at Braunschweig (Technical College) from 1932; Associate Professor at Freiburg/Brsg. from 1937; Professor and Faculty Director at the Institute of Chemistry, Tübingen, from 1944; turned down the same position as successor to H. Staudinger at Freiburg/Brsg.; accepted the same position at Heidelberg as successor to K. Freudenberg. Professor Emeritus since 1967.  Scientific Activities Textbook on stereochemistry, 1930. Papers on the subject of ring tension and double bonds as well as valency tautomerism. Main research into organic reactions of alkali metals and elaboration of carbon-based chemistry. Discovery of the halogen-metal exchange reaction (simultaneously with H. Gilman). Development of ylide chemistry and, together with that, study of the Stevens and Sommelet rearrangements as well as intra-anionic ether isomerisation. Through the synthesis of the pentaaryl derivatives of the elements of group 5, the phosphorous ylides were discovered and also, in 1953, the carbonylolefins which have since proven to be crucial for the manufacture of synthetic fabrics and also important in other industrial processes. In 1942 dehydrobenzol was proven to be a shortlived by-product, a fact demonstrated bye. D. Roberts in 1953 and by me, only this time using different means, viz. control experiments on [Diels – Alder](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1950/index.html) adducts. More recently the concept of the “at”-complexes as a counterpart to the “onium” complexes has led to the development of a new chemistry from which have come the sodium tetra phenylborates.  Honours Honorary Doctorate from the Sorbonne in 1957; Honorary Doctorate from the Universities of Tübingen and Hamburg in 1962; Adolf von Baeyer Memorial Medal from the German Chemical Society in 1953; Silver Medal from the University of Helsinki in 1957; Dannie Heinemann Award from the Göttingen Academy of Sciences in 1965; Otto Hahn Award for Chemistry and Physics in 1967; Silver Medal from the City of Paris in 1969; Paul Karrer Medal from the University of Zurich in 1972; Médaille de la Chaire Bruylants (University of Louvain) in 1972; Roger Adams Award from the American Chemical Society in 1973; Karl Ziegler Prize in 1975; Honorary Member of the Swiss Chemical Society in 1963; Honorary Member of the New York Academy of Sciences in 1965; Member of the Chemical Society of Peru, also in 1965; Honorary Fellow of the Chemical Society (London) in 1967; Member of the French Academy in 1971; Member of the Society of Medical Sciences, Córdoba (Argentina), in 1976. As well as these, member of several German academies: Bavarian Academy of Sciences, Heidelberg Academy of Sciences, German Academy of the Natural Scientist Leopoldina Halle. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0405 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | 0406 |
| **Biographical** | Peter Mitchell was born in Mitcham, in the County of Surrey, England, on September 29, 1920. His parents, Christopher Gibbs Mitchell and Kate Beatrice Dorothy (née) Taplin, were very different from each other temperamentally. His mother was a shy and gentle person of very independent thought and action, with strong artistic perceptiveness. Being a rationalist and an atheist, she taught him that he must accept responsibility for his own destiny, and especially for his failings in life. That early influence may well have led him to adopt the religious atheistic personal philosophy to which he has adhered since the age of about fifteen. His father was a much more conventional person than his mother, and was awarded the O.B.E. for his success as a Civil Servant.  Peter Mitchell was educated at Queens College, Taunton, and at Jesus college, Cambridge. At Queens he benefited particularly from the influence of the Headmaster, C.L. Wiseman, who was an excellent mathematics teacher and an accomplished amateur musician. The result of the scholarship examination that he took to enter Jesus College Cambridge was so dismally bad that he was only admitted to the University at all on the strength of a personal letter written by C.L. Wiseman. He entered Jesus College just after the commencement of war with Germany in 1939. In Part I of the Natural Sciences Tripos he studied physics, chemistry, physiology, mathematics and biochemistry, and obtained a Class III result. In part II, he studied biochemistry, and obtained a II-I result for his Honours Degree.  He accepted a research post in the Department of Biochemistry, Cambridge, in 1942 at the invitation of J.F. Danielli. He was very fortunate to be Danielli’s only Ph.D. student at that time, and greatly enjoyed and benefited from Danielli’s friendly and unauthoritarian style of research supervision. Danielli introduced him to David Keilin, whom he came to love and respect more than any other scientist of his acquaintance.  He received the degree of Ph.D. in early 1951 for work on the mode of action of penicillin, and held the post of Demonstrator at the Department of Biochemistry, Cambridge, from 1950 to 1955. In 1955 he was invited by Professor Michael Swann to set up and direct a biochemical research unit, called the Chemical Biology Unit, in the Department of Zoology, Edinburgh University, where he was appointed to a Senior Lectureship in 1961, to a Readership in 1962, and where he remained until acute gastric ulcers led to his resignation after a period of leave in 1963.  From 1963 to 1965, he withdrew completely from scientific research, and acted as architect and master of works, directly supervising the restoration of an attractive Regency-fronted Mansion, known as Glynn House, in the beautiful wooded Glynn Valley, near Bodmin, Cornwall – adapting and furnishing a major part of it for use as a research labotatory. In this, he was lucky to receive the enthusiastic support of his former research colleague Jennifer Moyle. He and Jennifer Moyle founded a charitable company, known as Glynn Research Ltd., to promote fundamental biological research and finance the work of the Glynn Research Laboratories at Glynn House. The original endowment of about £250,000 was donated about equally by Peter Mitchell and his elder brother Christopher John Mitchell.  In 1965, Peter Mitchell and Jennifer Moyle, with the practical help of one technician, Roy Mitchell (unrelated to Peter Mitchell), and with the administrative help of their company secretary, embarked on the programme of research on chemiosmotic reactions and reaction systems for which the Glynn Research Institute has become known. Since its inception, the Glynn Research Institute has not had sufficient financial resources to employ more than three research workers, including the Research Director, on its permanent staff. He has continued to act as Director of Research at the Glynn Research Institute up to the present time. An acute lack of funds has recently led to the possibility that the Glynn Research Institute may have to close.  Beside his interest in communication between molecules, Peter Mitchell has become more and more interested in the problems of communication between individual people in civilised societies, especially in the context of the spread of violence in the increasingly collectivist societies in most parts of the world. His own experience of small and large organisations in the scientific world has led him to regard the small organisations as being, not only more alive and congenial, but also more effective, for many (although perhaps not all) purposes. He would therefore like to have the opportunity to become more deeply involved in studies of the ways in which sympathetic communication and cooperative activity between free and potentially independent people may be improved. One of his specific interests in this field of knowledge is the use of money as an instrument of personal responsibility and of choice in free societies, and the flagrant abuse and basically dishonest manipulation of the system of monetary units of value practised by the governments of most nations.   |  | | --- | | Awards and affiliations | | CIBA Medal and Prize, British Biochemical Society, 1973 | | Member, European Molecular Biology Organisation, 1973 | | Fellowship of the Royal Society, 1974 | | Warren Triennial Prize, jointly with Efraim Racker, U.S.A., 1974 | | Louis and Bert Freedman Foundation Award, New York Academy of Sciences, 1974 | | Honorary Member, American Society of Biological Chemists, 1975 | | Foreign Honorary Member, American Academy of Arts and Sciences, 1975 | | Wilhelm Feldberg (Anglo/German) Foundation Prize, 1976 | | Dr. rerum naturalium honoris cause of the Technische Universität, Berlin, 1976 | | Lewis S. Rosenstiel Award, U.S.A., 1977 | | Foreign Associate, National Academy of Sciences, U.S.A., 1977 | | Honorary Degree of Doctor of Science, Exeter University, U.K., 1977 | | Sir Hans Krebs Lecture and Medal of the Federation of European Biochemical Societies, 1978 | | Honorary Degree of Doctor of Science, University of Chicago, 1978 | |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0406 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0407** |
| **Biographical** | In his memorable series *“Etudes sur le temps humain”*, Georges Poulet devoted one volume to the *“Mesure de l’instant”*.[1](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note1) There he proposed a classification of authors according to the importance they give to the past, present and future. I believe that in such a typology my position would be an extreme one, as I live mostly in the future. And thus it is not too easy a task to write this autobiographical account, to which I would like to give a personal tone. But the present explains the past.  In my Nobel Lecture, I speak much about fluctuations; maybe this is not unrelated to the fact that during my life I felt the efficacy of striking coincidences whose cumulative effects are to be seen in my scientific work.  I was born in Moscow, on the 25th of January, 1917 – a few months before the revolution. My family had a difficult relationship with the new regime, and so we left Russia as early as 1921. For some years (until 1929), we lived as migrants in Germany, before we stayed for good in Belgium. It was at Brussels that I attended secondary school and university. I acquired Belgian nationality in 1949.  My father, Roman Prigogine, who died in 1974, was a chemical engineer from the Moscow Polytechnic. My brother Alexander, who was born four years before me, followed, as I did myself, the curriculum of chemistry at the Université Libre de Bruxelles. I remember how much I hesitated before choosing this direction; as I left the classical (Greco-Latin) section of Ixelles Athenaeum, my interest was more focused on history and archaeology, not to mention music, especially piano. According to my mother, I was able to read musical scores before I read printed words. And, today, my favourite pastime is still piano playing, although my free time for practice is becoming more and more restricted.  Since my adolescence, I have read many philosophical texts, and I still remember the spell *“L’évolution créatrice”* cast on me. More specifically, I felt that some essential message was embedded, still to be made explicit, in [Bergson](https://www.nobelprize.org/nobel_prizes/literature/laureates/1927/index.html)‘s remark:  “The more deeply we study the nature of time, the better we understand that duration means invention, creation of forms, continuous elaboration of the absolutely new.”  Fortunate coincidences made the choice for my studies at the university. Indeed, they led me to an almost opposite direction, towards chemistry and physics. And so, in 1941, I was conferred my first doctoral degree. Very soon, two of my teachers were to exert an enduring influence on the orientation of my future work.  I would first mention Théophile De Donder (1873-1957).[2](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note2) What an amiable character he was! Born the son of an elementary school teacher, he began his career in the same way, and was (in 1896) conferred the degree of Doctor of Physical Science, without having ever followed any teaching at the university.  It was only in 1918 – he was then 45 years old – that De Donder could devote his time to superior teaching, after he was for some years appointed as a secondary school teacher. He was then promoted to professor at the Department of Applied Science, and began without delay the writing of a course on theoretical thermodynamics for engineers.  Allow me to give you some more details, as it is with this very circumstance that we have to associate the birth of the Brussels thermodynamics school.  In order to understand fully the originality of De Donder’s approach, I have to recall that since the fundamental work by Clausius, the second principle of thermodynamics has been formulated as an inequality: “uncompensated heat” is positive – or, in more recent terms, entropy production is positive. This inequality refers, of course, to phenomena that are *irreversible*, as are any natural processes. In those times, these latter were poorly understood. They appeared to engineers and physico-chemists as “parasitic” phenomena, which could only hinder something: here the productivity of a process, there the regular growth of a crystal, without presenting any intrinsic interest. So, the usual approach was to limit the study of thermodynamics to the understanding of equilibrium laws, for which entropy production is zero.  This could only make thermodynamics a “thermostatics”. In this context, the great merit of De Donder was that he extracted the entropy production out of this “sfumato” when related it in a precise way to the pace of a chemical reaction, through the use of a new function that he was to call “affinity”.[3](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#3)  It is difficult today to give an account of the hostility that such an approach was to meet. For example, I remember that towards the end of 1946, at the Brussels IUPAP meeting,[4](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note4) after a presentation of the thermodynamics of irreversible processes, a specialist of great repute said to me, in substance: “I am surprised that you give more attention to irreversible phenomena, which are essentially transitory, than to the final result of their evolution, equilibrium.”  Fortunately, some eminent scientists derogated this negative attitude. I received much support from people such as Edmond Bauer, the successor to [Jean Perrin](https://www.nobelprize.org/nobel_prizes/physics/laureates/1926/index.html) at Paris, and Hendrik Kramers in Leyden.  De Donder, of course, had precursors, especially in the French thermodynamics school of Pierre Duhem. But in the study of chemical thermodynamics, De Donder went further, and he gave a new formulation of the second principle, based on such concepts as affinity and degree of evolution of a reaction, considered as a chemical variable.  Given my interest in the concept of time, it was only natural that my attention was focused on the second principle, as I felt from the start that it would introduce a new, unexpected element into the description of physical world evolution. No doubt it was the same impression illustrious physicists such as Boltzmann[5](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note5) and [Planck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1918/index.html)[6](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#6) would have felt before me. A huge part of my scientific career would then be devoted to the elucidation of macroscopic as well as microscopic aspects of the second principle, in order to extend its validity to new situations, and to the other fundamental approaches of theoretical physics, such as classical and quantum dynamics.  Before we consider these points in greater detail, I would like to stress the influence on my scientific development that was exerted by the second of my teachers, Jean Timmermans (1882-1971). He was more an experimentalist, specially interested in the applications of classical thermodynamics to liquid solutions, and in general to complex systems, in accordance with the approach of the great Dutch thermodynamics school of van der Waals and Roozeboom.[7](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note7)  In this way, I was confronted with the precise application of thermodynamical methods, and I could understand their usefulness. In the following years, I devoted much time to the theoretical approach of such problems, which called for the use of thermodynamical methods; I mean the solutions theory, the theory of corresponding states and of isotopic effects in the condensed phase. A collective research with V. Mathot, A. Bellemans and N. Trappeniers has led to the prediction of new effects such as the isotopic demixtion of helium He3+ He4, which matched in a perfect way the results of later research. This part of my work is summed up in a book written in collaboration with V. Mathot and A. Bellemans, *The Molecular Theory of Solutions*. [8](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note8)  My work in this field of physical chemistry was always for me a specific pleasure, because the direct link with experimentation allows one to test the intuition of the theoretician. The successes we met provided the confidence which later was much needed in my confrontation with more abstract, complex problems.  Finally, among all those perspectives opened by thermodynamcis, the one which was to keep my interest was the study of irreversible phenomena, which made so manifest the “arrow of time”. From the very start, I always attributed to these processes a constructive role, in opposition to the standard approach, which only saw in these phenomena degradation and loss of useful work. Was it the influence of Bergson’s *“L’évolution créatrice”* or the presence in Brussels of a performing school of theoretical biology?[9](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note9) The fact is that it appeared to me that living things provided us with striking examples of systems which were highly organized and where irreversible phenomena played an essential role.  Such intellectual connections, although rather vague at the beginning, contributed to the elaboration, in 1945, of the theorem of minimum entropy production, applicable to non-equilibrium stationary states.[10](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note10) This theorem gives a clear explanation of the analogy which related the stability of equilibrium thermodynamical states and the stability of biological systems, such as that expressed in the concept of “homeostasy” proposed by Claude Bernard. This is why, in collaboration with J.M. Wiame,[11](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#11) I applied this theorem to the discussion of some important problems in theoretical biology, namely to the energetics of embryological evolution. As we better know today, in this domain the theorem can at best give an explanation of some “late” phenomena, but it is remarkable that it continues to interest numerous experimentalists.[12](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#12)  From the very beginning, I knew that the minimum entropy production was valid only for the linear branch of irreversible phenomena, the one to which the famous reciprocity relations of Onsager are applicable.[13](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note13) And, thus, the question was: What about the stationary states far from equilibrium, for which [Onsager](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1968/index.html) relations are not valid, but which are still in the scope of macroscopic description? Linear relations are very good approximations for the study of transport phenomena (thermical conductivity, thermodiffusion, etc.), but are generally not valid for the conditions of chemical kinetics. Indeed, chemical equilibrium is ensured through the compensation of two antagonistic processes, while in chemical kinetics – far from equilibrium, out of the linear branch – one is usually confronted with the opposite situation, where one of the processes is negligible.  Notwithstanding this local character, the linear thermodynamics of irreversible processes had already led to numerous applications, as shown by people such as J. Meixner,[14](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#14) S.R. de Groot and P. Mazur,[15](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#15) and, in the area of biology, A. Katchalsky.[16](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note16) It was for me a supplementary incentive when I had to meet more general situations. Those problems had confronted us for more than twenty years, between 1947 and 1967, until we finally reached the notion of “dissipative structure”. [17](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note17)  Not that the question was intrinsically difficult to handle; just that we did not know how to orientate ourselves. It is perhaps a characteristic of my scientific work that problems mature in a slow way, and then present a sudden evolution, in such a way that an exchange of ideas with my colleagues and collaborators becomes necessary. During this phase of my work, the original and enthusiastic mind of my colleague Paul Glansdorff played a major role.  Our collaboration was to give birth to a general evolution criterion which is of use far from equilibrium in the non-linear branch, out of the validity domain of the minimum entropy production theorem. Stability criteria that resulted were to lead to the discovery of critical states, with branch shifting and possible appearance of new structures. This quite unexpected manifestation of “disorder-order” processes, far from equilibrium, but conforming to the second law of thermodynamics, was to change in depth its traditional interpretation. In addition to classical equilibrium structures, we now face dissipative coherent structures, for sufficient far-from-equilibrium conditions. A complete presentation of this subject can be found in my 1971 book co-authored with Glansdorff.[18](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note18)  In a first, tentative step, we thought mostly of hydrodynamical applications, using our results as tools for numerical computation. Here the help of R. Schechter from the University of Texas at Austin was highly valuable.[19](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note19) Those questions remain wide open, but our centre of interest has shifted towards chemical dissipative systems, which are more easy to study than convective processes.  All the same, once we formulated the concept of dissipative structure, a new path was open to research and, from this time, our work showed striking acceleration. This was due to the presence of a happy meeting of circumstances; mostly to the presence in our team of a new generation of clever young scientists. I cannot mention here all those people, but I wish to stress the important role played by two of them, R. Lefever and G. Nicolis. It was with them that we were in a position to build up a new kinetical model, which would prove at the same time to be quite simple and very instructive – the “Brusselator”, as J. Tyson would call it later – and which would manifest the amazing variety of structures generated through diffusion-reaction processes.[20](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note20)  This is the place to pay tribute to the pioneering work of the late A. Turing,[21](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note21) who, since 1952, had made interesting comments about structure formation as related to chemical instabilities in the field of biological morphogenesis. I had met Turing in Manchester about three years before, at a time when M.G. Evans, who was to die too soon, had built a group of young scientists, some of whom would achieve fame. It was only quite a while later that I recalled the comments by Turing on those questions of stability, as, perhaps too concerned about linear thermodynamics, I was then not receptive enough.  Let us go back to the circumstances that favoured the rapid development of the study of dissipative structures. The attention of scientists was attracted to coherent non-equilibrium structures after the discovery of experimental oscillating chemical reactions such as the Belusov-Zhabotinsky reaction;[22](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note22) the explanation of its mechanism by Noyes and his co-workers;[23](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#23) the study of oscillating reactions in biochemistry (for example the glycolytic cycle, studied by B. Chance[24](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note24) and B. Hess[25](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#25)) and eventually the important research led by [M. Eigen](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1967/index.html).[26](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#26) Therefore, since 1967, we have been confronted with a huge number of papers on this topic, in sharp contrast with the total absence of interest which prevailed during previous times.  But the introduction of the concept of dissipative structure was also to have other unexpected consequences. It was evident from start that the structures were evolving out of fluctuations. They appeared in fact as giant fluctuations, stabilized through matter and energy exchanges with the outer world. Since the formulation of the minimum entropy production theorem, the study of non-equilibrium fluctuation had attracted all my attention.[27](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note27) It was thus only natural that I resumed this work in order to propose an extension of the case of far-from-equilibrium chemical reactions.  This subject I proposed to G. Nicolis and A. Babloyantz. We expected to find for stationary states a Poisson distribution similar to the one predicted for equilibrium fluctuations by the celebrated [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/index.html) relations. Nicolis and Babloyantz developed a detailed analysis of linear chemical reactions and were able to confirm this prediction.[28](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note28) They added some qualitative remarks which suggested the validity of such results for any chemical reaction.  Considering again the computations for the example of a non-linear biomolecular reaction, I noticed that this extension was not valid. A further analysis, where G. Nicolis played a key role, showed that an unexpected phenomenon appeared while one considered the fluctuation problem in nonlinear systems far from equilibrium: the distribution law of fluctuations depends on their scale, and only “small fluctuations” follow the law proposed by Einstein.[29](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note29) After a prudent reception, this result is now widely accepted, and the theory of non-equilibrium fluctuations is fully developing now, so as to allow us to expect important results in the following years. What is already clear today is that a domain such as chemical kinetics, which was considered conceptually closed, must be thoroughly rethought, and that a brand-new discipline, dealing with non-equilibrium phase transitions, is now appearing.[30](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note30), [31](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note31), [32](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note32)  Progress in irreversible phenomena theory leads us also to reconsideration of their insertion into classical and quantum dynamics. Let us take a new look at the statistical mechanics of some years ago. From the very beginning of my research, I had had occasion to use conventional methods of statistical mechanics for equilibrium situations. Such methods are very useful for the study of thermodynamical properties of polymer solutions or isotopes. Here we deal mostly with simple computational problems, as the conceptual tools of equilibrium statistical mechanics have been well established since the work of Gibbs and Einstein. My interest in non-equilibrium would by necessity lead me to the problem of the foundations of statistical mechanics, and especially to the microscopic interpretation of irreversibility.[33](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note33)  Since the time of my first graduation in science, I was an enthusiastic reader of Boltzmann, whose dynamical vision of physical becoming was for me a model of intuition and penetration. Nonetheless, I could not but notice some unsatisfying aspects. It was clear that Boltzmann introduced hypotheses foreign to dynamics; under such assumptions, to talk about a dynamical justification of thermodynamics seemed to me an excessive conclusion, to say the least. In my opinion, the identification of entropy with molecular disorder could contain only one part of the truth if, as I persisted in thinking, irreversible processes were endowed with this constructive role I never cease to attribute to them. For another part, the applications of Boltzmann’s methods were restricted to diluted gases, while I was most interested in condensed systems.  At the end of the forties, great interest was aroused in the generalization of kinetic theory to dense media. After the pioneering work by Yvon[34](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note34), publications of Kirkwodd[35](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note35), [Born](https://www.nobelprize.org/nobel_prizes/physics/laureates/1954/index.html) and Green[36](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note36), and of Bogoliubov[37](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note37) attracted a lot of attention to this problem, which was to lead to the birth of non-equilibrium statistical mechanics. As I could not remain alien to this movement, I proposed to G. Klein, a disciple of Fürth who came to work with me, to try the application of Born and Green’s method to a concrete, simple example, in which the equilibrium approach did not lead to an exact solution. This was our first tentative step in non-equilibrium statistical mechanics.[38](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note38) It was eventually a failure, with the conclusion that Born and Green’s formalism did not lead to a satisfying extension of Boltzmann’s method to dense systems.  But this failure was not a total one, as it led me, during a later work, to a first question: Was it possible to develop an “exact” dynamical theory of irreversible phenomena? Everybody knows that according to the classical point of view, irreversibility results from supplementary approximations to fundamental laws of elementary phenomena, which are strictly reversible. These supplementary approximations allowed Boltzmann to shift from a dynamical, reversible description to a probabilistic one, in order to establish his celebrated H theorem.  We still encountered this negative attitude of “passivity” imputed to irreversible phenomena, an attitude that I could not share. If – as I was prepared to think – irreversible phenomena actually play an active, constructive role, their study could not be reduced to a description in terms of supplementary approximations. Moreover, my opinion was that in a good theory a viscosity coefficient would present as much physical meaning as a specific heat, and the mean life duration of a particle as much as its mass.  I felt confirmed in this attitude by the remarkable publications of Chandrasekhar and von Neumann, which were also issued during the forties.[39](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note39) That was why, still with the help of G. Klein, I decided to take a fresh look at an example already studied by [Schrödinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/index.html), [40](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note40) related to the description of a system of harmonic oscillators. We were surprised to see that, for all such a simple model allowed us to conclude, this class of systems tend to equilibrium. But how to generalize this result to non-linear dynamical systems?  Here the truly historic performance of Léon van Hove opened for us the way (1955).[41](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note41) I remember, with a pleasure that is always new, the time – which was too short – during which van Hove worked with our group. Some of his works had a lasting effect on the whole development of statistical physics; I mean not only his study of the deduction of a “master equation ” for anharmonic systems, but also his fundamental contribution on phase transitions, which was to lead to the branch of statistical mechanics that deals with so-called “exact” results.[42](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note42)  This first study by van Hove was restricted to weakly coupled anharmonic systems. But, anyway, the path was open, and with some of my colleagues and collaborators, mainly R. Balescu, R. Brout, F. Hénin and P. Résibois, we achieved a formulation of non-equilibrium statistical mechanics from a purely dynamical point of view, without any probabilistic assumption. The method we used is summed up in my 1962 book.[43](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#43) It leads to a “dynamics of correlations”, as the relation between interaction and correlation constitutes the essential component of the description. Since then, these methods have led to numerous applications. Without giving more detail, here, I will restrict myself to mentioning two recent books, one by R. Balescu,[44](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note44) the other by P. Résibois and M. De Leener.[45](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#45)  This concluded the first step of my research in non-equilibrium statistical mechanics. The second is characterized by a very strong analogy with the approach of irreversible phenomena which led us from linear thermodynamics to non-linear thermodynamics. In this tentative step also, I was prompted by a feeling of dissatisfaction, as the relation with thermodynamics was not established by our work in statistical mechanics, nor by any other method. The theorem of Boltzmann was still as isolated as ever, and the question of the nature of dynamics systems to which thermodynamics applies was still without answer.  The problem was by far more wide and more complex than the rather technical considerations that we had reached. It touched the very nature of dynamical systems, and the limits of Hamiltonian description. I would never have dared approach such a subject if I had not been stimulated by discussions with some highly competent friends such as the late Léon Rosenfeld from Copenhagen, or G. Wentzel from Chicago. Rosenfeld did more than give me advice; he was directly involved in the progressive elaboration of the concepts we had to explore if we were to build a new interpretation of irreversibility. More than any other stage of my scientific career, this one was the result of a collective effort. I could not possibly have succeeded had it not been for the help of my colleagues M. de Haan, Cl. George, A. Grecos, F. Henin, F. Mayné, W. Schieve and M. Theodosopulu. If irreversibility does not result from supplementary approximations, it can only be formulated in a theory of transformations which expresses in “explicit” terms what the usual formulation of dynamics does “hide”. In this perspective, the kinetic equation of Boltzmann corresponds to a formulation of dynamics in a new representation.[46](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note46), [47](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note47), [48](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note48), [49](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#49)  In conclusion: dynamics and thermodynamics become two complementary descriptions of nature, bound by a new theory of non-unitary transformation. I came so to my present concerns; and, thus, it is time to end this intellectual autobiography. As we started from specific problems, such as the thermodynamic signification of non-equilibrium stationary states, or of transport phenomena in dense systems, we have been faced, almost against our will, with problems of great generality and complexity, which call for reconsideration of the relation of physico-chemical structures to biological ones, while they express the limits of Hamiltonian description in physics.  Indeed, all these problems have a common element: time. Maybe the orientation of my work came from the conflict which arose from my humanist vocation as an adolescent and from the scientific orientation I chose for my university training. Almost by instinct, I turned myself later towards problems of increasing complexity, perhaps in the belief that I could find there a junction in physical science on one hand, and in biology and human science on the other.  In addition, the research conducted with my friend R. Herman on the theory of car traffic[50](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note50) gave me confirmation of the supposition that even human behaviour, with all its complexity, would eventually be susceptible of a mathematical formulation. In this way the dichotomy of the “two cultures” could and should be removed. There would correspond to the breakthrough of biologists and anthropologists towards the molecular description or the “elementary structures”, if we are to use the formulation by Lévi-Strauss, a complementary move by the physico-chemist towards complexity. Time and complexity are concepts that present intrinsic mutual relations.  During his inaugural lecture, De Donder spoke in these terms:[51](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note51) “Mathematical physics represents the purest image that the view of nature may generate in the human mind; this image presents all the character of the product of art; it begets some unity, it is true and has the quality of sublimity; this image is to physical nature what music is to the thousand noises of which the air is full…”  Filtrate music out of noise; the unity of the spiritual history of humanity, as was stressed by M. Eliade, is a recent discovery that has still to be assimilated.[52](https://www.nobelprize.org/prizes/chemistry/1977/prigogine/biographical/#note52) The search for what is meaningful and true by opposition to noise is a tentative step that appears to be intrinsically related to the coming into consciousness of man facing a nature of which he is a part and which it leaves.  I have many times advocated the necessary dialogue in scientific activity, and thus the vital importance of my colleagues and collaborators in the journey that I have tried to describe. I would also stress the continuing support that I received from institutions which have made this work a feasible one, especially the Université Libre de Bruxelles and the University of Texas at Austin. For all of the development of these ideas, the International Institute of Physics and Chemistry founded by E. Solvay (Brussels, Belgium) and the Welch Foundation (Houston, Texas) have provided me with continued support.  The work of a theoretician is related in a direct way to his whole life. It takes, I believe, some amount of internal peace to find a path among all successive bifurcations. This peace I owe to my wife, Marina. I know the frailty of the present, but today, considering the future, I feel myself to be a happy man.   |  | | --- | | References | | 1. G. Poulet, *Etudes sur le temps humain*, Tone 4, Edition 10/18, Paris, 1949. | | 2. See the note on De Donder in the Florilège (pedant le XIXe siècle et le début du XXe), Acad. Roy. Belg., Bull. Cl. Sc., page 169, 1968. | | 3. Th. De Donder (Rédaction nouvelle par P. Van Rysselberghe), Paris, Gauthier- Villars, 1936.  See also:  I. Prigogine and R. Defay: Thermodynamique Chimique conformément aux méthodes de Gibbs et De Donder (2 Tomes), Liège, Desoer, 1944-1946.  Or the translation in English:  Chemical Thermodynamics, translated by D.H. Everett, Langmans 1954, 1962. | | 4. See Colloque de Thermodynamique, Union Intern. de Physique pure et appliquée (I.U.P.A.P.), 1948. | | 5. Bolzmann, L., Wien, Ber. *66*, 2275, 1872. | | 6. Planck, M., Vorlesaungen über Thermodynamik, Walter de Gruyter, Berlin, Leipzig, 1930. | | 7. Timmermans, J., Les Solutions Concentrées, Masson et Cie, Paris, 1936.  Let us also quote his thesis on experimental research on demixtion in liquid mixtures | | 8. Prigogine, I., The Molecular Theory of Solutions, avec A. Bellemans et V. Mathot; North-Holland Publ. Company, Amsterdam, 1957.  See also: Prigogine and Defay, Ref. 3. | | 9. Let us quote some remarkable works of this School:  Barchet, A., La Vie créatrice des formes, Alcan, Paris, 1927.  Dalcq, A., L’Oeuf et son dynamisme organisateur, Alban Michel. Paris, 1941.  Barchet, J., Embryologie Chimique, Desoer, Liège et Masson, Paris, 1946.  I was also much interested in the beautiful book by Marcel Florkin: L’Evolution biochimique, Desoer, Liège, 1944. | | 10. Prigogine, I., Acad. Roy. Belg. Bull. Cl. Sc. *31*, 600, 1945.  – Etude thermodynamique des phénomènes irréversibles. Thèse d’agrégation présentée en 1945 à l’Université Libre de Bruxelles.  Desoer, Liège, 1947.  – Introduction à la Thermodynamique des processus irréversibles, traduit de l’anglais par J. Chanu, Dunod, Paris, 1968. | | 11. Prigogine, I., and Wiame, J.M., Experientia, *2*, 451, 1946. | | 12. Nicolis, G. and Prigogine, I., Self Organization in Non-Equilibrium Systems (Chaps. III and IV), J. Wiley and Sons, New York, 1977. | | 13. Onsager, L. , Phys. Rev., *37*, 405, 1931. | | 14. Meixner, J., Ann. Physik, (5), *35*, 701, 1939; *36*, 103, 1939; *39*, 333, 1941; *40*, 165, 1941; Zeitsch Phys. Chim. B *53*, 235, 1943. | | 15. de Groot, S.R. and Mazur, P., Non-Equilibrium Thermodynamics, North-Holland, Amsterdam, 1962. | | 16. Katchalsky, A. and Curran, P.F., Non-Equilibrium Thermodynamics in Biophisics, Harvard Univ. Press, Cambridge, Mass., 1946. | | 17. Prigogine, I., Structure, Dissipation and Life.  Theoretical Physics and Biology, Versailles, 1967.  North-Holland Publ. Company, Amsterdam, 1969.  It is in this communication that the term “structure dissipative” is used for the first time. | | 18. Glansdorff, P. and Prigogine, I., Structure, Stabilité et Fluctuations, Masson, Paris, 1971.  – Thermodynamic Theory of Structure Stability and Fluctuations, Wiley and Sons, London, 1971.  – Traduction en langue russe: Mir, Moscou, 1973.  – Traduction en langue japonaise; Misuzu Shobo, 1977.  This book presents in detail the original work by the two authors, which led to the concept of dissipative structure. For a brief historical account, see also:  Acad. Roy. Belg., Bull. des Cl. Sc., LIX, *80*, 1973. | | 19. Schechter, R.S., The Variational Method in Engineering, McGraw-Hill, New York, 1967. | | 20. Tyson, J., Journ. of Chem. Physics, *58*, 3919, 1973. | | 21. Turing, A., Phil. Trans. Roy. Soc. London, Ser B, *237*, 37, 1952. | | 22. Belusov, B.P., Sb. Ref. Radiat. Med. Moscow, 1958.  Zhabotinsky, A.P., Biofizika, *9*, 306, 1964.  Acad. Sc. U.R.S.S. Moscow (Nauka), 1967. | | 23. Noyes, R.M. et al., Ann. Rev. Phys. Chem. *25*, 95, 1974. | | 24. Chance, B., Schonener, B. and Elsaesser, S., Proc. Nat. Acad. Sci. U.S.A. *52*, 337-341, 1964. | | 25. Hess, B., Ann. Rev. Biochem. *40*, 237, 1971. | | 26. Eigen, M., Naturwissenschaften, *58*, 465, 1971. | | 27. Prigogine, I. and Mayer, G., Acad. Roy. Belg. Bull. Cl. Sc., *41*, 22, 1955 | | 28. Nicolis, G. and Babloyantz, A., Journ. Chem. Phys., *51*, 6, 2632, 1969. | | 29. Nicolis, G. and Prigogine, I., Proc. Nat. Acad. Sci. U.S.A., *68*, 2102, 1971. | | 30. Prigogine, I., Proc. 3rd Symp. Temperature, Washington D.C., 1954.  Prigogine, I. and Nicolis, G., Proc. 3rd. Intern. Conference: From Theoretical Physics to Biology, Versailles, France, 1971. | | 31. Nicolis, G. and Turner, J.W., Proc. of the Conference on Bifurcation Theory, New York, 1977. To Appear. | | 32. Prigogine, I. and Nicolis, G., Non-Equilibrium Phase Transitions and Chemical Reactions, Scientific American. To Appear. | | 33. Prigogine, I., Non-Equilibrium Stastistical Mechanics, Interscience Publ., New York, London, 1962-1966. (For a brief history and original references.) | | 34. Yvon, J., Les Corrélations et l’Entropie en Mécanique Statistique Classique. Dunod, Paris, 1965. | | 35. Kirkwood. J.G., Journ, Chem. Physics, *14*, 180, 1946. | | 36. Born, M. and Green, H.S., Proc, Roy, Soc. London, A *188*, 10, 1946 and A *190*, 45, 1947. | | 37. Bogoliubov, N. N., Jour. Phys. U.S.S.R. *10*, 257, 265, 1949. | | 38. Klein, G. and Prigogine, I., Physica XIX 74-88; 88-100; 1053-1071, 1953. | | 39. Chandrasekhar, S., Stocastic Problems in Physics and Astronomy; Rev. of Mod. Physics, 15, no 1, 1943. | | 40. Shrödinger, E., Ann. der Physik, *44*, 916, 1914. | | 41. Van Hove, L., Physica, *21*, 512 (1955). | | 42. Van Hove, L., Physica, *16*, 137 (1950). | | 43. Prigogine, I., cf. Ref. 33. | | 44. Balascu, R., Equilibrium and Non-Equilibrium Statistical Mechanics, Wiley, Interscience, 1957. | | 45. Résibois, P. and De Leener, M., Classical Kinetic Theory of Fluids, Wiley, Interscience, New York, 1977. | | 46. Prigogine, I., George, C., Henin, F. and Rosenfeld, L., Chemica Scripta, *4*, 5-32, 1973. | | 47. Prigogine, I., George, C., Henin, F., Physica, *45*, 418-434, 1969 | | 48. Prigogine, I. and Grecos, A.P., The Dynamical Theory of Irreversible Processes, Proc. Intern. Conf. on Frontiers of Theor. Phys., New Delhi, 1976.  Kinetic Theory and Ergodic Properties in Quantum Mechanics, Abhandlungen der Akad. der Wiss., der D.D.R. Nr 7 n Berlin, Jahrgang 1977. | | 49. Grecos, A.P. and Prigogine, I., Thirteenth IUPAP Conference on Statisyical Physics, Haifa, August 1977. | | 50. Prigogine, I. and Herman, R., Kinetic Theory of Vehicular traffic, Elsevier, 1971. | | 51. For the reference, see note 2. | | 52. Mircéa Eliade, Historie des croyances et fies idées religieuseu Vol. I., p. 10, Payot, Paris, 1976. | |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0407 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0408** |
| **Biographical** | Although born in Cleveland, Ohio, USA, on December 9, 1919, I moved to Kentucky in 1920, and lived in Lexington through my university years. After my bachelors degree at the University of Kentucky, I entered graduate school at the California Institute of Technology in 1941, at first in physics. Under the influence of [Linus Pauling](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1954/index.html), I returned to chemistry in early 1942. From then until the end of 1945 I was involved in research and development related to the war. After completion of the Ph.D., I joined the faculty of the University of Minnesota in 1946, and moved to Harvard University in 1959. Harvard’s recognitions include the Abbott and James Lawrence Professorship in 1971, and the George Ledlie Prize in 1971.  The early research in borane chemistry is best summarized in my book “Boron Hydrides” (W.A. Benjamin, Inc., 1963), although most of this and late work is in several scientific journals. Since about 1960, my research interests have also been concerned with the relationship between three-dimensional structures of enzymes and how they catalyze reactions or how they are regulated by allosteric transformations.  Besides memberships in various scientific societies, I have received the Bausch and Lomb honorary science award in 1937; and, from the American Chemical Society, the Award for Distinguished Service in the Advancement of Inorganic Chemistry, and the Peter Debye Award in Physical Chemistry. Local sections of this Society have given the Harrison Howe Award and Remsen Award. The University of Kentucky presented to me the Sullivan Medallion in 1941, the Distinguished Alumni Centennial Award in 1965, and an honorary Doctor of Science degree in 1963. A Doctor Honoris Causa was awarded by the University of Munich in 1976. I am a member of the National Academy of Sciences U.S.A. and of the American Academy of Arts and Sciences, and a foreign member of the Royal Netherlands Academy of Sciences and Letters.  My other activities include tennis and classical chamber music as a performing clarinetist.  *William Lipscomb died on April 14, 2011.* |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0408 |
| **Interview** |  |
| Q4 | **Were you of the same opinion in ’64 that the field is finished now and I have to move?** |
|  | Absolutely not. Absolutely not. The field has grown enormously. The field of structure and bonding, and how atoms are held together to make chemical compounds has received a great deal of work and understanding and extended particularly towards how the molecules react, how they transform to other species, and even in recent years there has been a great transformation of computational chemistry and theoretical chemistry which allows one to predict structures, to guess reactions, almost predict reactions, so I think that will continue to improve. |
| Q4 | **What does it mean for our understanding of nature, that you can understand how material is structured on this level?** |
|  | The problem is how do molecules react, because if you want to transform a molecule into something useful or something that you’re interested in, it helps a lot to have the structure because you can tell where the reactivity parts are, where you can change the molecule most easily, especially if you know the structure and then if you know how the charges on the atoms change. You can then make predictions about the reactions and do the transformations that previously were only trial and error. Now you can, with a purpose. That means you can explore much more complex systems, much more complex reactions. |
| Q17 | **How do you get this kind of self-confidence that you can believe that my interpretation and my idea is right?** |
|  | Well, in order to make one of these kind of discoveries, you have to have read a lot, you have to know a lot and you have to suddenly, or maybe not suddenly, but gradually see something that’s different, that is implied or something that’s wrong, as I did in the case of the hydrides, I perceived it was wrong, but you then have to criticise it yourself before you test it or present it and as your confidence grows – it may take a long time, it may take 10 minutes, it may take years to develop your own confidence in these things – and sometimes you are wrong.  What is bad is to publish something that’s not very interesting …  It’s very important, that I learned from [Linus Pauling](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1954/pauling-facts.html), that it’s not a disgrace in science to publish something that’s wrong. What is bad is to publish something that’s not very interesting and so I learned from him that you really, he said once to me: It’s not what you can look up somewhere that counts, it’s what you really know that counts and if you know that, then that’s the basis for your discovery, and you won’t be shaken out of it if you have developed a certain amount of confidence and if you make some discoveries, then you can go on to make others and that is a criterion that we really believe that people who are creative remain creative. Now sometimes they don’t change areas, sometimes they are refining what they did earlier, but they’re still being creative. |
| Q3 | **If we go back in time to the start of your career, what made you enter science in general?** |
|  | When I was 11 years old, my mother bought me one of those chemistry sets and I stayed with it. My father was a doctor. I went to the drug store to buy chemicals to enlarge the set, to get other chemicals, and I had some very dangerous ones that you cannot buy now because of the regulations and I did all my experiments at home. When I came to high school two things happened. One is the instructor said: Yes you can take chemistry but you already know enough. All you have to do is show up for the final examination, you don’t have to do anything else.  So I did some research and the second aspect is that when I left the high school, I graduated, I gave my chemistry set to my high school. It more than doubled what they had already, as I had a pretty big set, so I was very interested in chemistry from very early days. And I tried to imagine how the molecules were really behaving in terms of structure and so on. |
| Q1 | **What do you think of students, that they’re coming to science now, to the university? What would you advise them to choose?** |
|  | Oh, they should choose to study something they’re really interested in and then learn to do it well. That’s exactly what I encourage in one; somebody is working for me and says I want to work on something else, I say you do it. Whether it’s a different field, completely different field, or whether it’s a different development and for a rather small number of my very best students, I have allowed them or encouraged them to find their own research problems and take them with them themselves to their new positions, to get a jump on the tenure situation.  Oh I see, so the motivation is coming from inside.  William Lipscomb: Exactly. I try to have them ask what their motivation is, what they’re really interested in, what they would like to be doing some years from now and encourage them to do it. |
| Q10 | **At the same time, science has become a more and more collective enterprise but what you’re pointing out is that it depends on how creative you are as an individual?** |
|  | Yes, that’s right because although you have collective, you have in my area, for example, doing protein structure work which is what I’m doing now to determine the structures of proteins and to try to figure out how enzymes speed up reactions by such large factors, that’s one of the things. Or how enzymes are regulated so they don’t run away with the metabolism on a molecular basis, that’s what I’m trying to do but there, I think, you need to try to imagine what is going on and you also need a fairly large group of people working to do all the things that need to be done.  It reaches its zenith in the case of the particle accelerators, the physicists who need very large amounts, but there’s some group of people who have to, or person who has to lead, who has to set the agenda to say what particles you’re going to work on, what enzymes are you going to study. It’s a very important choice and so I think that the role of individual thought is also present in these large group enterprises that you refer to.  This is the role of the leader.  William Lipscomb: The role of the leader, or the leader could be someone who is not the real leader, not the one who gets the money, but that may be the intellectual leader, maybe one of the students. It can be and it has happened. |
| Q10 | **So you can compare the leader of a research group to a conductor in an orchestra?** |
|  | Oh yes. Yes, that’s an interesting comparison. A lot of people think the orchestra is playing and the conductor doesn’t do very much but the conductor’s the person that gives shape to the music, gets the phrasing, and if he has really fine musicians in solo spots, the question is does he try to help them phrase, or does he let them go? He just decides that on the spot, depending on the musicians, sometimes he’s in control, sometimes he lets them be in control and there are interesting times because there’s a big difference among conductors. Some are very easy to understand and play under and some are very difficult and it’s the same for research groups, I think. |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0409** |
| **Biographical** | I was born on 7 September 1917 at Sydney in Australia. My father was English-born and a graduate of Oxford; my mother, born Hilda Eipper, was descended from a German minister of religion who settled in New South Wales in 1832. I was the second of four children.  Part of my childhood was spent in Sydney and part in rural New South Wales, at Armidale. When I was about ten years old the first signs of deafness (from otosclerosis) became noticeable. The total loss of hearing was a process that lasted more than a decade, but it was sufficiently gradual for me to attend Sydney Boys’ High School and to profit from the teaching there. In particular a good young teacher, Leonard Basser, influenced me in the direction of chemistry; and this seemed to offer a career where deafness might not be an insuperable handicap.  I entered Sydney University at the age of 16, and though by that time unable to hear any lecture I was attracted by laboratory work in organic chemistry (which I had done in an improvised laboratory at home since the age of 14) and by the availability of the original chemical literature. In 1937 I graduated with first-class honours and a University medal. After a year of post-graduate research I won an 1851 Exhibition scholarship to work at Oxford with [Robert Robinson](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1947/index.html). Two such scholarships were awarded each year, and the other was won by Rita Harradence, also of Sydney and also an organic chemist. This began an association which continues to this day. We were married in 1941, and have three children and two grandchildren.  War broke out as we journeyed to Oxford and after completing our work (on steroid synthesis) for doctorates we became part of the chemical effort on penicillin which was the major chemical project in Robinson’s laboratory during the war. We made contributions, and I helped to write *The Chemistry of Penicillin* (Princeton University Press, 1949), the record of a great international effort. However, I had earlier discovered what was to prove a key reaction for the synthesis of the sterols; and after the war I returned to this pursuit. The collaboration with Robinson continued after I joined (1946) the scientific staff of the Medical Research Council and worked at its National Institute, first at Hampstead and then at Mill Hill. In the end (1951) we were able to complete, simultaneously with [Woodward](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1965/index.html), the first total synthesis of the non-aromatic steroids.  At the National Institute for Medical Research I came into contact with biological scientists and formed collaborative projects with several of them. In particular George Popják and I shared an interest in cholesterol. At this time [Konrad Bloch](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1964/index.html) was beginning his work on the biosynthesis of the sterols and Popják and I began to concert experiments in which the disciplines of chemistry and biochemistry could be applied to this subject. We were led to devise a complete carbon-by-carbon degradation of the nineteen-carbon ring structure of cholesterol and to identify, by means of radioactive tracers, the arrangement of the acetic acid molecules from which the system is built. As knowledge of the intermediate stages became more complete our experiments could be planned to give more and more information.  In 1962 Popják and I left the service of the Medical Research Council and became co-directors of the Milstead Laboratory of Chemical Enzymology set up by Shell Research Ltd. Lord Rothschild was influential in the decision to establish this laboratory and I was his subordinate until he left Shell in 1970. At Milstead a project already conceived – the study of the stereochemistry of enzymic reactions by means of asymmetry artificially introduced by isotopic substitution – was developed. It continued after 1968, when Popják left Milstead to go to the University of California at Los Angeles, until 1975, when I left to take up my present position of Royal Society Research Professor at the University of Sussex. In 1967 I had formed a collaboration with Hermann Eggerer, then of München; and together we solved the problem of the “asymmetric methyl group”, and applied the solution in some of the many ways that have proved possible.  My work has received ample recognition as it progressed: I was elected to the Royal Society in 1953; the Chemical Society has awarded me its Corday Morgan medal (1953), Flintoff medal (1965), and Pedler (1968) and Robert Robinson (1971) lectureships; the American Chemical Society gave me its Ernest Guenther award (1968); and I received the Prix Roussel in 1972. Popják and I were jointly awarded the Biochemical Society’s Ciba medal (1965); the Stouffer prize (1967); and the Royal society’s Davy Medal (1968).  Throughout my scientific career my wife has been my most constant collaborator. Her experimental skill made major contributions to the work; she has eased for me beyond measure the difficulties of communication that accompany deafness; her encouragement and fortitude have been my strongest supports. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0409 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0410** |
| **Biographical** | I was born on July 23rd, 1906 in Sarajevo in the province of Bosnia, which then belonged to the Austrian-Hungarian Monarchy and later, in 1918, became part of Yugoslavia. In the western world my birthplace has a somewhat sinister reputation that was characterized by an older tax-inspector in the Midwest of America as “the place where all that mess started”. Actually, as an 8 years old boy I stood near to the spot where Archiduke Franz Ferdinand and his wife were assassinated. At the beginning of the first World War, in 1915, we moved to Zagreb, the capital of Croatia, where I attended the gymnasium. The period 1924 to 1929 was spent studying Chemistry at the Czech Institute of Technology in Prague, Czechoslovakia. The supervisor of my thesis was Professor Emil Votocek, one of the prominent founders of chemical research in Czechoslovakia. My mentor, however, was Rudolf Lukes, then lecturer and later successor of Votocek to the chair of organic chemistry. To Lukes I owe the greatest part of my early scientific education, and he remained my close friend until his premature death in 1960. In addition to these two “real” teachers I admired [Robert Robinson](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1947/index.html), Christopher Ingold and [Leopold Ruzicka](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1939/index.html), all of whom I considered as my “imaginary” teachers. In later years I was fortunate to become well acquainted with all three of these great chemists.  The close of my studies with a degree of a Dr. Ing. in 1929 coincided with the great economic crisis, and I was not able to find an academic position. I was therefore very grateful for a position in the newly created laboratory of G.J. Dríza in Prague where rare chemicals were produced on small scale. I had there also a modest opportunity to do some research, but I badly wanted to work in an academic environment. This is why I was so eager to accept the position of a lecturer at the University of Zagreb in 1935. I did not know that I had to fulfil there all the duties of a full professor and to live on a salary of an underpaid assistant, but it would probably not have affected my decision if I had known. With the help of a couple of enthusiastic young co-workers and of a developing small pharmaceutical factory, I had just managed to solve at least the most urgent problems for myself and my laboratory when the second World War broke out. After the German occupation of Zagreb in 1941 it became clear that I was likely to get into serious trouble if I remained there. At this critical point I received an invitation of [Richard Kuhn](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1938/index.html) to give some lectures in Germany, and shortly afterwards Leopold Ruzicka, whom I had asked for help, invited me to visit him on the way. With these two invitations, it was possible for me to escape with my wife to Switzerland. Through Ruzicka I soon obtained generous support of CIBA Ltd. and started work in the Organic Chemistry Laboratory at the Federal Institute of Technology (ETH) in Zurich. The cooperation with Ruzicka lasted many years and enabled me to make my slow progress up the academic hierarchical ladder. Starting as assistant, I became “Privat Dozent”, “Titularprofessor” associate (ausserordentlicher) professor and in 1952 full professor ad personam. Finally, in 1957, I succeeded Ruzicka as head of the Laboratory, a height that I never dreamt of when I was a student in Prague. In becoming director of the Laboratory I reached, according to Peter’s principle, the level of my incompetence and I tried hard for several years to step down. Surrounded and supported by a group of very able young colleagues, I finally succeeded in introducing a rotating chairmanship from which I was exempted. So far this has worked very satisfactorily and it may have helped some of my colleagues to resist tempting offers from other Universities.  My main interests were natural compounds, from adamantane and aialoids to rifamycins and boromycin. During the work on natural compounds stereochemical problems emerged from all sides. As E.L. Eliel pointed out, stereochemistry is not so much a branch of chemistry but rather a way of looking at chemistry. It was, and still is, great fun trying to find new points of view for it.  I travel a lot. Recently I counted that I have given lectures in more than 150 places, often several times. This in spite of the fact that I do not speak any language properly. I suspect that many people come to my lectures because they enjoy my strange accent and skill in managing without actually cheating.  I married my wife Kamila in Prague in 1933. A son Jan was born to us in Zürich in 1949.  For many years, when still a Yugoslav citizen, I was already a Swiss patriot and in 1959 I obtained Swiss citizenship. However, I consider myself a world citizen and I am very grateful to my adopted country that it allows me to be one.  The way from Sarajevo to Stockholm is a long one and I am fully aware that I have been very lucky to arrive there. The journey could not have been made without the generous help of friends, colleagues, co-workers and also of innumerable earlier chemists “on whose shoulders we stand”. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0410 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0411** |
| **Biographical** | I was born on 19 June, 1910, in Sterling, Illinois, of Huguenot-German parentage, mine being the sixth generation native to America. My father was Ezra Flory, a clergyman-educator; my mother, nee Martha Brumbaugh, had been a schoolteacher. Both were descended from generations of farmers in the New World. They were the first of their families of record to have attended college.  My interest in science, and in chemistry in particular, was kindled by a remarkable teacher, Carl W. Holl, Professor of Chemistry at Manchester College, a liberal arts college in Indiana, where I graduated in 1931. With his encouragement, I entered the Graduate School of The Ohio State University where my interests turned to physical chemistry. Research for my dissertation was in the field of photochemistry and spectroscopy. It was carried out under the guidance of the late Professor Herrick L. Johnston whose boundless zeal for scientific research made a lasting impression on his students.  Upon completion of my Ph.D. in 1934, I joined the Central Research Department of the DuPont Company. There it was my good fortune to be assigned to the small group headed by Dr. Wallace H. Carothers, inventor of nylon and neoprene, and a scientist of extraordinary breadth and originality. It was through the association with him that I first became interested in exploration of the fundamentals of polymerization and polymeric substances. His conviction that polymers are valid objects of scientific inquiry proved contagious. The time was propitious, for the hypothesis that polymers are in fact covalently linked macromolecules had been established by the works of Staudinger and of Carothers only a few years earlier.  A year after the untimely death of Carothers, in 1937, I joined the Basic Science Research Laboratory of the University of Cincinnati for a period of two years. With the outbreak of World War II and the urgency of research and development on synthetic rubber, supply of which was imperiled, I returned to industry, first at the Esso (now Exxon) Laboratories of the Standard Oil Development Company (1940-43) and later at the Research Laboratory of the Goodyear Tire and Rubber Company (1943-48). Provision of opportunities for continuation of basic research by these two industrial laboratories to the limit that the severe pressures of the times would allow, and their liberal policies on publication, permitted continuation of the beginnings of a scientific career which might otherwise have been stifled by the exigencies of those difficult years.  In the Spring of 1948 it was my privilege to hold the George Fisher Baker Non-Resident Lectureship in Chemistry at Cornell University. The invitation on behalf of the Department of Chemistry had been tendered by the late Professor [Peter J.W. Debye](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1936/index.html), then Chairman of that Department. The experience of this lectureship and the stimulating associations with the Cornell faculty led me to accept, without hesitation, their offer of a professorship commencing in the Autumn of 1948. There followed a most productive and satisfying period of research and teaching “Principles of Polymer Chemistry,” published by the Cornell University Press in 1953, was an outgrowth of the Baker Lectures.  It was during the Baker Lectureship that I perceived a way to treat the effect of excluded volume on the configuration of polymer chains. I had long suspected that the effect would be non-asymptotic with the length of the chain; that is, that the perturbation of the configuration by the exclusion of one segment of the chain from the space occupied by another would increase without limit as the chain is lengthened. The treatment of the effect by resort to a relatively simple “smoothed density” model confirmed this expectation and provided an expression relating the perturbation of the configuration to the chain length and the effective volume of a chain segment. It became apparent that the physical properties of dilute solutions of macromolecules could not be properly treated and comprehended without taking account of the perturbation of the macromolecule by these intramolecular interactions. The hydrodynamic theories of dilute polymer solutions developed a year or two earlier by Kirkwood and by Debye were therefore reinterpreted in light of the excluded volume effect. Agreement with a broad range of experimental information on viscosities, diffusion coefficients and sedimentation velocities was demonstrated soon thereafter.  Out of these developments came the formulation of the hydrodynamic constant called theta, and the recognition of the Theta point at which excluded volume interactions are neutralized. Criteria for experimental identification of the Theta point are easily applied. Ideal behavior of polymers, natural and synthetic, under Theta conditions has subsequently received abundant confirmation in many laboratories. These findings are most gratifying. More importantly, they provide the essential basis for rational interpretation of physical measurements on dilute polymer solutions, and hence for the quantitative characterization of macromolecules.  In 1957 my family and I moved to Pittsburgh where I undertook to establish a broad program of basic research at the Mellon Institute. The opportunity to achieve this objective having been subsequently withdrawn, I accepted a professorship in the Department of Chemistry at Stanford University in 1961. In 1966, I was appointed to the J.G. Jackson – C.J. Wood Professorship in Chemistry at Stanford.  The change in situation upon moving to Stanford afforded the opportunity to recast my research efforts in new directions. Two areas have dominated the interests of my co-workers and myself since 1961. The one concerns the spatial configuration of chain molecules and the treatment of their configuration-dependent properties by rigorous mathematical methods; the other constitutes a new approach to an old subject, namely, the thermodynamics of solutions.  Our investigations in the former area have proceeded from foundations laid by Professor M.V. Volkenstein and his collaborators in the Soviet Union, and were supplemented by major contributions of the late Professor Kazuo Nagai in Japan. Theory and methods in their present state of development permit realistic, quantitative correlations of the properties of chain molecules with their chemical constitution and structure. They have been applied to a wide variety of macromolecules, both natural and synthetic, including polypeptides and polynucleotides in the former category. The success of these efforts has been due in no small measure to the outstanding students and research fellows who have collaborated with me at Stanford during the past thirteen years. A book entitled “Statistical Mechanics of Chain Molecules”, published in 1969, summarizes the development of the theory and its applications up to that date.  Mrs. Flory, the former Emily Catherine Tabor, and I were married in 1936. We have three children: Susan, wife of Professor George S. Springer of the Department of Mechanical Engineering at the University of Michigan; Melinda, wife of Professor Donald E. Groom of the Department of Physics at the University of Utah; and Dr. Paul John Flory, Jr., currently a post-doctoral Research Associate at the Medical Nobel Institute in Stockholm. We have four grandchildren: Elizabeth Springer, Mary Springer, Susanna Groom and Jeremy Groom. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0411 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0412** |
| **Biographical** | I was born in Solln, near Munich, on 10 November 1918 as the third child of the Professor of Physics at the Technical College of Munich, Dr. Karl T. Fischer (died 1953), and his wife, Valentine, *née* Danzer (died 1935). After completing four years at elementary school I went on to grammar school in 1929, from which I graduated in 1937 with my *Abitur*. Following a subsequent period of “work service” and shortly before the end of my two years’ compulsory military service, the Second World War broke out. I served in Poland, France and Russia. In the winter of 1941/2 I began to study Chemistry at the Technical College in Munich during a period of study leave. I was released by the Americans in the autumn of 1945, and resumed my study of Chemistry in Munich after the reopening of the Technical College in 1946. I graduated in 1949. I took up a position as scientific assistant to Professor Walter Hieber in the Inorganic Chemistry Department, and under his guidance I dedicated myself to working on my doctoral thesis, “The Mechanisms of Carbon Monoxide Reactions of Nickel II Salts in the Presence of Dithionites and Sulfoxylates”. After receiving my doctorate in 1952, I was invited by Professor Hieber to continue my activities at the college and consequently chose to specialise in the study of transition metal and organo-metallic chemistry. I wrote my university teaching thesis on “The Metal Complexes of Cyclopentadienes and Indenes”. I was appointed a lecturer at the Technical College in 1955 and in 1956 I completed a scientific sojourn of many months in the United States. In 1957 I was appointed Professor at the University of Munich. After turning down an offer of the Chair of Inorganic Chemistry at the University of Jena I was appointed Senior Professor at the University of Munich in 1959 . In 1957 I was awarded the Chemistry Prize by the Göttingen Academy of Sciences. The Society of German Chemists awarded me the Alfred Stock Memorial Prize in 1959. In 1960 I refused an appointment as Senior Professor in the Department of Inorganic Chemistry at the University of Marburg. In 1964 I took the Chair of Inorganic Chemistry at the Technical College of Munich, which had been vacated by Professor Hieber. In the same year I was elected a member of the Mathematics/Natural Science section of the Bavarian Academy of Sciences; in 1969 I was appointed a member of the German Academy of Scientists Leopoldina. In 1972 I was given an honorary doctorate by the Faculty of Chemistry and Pharmacy of the University of Munich.  Lectures on my fields, particularly those on metallic complexes of cyclopentadienes and indenes, metal-p-complexes of six-ringed aromatics, mono-, di- and oligo-olefins and most recently metalcarbonyl carbene and carbyne complexes, led me on lecture tours of the United States, Australia, Venezuela, Brazil, Israel and Lebanon, as well as numerous European countries, including the former Soviet Union. In 1969 I was Firestone Lecturer at the University of Wisconsin, Madison,Wisconsin, USA; in 1971 Visiting Professor at the University of Florida, Gainesville, USA, as well as the first Inorganic Chemistry Pacific West Coast Lecturer. In the spring of 1973 I held lectures as the Arthur D. Little Visiting Professor at the Massachusetts Institute of Technology, Cambridge, Massachusetts, USA; and that was followed by a period when I was Visiting Distinguished Lecturer at the University of Rochester, Rochester, New York, USA. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0412 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0413** |
| **Biographical** | I was born in Springside, a village close to Todmorden in west Yorkshire on July 14th, 1921. The house where I was born and indeed most of the village has been demolished by the local council as being unfit for habitation. My father, and his father, also Geoffrey, were both master house painters and decorators, the latter, youngest of twelve children having migrated from Boroughbridge in Yorkshire about 1880. My mother’s family were originally of hill farming stock but many of my relations were weavers in the local cotton mills and indeed my mother went into the mill at an early age. My first introduction to chemistry came at a quite early age through my mother’s elder brother. A well known organist and choirmaster he had married into a family that owned a small chemical company making Epsom and Glauber’s salt for the pharmaceutical industry. I used to play around in their small laboratory as well as go with my uncle on visits to various chemical companies.  The oldest of three children, I was educated in the local council primary school and after winning a County Scholarship in 1932, went to Todmorden Secondary School. This small school has had an unusual record of scholarly achievement, including two Nobel Laureates within 25 years. I actually had the same Physics teacher as [Sir John Cockcroft](https://www.nobelprize.org/nobel_prizes/physics/laureates/1951/index.html), but physics was never my favourite subject.  In 1939 I obtained a Royal Scholarship for study at the Imperial College of Science and Technology where I graduated in 1941. As it was wartime, I was directed to stay on and did some research under the supervision of my predecessor, Professor H.V.A. Briscoe. In late 1942, Professor F.A. Paneth was recruiting young chemists for the nuclear energy project which I joined. I was sent out to Canada in January 1943 and remained in Montreal and later Chalk River until I could leave in 1946. Having been attracted by the prospect of California, I wrote to, and was accepted by [Professor Glenn T. Seaborg](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1951/index.html). For the next four years in Berkeley I was engaged mostly on nuclear taxonomy and made many new neutron deficient isotopes using the cyclotrons of the Radiation Laboratory.  On a visit in 1949 to England, Briscoe advised me that I was unlikely to get an academic position in England in nuclear chemistry so that when I went as Research Associate to the Massachusetts Institute of Technology in 1950, I began to return to my first interest as a student – transition metal complexes such as carbonyls and olefin complexes.  In 1951 I was offered an Assistant Professorship at Harvard University, largely because of my nuclear background. I was at Harvard from September 1951 until I returned to England in December, 1955, with a sabbatical break of nine months in Copenhagen in Professor Jannik Bjerrum’s laboratory as a John Simon Guggenheim Fellow. At Harvard, I still did some nuclear work on excitation functions for protons on cobalt but I had already begun to work on olefin complexes so that I was primed for the appearance of the celebrated Kealy and Pauson note on dicyclopentadienyliron in Nature in early 1952.  In June 1955, I was appointed to the chair of Inorganic Chemistry at Imperial College in the University of London, which, at that time was the only established chair in the subject in the United Kingdom and took up the position in January 1956. I have been at the College ever since and have worked, with a relatively few students and postdoctoral fellows, almost entirely on the complexes of transition metals. I have been much interested in the complex chemistry of ruthenium, rhodium and rhenium, in compounds of unsaturated hydrocarbons and with metal to hydrogen bonds. The latter led to work on homogeneous catalytic reactions such as hydrogenation and hydroformylation of olefins.  In 1952 I married Lise Sølver, only daughter of Professor and Mrs. Svend Aage Schou, lately Rector of Denmark’s Pharmaceutical High School and we have two daughters. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0413 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0414** |
| **Biographical** | Born in Monessen, Pennsylvania, March 26, 1916 Dr. Anfinsen obtained a B.A. degree from Swarthmore College in 1937 and an M.S. in organic chemistry in 1939 from the University of Pennsylvania. He spent the year 1939-40 as a Visiting Investigator at the Carlsberg Laboratory in Copenhagen. In 1943, he received a Ph.D. from Harvard Medical School in biochemistry and spent the next seven years at Harvard Medical School; first as Instructor and then as Assistant Professor of Biological Chemistry. During this time, he spent a year (1947-48) as a Senior Fellow of the American Cancer Society working with Dr. Hugo Theorell at the Medical Nobel Institute. Dr. Anfinsen left Harvard in 1950 to become Chief of the Laboratory of Cellular Physiology and Metabolism in the National Heart Institute of the National Institutes of Health. He was again at Harvard Medical School as Professor of Biological Chemistry in 1962-63 and then returned to the National Institutes of Health to assume his present position.  In Anfinsen’s early work, he and Steinberg studied the non-uniform labelling in newly synthesized proteins – a technique which later permitted Dintzis, Canfield and others to determine that proteins are synthesized sequentially from the amino-terminal and *in vivo*, and to calculate the rate at which amino acids are polymerized.  In the mid 1950’s Anfinsen began to concentrate on the problem of the relationship between structure and function in enzymes. On the basis of studies on ribonuclease with Sela and White, he proposed that the information determining the tertiary structure of a protein resides in the chemistry of its amino acid sequence. Investigations on reversible denaturation of several proteins served to verify this proposal experimentally. It was demonstrated that, after cleavage of disulfide bonds and disruption of tertiary structure, many proteins could spontaneously refold to their native forms. This work resulted in general acceptance of the “thermodynamic hypothesis”. Studies on the rate and extent of renaturation *in vitro* led to the discovery of a microsomal enzyme which catalyzes sulfhydryl-disulfide interchange and thereby accelerates, *in vitro*, the refolding of denatured proteins containing disulfide bonds. In the presence of this enzyme the rate of renaturation approaches that sufficient to account for folding of newly completed polypeptide chains during protein biosynthesis. These findings have given important impetus to studies on the organic synthesis of proteins, since they demonstrate that, under physiological conditions of environment, attainment of the native structure rests solely upon the correct sequential polymerization of the amino acids.  In addition to his research activities, Dr. Anfinsen is an editor of *Advances in Protein Chemistry*, served on the Editorial Board of the *Journal of Biological Chemistry* and wrote “The Molecular Basis of Evolution” which was published in 1959. He is active as a member of the Board of Governors of the Weizmann Institute of Science in Rehovot, Israel, and was elected President of the American Society of Biological Chemists for the Academic Year 1971-72. His honors include a Rockefeller Foundation Public Service Award in 1954, a Guggenheim Fellowship in 1958, election to the National Academy of Sciences in 1963 and the Royal Danish Academy in 1964, and Honorary Doctor of Science degrees from Swarthmore College (1965), Georgetown University (1967), and New York Medical College (1969).  In recent years, Anfinsen has devoted himself primarily to comprehensive investigations of an extracellular nuclease of *Staphylococcus aureus*. He and his colleagues have determined the sequence of its 149 amino acids and have described its fundamental enzymological, physical, and immunological properties. They have used an extensive range of spectroscopic and chemical techniques, including new methods of affinity labeling and cross-linking, to delineate the identity and relationship of amino acids in its active site. Dr. Anfinsen has collaborated closely with a crystallographic group at M.I.T., under Professor F.A. Cotton, which has determined the three-dimensional structure of nuclease at high resolution.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html) *en 1972*, Editor Wilhelm Odelberg, [Nobel Foundation], Stockholm, 1973  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 1972 **Addendum, January 2003** *(kindly provided by Libby Anfinsen)*   |  |  | | --- | --- | | Honors received | | | 1969 | Honorary Fellow, Weizmann Institute of Science, Rehovot, Israel  Leon Lecture, University of Pennsylvania  Hadassah Myrtle Leaf Award | | 1970 | EMBO Lecture for Sweden  Visiting Fellow, All Souls College, Oxford, England | | 1972 | Jubilee Lecture, British Biochemical Society, England  Nobel Prize in Chemistry | | 1973 | Doctor of Science, (Honorary) University of Pennsylvania | | 1974 | Mathers Lectures, Indiana University, Bloomington, Indiana  Feinberg Graduate School, Christian B. Anfinsen Scholarship established  Scientific Advisory Committee of Weizmann Institute Chairman | | 1975 | Doctor of Science (Honorary), Gustavus Adolphus College – Lecture  Kempner Lectureship, University of Texas Medical Branch, Galveston, Texas  Participant, Encounter for the Universality of UNESCO, Paris | | 1976 | Bicentennial Exhibit Chosen for 20th Century Scientist, Maryland Academy of Science | | 1977 | Doctor of Science, (Honorary), Brandeis University  Naff Lectures, University of Kentucky, Lexington, Kentucky | | 1978 | Doctor of Science, (Honorary), Providence College, Rhode Island  Herbert A. Sober Memorial Lectureship  Consultant to ABC Cancer Foundation | | 1979 | Berson Memorial Lecture, Mount Sinai School of Medicine, New York City, New York  Fritz Lippmann Life Sciences Medal, Paris, France | | 1980 | Case Institute of Technoogy, Centennial Medal | | 1981 | Pontifical Academy of Science, The Vatican  NIH Scientist Emeritus Award  Fogarty International Symposium in honor of CBA on Contributions of Chemical Biology to Biomedical Science | | 1982 | Doctor of Medicine, M.D. (Honorary), University of Naples, Italy  Doctor of Science, (Honorary) Yeshiva University, New York City, New York | | 1984 | Distinguished Scientist Lectures Series, Bard College, New York  Theobald Smith Lecture Award, Albany College of Medicine, New York | | 1985 | Hebrew University of Jerusalem Medal | | 1986 | National Library of Medicine Medal, Bethesda, Maryland | | 1987 | Doctor of Science (Honorary), Adelphi University, New York | | 1988 | Genetic Engineering Lectures, Kon Kuk University, Seoul, Korea | | 1990 | Pioneer in Science Award, Bar Ilan University, Ramat Aviv, Israel  Participant in Oslo Conference, “Anatomy of Hate”, Norwegian Nobel Committee & Elie Wiesel Fund | | 1991 | Morris Brown College Research Symposium Award, Outstanding Nobelist  University of Texas Medical Branch, Centennial Symposium Medal, Galveston, Texas | | 1992 | Protein Folding Symposium to Honor Christian B. Anfinsen, NIH, Bethesda, Maryland | | 1993 | Doctor of Science, University of Las Palmas, Canary Islands (Honorary) | | 1994 | National Institutes of Health/Israel Alumni Association Lectureship established in honor of Christian B. Anfinsen, Rehovot, Israel: Memorial Biennial Lecture  Johns Hopkins University Lectureship honoring CBA established, Baltimore, Maryland  National Institutes of Health Anfinsen Lectureship established  Department of Health, Education and Welfare, Distinguished Service Medal  Department of Health, Education and Welfare, Superior Service Medal |   *Christian Anfinsen died on May 14, 1995.* |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0414 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0415** |
| **Biographical** | Stanford Moore was born in 1913 in Chicago, Illinois, and grew up in Nashville, Tennessee, where his father was a member of the faculty of the School of Law of Vanderbilt University. His developmental years were in a home environment which made the pursuit of knowledge an eagerly adopted undertaking. He had the opportunity to attend a high school administered by the George Peabody College for Teachers in Nashville. A skilled teacher of science, R.O. Beauchamp, kindled an interest in chemistry. Moore entered Vanderbilt University undecided between a career in chemistry or aeronautical engineering. The courses which he took in the engineering school presaged a concern for instrumentation. But a gifted professor of organic chemistry, Arthur Ingersoll, succeeded in presenting the study of molecular architecture as an even more appealing discipline. Moore graduated from Vanderbilt (B.A. 1935, *summa cum laude* ) with a major in chemistry. The faculty recommended him for a Wisconsin Alumni Research Foundation Fellowship which took him to the University of Wisconsin where he received his Ph.D. in organic chemistry in 1938.  His thesis research was in biochemistry in the laboratory of Karl Paul Link. The first lessons that the young graduate student received from the skilled hands of his professor were in the microanalytical methods of Pregl for the determination of C, H, and N; Link had recently returned from Europe where he had studied in the laboratory of Fritz Pregl in Graz. This training from Link in microchemistry was especially valuable for a student who was later to be concerned with the quantitative analysis of proteins. Moore’s thesis was on the characterization of carbohydrates as benzimidazole derivatives. The experience of bringing that work from the bench to the printed page under Link’s guidance marked Moore’s transition from a student to a productive scholar.  Karl Paul Link was a friend of Max Bergmann, who had recently arrived from Germany to lead a laboratory at the Rockefeller Institute for Medical Research in New York. Through that friendship, Moore was encouraged to join the Bergmann Laboratory in 1939, which was an internationally renowned center of research on the chemistry of proteins and enzymes. During [Emil Fischer’s](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1902/index.html) last years Max Bergmann had been his senior research associate, and Bergmann had attracted to Rockefeller a group of versatile chemists who maintained a tradition of innovative research and high productivity. After nearly three valuable years in such company, which included William H. Stein, the advent of World War II drew Moore out of the laboratory to serve as a junior administrative officer in Washington for academic and industrial chemical projects administered by the Office of Scientific Research Development. At the close of the war, he was on duty with the Operational Research Section attached to the Headquarters of the United States Armed Forces in the Pacific Ocean Area, Hawaii.  During the war years, the situation at the Rockefeller Institute had changed. The untimely death of Max Bergmann in 1944 had brought to a close the major chapter in biochemistry which the contributions of his laboratory comprised. Moore’s decision to return to Rockefeller was influenced by Herbert Gasser, then the Director of The Rockefeller Institute, who offered to give modest space to Moore and Stein to pursue the theme of research which they had begun with Bergmann or any new lines of investigation that appealed to them. Thus began the collaboration that led to the development of quantitative chromatographic methods for amino acid analysis, their automation, and the utilization of such techniques, in cooperation with younger associates, in the researches in protein chemistry summarized in the Nobel Lecture by Moore and Stein.  The investigations were conducted in an atmosphere at Rockefeller that encouraged interdepartmental cooperation and international consultation that would expedite research. Interludes included Moore’s tenure of the Francqui Chair at the University of Brussels in 1950, where, at the generous invitation of E.J. Bigwood, a laboratory of amino acid analysis was organized in the School of Medicine. Moore had the opportunity to round out the year in Europe with six months in England at the University of Cambridge where he shared part of a laboratory with Frederick Sanger during the time of the pioneering studies on insulin. In 1968, Moore was a Visiting Professor of Health Sciences at the Vanderbilt University School of Medicine.  Memberships and Activities American Society of Biological Chemists (Treasurer, 1956-59; Editorial Board, 1950-60; President, 1966), American Chemical Society, hon. member Belgian Biochemical Society, foreign correspondent Belgian Royal Academy of Medicine, Biochemical Society (Great Britain), U.S. National Academy of Sciences (Chairman, Section of Biochemistry, 1970), American Academy of Arts and Sciences, Harvey Society, Chairman of Panel on Proteins of the Committee on Growth of the National Research Council (1947-49), Secretary of the Commission on Proteins of the International Union of Pure and Applied Chemistry (1953-57), Chairman of the Organizing Committee for the Sixth International Congress of Biochemistry (1964), President of the Federation of American Societies for Experimental Biology (1970).  Honors Docteur *honoris causa* from the Faculty of Medicine of the University of Brussels (1954) and from the University of Paris (1964). Award shared with William H. Stein: American Chemical Society Award in Chromatography and Electrophoresis, 1964; Richards Medal of the American Chemical Society, 1972; Linderstrom-Lang Medal, Copenhagen, 1972. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0415 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0416** |
| **Biographical** | I was born June 25, 1911 in New York City, the second of three children, to Freed M. and Beatrice Borg Stein. My father was a business man who was greatly interested in communal affairs, particularly those dealing with health, and he retired quite early in life in order to devote his full time to such matters as the New York Tuberculosis and Health Association, Montefiore Hospital and others. My mother, too, was greatly interested in communal affairs and devoted most of her life to bettering the lot of the children of New York City. During my childhood, I received much encouragement from both of my parents to enter into medicine or a fundamental science.  My early education was at the Lincoln School of Teachers College of Columbia University in New York City, a school which was considered progressive for that time and which fostered in me an active interest in creative arts, music, and writing. There I had my first course in chemistry which proved to be an extremely valuable and interesting one. I left this school when I was about sixteen and went to an excellent preparatory school in New England, namely Phillips Exeter Academy, which was at the time, although it has changed since, a much more rigid and much more demanding educational experience than I had had at Lincoln. It was at Exeter that I was introduced to standards of precision of writing, and of work generally which I think has stood me in very good stead, and I believe that the combination of a progressive school and a more demanding school such as I enjoyed was an ideal preparation. From Exeter I went to Harvard where I had a very enjoyable, although not a very academically distinguished career, and graduated from the college in 1933 at the depths of the economic depression. I had majored in chemistry at college and decided to continue on at Harvard as a graduate student in that subject. This proved to be a rather unfortunate experience because my first graduate year was undistinguished, to say the very least. I was almost ready to abandon a career in science when it was suggested to me that I might enjoy biochemistry much more than straight organic chemistry.  The next year, I transferred to the Department of Biochemistry, then headed by the late Hans Clarke at the College of Physicians and Surgeons, Columbia University in New York. The department at Columbia was an eye-opener for me. Professor Clarke had succeeded in surrounding himself with a fascinating and active faculty and an almost equally stimulating group of graduate students. From both of these I learned a tremendous amount in a short time. My thesis involved the amino acid analysis of the protein elastin, which was then thought to play a role in coronary artery disease and I completed the requirements for my degree at Columbia late in 1937 and went directly to the laboratory of Max Bergmann at the Rockefeller Institute.  While still a graduate student, I had the extreme good fortune to marry, in 1936, Phoebe Hockstader who has been of enormous support to me ever since. We have three sons, William H. Jr., 35; David F., 33; Robert J., 28.  Bergmann was, I still feel, one of the very great protein chemists of this century and he, too, had the ability to surround himself with a most talented group of postdoctoral colleagues. In the laboratory at the time that I was there were, of course, Dr. Moore, and, in addition, Dr Joseph S. Fruton, Dr Emil L. Smith, Dr. Klaus Hofmann, Dr. Paul Zamecnik, and many others. It was impossible not to learn a great deal about the business of research in protein chemistry from Bergmann, himself, and from the outstanding group he had around him.  The task of Moore and myself was to devise accurate analytical methods for the determination of the amino acid composition of proteins, because Bergmann firmly believed, as did we, that the amino acid analysis of proteins bore the same relationship to these macromolecules that elementary analysis bore to the chemistry of simpler organic substances. It was during this period in the mid-thirties that Bergmann and Fruton and their colleagues were working out the specificity of proteolytic enzymes, work which has had a profound effect upon our knowledge of how enzymes function and has made it possible to use these proteolytic enzymes as tools for the degradation and subsequent derivation of structure of protein molecules ever since.  Work on proteins was suspended during the war for other more pressing matters and Dr. Moore left the laboratory in order to be of assistance in Washington and elsewhere. Our entire group was engaged in working for the Office of Scientific Research and Development. Bergmann’s death in 1944 robbed the world of a distinguished chemist and, of course, left the laboratory without a chief. The group continued to function until the end of the war at which time Moore and I had the very great good fortune to be asked by [Dr. Herbert Gasser](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1944/index.html), Director of the Institute, to stay on at Rockefeller with the freedom to do anything we pleased in the biochemical field.  In the meantime, had come the remarkable developments in England on the separation of amino acids by paper chromatography by [Martin and Synge](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1952/index.html) and [Sanger](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1958/index.html) had started his classical work on the derivation of the structure of insulin. It was then, perhaps, not surprising that Moore and I resumed our collaboration, and following a suggestion of Synge began to try to separate amino acids on columns of potato starch. We were very fortunate in hitting upon a type of potato starch which was well-suited to our needs almost immediately, and from that day on began to work first on the amino acid analysis, and then on the structural analysis of proteins. From columns of potato starch, we progressed to columns of ion exchange resins, developed the automatic amino acid analyzer, and together with a group of very devoted and extremely skillful collaborators, began work on the structure of ribonuclease. These columns were also used for other purposes. In the course of the early work, we developed a drop-counting automatic fraction collector which is now a common instrument in most biochemical laboratories throughout the world.  I should like to emphasize that the development of methods grew out of a need rather than a particular desire to develop methods as ends in themselves. We needed to know the amino acid composition of proteins, we needed to be able to separate and analyze peptides in good yield, and we needed to be able to purify proteins chromatographically. Since there were no methods for doing any of these things at the time that we started, we had to devise them ourselves. We not only wanted to know what the amino acid sequence of an enzyme such as ribonuclease was, but we tried to find out as much as we could about what made it an enzyme and after we had taken that particular enzyme about as far as we thought we could profitably go, we turned to a number of others which have been listed in the Nobel Lecture.  During all of this time, we had the undeviating support of an enlightened administration at Rockefeller who believed in allowing us to do those things which we thought to be important, and, during the last years of this work, we also have had great financial assistance from the NIH. For this and particularly for the very large number of devoted and talented colleagues which we have had in the laboratory we shall be forever grateful.  During all of this time, each of us, naturally, developed interests outside of the laboratory. I, for example, became greatly concerned about the promulgation of scientific information and have been attached, in one way or another, to the Journal of Biological Chemistry for a matter of over fifteen years. During this time it has been my privilege to work with a knowledgeable and dedicated group of biochemists who have devoted themselves unselfishly to serving the interests of their fellow biochemists throughout the world.  *Scientific Societies* – National Academy of Sciences, American Academy of Arts and Sciences, American Society of Biological Chemists, Biochemical Society of London, American Chemical Society, American Association for the Advancement of Science, Harvey Society of New York.  I was a member of the Editorial Committee of the Journal of Biological Chemistry, which is an elective office, for six years and Chairman of this Committee for three, 1958-61. After the conclusion of my work on the Editorial Committee, I became a member of the Editorial Board of the Journal of Biological Chemistry in 1962, and then an Associate Editor from 1964 until 1968. I assumed the Editorship, succeeding John T. Edsall, in 1968, a post I was forced to relinquish by illness in 1971.  *Other Activities* – Member of the Council of the Institute of Neurological Diseases and Blindness of the NIH, 1961-66; Chairman of the U.S. National Committee for Biochemistry, 1968-69; Philip Schaffer Lecturer at Washington University at St. Louis, 1965; Harvey Lecturer, 1956; Phillips Lecturer at Haverford College, 1962; Visiting Professor at the University of Chicago, 1961; Visiting Professor at Harvard University, 1964; Member of Medical Advisory Board, Hebrew University-Hadassah Medical School, 1957-1970; Trustee, Montefiore Hospital.  *Awards (shared with Stanford Moore)*: American Chemical Society Award in Chromatography and Electrophoresis, 1964; Richards Medal of the American Chemical Society, 1972; Kaj Linderstrøm-Lang Award, Copenhagen, 1972. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0416 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0417** |
| **Biographical** | Gerhard Herzberg was born in Hamburg, Germany, on 25 December, 1904. He was married in 1929 to Luise Herzberg *neé* Oettinger and has two children. He was widowed in 1971.  Herzberg received his early training in Hamburg and subsequently studied physics at the Darmstadt Institute of Technology where in 1928 he obtained his Dr.Ing. degree under H. Rau (a pupil of [W. Wien](https://www.nobelprize.org/nobel_prizes/physics/laureates/1911/index.html)). From 1928 to 1930 he carried out post-doctorate work at the University of Göttingen under [James Franck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1925/index.html) and [Max Born](https://www.nobelprize.org/nobel_prizes/physics/laureates/1954/index.html) and the University of Bristol. In 1930 he was appointed Privatdozent (lecturer) and senior assistant in the Physics Department of the Darmstadt Institute of Technology.  In August 1935 Herzberg was forced to leave Germany as a refugee and took up a guest professorship at the University of Saskatchewan (Saskatoon, Canada), for which funds had been made available by the Carnegie Foundation. A few months later he was appointed research professor of physics, a position he held until 1945. From 1945 to 1948 Herzberg was professor of spectroscopy at the Yerkes Observatory of the University of Chicago. He returned to Canada in 1948 and was made Principal Research Officer and shortly afterwards Director of the Division of Physics at the National Research Council. In 1955, after the Division had been divided into one in pure and one in applied physics, Herzberg remained Director of the Division of Pure Physics, a position which he held until 1969 when he was appointed Distinguished Research Scientist in the recombined Division of Physics.  Herzberg’s main contributions are to the field of atomic and molecular spectroscopy. He and his associates have determined the structures of a large number of diatomic and polyatomic molecules, including the structures of many free radicals difficult to determine in any other way (among others, those of free methyl and methylene). Herzberg has also applied these spectroscopic studies to the identification of certain molecules in planetary atmospheres, in comets, and in interstellar space.  Herzberg has been active as President or Vice President of several international commissions dealing with spectroscopy. He was also Vice President of the International Union of Pure and Applied Physics from 1957 to 1963. He held the offices of President of the Canadian Association of Physicists for the year 1956-57 and President of the Royal Society of Canada for the year 1966-67.  Herzberg was elected a Fellow of the Royal Society of Canada in 1939 and of the Royal Society of London in 1951. He was Bakerian Lecturer of the Royal Society of London in 1960 and received a Royal Medal from the Society in 1971. He was George Fischer Baker Non-Resident Lecturer in Chemistry at Cornell University in 1968, and Faraday Medallist and Lecturer of the Chemical Society of London in 1970. He is Honorary Member or Fellow of a number of scientific societies, including the American Academy of Arts and Sciences, the Optical Society of America and the Chemical Society. He is also a Foreign Associate of the National Academy of Sciences in Washington and a member of the Pontifical Academy of Sciences. He is a Companion of the Order of Canada. He has received many other medals and awards and holds Honorary Degrees from a number of universities in Canada and abroad, including one from the University of Stockholm. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0417 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0418** |
| **Biographical** | Luis F. Leloir was born in Paris of Argentine parents on September 6, 1906 and has lived in Buenos Aires since he was two years old. He graduated as a Medical Doctor in the University of Buenos Aires in 1932 and started his scientific career at the Institute of Physiology working with Professor Bernardo A. Houssay on the role of the adrenalin carbohydrate metabolism. In 1936 he worked at the Biochemical Laboratory of Cambridge, England, which was directed by [Sir Frederick Gowland Hopkins](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1929/index.html). There he collaborated with Malcom Dixon, N.L. Edson and D.E. Green. On returning to Buenos Aires he worked with J.M. Muñoz on the oxidation of fatty acids in liver, and also together with E. Braun Menéndez, J.C. Fasciolo and A.C. Taquini on the formation of angiotensin. In 1944 he was Research Assistant in Dr. Carl F. Cori’s laboratory in St. Louis, United States and thereafter worked with D.E. Green in the College of Physicians and Surgeons, Columbia University, New York. Since then he has been Director of the Instituto de Investigaciones Bioquímicas, Fundación Campomar. With his early collaborators, Ranwel Caputto, Carlos E. Cardini, Raúl Trucco and Alejandro C. Paladini work was started on the metabolism of galactose which led to the isolation of glucose 1,6-diphosphate and uridine diphosphate glucose. The latter substance was then found to act as glucose donor in the synthesis of trehalose (with Enrico Cabib, 1953 ) and sucrose (with Carlos E. Cardini and J.Chiriboga, 1955). Other sugar nucleotides such as uridine diphosphate acetylglucosamine and guanosine diphosphate mannose were also isolated. Further work showed that uridine diphosphate glucose is involved in glycogen synthesis and adenosine diphosphate glucose in that of starch.  More recent investigations (with Nicolás Behrens) have dealt with the role of a polyprenol, dolichol, in glucose transfer in animal tissues.  Luis Leloir was married in 1943 to Amelia Zuberbuhler and has a daughter, Amelia. At present Leloir is Professor in the Faculty of Sciences, University of Buenos Aires. He is a member of the following academies; National Academy of Sciences, American Academy of Arts and Sciences, Academia Nacional de Medicina, American Philosophical Society, Pontificial Academy of Sciences, and Honorary Member of the Biochemical Society (England). He has received honorary degrees of the following universities: Granada (Spain), Paris (France), Tucuman (Argentina) and La Plata (Argentina). Prof. Leloir has received the following awards: Argentine Scientific Society, Helen Hay Whitney Foundation (United States), Severo Vaccaro Foundation (Argentina), Bunge and Born Foundation (Argentina), Gairdner Foundation (Canada), Louisa Gross Horowitz (United States), Benito Juarez (Mexico); and at present he is President of the Pan-American Association of Biochemical Societies. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0418 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0419** |
| **Biographical** | Derek Harold Richard Barton was born on 8 September 1918, son of William Thomas and Maude Henrietta Barton. In 1938 he entered Imperial College, University of London, where he obtained his B.Sc.Hons. (1st Class) in 1940 and Ph.D. (Organic Chemistry) in 1942. From 1942 to 1944 he was a research chemist on a government project, from1944-1945 he was with Messrs. Albright and Wilson, Birmingham. In 1945 he became assistant lecturer in the Department of Chemistry of Imperial College, from 1946-1949 he was I.C.I. Research Fellow. In 1949 he obtained his D.Sc. from the same University. During 1949-1950 he was Visiting Lecturer in the Chemistry of Natural Products, at the Department of Chemistry, Harvard University (U.S.A.). In 1950 he was appointed Reader in Organic Chemistry and in 1953 Professor at Birkbeck College. In 1955 he became Regius Professor of Chemistry at the University of Glasgow, in 1957 he was appointed Professor of Organic Chemistry at Imperial College, which position he still holds.  In 1950, in a brief paper in *Experientia* entitled “The Conformation of the Steroid Nucleus”, Professor Barton showed that organic molecules in general and steroid molecules in particular could be assigned a preferred conformation based upon results accumulated by chemical physicists, in particular by Odd Hassel. Having chosen a preferred conformation, it was demonstrated that the chemical and physical properties of a molecule could be interpreted in terms of that preferred conformation. In molecules containing fixed rings, such as the steroids, there resulted a simple relationship between configuration and conformation, such that configurations could be predicted once the possible conformations for the products of a reaction could be analysed. Thus the subject “conformational analysis” had begun. Barton later determined the geometry of many other natural product molecules using this method. Conformational analysis is useful in the elucidation of configuration, in the planning of organic synthesis, and in the analysis of reaction mechanisms. It will be fundamental to a complete understanding of enzymatic processes.  Prof. Barton was invited to deliver the following special lectures: 1956, Max Tischler Lecturer at Harvard University; 1958, First Simonsen Memorial  Lecturer of the Chemical Society; 1961, Falk-Plaut Lecturer, Columbia University; 1962, Aub Lecturer at Harvard Medical School; Renaud Lecturer at Michigan State University; Inaugural 3 M’s Lecturer, University of Western Ontario; 1963, Hugo Müller Lecturer of the Chemical Society; 3 M’s Lecturer at the University of Minnesota; 1967, Pedler Lecturer of the Chemical Society; 1969, Sandin Lecturer at the University of Alberta; 1970, Graham Young Lectureship, Glasgow.  In 1958 Prof. Barton was Arthur D. Little Visiting Professor at Massachusetts Institute of Technology, Cambridge, Mass.; in 1959 Karl Folkers Visiting Professor at the Universities of Illinois and Wisconsin.  In 1954 Derek Barton was elected to Fellowship of the Royal Society, in 1956 he became Fellow of the Royal Society of Edinburgh; in 1965 he was appointed member of the Council for Scientific Policy of the U. K.; in 1969 he became President of Section B, British Association for the Advancement of Science, and President of the Organic Chemistry Division of the International Union of Pure and Applied Chemistry.  Professor Barton holds the following honours and awards: 1951, First Corday-Morgan Medal of the Chemical Society; 1956, Fritzsche Medal of the American Chemical Society; 1959, First Roger Adams Medal of the American Chemical Society; 1960, Foreign Honorary Member of the American Academy of Arts and Sciences; 1961, Davy Medal of the Royal Society; 1962, D. Sc.h.c. Montpellier; 1964, D. *Sc.h.c.* Dublin; 1967, Honorary Fellow of the Deutsche Akademie der Naturforscher “Leopoldina”; 1969, Honorary Member of Sociedad Quimica de Mexico; 1970, D.Sc.*h.c.* St. Andrews: Fellow of Birkbeck College; Honorary Member of the Belgian Chemical Society; Foreign Associate of the National Academy of Sciences; Honorary Member of the Chilean Chemical Society; D.Sc.*h.c.*, Columbia University, New York; 1971, First award in Natural Product Chemistry, Chemical Society (London); D.Sc.*h.c*., Coimbra (Portugal); Elected Foreign Member of the Academia das Ciencias de Lisboa; 1972, D. Sc*.h.c.* University of Oxford; Longstaff Medal of the Chemical Society.  Derek Barton was first married to Jeanne Kate Wilkins but this marriage was later dissolved. He is now married to Christiane Cognet, a Professor of the Lycée français de Londres. He has one son, W.G.L. Barton, by his first marriage. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0419 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0420** |
| **Biographical** | Odd Hassel was born in Kristiania (now Oslo), Norway, I7 May, 1897. His father was Ernst Hassel, a physician who specialized in gynaecology, his mother Mathilde née Klaveness.  In 1915 he entered the University of his native town where he studied mathematics and physics with chemistry as his chief subject and graduated as a cand. real. in 1920. After a year of leisure in France and Italy he went to Germany in the autumn of 1922 where he first spent more than half a year in Munich in the laboratory of Professor K. Fajans. Work on the sensibilisation of silver halides by organic dyes led to the detection of what is now termed the “adsorption indicators” After moving to Berlin Hassel worked at the Kaiser Wilhelm Institute in Dahlem, carrying out X-ray crystallographic work. During that time he obtained, on the proposal of [Fritz Haber](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1918/index.html), a Rockefeller Fellowship. In 1924 he graduated as Dr. Phil. at the Berlin University. From 1925 to 1926 he worked at the University of Oslo in the capacity of “universitetsstipendiat”, from 1926 to 1934 as “dosent” in physical chemistry and electrochemistry. From 1934 to 1964 he had the chair of physical chemistry in Oslo, the first of its kind in Norway, and headed the department of physical chemistry started in 1934.  Hassel’s main interest during the first years of his teaching at the University of Oslo dealt with inorganic chemistry, but from 1930 onwards his work was concentrated on problems connected with molecular structure, particularly the structure of cyclohexane and its derivatives and other substances containing six-membered rings related to that of cyclohexane.  In order to supplement the experimental methods available two additional methods not previously used in Norway were introduced; the measurements of electric dipole moments and electron diffraction by vapours. Sufficient experimental material had been gathered by 1943 to allow more general conclusions regarding the possible configurations (conformations) and the transition between them to be drawn. A short paper had just been published in a Norwegian journal when Hassel was arrested by Norwegian Nazis and later taken into custody by the German occupants. Released in November 1944 he found the institute almost deserted. After the war experimental work could be taken up again and in particular electron-diffraction work based on the rotating sector method.  During the early 1950’s Hassel opened a new field of structure investigation, namely that of the charge-transfer compounds. Compounds formed by organic electron- donor molecules like ethers and amines and electron acceptors as halogen molecules or organic halides had mainly been investigated by spectroscopic methods. Information on the steric structures was scarce, however, and a series of structure determinations was undertaken. After some years work he was able to set up rules for the geometry of this kind of addition compounds, and this field still remains his main interest in structural chemistry.  Hassel holds honorary degrees from the Universities of Copenhagen and Stockholm. He is an honorary Fellow of the Norwegian Chemical Society and of the Chemical Society, London.  He is a Fellow of the Norwegian Academy of Sciences, the Royal Danish Academy of Sciences, the [Royal Swedish Academy of Sciences](http://www.kva.se/) and the Royal Norwegian Academy of Science. In 1964 he received the Guldberg-Waage Medal from the Norwegian Chemical Society and the Gunnerus Medal from the Royal Norwegian Academy of Sciences.  He is a Knight of the Order of St. Olav.  From 1967 a lecture is given yearly by distinguished scientists from abroad to his honour at the University of Oslo (“The Hassel Lecture”). |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0420 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0421** |
| **Biographical** | Lars Onsager was born in Oslo, Norway, November 27, 1903 to parents Erling Onsager, Barrister of the Supreme Court of Norway, and Ingrid, née Kirkeby. In 1933 he married Margarethe Arledter, daughter of a well-known pioneer in the art of paper making, in Cologne, Germany. They have sons Erling Frederick, Hans Tanberg, and Christian Carl, and a daughter Inger Marie, married to Kenneth Roy Oldham.  After three years with the experienced educators Inga and Anna Platou in Oslo, one year at a deteriorating private school in the country and a few months of his mother’s tutoring, he entered Frogner School as the family returned to Oslo. There he was soon invited to jump a grade, so that he was able to graduate in 1920.  Admitted to Norges tekniske høgskole in the fall of that year as a student of chemical engineering, he entered a stimulating environment; the department had attracted outstanding students over a period of years. Among the professors particularly O.E. Collenberg and J.P. Holtsmark encouraged his efforts in theory and helped him in the evaluation of background knowledge.  After graduation in 1925 he accompanied Holtsmark on a trip to Denmark and Germany, then proceeded to Zurich, where he remained for a couple of months with [Debye](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1936/index.html) and Hückel and returned the following spring, for a stay of nearly two years. There he organized his results in the theory of electrolytes for publication, broadened his knowledge of physics and became acquainted with a good many leading physicists.  In 1928 he went to Baltimore and served for the spring term as Associate in Chemistry at Johns Hopkins University. The appointment was not renewed; but C.A. Kraus at Brown University engaged him as an instructor, and he remained in that position for five years. During this time he gave lectures on statistical mechanics, published the reciprocal relations and made progress on a variety of problems. Some of the results were published at the time, one with the able assistance of R.M. Fuoss; others formed the basis for later publications. In 1933 he accepted a Sterling Fellowship at Yale University, where he remained to serve as Assistant Professor 1934-1940, Associate Professor 1940-1945 and JosiahWillard Gibbs Professor of Theoretical Chemistry 1945-1972. Incidentally, he obtained a Ph.D. degree in Chemistry from Yale in 1935; his dissertation consisted of the mathematical background for his interpretation of deviations from Ohm’s law in weak electrolytes.  Over the years, the subjects of his interest came to include colloids, dielectrics, order-disorder transitions, metals and superfluids, hydrodynamics and fractionation theory. In 1951-1952 he spent a year’s leave of absence as a Fulbright Scholar with David Schoenberg at the Mond Laboratory in Cambridge, England, a leading center for research in low temperature physics. In the Spring of 1961 he served as Visiting Professor of Physics at the University of California in San Diego. Of his sabbatical leave 1967-1968 he spent the first three months as Visiting Professor at Rockefeller University and the last three as Gauss Professor in Göttingen. In 1962, at the suggestion of Manfred Eigen, he joined Neuroscience Associates, a small interdisciplinary group organized by F.O. Schmitt in Cambridge, Massachusetts.  Lars Onsager holds honary degrees of Doctor of Science from Harvard University (1954), Rensselaer Polytechnic Institute (1962), Brown University (1962), Rheinisch-Westfahlische Technische Hochschule (1962), the University of Chicago (1968), Ohio State University (Cleveland, 1969), Cambridge University (1970) and Oxford University (1971), and Doctor technicae from Norges tekniske høgskole (1960).  In 1953 he received the Rumford Medal from the American Academy of Arts and Sciences, in 1958 The Lorentz Medal from The Royal Netherlands Academy of Sciences, in 1966 the Belfer Award in Science from Yeshiva University, in 1965 the Peter Debye Award in Physical Chemistry from the American Chemical Society, in 1962 the Lewis Medal from its California Section, the Kirkwood Medal from the New Haven Section and the Gibbs Medal from the Chicago Section, in 1964 the Richards Medal from the Northeastern Section.  In 1969 he received the National Science Medal, and he became an honorary member of The Bunsen Society for Physical Chemistry. During Spring 1970 he was Lorentz Professor in Leiden (The Netherlands).  Onsager is a Fellow of the American Physical Society and The New York Academy of Sciences, a member of The American Chemical Society, The Connecticut Academy of Arts and Sciences, The National Academy of Sciences, The American Academy of Arts and Sciences and The American Philosophical Society, a Foreign Member of the Norwegian Academy of Sciences, The Royal Norwegian Academy of Sciences, The Norwegian Academy of Technical Sciences, [the Royal Swedish Academy of Sciences](http://www.kva.se/) and The Royal Science Society in Uppsala, and an Honorary Member of The Norwegian Chemical Society. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0421 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0422** |
| **Biographical** | Manfred Eigen was born in Bochum on 9 May 1927, the son of the chamber musician Ernst Eigen and his wife Hedwig, née Feld. He received his schooling at the Bochum humanistic Gymnasium.  In the autumn of 1945 he commenced the physics and chemistry course at the Georg-August University in Göttingen and obtained his doctorate in natural science in 1951. He wrote his dissertation on the specific heat of heavy water and aqueous electrolyte solutions under the guidance of Arnold Eucken. After two years as an assistant lecturer at the physical chemistry department of the university under Ewald Wicke, he transferred to the Max-Planck Institut für physikalische Chemie, which had moved to Göttingen under the Directorship of Karl Friedrich Bonhoeffer. The influence of Bonhoeffer, who provided him with magnificent working conditions at the Institut, is reflected in his later work in the field of biophysical chemistry.  Eigen began his work on the problem of fast ionic reactions in solution in the period 1951-1953, encouraged to do so by the ultrasound absorption measurements carried out by his colleagues Konrad Tamm and Walter Kurtze. During the following years he developed a series of measuring techniques involving times down to the order of a nanosecond. He developed many of these techniques with Leo de Maeyer, who joined him in the autumn of 1954, and with whom he is still collaborating closely at the Göttingen MaxPlanck-Institut. The Max-Planck-Gesellschaft appointed Eigen a Scientific Member in 1957 and head in 1964. In 1967 he was elected Managing Director of the Institute for a period of three years. At the same time he was appointed to the Scientific Council of the German Federal Republic.  Eigen’s scientific development is reflected in the close on 100 papers he has published. The subject matter of these works ranges from the thermodynamic properties of water and aqueous solutions, and the theory of electrolytes, through thermal conductivity and sound absorption, to fast ionic reactions.  In the years 1953-1963 followed the description of a series of novel measuring techniques used for the study of very fast reaction in the range from one second to one nanosecond. The gap between the region of classical reaction kinetics and spectroscopy was thus closed. Eigen was particularly interested in proton reactions: together with De Maeyer he was the first to determine the neutralization rate and found the anomalous conduction characteristics of protons in ice crystals. The development of the theory of relaxation of multi stage processes was followed by studies on metal complex reactions, in which the fast reactions of a large number of metal ions were investigated in relation to their position in the periodic table. Around 1960 the emphasis in his work shifted towards physical-organic chemistry. The individual steps of a series of reaction mechanisms were elucidated, and a general theory of acid-base catalysis was verified experimentally.  At the same time, however, his attention turned also to biochemical questions, which now claimed his chief interest. These questions ranged from hydrogen bridges of nucleic acids, through the dynamics of code transfer, to enzymes and lipid membranes. Biological control and regulation processes, and the problem of the storage of information in the central nervous system also occupy his attention. Practically every year he travels together with his friend and colleague Leo de Maeyer to Boston to discuss topics of common interest with American neurologists, biochemists, and biophysicists.  Eigen holds the following honours and distinctions: Bodenstein prize of the Deutsche Bunsengesellschaft, 1956; Otto-Hahn Prize for Chemistry and Physics, 1962; Kirkwood Medal (American Chemical Society), 1963; Harrison Howe Award (American Chemical Society), 1965; Andrew D. White Professor at large at Cornell University, Ithaca, N.Y., 1965; Honorary Professor at the Technische Hochschule, Braunschweig, 1965; Foreign Honorary Member of the American Academy of Arts and Sciences, 1964; Member of the “Leopoldina”, Deutsche Akademie der Naturforscher in Halle, 1964; Member of the Göttingen Akademie der Wissenschaften, 1965; Honorary Member of the American Association of Biological Chemists, 1966; Honorary degree of doctor of science at Harvard University, U.S.A., 1966; Honorary degree of doctor of science at Washington University, U.S.A., 1966; Foreign Associate of the National Academy of Sciences, Washington, U.S.A. 1966; Honorary degree of doctor of science, University of Chicago, U.S.A., 1966; Carus Medal of the Deutsche Akademie der Naturforscher “Leopoldina”, Halle, 1967; Linus Pauling Medal of the American Chemical Society, 1967.  Manfred Eigen is married to Elfriede, née Müller. They have two children, Gerald (born 1952) and Angela (born 1960). In his free time he is a keen amateur musician. His favorite holiday pastime is mountaineering. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0422 |
| **Interview** |  |
| **Q3** | **So why then not start with the sort of traditional question: how did you come into research and what made you chose the field you chose?** |
|  | That’s a difficult question because I was grown up in a family of musicians and I learnt to play the piano from my 5th year of age and I always thought I would have to study music. And at 12 I played already concerts, but then there was a war, and I was about between my age of 15 and 18 I couldn’t see a piano, and this is the most important time for a musician because he has to print his /- – -/.  So when the war was over, one day later I got 18 years old, and of course before that I have thought I like as much mathematics, physics, chemistry, and funnily enough I always thought if I am going to study natural sciences it has to be in Göttingen. So I went to Göttingen and it was the first university to open after the war, and they told me I am too young and still would have to go back to school, but I asked them whether I could do it with an examination and so I was accepted and started in 1945 and then it was my teachers who convinced me. I had very good teachers. |
| Q6 | **But you also told me that you made a very impressive and short career to get your thesis. Could you please tell the story about your thesis?** |
|  | My student time the first semester was physics. I was the student of [Heisenberg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1932/heisenberg-facts.html) and of Richard Becker and in experimental physics of Kopfermann and Paul, and in physical chemistry Arnold Eucken, these are all great heroes, and I learnt a lot from them. So I started quite early to do a master thesis in 1947 already, and had to matter the specific heat of heavy water over a large temperature range up to as high as possible – 150, 180 centigrade – limited by the vapour pressure since that was a big glass vessel in glass which could stand perhaps pressure of 5 or 10 atmospheres, and there was a debate between Arnold Eucken, my teacher, and his assistant. His assistant said “No, it wouldn’t go above 140, 150 centigrade” and Eucken “Oh, you can go at least 200”, because he wanted to have the data. And it was precision management up to 5th decimal because he wanted to test his theory of water and he needed the isotope effect. So I decided in my first experiment to go up to 175 whatever could a poor student do …  And it was at the time when heavy water was very limited and it was very precious.  Manfred Eigen: Oh, it wasn’t even available in Germany then any more. He had still something, 99% purity, and it couldn’t be paid anyway. So I had 450 gram, 450 millilitre of that stuff he gave me, I think his hands were …  Shaking.  … there was a big explosion and the water evaporated and the machine was under the ceiling and it was really a big bang …  Manfred Eigen: … shaking. So it happened at 169 there was a big explosion and the water evaporated and the machine was under the ceiling and it was really a big bang and everybody came to my room and “Oh, poor fellow”, and Eucken came also and said “What did you do with my heavy water”? I said “Yes, but you were the one who told me 200 centigrade”. “But not in the first experiment”, he said.  Well, I wanted to tell him something but he shouted at me and I couldn’t say anything, so I finally went home and next morning came to clean up the room and he came right at first and say “What am I going to do with you now the whole thesis is gone?” I said “Well, I will have built a new calorimeter in 3 or 4 weeks. I know now how it works and “Oh” he said “don’t talk nonsense it’s not the calorimeter, it’s the heavy water”. I said “No wait, don’t start again”, went to the board and said “Here you have your heavy water”. He wouldn’t believe that I did my first experiment with heavy water.  You were clever.  Manfred Eigen: So then I got quite some freedom in doing my work. So it happened that I finished that work very soon with heavy water and accuracy and not as he suggested to put it on large graph paper – I worked out some /- – -/ functions by which you can approximate it in a suitable way, but then he said “Well, I want to have some data on electrolytes also” I say “Yes, but let me first do my examination”. “Oh” he said, “then we lose time. Why do you need that examine and go on as a doctor please”. “Yes, but what will the faculty say?” He said “That’s my business”, so I finished very early. |
| Q17 | **So then in something like three years you passed your thesis and you were 21 when you had your PhD?** |
|  | I finished my thesis was 21 and got my examination was 22 years.  Excellent.  Manfred Eigen: And yes, at 27 I got my first offer for a professorship.  Yes, good.  Manfred Eigen: And so I started very early then having all the students were older than me.  Yes. And then also you have become sort of the father of physical calculations on the evolution of life and origin of …  Manfred Eigen: Oh, that was much later. The Nobel Prize was for fast reactions.  I know that.  Manfred Eigen: So it was in Eucken’s textbook where it said unmeasurably fast reactions and he thought that you mixed two substances it takes a millisecond about because you get tubular and flow and if you do it under pressure you know, so that’s no way. The mixing process takes that much time and if the reaction is faster than you can’t follow it. And I didn’t know how to do it but as a young man you don’t believe what they tell you. I say “There’s nothing which is immeasurably fast” and so I thought of it but couldn’t find a solution. But I learnt by studying electrolytes about the salvation and the interactions and there was a problem in physics which was brought to me that is the high sound absorption of sea water which is, one didn’t have an explanation. I knew it is not the sodium chloride, not the salt in seawater it is …  Well, we soon found out it must be the magnesium sulphur that is the next, because pure water has very little sound absorption and it has two maxima so there must be two processes going on. And there was a suggestion that one was the magnesium and the other the sulphate iron so I said “Well, that’s easy to decide, take magnesium chloride, the chloride iron doesn’t do anything, sodium chloride, and take sodium sulphate and if it’s the sulphate iron the sodium doesn’t do anything”.  And that worked?  Manfred Eigen: No.  It didn’t.  Manfred Eigen: No absorption at all. So my conclusion was it is the interaction and since there were two maxima I thought it must be a coupled reaction. I worked out all the theory like in coupled oscillators with normal modes and so on and it was a quantitative explanation. So at that time I remembered Eucken’s words of “There’s no way to study these fast reactions”. I said “Well, the frequencies we use are one maximum, one megacycle it’s a millionth of the second, the other is 100 megacycles almost a billionth of a second. I said “Here you see the reaction going on” so the trick is you don’t mix your substances. You start from equilibrium as you do it in a sound wave by the pressure wave and that’s how we started.  … it turned out to be the fastest reaction we know …  And I went to Bonhoeffer – Eucken had died – and proposed to study fast reaction this way and he was enthusiastic about getting me a laboratory and so that was the start of the work for the Nobel Prize and in 1955 we did a very famous experiment, we measured the rate of neutralisation. That means the proton hydroxaline forming water molecule and nobody could measure that before. And it turned out to be the fastest reaction we know. Even faster than an electron with the hydrogen atom or so on. It turns out not only to be controlled by the diffusion of the two partners to each, then when they reach a certain distance where the salvations freeze up they tunnel through it and so it is really the fastest reaction we know. And so you see at the time it was almost Olympic discipline to get the fastest possible reaction.  And then all the inorganic chemists came to us: you can now study the rates of complex formation, then the organic chemists came to measure acid-base catalysis of all types of organic reaction and the bio-chemists came and we did the first studies on allosteric enzymes to measure the control of enzymes, all the elementary steps because this relaxation spectrum is a linearized spectrum gives you like in optics – a spectrum of time content you can resolve mechanisms and then we started to think how can this come about those finely tuned reactions with controlled, and so who did it? When our biologists said of course Darwin explained it but we said but Darwin talked about living beings these are molecules and molecules didn’t know about Darwin as much as Darwin knew about molecules. So that’s when we started to think of evolution and we worked out a theory of molecular evolution which you can formulate in mathematical terms. So we are at your subject now. |
| Q9 | **That’s very good. How were you told about the Nobel Prize? Can you remember, of course you do, how the initial message came?** |
|  | About my Nobel Prize?  Yes.  … call them damn fast reactions and if you still have fast ones, call them damn fast indeed …  Manfred Eigen: I will tell you. It was 1967, so the essential work on which it was raised was already in -55, -56, -57, in 1960’s turned out that it’s a generally applicable method and we went down to even nanoseconds. Nowadays they go to femtoseconds, as late as … but they were not yet invented at the time. I remember going in -54 to a meeting of Faraday Society where they talked about fast reactions and Hartridge and Roughton talked about this mixing millisecond, he called that very fast because /- – -/ before that said something about a second that’s a fast and then [Norrish](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1967/norrish-facts.html) and [George Porter](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1967/porter-facts.html) came with the flash photolysis get to the microsecond range and called it extremely fast reactions. I said “My English is not good enough, can anybody tell me how to call … I was going to call these reactions slow reactions … I’m going to …”. “Oh”, they said “No difficulty, call them damn fast reactions and if you still have fast ones, call them damn fast indeed”.  So people told me that there was a Nobel Prize coming up and our cleaning woman the year before had baked cakes because they thought I would get it and there was nothing. So I told them “Relax, that takes many years so there’s no hope”, and then the next year I got it. And I got a phone call from a camera team, that was the first I got: Can we have an interview with you?”, was it 12 o’clock, mid-noon, I said “Yes”. “We’re from Stockholm.” I said “Yes, which day tomorrow?” “No”, they said, “right now”. I said “But you have to come from Stockholm here, as I said we are already at Göttingen”. And then an hour later there was a phone call.  And that was the same day as it was announced?  Manfred Eigen: Yes.  So they knew in advance too?  Manfred Eigen: Somehow they must have, or they have guessed and perhaps they sent several teams to several cities.  Okay.  Manfred Eigen: But anyway, so was it. And then the next weeks I couldn’t do any work because there were always interviews and such things. |
| Q9 | They sometimes say that the Nobel Prize changes their lives? Did it do that? Do you felt it changed your life? |
|  | No, I was happy to be very sick. I had very serious stomach trouble.  So you didn’t notice a change over here?  Manfred Eigen: Oh yes, I noticed a change and at the Nobel dinner they gave special meals to me and that took place and soon later it happened that I got a bleeding and I had to be taken to an emergency operation and so they took me out for several months and every single … for the interruption were finished, my secretary said no he’s not available he’s sick and so when I came back from the hospital, I could go on with my daily life.  And you didn’t come in to all committees and didn’t have to answer all questions?  Manfred Eigen No, I didn’t, I had good excuses.  So should we then switch to the self-replication of the RNA and the further work on evolution?  Manfred Eigen: Yes, there was much discussion what came first, you know, the egg … if the egg is the DNA and the hen is the protein that means the function or the information. Then of course this was back to our present system it’s the wrong question, they had to come in both, but it seems to be if you ask what is the first prerequisite, is it metabolism or is it self-organisation, it’s coping, it’s selection natural selection, and I would say metabolism can be realised in a very primitive way if you have energy rich substances. They will be used up very soon and then you have to incorporate mechanisms which make the sun energy available but you cannot do without self-reproduction.  You cannot do without selection and self-reproduction is the prerequisite of selection. You get really the competing behaviour by the growth laws and so self-reproduction is necessary by two reasons. It yields a mechanism of selection and it keeps information conserved. Without reproduction you lose the information. Everything has a final lifetime, if you don’t reproduce given the huge number of possibilities, you lose it. And so our first interest was directed towards self-reproducing system like an RNA is the simplest one. DNA is already more complicated and evolution probably came much later than RNA. |
| Q4 | **So when did you do the first RNA experiments?** |
|  | Beginning of the 1980’s. But we again did also much theoretical work. In other words, we worked out methods to study the evolution of, and we found that the usual way of constructing trees presupposes that there is a tree, the programme, and we wanted to have a method which decides whether it is a tree or some other kind of diverence. And so we worked out something which is called statistical geometry, Ruthild Winkler-Oswatitsch worked on this and Andreas Dress, he’s a mathematician, and we came out with a new method which we called statistical geometry which tells you what kind of divergences it is and also allows you then to reconstruct nodes in the tree or centres of evolution.  One of the works we studied is all the available sequences of tRNA which we realised must be one of the very early molecules, because it adapts the amino acid to the codon. At that time about 1000 sequences were available so then you could study phylogeny of a given tRNA or you can start the diversions of a family of all the 40 tRNA organisms through all the steps of co-organisms and by combining this we found out that the age of the first divergence, that’s say the divergence between eukaryotes and archaebacteria, was at the time and if you also they could find the first divergence of the families, that means the origin of the code with only 25% longer than that.  So if the first divergence of cell was 3 billion years ago, the code couldn’t be older than 4 billion years and everything now points in the direction that this correct. This was the first time we could get measured without assuming anything. Just numbers from experimental data which taught us. So we went on on the RNA /- – -/ and Peter Schuster is going on now to studying the secondary structure formation and third structures in RNA’s and that’s a wonderful evolutionary problem. |
| Q18 | **Do you know several people like you who study evolution sometimes meet religious groups and they have other opinion.** |
|  | Oh sure, sure. Every week I get letters. I have no problems with them because I tell them “Look, I’m looking how it was created by the Lord, but you want to tell the Lord how he has to do it”.  Yes okay yes.  Manfred Eigen: And that’s even is contradictory to the first commandment. |
| Q10 | **What’s your view on the discussion we had today whether research in general should be funded by all this that we sell ourselves to companies or contra the old system…?** |
|  | That’s entirely wrong. I mean everything which is new has to come out of fundamental research otherwise it’s not new.  Yes.  … if you really want to find something new, it can only come out of fundamental research …  Manfred Eigen: If you know before that you can develop a certain thing then okay you go ahead. It’s important. It’s important for industry that they do purposeful research and so, but if you really want to find something new, it can only come out of fundamental research and it often comes out at much later time it turns out to be. |
| Q10 | **Do you see any risk in the support from the industry to academy and to universities?** |
|  | I don’t see a risk if it is done in the proper way if it is too much goal directed and if you only get money from industry and even from government if you can prove that it has a useful purpose, that would be better. It would be the end of fundamental research. |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0423** |
| **Biographical** | Ronald George Wreyford Norrish was born in Cambridge on November 9th, 1897. His father, a native of Crediton, Devonshire, came to Cambridge as a young Pharmacist to open one of the early shops of Boots, the Chemists, and remained there for the rest of his long life.  After spending his early years at the local Board school, Norrish obtained a scholarship to the Perse Grammar School in 1910. He remembers with deep gratitude his early teachers, in particular Rouse, Turnbull and Hersch, who gave dedicated and individual help to promising young scholars. In 1915 he obtained an entrance scholarship to Emmanuel College, Cambridge in Natural Sciences, but left in 1916 with a commission in the Royal Field Artillery for service in France. He was made prisoner of war in March 1918 and spent the rest of the war in Germany, first at Rastatt and later at Graudenz in Poland. Repatriated in 1919, he returned to Emmanuel College where he has remained ever since, first as a student and after 1925 as a Fellow. Norrish’s early research was inspired by Eric Redeal (now Sir Eric Redeal) under whose lively supervision he first took up the study of Photochemistry.  In 1925 he was made Demonstrator and in 1930, Humphrey Owen Jones Lecturer in Physical Chemistry in the Department of Chemistry at Cambridge and upon the death of the first Professor of Physical Chemistry, Dr. T.M. Lowry, was appointed to the Professorship in 1937. He occupied the chair until 1965 when he retired as Emeritus Professor of Physical Chemistry in the University.  Norrish has had the good fortune to work with many gifted students and with them has carried out a wide range of research in the fields of Photochemistry and Reaction Kinetics, including Combustion and Polymerisation. As the study of Chemical Kinetics developed, there was a fortunate integration in the various aspects of the study in which his school of work was engaged, as a result of which the importance of Photochemistry and Spectroscopy to Chemical Kinetics in general emerged. All this was sadly brought to a temporary halt in 1940. During the second world war, while still continuing to direct the Department of Physical Chemistry and to teach, Norrish was concerned with a good deal of research work in connection with various ministries and was able to collaborate with his colleagues on various government committees. It was after the war in 1945 when research was recommenced that work was started with the object of observing short lived transients in chemical reactions. In collaboration with his student, now Professor George Porter, this led to the development of Flash Photolysis and Kinetic Spectroscopy which has had considerable influence on the subsequent development of Photochemistry and Reaction Kinetics, and in the hands of workers in many parts of the world is continuing to develop as a powerful technique for the study of all aspects of chemical reaction.  In 1926 Ronald Norrish married Annie Smith who was Lecturer in the Faculty of Education in the University of Wales in Cardiff. They have two daughters and four grandchildren. Much of their time has been spent in travel.  Norrish has served on the Councils of the Chemical Society, the Faraday Society of which he became President in 1951-1955 and on the Council of the Royal Institute of Chemistry of which he was Vice President from 1957 to 1959. He delivered the Liversidge Lecture to the Chemical Society in 1958, the Faraday Memorial Lecture to the Chemical Society in 1965, and the Bakerian Lecture to the Royal Society in 1966. He was President of the British Association Section B (Chemistry) in 1961, and in the same year was made Liveryman of the Worshipful Company of Gunmakers. In 1958 he received the honorary degree of D. de l’U. at the Sorbonne in Paris and also honorary degrees D. Sc. in Leeds and Sheffield in 1965, Liverpool and Lancaster (1968) and British Columbia (1969). He is an honorary member of the Polish Chemical Society and Membre d’honneur of the Société de Chimie Physique in Paris. He is a foreign member of the Polish and the Bulgarian Academies of Sciences, a corresponding member of the Academy of Sciences in Göttingen and of the Royal Society of Sciences in Liege. He is a honorary member of the Royal Society of Sciences in Uppsala and the New York Academy of Sciences. He has received the Meldola medal of the Royal Institute of Chemistry (1926), the Davy medal of the Royal Society (1958), the Lewis medal of the Combustion Institute (1964), the Faraday medal of the Chemical Society (1965) and their Longstaff medal (1969). He was elected Fellow of the Royal Society in 1936 and is still endeavouring to continue to prosecute his scientific activities in Cambridge.  To mark his retirement in 1965, many of his old friends and younger colleagues now occupying distinguished positions in academic and industrial work in Great Britain and abroad collaborated to publish a work entitled “Photochcmistry and Reaction Kinetics”. To them and to all others with whom he has worked for over 50 years he is deeply grateful. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0423 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0424** |
| **Biographical** | George Porter was born in the West Riding of Yorkshire on the 6th December 1920. He married Stella Jean Brooke on the 25th August 1949 and they have two sons, John and Andrew.  His first education was at local primary and grammar schools and in 1938 he went, as Ackroyd Scholar, to Leeds University. His interest in physical chemistry and chemical kinetics grew during his final year there and was inspired to a large extent by the teaching of M.G. Evans. During his final honours year he took a special course in radio physics and became, later in the year, an Officer in the Royal Naval Volunteer Reserve Special Branch, concerned with radar. The training which he received in electronics and pulse techniques was to prove useful later in suggesting new approaches to chemical problems.  Early in 1945, he went to Cambridge to work as a postgraduate research student with Professor R.G.W. Norrish. His first problem involved the study, by flow techniques, of free radicals produced in gaseous photochemical reactions. The idea of using short pulses of light, of shorter duration than the lifetime of the free radicals, occurred to him about a year later. He began the construction of an apparatus for this purpose in the early summer of 1947 and, together with Norrish, applied this to the study of gaseous free radicals and to combustion. Their collaboration continued until 1954 when Porter left Cambridge.  During 1949 there was an exciting period when the method was applied to a wide variety of gaseous substances. Porter still remembers the first appearance of the absorption spectra of new, transient substances in time resolved sequence, as they gradually appeared under the safelight of a dark room, as one of the most rewarding experiences of his life.  His subsequent work has been mainly concerned with showing how the flash-photolysis method can be extended and applied to many diverse problems of physics, chemistry and biology. He has made contributions to other techniques, particularly that of radical trapping and matrix stabilisation.  After a short period at the British Rayon Research Association, where he applied the new methods to practical problems of dye fading and the phototendering of fabrics, he went, in 1955, to the University of Sheffield, as Professor of Physical Chemistry, and later as Head of Department and Firth Professor. In 1966 he became Director and Fullerian Professor of Chemistry at the Royal Institution in succession to [Sir Lawrence Bragg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1915/index.html). He is Director of the Davy Faraday Research Laboratory of the Royal Institution. Here his research group is applying flash photolysis to the problem of photosynthesis and is extending these techniques into the nanosecond region and beyond.  Porter became a fellow of Emmanuel College, Cambridge, in 1952, and an honorary fellow in 1967. He was elected a Fellow of the Royal Society in 1960 and awarded the Davy Medal in 1971. He received the Corday-Morgan Medal of the Chemical Society in 1955, and was Tilden Lecturer of the Chemical Society in 1958 and Liversidge Lecturer in 1969. He has been President of the Chemical Society since 1970. He is Visiting Professor of University College London since 1967, and Honorary Professor of the University of Kent at Canterbury since 1966.  Porter holds Honorary D.Sc.’s from the following Universities: 1968, Utah, Salt Lake City (U.S.A.), Sheffield; 1970, East Anglia, Surrey and Durham; 1971, Leeds, Leicester, Heriot-Watt and City University. He is an honorary member of the New York Academy of Sciences (1968) and of the Academy “Leopoldina”. He is President of the Comité International de Photobiologie since 1968. He was Knighted in January 1972.  He is interested in communication between scientists of different disciplines and between the scientist and the non-scientist, and has contributed to many films and television programmes. His main recreation is sailing. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0424 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0425** |
| **Biographical** | Robert Sanderson Mulliken was born in Newburyport, Massachusetts, on June 7, 1896, the son of Samuel Parsons Mulliken, Professor of Organic Chemistry, and Katherine W. Mulliken. He married Mary Helen von Noè, December 24, 1929. Their children are Lucia Maria (Mrs. John P. Heard) and Valerie Noè.  Mulliken took a B.Sc. Degree in 1917 at the Massachusetts Institute of Technology, Cambridge, Mass., and a Ph.D. degree at the University of Chicago, Ill., in 1921.  Mulliken has been deeply interested in valence theory and molecular structure. His earlier work on isotopes and on diatomic band spectra was followed by theoretical systematization of the electronic states of molecules, mainly in terms of the idea of molecular orbitals. This included work on electronegativities, dipole moments, and valence-state energies. A subsequent series of papers on the theoretical interpretation of absolute intensities of electronic spectra led him to computations on conjugated organic molecules, and to the quantum-mechanical statement of the concept of hyperconjugation. There followed work on quantum-mechanical questions underlying molecular-orbital theory and on the use of interatomic overlap integrals as measures of bond energies. His more recent work has dealt extensively with the structure and spectra of molecular complexes, on the one hand, and on the other hand (extending and developing earlier work) with the structure and spectra of hydrogen, helium, nitrogen and other small molecules.  Mulliken was National Research Council Fellow, University of Chicago, and Harvard University, 1921-1925; Guggenheim Fellow, Germany and Europe, 1930 and 1932-1933; Fulbright Scholar, Oxford University, 1952-1954; Visiting Fellow, St. John’s College, Oxford, 1952-1953; Junior Chemical Engineer, Bureau of Mines, U.S. Department of Interior, Washington, D.C. 1917-1918; Assistant in Rubber Research, New Jersey Zinc Company, Pennsylvania, 1919.  His academic career includes the following positions: Assistant Professor of Physics, Washington Square College, New York University, 1926-1928; Associate Professor of Physics, University of Chicago, 1928-1931; Professor of Physics, University of Chicago, 1931-1961, and Chemistry, 1961; Ernest de Witt Burton Distinguished Service Professor, University of Chicago, 1956-1961; Distinguished Service Professor of Physics and Chemistry, University of Chicago, since 1961; Distinguished Research Professor of Chemical Physics, Florida State University (Jan.-March), since 1964. Other professional positions held: Director, Editorial Work and Information, Plutonium Project, University of Chicago, 1942-1945; Scientific Attaché, U.S. Embassy, London, 1955; Baker Lecturer, Cornell University, 1960; Silliman Lecturer, Yale University, Spring, 1965.  Mulliken received honorary degrees at Columbia University, 1939 (Sc.D.); the University of Stockholm, 1960 (Ph.D.); Marquette University, 1967 (Sc.D.); Cambridge University, 1967 (Sc.D.) ; and he holds several professional awards and honours of which a few are listed here: Bronze Medal Award, University of Liege, 1948; Peter Debye Award, California Section of the American Chemical Society, 1963; Willard Gibbs Medal, Chicago Section of the American Chemical Society, 1965; Gold Medal Award for Scientific Achievement, City College Chemistry Alumni Association, and 15th Bicentennial Lecturer, City College of New York, 1965.  He is a Member of the American Academy of Arts and Sciences, American Chemical Society, American Philosophical Society, National Academy of Sciences, Cosmos Club (Washington, D.C.), Quadrangle Club (Chicago, Ill.); a Fellow of the American Physical Society and the American Academy for the Advancement of Science; an Honorary Fellow of the Chemical Society of Great Britain (London) and the Indian National Academy of Science; a Foreign Member of the Royal Society of Great Britain; an Honorary Member of the Société de Chimie Physique; and a Corresponding Member of the Société Royale des Sciences de Liége.  His recreational interests include: driving a car, oriental rugs, and art. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0425 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0426** |
| **Biographical** | Robert Burns Woodward was born in Boston on April 10th, 1917, the only child of Margaret Burns, a native of Glasgow, and Arthur Woodward, of English antecedents, who died in October, 1918, at the age of thirty-three.  Woodward was attracted to chemistry at a very early age, and indulged his taste for the science in private activities throughout the period of his primary and secondary education in the public schools of Quincy, a suburb of Boston. In 1933, he entered the Massachusetts Institute of Technology, from which he was excluded for inattention to formal studies at the end of the Fall term, 1934. The Institute authorities generously allowed him to re-enroll in the Fall term of 1935, and he took the degrees of Bachelor of Science in 1936 and Doctor of Philosophy in 1937. Since that time he has been associated with Harvard University, as Postdoctoral Fellow (1937-1938), Member of the Society of Fellows (1938-1940), Instructor in Chemistry (1941-1944), Assistant Professor (1944-1946), Associate Professor (1946-1950), Professor (1950-1953), Morris Loeb Professor of Chemistry (1953-1960), and Donner Professor of Science since 1960. In 1963 he assumed direction of the Woodward Research Institute at Basel. He was a member of the Corporation of the Massachusetts Institute of Technology (1966-1971), and he is a Member of the Board of Governors of the Weizmann Institute of Science.  Woodward has been unusually fortunate in the outstanding personal qualities and scientific capabilities of a large proportion of his more than two hundred and fifty collaborators in Cambridge, and latterly in Basel, of whom more than half have assumed academic positions. He has also on numerous occasions enjoyed exceptionally stimulating and fruitful collaboration with fellow-scientists in laboratories other than his own. His interests in chemistry are wide, but the main arena of his first-hand engagement has been the investigation of natural products – a domain he regards as endlessly fascinating in itself, and one which presents unlimited and unparalleled opportunities for the discovery, testing, development and refinement of general principles.  Prof. Woodward holds more than twenty honorary degrees of which only a few are listed here: D.Sc. Wesleyan University, 1945; D. Sc. Harvard University, 1957; D. Sc. University of Cambridge (England), 1964; D. Sc. Brandeis University, 1965; D. Sc. Israel Institute of Technology (Haifa), 1966; D.Sc. University of Western Ontario (Canada), 1968;D.Sc. Universite de Louvain (Belgium), 1970.  Among the awards presented to him are the following: John Scott Medal (Franklin Institute and City of Philadelphia), 1945; Backeland Medal (North Jersey Section of the American Chemical Society), 1955; Davy Medal (Royal Society), 1959; Roger Adams Medal (American Chemical Society), 1961; Pius XI Gold Medal (Pontifical Academy of Sciences), 1969; National Medal of Science (United States of America), 1964; Willard Gibbs Medal (Chicago Section of the American Chemical Society), 1967; Lavoisier Medal (Societe Chimique de France), 1968; The Order of the Rising Sun, Second Class (His Majesty the Emperor of Japan), 1970; Hanbury Memorial Medal (The Pharmaccutical Society of Great Britain), 1970; Pierre Brnylants Medal (Université de Louvain), 1970.  Woodward is a member of the National Academy of Sciences; Fellow of the American Academy of Arts and Sciences; Honorary Member of the German Chemical Society; Honorary Fellow of The Chemical Society; Foreign Member of the Royal Society; Honorary Member of the Royal Irish Academy; Corresponding Member of the Austrian Academy of Sciences; Member of the American Philosophical Society; Honorary Member of the Belgian Chemical Society; Honorary Fellow of the Indian Academy of Sciences; Honorary Member of the Swiss Chemical Society; Member of the Deutsche Akademie der Naturforscher (Leopoldina); Foreign Member of the Accademia Nazionale dei Lincei; Honorary Fellow of the Weizmann Institute of Science; Honorary Member of the Pharmaceutical Society of Japan.  Woodward married Irja Pullman in 1938, and Eudoxia Muller in 1946. He has three daughters: Siiri Anne (b. 1939), Jean Kirsten (b. 1944), and Crystal Elisabeth (b. 1947), and a son, Eric Richard Arthur (b. 1953). |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0426 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0427** |
| **Biographical** | Dorothy Crowfoot was born in Cairo on May 12th, 1910 where her father, John Winter Crowfoot, was working in the Egyptian Education Service. He moved soon afterwards to the Sudan, where he later became both Director of Education and of Antiquities; Dorothy visited the Sudan as a girl in 1923, and acquired a strong affection for the country. After his retirement from the Sudan in 1926, her father gave most of his time to archaeology, working for some years as Director of the British School of Archaeology in Jerusalem and carrying out excavations on Mount Ophel, at Jerash, Bosra and Samaria.  Her mother, Grace Mary Crowfoot (born Hood) was actively involved in all her father’s work, and became an authority in her own right on early weaving techniques. She was also a very good botanist and drew in her spare time the illustrations to the official Flora of the Sudan. Dorothy Crowfoot spent one season between school and university with her parents, excavating at Jerash and drawing mosaic pavements, and she enjoyed the experience so much, that she seriously considered giving up chemistry for archaeology.  She became interested in chemistry and in crystals at about the age of 10, and this interest was encouraged by Dr. A.F. Joseph, a friend of her parents in the Sudan, who gave her chemicals and helped her during her stay there to analyse ilmenite. Most of her childhood she spent with her sisters at Geldeston in Norfolk, from where she went by day to the Sir John Leman School, Beccles, from 1921-28. One other girl, Norah Pusey, and Dorothy Crowfoot were allowed to join the boys doing chemistry at school, with Miss Deeley as their teacher; by the end of her school career, she had decided to study chemistry and possibly biochemistry at university.  She went to Oxford and Somerville College from 1928-32 and became devoted to Margery Fry, then Principal of the College. For a brief time during her first year, she combined archaeology and chemistry, analysing glass tesserae from Jerash with E.G.J. Hartley. She attended the special course in crystallography and decided, following strong advice from F.M. Brewer, who was then her tutor, to do research in X-ray crystallography. This she began for part II Chemistry, working with H.M. Powell, as his first research student on thallium dialkyl halides, after a brief summer visit to Professor Victor Goldschmidt’s laboratory in Heidelberg.  Her going to Cambridge from Oxford to work with J.D. Bernal followed from a chance meeting in a train between Dr. A.F. Joseph and Professor Lowry. Dorothy Crowfoot was very pleased with the idea; she had heard Bernal lecture on metals in Oxford and became, as a result, for a time, unexpectedly interested in metals; the fact that in 1932 he was turning towards sterols, settled her course.  She spent two happy years in Cambridge, making many friends and exploring with Bernal a variety of problems. She was financed by her aunt, Dorothy Hood, who had paid all her college bills, and by a £75 scholarship from Somerville. In 1933, Somerville, gave her a research fellowship, to be held for one year at Cambridge and the second at Oxford. She returned to Somerville and Oxford in 1934 and she has remained there, except for brief intervals, ever since. Most of her working life, she spent as Official Fellow and Tutor in Natural Science at Somerville, responsible mainly for teaching chemistry for the women’s colleges. She became a University lecturer and demonstrator in 1946, University Reader in X-ray Crystallography in 1956 and Wolfson Research Professor of the Royal Society in 1960. She worked at first in the Department of Mineralogy and Crystallography where H.L. Bowman was professor. In 1944 the department was divided and Dr. Crowfoot continued in the subdepartment of Chemical Crystallography, with H.M. Powell as Reader under Professor [C.N. Hinshelwood](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1956/index.html).  When she returned to Oxford in 1934, she started to collect money for X-ray apparatus with the help of [Sir Robert Robinson](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1947/index.html). Later she received much research assistance from the Rockefeller and Nuffield Foundations. She continued the research that was begun at Cambridge with Bernal on the sterols and on other biologically interesting molecules, including insulin, at first with one or two research students only. They were housed until 1958 in scattered rooms in the University museum. Their researches on penicillin began in 1942 during the war, and on vitamin B12 in 1948. Her research group grew slowly and has always been a somewhat casual organisation of students and visitors from various universities, working principally on the X-ray analysis of natural products.  Dorothy Hodgkin took part in the meetings in 1946 which led to the foundation of the International Union of Crystallography and she has visited for scientific purposes many countries, including China, the USA and the USSR. She was elected a Fellow of the Royal Society in 1947, a foreign member of the Royal Netherlands Academy of Sciences in 1956, and of the American Academy of Arts and Sciences (Boston) in 1958.  In 1937 she married Thomas Hodgkin, son of one historian and grandson of two others, whose main field of interest has been the history and politics of Africa and the Arab world, and who is at present Director of the Institute of African Studies at the University of Ghana, where part of her own working life is also spent. They have three children and three grandchildren. Their elder son is a mathematician, now teaching for a year at the University of Algiers, before taking up a permanent post at the new University of Warwick. Their daughter (like many of her ancestors) is an historian-teaching at girls’ secondary school in Zambia. Their younger son has spent a pre-University year in India before going to Newcastle to study Botany, and eventually Agriculture. So at the present moment they are a somewhat dispersed family. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0427 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0428** |
| **Biographical** | Karl Ziegler was born in Helsa near Kassel in Germany, on November 26, 1898. He graduated in 1920 under Prof. von Auwers at the University of Marburg/Lahn, and qualified as a lecturer in 1923. After working for a short period at the University of Frankfurt/Main, he spent 10 years as a lecturer at Heidelberg.  His research work in the field of radicals with trivalent carbon and his syntheses of multi-membered ring systems earned him the Liebig Medal in 1935. In 1936 he became Professor and Director of the Chemisches Institut at the University of Halle/Saale. In the same year he lectured as a visiting professor at Chicago University.  From 1943 until 1969 he was Director of the Max-Planck-Institut fur Kohlenforschung (formerly known as the Kaiser-Wilhelm-Institut fur Kohlenforschung) in Mülheim/Ruhr. He continues his active association with the Institute in his capacity of Scientific Member. After the war he was instrumental in the foundation of the Gesellschaft Deutscher Chemiker, whose president he was for five years. From 1954 until 1957 he was president of the Deutsche Gesellschaft für Mineralölwissenschaft und Kohlechemie.  His research work at the above-mentioned institute over the past 20 years on syntheses and reactions in the chemistry of organ aluminium compounds, his discovery of organometallic mixed catalysts for the polymerization of olefins (*e.g*. the synthesis of high-density polyethylene) – all these are widely known.  Many honours have been bestowed upon him. He holds honorary doctorates at the Technische Hochschulen of Hannover and Darmstadt, and of the Universities of Heidelberg and Giessen. He has received the Liebig Medal from the Verein Deutscher Chemiker, the Carl Duisberg Plakette from the Gesellschaft Deutscher Chemiker, the Carl Engler Medal from the Deutsche Gesellschaft für Mineralölwissenschaft und Kohlechemie, and the Lavoisier Medal from the Société Chimique de France. The Werner von Siemens Foundation awarded him the Siemens Ring. His Nobel Prize was followed by the award of a distinguished order by the German Federal Government, the Swinburne Medal by the Plastics Institute, London, the International Synthetic Rubber Medal by *Rubber and Plastics Age*, London, and, in 1971, the Carl Dietrich Harries Plakette by the Deutsche Kautschuk Gesellschaft, as well as the Wilhelm Exner Medal by the Österreichischer Gewerbeverein, Vienna. He is an honorary senator of the Max-Planck Gesellschaft, founder president (1970-1972 ) of the Rheinisch-Westfalische Akademie der Wissenschaften, as well as member or honorary member of various German and foreign scientific societies and academies. In 1971, The Royal Society, London, elected him as a Foreign Member.  On the death of [Otto Hahn](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1944/index.html), Karl Ziegler was appointed the latter’s successor to the Order “Pour le mérite für Wissenschaften und Künste”.  Karl Ziegler has been married to Maria Kurtz, since 1922. His daughter, Marianne Witte, is a doctor of medicine and is married to the chief physician of a children’s hospital in the Ruhr. His son, Dr. Erhart Ziegler, is a physicist and patent attorney. Karl Ziegler has five grandchildren by his daughter and five by his son. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0428 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0429** |
| **Biographical** | Giulio Natta was born at Imperia on February 26, 1903. He graduated in Chemical Engineering at the Polytechnic of Milan in 1924 and passed the examinations entitling him to teach there in 1927. In 1933 he was established on the staff of Pavia University as a full professor and at the same time was appointed director of the Institute of General Chemistry at that University, where he stayed till 1935, that is until he was appointed full professor in physical chemistry at the University of Rome. From 1936 to 1938 he was full professor and director of the Institute of Industrial Chemistry at the Polytechnic of Turin. He has been full professor and director of the Department of Industrial Chemistry at the Milan Polytechnic since 1938.  Now a world famous scientist, Prof. Natta began his career with a study of solids by means of X-rays and electron diffraction. He then used the same methods for studying catalysts and the structure of some high organic polymers (the latter from 1934). His kinetic research on methanol synthesis, on selective hydrogenation of unsaturated organic compounds and on oxosynthesis led to an understanding of the mechanism of these reactions and to an improvement in the selectivity of catalysts.  In 1938 Prof. Natta began to study the production of synthetic rubber in Italy; he took part in research work on butadiene and was the first to accomplish physical separation of butadiene from 1-butadiene by a new method of extractive distillation.  In 1938 he began to investigate the polymerisation of olefins and the kinetics of subsequent concurrent reactions. In 1953, with financial aid from a large Italian chemical company, Montecatini, Prof. Natta extended the research conducted by Ziegler on organometallic catalysts to the stereospecific polymerization, thus discovering new classes of polymers with a sterically ordered structure, *viz*. isotactic, syndiotactic and di-isotactic polymers and linear non branched olefinic polymers and copolymers with an atactic (or sterically nonordered) structure. These studies, which were developed for industrial application in Montecatini’s laboratories, led to the realisation of a thermoplastic material, isotactic polypropylene, which Montecatini were the first to produce on an industrial scale, in 1957, in their Ferrara plant. This product has been marketed successfully as a plastic material, by the name of Moplen, as a synthetic fibre, by the name of Meraklon, as a monofilament by the name of Merakrin and as packing film, by the name of Moplefan.  By X-ray investigations, Prof. Natta has also succeeded in determining the exact arrangement of chains in the lattice of the new crystalline polymers he has discovered.  No less important is his later research which led to the synthesis of completely new elastomers, in two different ways: by polymerization of butadiene into *cis*-1,4 polymers with a very high degree of steric purity and by copolymerization of ethylene with other a-olefins (propylene), originating extremely interesting materials such as saturated synthetic rubbers. The vulcanisation of these rubbers was made possible by the usual methods used for natural rubber, with the introduction of unsaturated monomeric units (terpolymers containing ethylene and propylene). The processes for the asymmetric synthesis, which allow the production of optically active macromolecules from optically inactive monomers, are of great scientific importance, due to their similarity to the natural biological processes. Other interesting results obtained by Natta in the field of macromolecular chemistry concern the synthesis of crystalline alternating copolymers of different couples of monomers and the synthesis of various sterically ordered polymers of non-hydrocarbon monomers.  Prof. Natta’s scientific and technical activity is documented in over 700 published papers, of which about 500 concern stereoregular polymers, and by a large number of patents in many different countries. In 1961 he was made an honorary life member of the New York Academy of Sciences of which he had been a fellow since 1958. In 1955 he became a “national member” of the Accademia dei Lincei; he is also a member of the Istituto Lombardo di Scienze e Lettere and of the Accademia delle Scienze of Turin. He was made honorary member of the Austrian (1960), Belgian (that awarded him the STAS medal) (1962), and Swiss (1963) Chemical Societies. Professor Natta received a gold medal from the town of Milan (1960), from the President of the Italian Republic (1961, reserved to those who gained merits in the field of school, culture and art), the first international gold medal of the synthetic rubber industry (1961); a gold medal from the Milan district (1962) and from the Society of Plastic Engineers (New York, 1963), the Perrin medal from the French Chemical Physical Society, and the Lavoisier medal from the Chemical Society of France (both in 1963), the Perkin gold medal of the English Society of Dyers and Colourists (1963), the John Scott award from the Board of Directors of the City Trust of Philadelphia, and the Medal “Leonardus Vincius Florentinus Doctor Ingenieurs” of FIDIIS, Paris (1971). The Turin University gave him an honorary degree in pure chemistry, and in 1963 Prof. Natta received an honorary degree from Mainz University.  Prof. Natta is a honorary member of the Industrial Chemical Society of Paris (1966) and of the Chemical Society of London (1970); an honorary member of the Rotary Club; associated foreign member of the Académie des Sciences de l’Institut de France (1964); member of the National Academy of XL, Rome (1964); joined member of the International Academy of Astronautics, Paris (1965); foreign member of the Academy of Sciences of Moscow, U.S.S.R. (1966); honorary president of the Italian Section of the Society of Plastics Engineers (SPE). He holds the following awards and honorary degrees: gold medal of the Union of Italian Chemists (1964); gold medal “Lomonosov” of the Moscow Academy of Sciences (1969); the “Carl-Dietrich-Harries-Plakette, of the Deutsche Kautschuk Gesellschaft, Frankfurt/Main (1971); honorary degrees from the University of Genoa (1964), the Polytechnic Institute of Brooklyn, New York (1964), the Catholic University of Louvain, Belgium (1965), and in 1971 from ESPCI, University of Paris. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0429 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0430** |
| **Biographical** | Max Ferdinand Perutz was born in Vienna on May 19th, 1914. Both his parents, Hugo Perutz and Dely Goldschmidt, came from families of textile manufacturers who had made their fortune in the 19th century by the introduction of mechanical spinning and weaving into the Austrian monarchy. He was sent to school at the Theresianum, a grammar school derived from an officers academy of the days of the empress Maria Theresia, and his parents suggested that he should study law in preparation for entering the family business. However, a good schoolmaster awakened his interest in chemistry, and he had no difficulty in persuading his parents to let him study the subject of his choice.  In 1932, he entered Vienna University, where he, in his own words, “wasted five semesters in an exacting course of inorganic analysis”. His curiosity was aroused, however, by organic chemistry, and especially by a course of organic biochemistry, given by F. von Wessely, in which Sir F.G. Hopkins’ work at Cambridge was mentioned. It was here that Perutz decided that Cambridge was the place where he wanted to work for his Ph.D. thesis. With financial help from his father he became a research student at the Cavendish Laboratory in Cambridge under J.D. Bernal in September 1936, and he has stayed at Cambridge ever since.  After Hitler’s invasion in Austria and Czechoslovakia, the family business was expropriated, his parents became refugees, and his own funds were soon exhausted. He was saved by being appointed research assistant to [Sir Lawrence Bragg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1915/index.html), under a grant from the Rockefeller Foundation, on January 1st, 1939. The grant continued, with various interruptions due to the war, until 1945, when Perutz was given an Imperial Chemical Industries Research Fellowship. In October 1947, he was made head of the newly constituted Medical Research Council Unit for Molecular Biology, with J.C. Kendrew representing its entire staff. He continued holding this post until he was made Chairman of the Medical Research Council Laboratory of Molecular Biology, in March 1962. His collaboration with Sir Lawrence Bragg has continued through all these years.  The scientific work of Perutz on the structure of haemoglobin started as a result of a conversation with F. Haurowitz in Prague, in September 1937. G.S. Adair made him the first crystals of horse haemoglobin, and Bernal and I. Fankuchen showed him how to take X-ray pictures and how to interpret them. Early in 1938, Bernal, Fankuchen, and Perutz [*Nature*, 141 (1938) 523] published a joint paper on X-ray diffraction from crystals of haemoglobin and chymotrypsin. The chymotrypsin crystals were twinned and therefore difficult to work with, and so Perutz continued with haemoglobin. D. Keilin, then Professor of Biology and Parasitology at Cambridge, soon became interested in the work and provided Perutz and his colleagues with the biochemical laboratory facilities which they lacked at the Cavendish. Thus from 1938 until the early fifties the protein chemistry was done at Keilin’s Molteno Institute and the X-ray work at the Cavendish, with Perutz busily bridging the gap between biology and physics on his bicycle. The rest of the story is well-known and forms the subject of his Nobel discourse.  Perutz has persued one sideline concerned with glaciers, studying their crystal texture and mechanism of flow, but this was mainly an excuse for working in the mountains: he is a keen mountaineer, his other recreations being walking, skiing and gardening. Scientifically, his overwhelming interest lies on the side of molecular biology. He is grateful for having had the good fortune of being joined by colleagues of great ability, several of whom have now been honoured with the Nobel Prize at the same time as Perutz himself. Kendrew came in 1946, [Crick](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1962/index.html) in 1948, and [Watson](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1962/index.html) arrived as a visitor in 1951. Recently [F. Sanger](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1958/index.html), who received the Nobel Prize in 1958, also joined forces with them. Perutz is extremely happy at the generous recognition given by the Swedish Academy of Sciences and the Royal Karolinska Institute to their great common adventures and hopes that it will spur them to new endeavours.  Perutz, who is a Fellow of the Royal Society, was made Commander of the British Empire in 1962. He is also an honorary member of the American Academy of Arts and Sciences.  In 1942, Perutz married Gisela Peiser. They have two children, Vivien (b. 1944) and Robin (b. 1949). |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0430 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0431** |
| **Biographical** | John Cowdery Kendrew was born on 24th March, 1917, in Oxford. His father, Wilfrid George Kendrew, was Reader in Climatology in the University of Oxford; his mother, Evelyn May Graham (Sandberg) Kendrew, was an art historian, for many years resident in Florence, Italy, where she published works on the Italian Primitives under the *nom de plume* Evelyn Sandberg Vavals.  He was educated at the Dragon School, Oxford (1923-1930) and Clifton College, Bristol (1930-1936), and went to Trinity College, Cambridge in 1936 as a Major Scholar. He graduated in Chemistry in 1939, and spent the first few months of the war doing research on reaction kinetics in the Department of Physical Chemistry at Cambridge under the supervision of Dr. E.A. Moelwyn-Hughes. He then became a member of the Air Ministry Research Establishment (later Telecommunication Research Establishment) and worked on radar. In 1940 he joined the staff of Sir Robert Watson-Watt (Scientific Adviser to the Air Ministry) and for the rest of the war was engaged in operational research at Royal Air Force headquarters, successively in Coastal Command, Middle East, and South East Asia (where he was Scientific Adviser to the Allied Air Commander-in-Chief); he held the honorary rank of Wing Commander R.A.F.  During the war years his interests became more biological, and largely owing to the influence of J.D. Bernal and [L. Pauling](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1954/index.html) he decided to work on the structure of proteins. He returned to Cambridge in 1946 and, in the Cavendish Laboratory, began a collaboration with Max Perutz, under the direction of [Sir Lawrence Bragg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1915/index.html). He took his Ph.D. degree in 1949 and his Sc.D. in I962. He and Perutz were the first two members of the Medical Research Council Unit at the Cavendish Laboratory, which has now achieved separate existence as the Medical Research Council Laboratory of Molecular Biology; he was Deputy Director of the former, and is now Deputy Chairman of the latter, and Director of its Division of Structural Studies.  He became a Fellow of Peterhouse in 1947, Reader at the Davy-Faraday Laboratory of the Royal Institution in London in 1954, Fellow of the Royal Society in 1960, and an honorary member of the American Society of Biological Chemists in 1962. Since 1960 he has been (part-time) Deputy to the Chief Scientific Adviser, Ministry of Defence. He is Founder and Editor-in-Chief of the *Journal of Molecular Biology*, and Honorary Secretary of the British Biophysical Society. In 1962, he was made Companion of the British Empire.  His research has been in the field of protein structure, and has mostly centred on the X-ray analysis of myoglobin. This project culminated in the production of a three-dimensional model of myoglobin at 6Å resolution in 1957, and an almost complete structure in 1960.  Kendrew is unmarried. His recreations are music, history of art (following his mother’s footsteps particularly Italian art), and travelling in Italy. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0431 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0432** |
| **Biographical** | Melvin Calvin was born in St. Paul, Minnesota, April 8, 1911, of Russian emigrant parents. He received the B.S. degree in Chemistry in 1931 at the Michigan College of Mining and Technology, and the Ph.D. degree in Chemistry from the University of Minnesota in 1935. He spent the academic years 1935-1937 at the University of Manchester, England. He began his academic career at the University of California at Berkeley in 1937, as an instructor, and has been a full professor since 1947. He has served as Director of the big-organic chemistry group in the Lawrence Radiation Laboratory since 1946. This group became the Laboratory of Chemical Biodynamics in 1960.  He has been the recipient of a number of medals, awards, and lectureships, and holds membership in numerous learned societies. In addition, he has been elected to the National Academy of Sciences, the American Philosophical Society, the American Academy of Arts and Sciences, the Royal Society of London, the Royal Netherlands Academy of Sciences and Letters, and the German Academy of Scientists, Leopoldina. He holds honorary D.Sc. degrees from Michigan College of Mining and Technology, the University of Nottingham, Oxford University, and Northwestern University.  His scientific life began with a thesis on the electron affinity of halogens, done under the direction of Professor George A. Glocker at the University of Minnesota and completed in 1935. The following two-year postdoctoral period was spent with Professor Michael Polanyi at the University of Manchester, at which time his interest in coordination catalysis, particularly metalloporphyrins, was awakened. This interest is still paramount and has resulted both in theoretical *(The Chemistry of Metal Chelate Compounds)* and practical (oxygen-carrying synthetic chelate compounds) applications. The investigation of the electronic, photoelectric and photochemical behaviour of such materials now occupies a good fraction of his time.  Upon coming to Berkeley, California, at the invitation of Professor Gilbert N. Lewis, his interest turned to general theoretical aspects of organic molecular structure and behaviour.There were two prime publications of this period. The first, with Professor Gilbert N. Lewis, was on *The Color of Organic Substances*, and the second, with Professor G.E.K. Branch, was *The Theory of Organic Chemistry*. It was from these men that the fundamental interest in the behaviour of organic molecules in their most detailed terms was derived.  This interest combined with the earlier one on the catalytic behaviour of coordination compounds were the natural parents of his present preoccupation with the problem of photosynthesis. The ready availability of carbon-14 which began in 1945 channeled the early work to development of techniques for its use *(Isotopic Carbon)* and its application to the exploration of photosynthetic carbon dioxide reduction *(The Path of Carbon in Photosynthesis)*.  An extension of his interest from here into the general problems of biology was unavoidable, and thus his laboratory is at present peopled by emigrants from all areas of science on both sides of chemistry – physics on the one hand and biology on the other.  Dr. Calvin is married to the former Genevieve Jemtegaard, daughter of Norwegian emigrant parents, they have two daughters, Elin and Karole, and one son, Noel. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0432 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | 0433 |
| **Biographical** | Willard Frank Libby was born in Grand Valley, Colorado, on 17th December, 1908, to Ora Edward Libby and his wife Eva May (*née* Rivers).  He attended grammar and high schools near Sebastopol, California, between 1913 and 1926, moving to the University of California at Berkeley in 1927, where he studied till 1933, taking his B.Sc. and Ph.D. degrees in 1931 and 1933 respectively. He was appointed Instructor in the Department of Chemistry at California University (Berkeley) in 1933 and during the next ten years was promoted successively to Assistant and then Associate Professor of Chemistry. He was awarded a Guggenheim Memorial Foundation Fellowship in 1941 and elected to work at Princeton University, but on 8th December, 1941, this Fellowship was interrupted for war work on America’s entry into World War II, and Libby went to Columbia University on the Manhattan District Project, on leave from the Department of Chemistry, California University, till 1945.  At the end of the war, in 1945, Libby accepted the post of Professor of Chemistry in the Department of Chemistry and Institute for Nuclear Studies (now the Enrico Fermi Institute for Nuclear Studies) of University of Chicago, remaining there till his appointment by President Eisenhower on 1st October, 1954, as a member of the U.S. Atomic Energy Commission.  This appointment was renewed by the President for a further five-year term on 19th June, 1956, but Libby resigned from it on 30th June, 1959, to become Professor of Chemistry in the University of California at Los Angeles, being appointed Director of the Institute of Geophysics and Planetary Physics on 1st January, 1962.  Libby has performed a wide range of scientific advisory and technical consultant work with industrial firms associated with the Institute for Nuclear Studies, as well as with defence departments, scientific organizations and universities. From 1945 to 1952 he was a Member of the Committee of Senior Reviewers of the Atomic Energy Commission; from 1950 to 1954 he was a Member of the Commission’s General Advisory Committee and was re-appointed to this Committee by President Eisenhower in June, 1960. He has been a Member of the Plowshare Advisory Committee since 1959; a Guggenheim Memorial Foundation Fellow in 1941, 1951 and 1959-1962; a Member of the Advisory Board of the Guggenheim Memorial Founclation, being re-elected in May, 1960, for a further term of four years; a Research Associate of the Carnegie Institute of Washington Geophysical Laboratory from 1954 to 1959. He has served as Consultant to the Office of Civil and Defence Mobilization (1959-1961) and also as a Member of the Advisory Council to the Coordinator of Atomic Activities of the State of California since 1959. He is, since 1963, Director of the Douglas Aircraft Company, and Member of the National Science Foundation’s General Commission on Science, the Federal Government and the Academic Institution.  Libby is a Physical Chemist, and specialist in radiochemistry, particularly hot atom chemistry, tracer techniques, and isotope tracer work. He became well-known at University of Chicago for his work on natural carbon-14 (radiocarbon) and its use in dating archaeological artifacts, and natural tritium, and its use in hydrology and geophysics.  Besides the Nobel Prize in Chemistry for 1960, he received other distinctions, including the Research Corporation Award for 1951 for the radiocarbon dating technique; the Chandler Medal of Columbia University for outstanding achievement in the field of chemistry (1954); the American Chemical Society Award for Nuclear Applications in Chemistry (1956); the Elliott Cresson Medal of the Franklin Institute (1957); the American Chemical Society’s Willard Gibbs Medal Award (1958); the Albert Einstein Medal Award (1959); the Day Medal of the Geological Society of America (1961).  Libby’s book, *Radiocarbon Dating*, was published by the University of Chicago Press in 1952, and a second edition appeared in 1955. He is also the author of numerous articles, which appeared principally in scientific journals. Since 1960 he is member of the Editorial Board of the *Proceedings of the National Academy of Sciences*, and since 1962 member of the Editorial Board of *Science*.  Professor Libby holds memberships of numerous learned societies in the United States; he is also Member of the Heidelberg Academy of Sciences, of the Bolivian Society of Anthropology, and is Foreign Member of the Royal Swedish Academy of Sciences (1960).  He is married to the former Leonor Hickey of King City, California. They have twin daughters Janet and Susan (b. 1945) |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0433 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0434** |
| **Biographical** | Jaroslav Heyrovsky was born in Prague on 20th December, 1890, the fifth child of Leopold Heyrovsky, Professor of Roman Law at the Czech University of Prague, and his wife Clara, *née* Hanl.  He obtained his early education at secondary school till 1909 when he began his study of chemistry, physics and mathematics at the Czech University, Prague. From 1910 to 1914 he continued his studies at University College, London, under Professors [Sir William Ramsay](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1904/index.html), W.C.Mc.C. Lewis and F.G. Donnan, taking his B.Sc. degree in 1913. He was particularly interested in working with Professor Donnan, on electrochemistry.  During the First World War Heyrovsky did his war service in a military hospital as dispensing chemist and radiologist, which enabled him to continue his studies and to take his Ph.D. degree in Prague in 1918 and D.Sc. in London in 1921.  Heyrovsky started his university career as assistant to Professor B. Brauner in the Institute of Analytical Chemistry of the Charles University, Prague; he was promoted to Associate Professor in 1922 and in 1926 he became the first Professor of Physical Chemistry at this University.  Heyrovsky’s invention of the polarographic method dates from 1922 and he concentrated his whole further scientific activity on the development of this new branch of electrochemistry. He formed a school of Czech polarographers in the University, and was himself in the forefront of polarographic research.  In 1950 the Professor was appointed Director of the newly established Polarographic Institute which has been incorporated into the Czechoslovak Academy of Sciences since 1952.  Many universities and seats of learning have honoured Professor Heyrovsky. He was elected Fellow of University College, London, in 1927, and received honorary doctorates of the Technical University, Dresden, in 1955, the University of Warsaw in 1956, the University Aix-Marseille in 1959, and the University of Paris in 1960. He was granted honorary membership of the American Academy of Arts and Sciences, Boston, Mass., in 1933; of the Hungarian Academy of Sciences in 1955; the Indian Academy of Sciences, Bangalore, in 1955; the Polish Academy of Sciences, Warsaw, in 1962; was elected Corresponding Member of the German Academy of Sciences, Berlin, in 1955; member of the German Academy of Natural Scientists, Leopoldina (Halle-Saale) in 1956; Foreign Member of the Royal Danish Academy of Sciences, Copenhagen, in 1962; Vice-President of the International Union of Physics from 1951 to 1957; President and first honorary member of the Polarographic Society, London; honorary member of the Polarographic Society of Japan; honorary member of the Chemical Societies of Czechoslovakia, Austria, Poland, England and India.  In Czechoslovakia he was awarded the State Prize, First Grade, in 1951, and in 1955 the Order of the Czechoslovak Republic.  Heyrovsky has lectured on polarography in the United States of America in 1933, the USSR in 1934, England in 1946, Sweden in 1947, the People’s Republic of China in 1958, and in U.A.R. (Egypt) in 1960 and 1961.  In 1926 Professor Heyrovsky married Marie Koranová, and there are two children of the marriage, a daughter, Judith, and a son, Michael. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0434 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | 0**435** |
| **Biographical** | Frederick Sanger was born on August 13, 1918, at Rendcombe in Gloucestershire, the second son of Frederick Sanger, M.D., a medical practitioner and his wife Cicely. He was educated at Bryanston School and at St. John’s College, Cambridge, where he took his B.A. degree in natural sciences in 1939. Since 1940 he has carried out research in the Department of Biochemistry at Cambridge. From 1940 to 1943 he worked with Dr. A. Neuberger on the metabolism of the amino acid lysine and obtained a Ph.D. degree in 1943. From 1944 to 1951 he held a Beit Memorial Fellowship for Medical Research and since 1951 he has been a member of the External Staff of the Medical Research Council. His present position is Head of the Division of Protein Chemistry in the M.R.C. Laboratory for Molecular Biology at Cambridge.  Since 1943 his work has been concerned largely with problems related to the determination of the structure of proteins. These studies resulted in the determination of the structure of insulin.  Sanger was awarded the Corday-Morgan Medal and Prize of the Chemical Society in 1951. In 1954 he became a Fellow of the Royal Society and a Fellow of King’s College, Cambridge. He is an Honorary Foreign Member of the American Academy of Arts and Sciences; Honorary Member of the American Society of Biological Chemists, Member of the Academies of Science of Argentina and Brazil, Honorary Member of the Japanese Biochemical Society, and Corresponding Member of the Association Qulmica Argentina.  In 1940, he married Margaret Joan Howe; they have two sons and one daughter.  From [*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*, Chemistry 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 1958 **Addendum, May 2005** Following the work on insulin he developed further methods for studying proteins and particularly the active centres of some enzymes. Around 1960 he turned his attention to the nucleic acids, RNA and DNA. He developed methods for determining small sequences in RNA. The work culminated in the development of the “dideoxy” technique for DNA sequencing around 1975. This was a relatively rapid method and was used to determine the DNA sequence of the bacteriophage f*x* 174 of 5375 nucleotides in 1977, of human mitochrondrial DNA (16,338 nucleotides) and of bacteriophage l (48,500 nucleotides). The method has been improved and automated in other human genome (3 billion nucleotides).  Sanger retired in 1983.  *Frederick Sanger received a second* [*Nobel Prize in Chemistry in 1980*](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1980/index.html)*.*  *Frederick Sanger died on 19 November 2013.* |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0435 |
| **Interview** |  |
| Q9 | **Dr Frederick Sanger, welcome to this Nobel interview. You are one of the very few persons that have been awarded Nobel Prize twice. So I suppose that maybe you know how to do it. Many people would like to get Nobel Prize but they don’t. Can you share your secret with us?** |
|  | Well, I don’t think there are any secrets. First thing, you know, people ask me this sometimes and I tell them the first thing is to get a prize. And that’s much more difficult to get the first prize than to get the second one. Because if you’ve already got a prize then you can get facilities for work and you can get collaborators and everything is much easier. And I think I was lucky in that respect that I got my first prize when I was rather young, about 40, and most people when they get a prize they get a good job, they become professors or directors of a large group. And I didn’t because I wouldn’t be any good at that.  So really you’ve got to be bad at something, but I was quite good at doing research and I could go on doing research for another 20 years and I didn’t have to apply for grants and that sort of thing. Nowadays there’s a lot of people that when they’re doing science they have to apply for grants, spend a lot of time doing paperwork which I never had to do after I had won the first prize. So I had 20 years when I could just do what I wanted. And I started on a rather difficult project and spent all my time at it and I was very lucky at that time, succeeded in certain parts of the work. |
| Q2 | **But you switched your fields after the first prize?** |
|  | It wasn’t such a serious switch. My first work was on proteins, you know, things we’re actually made of. And the problem was to do what we call sequencing, actually measure the structure of the proteins, and I worked on proteins. And after that I attacked a much more difficult problem which was the nucleic acids and particularly the DNA. And I eventually came up with a method for sequencing DNA and learning about its structure and that’s what I got the second prize for, together with Wally Gilbert who was also working on the same thing.  And Paul Burke got the prize, the same work. But you shared the prize.  Frederick Sanger: Well yes, I got the prize the same year. |
| Q2 | **So you were working really on fundamental questions about life and where life comes from.** |
|  | That’s correct, yes. Rather fundamental questions which were quite mysterious when I started. When I started there were no sequences known at all either in proteins or in nucleic acids. I started about 1940, quite a long time ago. And started working on the proteins and I obtained a sequence of the first protein insulin that was ever done. And that was sequencing. And then after 1958 when I got the first prize I decided to attack the nucleic acids, which was a real challenge. And we did get a method for doing the sequencing. And it was about 1980 I got that prize. And now of course that same method is being used to do the human genome which there’s been a lot of interest in. And that is of course the ultimate thing to understand, the sequence and all the details of the DNA in human beings, and we hope that’s going to do a lot of good for medicine. |
| Q15 | **The laboratory in Cambridge that was working on human genome project bears your name also, Frederick Sanger’s Laboratory. So how do you view this development in modern biology?** |
|  | It’s very exciting I think. I think there’s going to be a lot of use for medicine if we can really understand DNA and you know it is very complicated. There’s 3,000 million letters, as it were, in the DNA and you’ve got to read all that. And of course there are a lot of people working on that now to try and decipher the means and how it works. And I think that’s going to be of use to medicine. But we don’t know exactly what the applications will be. There’s a lot of talk of course in newspapers and wild talk about we’re going to cure all sorts of diseases and things.  Many promises.  Frederick Sanger: But you don’t want to believe that yet. But I think it’s sensible to understand that, you know, if you understand the thing better and if you know how it works then it will be of use to medicine. |
| Q18 | **What do you think, do you understand the concerns or fears that people feel when reading about modern biology?** |
|  | Not really, no. I mean there are certain things of course you’ve got to think about. But as regards to the human genome the only thing that I’ve heard about is that some people may have to pay more for their insurance policies. And I don’t think that’s a very serious thing. I mean that’s a question for society to decide whether or what things should be used for. But surely it’s much more significant that one should consider what might happen, what good things might happen, that people might be more healthy and more happy. That’s what science is about, to try and improve conditions for humanity on this earth. And I think we should think about that ultimately. And against that you can worry a bit about what insurance you’re going to have to pay. And I think that’s for the insurance industry and not a very serious thing is it? |
| Q18 | **Modern molecular biology, it was very successful, not only in research and in science but also commercially very successful field, and biotechnology has developed since then with big commercial pressures. What do you think about that?** |
|  | Of course it’s I think essential thing that happens. I mean that is a question for society to decide, isn’t it? I mean that’s the way capitalism works, isn’t it, that people want to make money. And if they’re working on some good, useful work then that should be very agreeable. But it would be nice to know everything that’s going on and there is a lot of work going on now using the human genome work, which is kept secret now until it’s used.  So it’s not really the ethos of science.  Frederick Sanger: Not of fundamental science but it is understandable I think that people want to make money and we live in a capitalist world, don’t we, and we have to face up to it and it seems to work fairly well. |
| Q18 | **I have heard that at least in 1980 before your prize and about this time that you were quite reluctant to take in patents on discoveries in science.** |
|  | I never had an opportunity really. I worked for the Medical Research Council and we were not allowed to take patents. And anyhow I wouldn’t have wanted to I don’t think because I wouldn’t want to keep my work secret. I was paid by the Medical Research Council and I had, I was paid for life really by the Medical Research Council. And that was my reward for the work I did. I don’t think it would be fair to expect to keep my work secret. And it was up to the English tax payer who supported me, gave me this opportunity. And I think that was fair enough. |
| Q6 | **I have another question. Einstein, people say that Einstein had a strong religious belief and you’re a Quaker. What is the place of religion within your scientific philosophy?** |
|  | Well, that’s rather difficult to say I think. I was brought up as a Quaker. My father was a Quaker and I think I was never actually a sort of Quaker, I was never a member of the Society of Friends. And when I got to university and stuff and started studying science I gradually lost interest really in religion and Quakerism. But it has influenced I think my philosophy in two ways really. The Quakers are pacifists. They will not take human life. And I still go along with that, and I believe that.  And the Quakers are also very keen on truth and they believe in fundamental truth is one of the virtues. And I was brought up with that philosophy, that I must not tell lies when I was a little boy. And I think this is very important for scientists, because a scientist is really studying truth and you’ve got to be very certain about what you say is truthful. And I think this is a philosophy which is worthwhile because a lot of people I think nowadays are quite happy to say something that’s wrong and it can get them into difficulty and it can get other people into difficulty. I actually have a sort of fixation about it rather, even when somebody asks me, you know they say to me how are you Fred? And I say oh I’m terrible. I should say I’m fine thanks. It’s a standard thing.  It’s a convention.  Frederick Sanger: If I’m not, I find it a little difficult to say that. |
| Q6 | **You have now left science and you have been retired in 20 years as you said before, but you have developed some other interests as gardening and as you call it ‘messing about in boats’. What do you do when you’re messing about?** |
|  | Well, I used to do a bit of sailing and messing about really. I had a boat, a sailing boat, and I had a motor boat. Now I have given up sailing, I don’t do any sailing, but I still have a motor boat and I go on the river and mess about, just being on the river and cruising away from the roads is very pleasant. But gardening is what I do mostly and just living it.  Are you a successful gardener?  … I’m actually much busier, I think, than I was when I was working …  Frederick Sanger: No, I find it’s … no I’m actually much busier, I think, than I was when I was working because I’ve got time to do all sorts of things. I was going to do lots of different things like painting and building. And I used to do a lot of carpentry at one time, used to do a lot of carpentry. I find that there’s too many things to do. And I’ve got rather a large garden and that takes most of my time now.  So what is your biggest success in the garden?  Frederick Sanger: Biggest success? I don’t know really. I think just enjoying myself. And it’s nice for my family. |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0436** |
| **Biographical** | Sir Alexander Robertus Todd was born in Glasgow on October 2, 1907, the elder son of Alexander Todd, a business man of that city, and his wife Jean Lowrie. He was educated at Allan Glen’s School and Glasgow University, where he took his B.Sc. degree in 1928 and, after a short initial research training with T.S. Patterson he proceeded to the University of Frankfurt-on-Maine. Here he studied under W. Borsche and obtained his Ph.D. (Dr.Phil.nat.) in 1931 for a thesis on the chemistry of the bile acids.  Returning to England he worked from 1931-1934 on anthocyanins and other colouring matters with [Sir Robert Robinson](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1947/index.html), the Nobel Prize winner, and took a Ph.D. degree at Oxford University in 1933.  Todd went back to Scotland in 1934 when he joined the staff of Edinburgh University under G. Barger. Two years later, i.e. in 1936 he moved to the Lister Institute of Preventive Medicine, Chelsea, and became Reader in Biochemistry in the University of London in 1937.  In 1938 he was appointed as Sir Samuel Hall Professor of Chemistry and Director of the Chemical Laboratories of the University of Manchester, which position he held until 1944, when he accepted an appointment as Professor of Organic Chemistry at Cambridge University and Fellow of Christ’s College.  Todd’s work has gained him recognition in many universities and countries. He holds the D.Sc. degree of Glasgow University and has had bestowed upon him honorary doctorates from the Universities of Kiel (Dr.rer.nat.), Glasgow (LL.D.), Hon.D.Sc. London (1958), Madrid (1959), Exeter (1960), Leicester (1960), Aligarh (1960), and in 1961 Wales, Yale and Sheffeld; also Hon.LL.D. from Melbourne in 1960. He is a Fellow of the Royal Society, foreign member of the National Academy of Sciences, the American Academy of Arts and Sciences, the Austrian Academy of Sciences and the Spanish Council of Scientific Investigation, and an honorary member of the French, German and Spanish chemical societies and member of the Deutsche Akad. Naturforscher Leopoldina, Halle, (1959). He holds the Meldola Medal of the Royal Institute of Chemistry and the Society of Maccabeans; the Davy Medal and Royal Medal of the Royal Society, the Cannizaro Medal of the Italian Chemical Society and the Lavoisier Medal of the French Chemical Society. He has been Tilden Lecturer and Pedler Lecturer of the Chemical Society, Bakerian Lecturer of the Society of Chemical Industry, also visiting professor at California Institute of Technology (1938), the University of Chicago (1948), Sidney University (1950), Massachusetts Institute of Technology (1954) and the University of California (1957). He was elected Hon. Member, New York Academy of Sciences (1959), Hon. Fellow of the Royal Australian Chemical Institute (1960), President of the Chemical Society, London, 1960-1962, Master of the Worshipful Company of Salters, 1961-1962.  Todd has taken considerable interest in international scientific affairs; he is President of the International Union of Pure and Applied Chemistry, and Chairman of the British National Committee for Chemistry. He has served on many Government Committees and in 1952 was elected Chairman of the British Government’s Advisory Council on Scientific Policy. He is a Managing Trustee of the Nuff’eld Foundation.  The main subjects of Todd’s researches have been the chemistry of natural products of biological importance and, in addition to the nucleotide and nucleotide coenzyme studies described in his Nobel Lecture, the chemistry of vitamins B1, E and B12, the constituents of Cannabis species, insect colouring matters, factors influencing obligate parasitism and various mould products.  Knighted in 1954, he was raised to the Peerage in March, 1962, being created Baron Todd of Trumpington.  Lord Todd is married to Alison Sarah, daughter of Nobel Prize winner Sir Henry Dale, and they have a son, Alexander Henry, and two daughters, Helen Jean and Hilary Alison. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0436 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0437** |
| **Biographical** | Sir Cyril Norman Hinshelwood was born in London on June 19, 1897. He was educated at Westminster City School and Oxford University where he gained Master of Arts and Doctor of Science degrees. He held successive fellowships at Balliol, Trinity, and Exeter Colleges; he was tutor of Trinity College from 1921 to 1937 and since 1937 he has been Dr. Lee’s Professor of Chemistry, University of Oxford. He is a delegate of the Clarendon Press and he has served as a member of several Advisory Councils on scientific matters to the British Government. He was elected Fellow of the Royal Society in 1929, serving as Foreign Secretary from 1950 to 1955, and as President from 1955 to 1960. He was knighted in 1948 and appointed to the Order of Merit in 1960.  His early studies of molecular kinetics led to the publication of *Thermodynamics for Students of Chemistry* and *The Kinetics of Chemical Change* in 1926, the latest (fourth) edition of the latter appearing in 1940, and he subsequently worked on chemical changes in the bacterial cell, producing physicochemical explanations for the biological responses of bacteria to changes in environment. His findings proved to be of great importance in later research work on antibiotics and therapeutic agents, and his book on the topic *The Chemical Kinetics of the Bacterial Cell* was published in 1946. He has contributed a great number of original papers and reviews to journals of learned societies and to other scientific periodicals, and his latest book, *The Structure of Physical Chemistry* appeared in 1951.  Sir Cyril was President of the Chemical Society from 1946 to 1948, and President of the Faraday Society from 1961 to 1962, and included amongst the many awards he has gained are Lavoisier Medal, Société Chimique de France, 1935; Davy Medal, Royal Society, 1943; Royal Medal, 1947; Longstaff Medal, Chemical Society, 1948; Guldberg Medal, Oslo University, 1952; Faraday Medal, 1953; Avogadro Medal, Accademia dei Lincei, Rome, 1956; and Leverhulme Medal, Royal Society, 1960.  Honorary degrees conferred on Sir Cyril include Doctor of Civil Law (Oxford) and the Doctor of Science degrees of Bristol, Cambridge, Dublin, Hull, Leeds, London, Sheffield, Wales, and Ottawa Universities. He holds honorary memberships of the major scientific societies of many countries.  Sir Cyril is unmarried. He is fluent in many languages and his main hobbies are painting, collecting Chinese pottery, and foreign literature. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0437 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0438** |
| **Biographical** | Nikolai Nikolaevic Semenov was born in Saratov on April 3, 1896. He graduated from Petrograd University in 1917 and in 1920 he took charge of the electron phenomena laboratory of the Leningrad Physico-Technical Institute. He lectured at the Polytechnical Institute and was appointed Professor in 1928. In 1931, he became Director of the Institute of Chemical Physics of the U.S.S.R. Academy of Sciences (which has moved to Moscow in 1943); from 1944 he has been a Professor at the Moscow State University.  Semenov’s outstanding work on the mechanism of chemical transformation includes an exhaustive analysis of the application of the chain theory to varied reactions and, more especially, to combustion processes. He proposed a theory of degenerate branching which led to a better understanding of the phenomena associated with the induction periods of oxidation processes. Semenov has made valuable contributions to the field of molecular physics; he has also carried out investigations on electron phenomena, dielectric breakdown and the propagation of explosive waves.  Semenov has written two important books concerned with his work. *Chemical Kinetics and Chain Reactions* was published in 1934 with an English edition in 1935. It was the first book in the U.S.S.R. to develop a detailed theory of unbranched and branched chain reactions in chemistry. *Some Problems of Chemical Kinetics and Reactivity*, first published in 1954, was revised in 1958; there are also English, American, German, and Chinese editions.  He became a Corresponding Member of the U.S.S.R. Academy of Sciences in 1929 and Academician in 1932: he was awarded five Orders of Lenin and the Order of Red Banner of Labour. He is a member of the Chemical Society (London), Foreign Member of the Royal Society, and foreign member of the American, Indian, German, and Hungarian Academies of Sciences. He also holds Honorary Doctorate degrees of Oxford and Brussels Universities, and since 1960 he has been Chairman of the All-Union Society for Propagation of Political and Scientific Knowledge.  He married Natalya Nikolaevna Semenova; they have one son and one daughter. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0438 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0439** |
| **Biographical** | Vincent du Vigneaud was born in Chicago, Illinois, on 18th May, 1901, the son of the late inventor and machine designer Alfred J. du Vigneaud and his wife, Mary Theresa. He studied under Professor C.S. Marvel at The University of Illinois and took his B.Sc. degree in 1923 and M.Sc. in 1924.  During the year 1924-1925 he was assistant biochemist to Dr. W.G. Farr at the Philadelphia General Hospital and served on the Staff of the Graduate School of Medicine of Pennsylvania University. In 1927 he worked with Professor J.R. Murlin and submitted a thesis to the School of Medicine of Rochester University which earned him the Ph.D. As a Fellow of the National Research Council he worked with Professor J.J. Abel at Johns Hopkins University Medical School, with Professor George Barger at Edinburgh University Medical School and with Professor Charles R. Harington at London University College Hospital.  On his return to America, du Vigneaud joined the Physiological Chemical Staff at The University of Illinois under Professor W.C. Rose and in 1932 he became Head of the Biochemistry Department at the George Washington University School of Medicine. The Cornell University Medical College offered him a Professorship as Head of the Biochemistry Department in 1938.  Du Vigneaud has held many lectureships in universities in the United States and England, among the latter the Liversidge Lectureship at Cambridge, and in the summer of 1947 he was Visiting Lecturer of the American Swiss Foundation for Scientific Exchange in Switzerland. His Messenger Lectures at Cornell University in 1950 were published in 1952 as *A Trail of Research in Sulphur Chemistry and Metabolism and Related Fields*. Many learned chemical societies in America have conferred awards on du Vigneaud and he received the Chandler Medal of Columbia University in 1955 and the Willard Gibbs Medal of the American Chemical Society a year later. Honorary science doctorates were bestowed on him by New York and Yale Universities in 1955, and by The University of Illinois at Urbana-Champaign in 1960.  Honorary fellowships have been conferred on du Vigneaud by the Royal Society of Edinburgh, the Chemical Society and the Royal Institute of Chemistry, London. He has been elected to membership of many scientific academies among the most noteworthy being the Board of Trustees of the Rockefeller Institute and the National Institute of Arthritis and Metabolic Diseases.  His researches have centred mainly about sulphur-containing compounds of biochemical importance, being concerned originally with the sulphur of insulin and more recently with two hormones of the posterior pituitary gland-oxytocin and vasopressin. He has also studied intermediary metabolism, amino acids and peptides, transmethylation and metabolism of onecarbon compounds, transsulphuration, biotin and penicillin.  Du Vigneaud married Zella Zon Ford in 1924; they have a son, Vincent, Jr. (b. 1933) and a daughter, Marilyn Renée (b. 1935). |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0439 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0440** |
| **Biographical** | Linus Carl Pauling was born in Portland, Oregon, on 28th February, 1901, the son of a druggist, Herman Henry William Pauling, who, though born in Missouri, was of German descent, and his wife, Lucy Isabelle Darling, born in Oregon of English-Scottish ancestry.  Linus attended the public elementary and high schools in the town of Condon and the city of Portland, Oregon, and entered the Oregon State College in 1917, receiving the degree of B.Sc. in chemical engineering in 1922. During the years 1919-1920 he served as a full-time teacher of quantitative analysis in the State College, after which he was appointed a Teaching Fellow in Chemistry in the California Institute of Technology and was a graduate student there from 1922 to 1925, working under Professor Roscoe G. Dickinson and Richard C. Tolman. In 1925 he was awarded the Ph.D. (*summa cum laude*) in chemistry, with minors in physics and mathematics.  Since 1919 his interest lay in the field of molecular structure and the nature of the chemical bond, inspired by papers by [Irving Langmuir](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1932/index.html) on the application of the Lewis theory of the sharing of pairs of electrons between atoms to many substances. In 1921 he suggested, and attempted to carry out, an experiment on the orientation of iron atoms by a magnetic field, through the electrolytic deposition of a layer of iron in a strong magnetic field and the determination of the orientation of the iron crystallises by polishing and etching the deposit, and microscopic examination of the etch figures. With Professor Dickinson, he began in 1922 the experimental determination of the structures of some crystals, and also started theoretical work on the nature of the chemical bond.  Since his appointment to the Staff of California Institute of Technology, Professor Pauling was elected Research Associate in 1925; National Research Fellow in Chemistry, 1925-1926; Fellow of the John Simon Guggenheim Memorial Foundation, 1926-1927 (through this last he worked in European Universities with Sommerfeld, [Schrödinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/index.html), and [Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html)); Assistant Professor of Chemistry, 1927-1929; Associate Professor, 1929-1931; Professor, 1931, when he was the first recipient of the American Chemical Society Award in Pure Chemistry – the Langmuir Prize – and Chairman of the Division of Chemistry and Chemical Engineering, and Director of the Gates and Crellin laboratories of Chemistry, 1936-1958. In 1963, he was awarded the [Nobel Peace Prize](https://www.nobelprize.org/nobel_prizes/peace/laureates/1962/index.html).  Pauling is a member of numerous professional societies in the U.S.A. as well as in many European countries, India, Japan and Chile. Awards, medals, and honorary degrees were showered upon him in America and Europe, and in addition he was elected Rationalist of the Year for 1960 and Humanist of the Year for 1961. Several books have come from his pen, ranging from his most famous one *The Nature of the Chemical Bond, and the Structure of Molecules and Crystals* (1939, 1949, 1960) via *General Chemistry* (1947, 1953), which was translated into nine languages, to *No More War!* (1958, 1959,1962).  The subjects of the papers he published reflect his great scientific versatility: about 350 publications in the fields of experimental determination of the structure of crystals by the diffraction of X-rays and the interpretation of these structures in terms of the radii and other properties of atoms; the application of quantum mechanics to physical and chemical problems, including dielectric constants, X-ray doublets, momentum distribution of electrons in atoms, rotational motion of molecules in crystals, Van der Waals forces, etc.; the structure of metals and intermetallic compounds, the theory of ferromagnetism; the nature of the chemical bond, including the resonance phenomenon in chemistry; the experimental determination of the structure of gas molecules by the diffraction of electrons; the structure of proteins; the structure of antibodies and the nature of serological reactions; the structure and properties of hemoglobin and related substances; abnormal hemoglobin molecules in relation to the hereditary hemolytic anemias; the molecular theory of general anesthesia; an instrument for determining the partial pressure of oxygen in a gas; and other subjects.  Pauling married Ava Helen Miller of Beaver Creek, Oregon, in 1923. She is of English-Scottish and German descent. They have four children, Linus (Carl) Jr. (1925), Peter Jeffress (1931), Linda Helen (1932) and Edward Crellin (1937), and thirteen grandchildren. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0440 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0441** |
| **Biographical** | Hermann Staudinger was born in Worms on the 23rd of March 1881. His father was Dr. Franz Staudinger.  Staudinger was educated in Worms, matriculated in 1899, and continued his studies first at the University of Halle, later at Darmstadt and Munich. He graduated at Halle in 1903 and qualified for inauguration as academic lecturer under Professor Thiele at Strasbourg University in spring 1907. In November 1907 he was appointed Professor of Organic Chemistry at the Institute of Chemistry of the Technische Hochschule in Karlsruhe. For fourteen years, from 1912, he was lecturer at the Eidgenössische Technische Hochschule in Zurich, and in 1926 followed the invitation of the University of Freiburg i. Br. to become Lecturer of Chemistry. ln this city, he remained all through his further career. From 1940 onwards he held an additional appointment as Principal of the Research Institute for Macromolecular Chemistry. Staudinger resigned from his post as Principal of the Chemical Laboratories of the University in April 1951, and accepted the honorary appointment as Head of the State Research Institute for Macromolecular Chemistry, which he held until April 1956.  Staudinger was a prolific writer and the following books by him have been published: *Die Ketene* (Ketenes), published by Enke, Stuttgart, 1912; *Anleitung zur organischen qualitativen Analyse* (Introduction to organic qualitative analysis), published by Springer, Berlin, 1st edition 1923, 5th edition 1948, 6th edition 1955; *Tabellen zu den Vorlesungen über allgemeine und anorganische Chemie* (Tables for the lectures on general and inorganic chemistry), published by Braun, Karlsruhe, 1st edition 1927, 5th edition 1947; *Die hochmolekularen organischen Verbindungen, Kautschak und Cellulose* (The high-molecular organic compounds, rubber and cellulose), published by Springer, Berlin, 1932; *Organische Kolloidchemie* (Organic colloid chemistry), published by Vieweg, Braunschweig, 1st edition 1940, 3rd edition 1950; *Fortschritte der Chemie, Physik und Technik der makromolekularen Stoffe* (Progress of the chemistry, physics and technique of the macromolecular substances), jointly with Professor Vieweg and Professor Röhrs, Volume 1, 1939, Volume II, 1942, publisher Lehmann, Munich; *Makromolekulare Chemie und Biologie* (Macromolecular chemistry and biology), publisher Wepf & Co., Basle, 1947; *Vom Aufstand der technischen Sklaven* (The uprising of the technical slave), published by Chamier, Essen-Freiburg, 1947.  Since September 1947 Staudinger has edited the periodical *Die makromolekulare Chemie* (Macromolecular chemistry), published by Dr. A. Hüthig, Heidelberg and Wepf & Co., Basle.  In 1961 his book *Arbeitserinnerungen* (Working memoirs) appeared, published by Dr. A. Hüthig, Heidelberg.  Besides the books, Staudinger published a great number of scientific papers. Among these were fifty on ketenes, also works on oxalyl chloride, autoxidation, aliphatic diazo-compounds, explosions, insecticides, synthetic pepper and coffee aroma. Since 1920 he has written approximately 500 papers on macromolecular compounds, about 120 of these on cellulose, about 50 on rubber and isoprene.  For his work Staudinger received many honours and awards; to mention but a few – he is Dr. Ing. h.c. of the Technische Hochschule Karlsruhe; Dr. rer. nat. h.c. of the University of Mainz; Dr. (C) h.c. of the University of Salamanca; Dr. chem. h.c. of the University of Torino; Dr. sc. techn. h.c. of the Eidgenössische Technische Hochschule Zurich; and Dr. h.c. of the University of Strasbourg. In 1953 he was awarded the Nobel Prize in Chemistry for his discoveries in the field of macromolecular chemistry. In 1933 he was honoured with the Cannizzarro Prize of the Reale Accademia Nazionale dei Lincei in Roma. He is member of the Institut de France, and member and honorary member of many Chemical Societies and the Society of Macromolecular Chemistry in Tokyo.  Hermann Staudinger is married to Magda Woit, who is for many years his co-worker and co-author of numerous publications. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0441 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0442** |
| **Biographical** | Archer John Porter Martin was born on March 1st, 1910, in London where his father was a general medical practitioner. He attended Bedford School from 1921 to 1929 when he entered Cambridge University to graduate in 1932. After a year in the Physical Chemistry Laboratory he obtained a post at the Dunn Nutritional Laboratory, where he worked under L.J. Harris and Sir Charles Martin, and in 1938 he moved to the Wool Industries Research Association at Leeds. From 1946 to 1948 he was Head of the Biochemistry Division of the Research Department of Boots Pure Drug Company at Nottingham and in 1948 he joined the staff of the Medical Research Council, first at the Lister Institute and later at the National Institute for Medical Research. He was appointed Head of the Division of Physical Chemistry at the Institute in 1952 and he was Chemical Consultant from 1956 to 1959. Since 1959 he has been a Director of Abbotsbury Laboratories Ltd.  Martin entered Cambridge University with the intention of becoming a chemical engineer but, due to the influence of Professor J.B.S. Haldane, then Reader of Biochemistry at Cambridge, he eventually specialized in biochemistry. His first researches, as an undergraduate, resulted in a method of detecting pyro-electricity by observing the attraction of a metal plate for crystals that had been immersed in liquid air. At Cambridge he worked on ultraviolet adsorption spectra and at the Nutritional Laboratory he was concerned with the attempted isolation of Vitamin E and in the pathological effects of prolonged Vitamin E deficiencies. In these latter studies he used solvent extraction and chromatographic methods which were to lay the foundation for his later work on chromatography. He also worked, along with others, on the B2 group of vitamin deficiencies in pigs.  At the Wool Industries Research Association he worked on the felting of wool, first with R.L.M. Synge and later with Consden and Gordon, and on amino-acid analysis. It was here that he developed his method of partition chromatography; more recently, with A.T. James, he has developed the method of gas-liquid chromatography.  Dr. Martin, a Fellow of the Royal Society (1950), was made Companion of the British Empire in 1960. He received the Berzelius Medal of the Swedish Medical Society (1951), the John Scott Award (1958), the John Price Wetherill Medal (1959), the Franklin Institute Medal (1959), and the Leverhulme Medal (1963).  In 1963, he was appointed to deliver special lectures (as “buitengewoon hoogleraar”) at the Technological University of Eindhoven, The Netherlands.  In 1943 he married Judith Bagenal; they have one son and three daughters. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0442 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0443** |
| **Biographical** | Richard Laurence Millington Synge was born at Liverpool on October 28th, 1914, as the son of Laurence Millington Synge, of Liverpool Stock Exchange, and Katharine Charlotte Swan. In 1928 he went to Winchester College, where he studied mainly classics until 1931, thereafter natural science. In 1933 he entered Trinity College, University of Cambridge and studied physics, chemistry and physiology for Part I of the Natural Sciences Tripos (1935) and biochemistry for Part II (1936). During 1936-1939 he was a research student under supervision of Mr. N.W. Pirie in the University Biochemical Laboratory headed by [Sir Frederick G. Hopkins](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1929/index.html), and during 1939-1941 at the Wool Industries Research Association at Leeds. He obtained his Ph.D. degree at Cambridge in 1941. In the same year, he joined the staff of the Wool Industries Research Association at Leeds and in 1943 that of the Lister Institute of Preventive Medicine, London, in the Biochemistry Department under W.T.J. Morgan. Since 1948, he has been Head of the Department of Protein Chemistry at the Rowett Research Institute at Bucksburn, Aberdeen.  The circumstances of his work up to 1945, including the collaborative work on partition chromatography and related topics, are described in the Nobel Lectures by A.J.P. Martin and himself. They gave the first demonstration of partition chromatography to the Biochemical Society at its meeting at the National Institute for Medical Research, London, on June 7th, 1941 [*Chem.Ind.(Lond.),* 19 (1941) 487], the first published description appearing in the *Biochemical Journal,* 35 (1941) 1358.  Since 1945 Dr. Synge has been mainly interested in analytical problems concerning the larger peptide molecules, as antibiotics and as intermediates in protein metabolism. From 1942 to 1948 he worked almost exclusively with the antibiotic peptides of the gramicidin group. In 1946-1947 he spent eight months with Professor [Tiselius](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1948/index.html) at Uppsala, studying the application of his adsorption methods to these compounds.  At the Rowett Research Institute, directed by D.P. Cuthbertson, he has been particularly concerned with the digestion of proteins by the ruminant animal and its associated micro-organisms, with peptides, proteins and other components of plant material, and with physico-chemical methods for the purification of intermediates in the metabolism of proteins. Work begun about 1950 with D.L. Mould and A. Tiselius on electrokinetic ultrafiltraltion of various polysaccharides has been developed in a number of directions to take advantage of molecular-sieve effects, especially in the presence of hydrogen-bond breaking solvents.  In 1958-1959, he spent a year at Ruakura Animal Research Station, Hamilton, New Zealand, working with E.P. White on isolation of the toxic fungal component sporidesmin.  Dr. Synge was made a Fellow of the Royal Society in 1950 and of the Royal Institute of Chemistry in 1952. He is an honorary member of the American Society of Biological Chemists.  In 1943 he married Ann Stephen, daughter of the late Adrian and Karin Stephen, psychoanalysts. They have four daughters and three sons, in order of decreasing age: Jane, Elizabeth, Matthew Millington, Patrick Millington, Alexander Millington, Charlotte, and Mary. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0443 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0444** |
| **Biographical** | Edwin Mattison McMillan was born on 18th September, 1907, at Redondo Beach, California. He is the son of Dr. Edwin Harbaugh McMillan, a physician, and his wife, Anne Marie McMillan, *née* Mattison, who both came from the State of Maryland and were both of English and Scottish descent. The boy spent his early years in Pasadena, California, and obtained his education in that state.  McMillan attended the California Institute of Technology, obtaining a B.Sc. degree in 1928, and taking his M.Sc. degree a year later, then transferring to Princeton University for Ph.D. in 1932. The same year he entered the University of California at Berkeley as a National Research Fellow. The thesis he submitted for Ph.D. was in the field of molecular beams, and the problem he undertook as a National Research Fellow was the measurement of the magnetic moment of the proton by a molecular beam method. After two years on this work and one as a research associate he became a Staff Member of the Radiation Laboratory under Professor [E.O. Lawrence](https://www.nobelprize.org/nobel_prizes/physics/laureates/1939/index.html), studying nuclear reactions and their products, and helping in the design and construction of cyclotrons and other equipment, and a member of the Faculty in the Department of Physics at Berkely, being appointed Instructor in 1935, Assistant Professor in 1936, Associate Professor, 1941, and Professor in 1946.  During the Second World War, McMillan was on leave from November, 1940, to September, 1945, engaged on national defence research, serving (1940-1941) in the Radiation Laboratory, Massachusetts Institute of Technology; (1941-1942) U. S. Navy Radio and Sound Laboratory, San Diego; (1942-1945) Manhattan District, Los Alamos.  It was during 1945 that he had the idea of “phase stability” which led to the development of the synchroton and synchro-cyclotron; these machines have already extended the energies of artificially accelerated particles into the region of hundreds of MeV and have made possible many important researches.  McMillan returned to the University of California Radiation Laboratory as Associate Director from 1954-1958, when he was raised to Deputy Director and finally Director, in the same year.  In 1951 he received the 1950 Research Corporation Scientific Award, and in 1963 he shared the Atoms for Peace Award with Professor V. I. Veksler.  Professor McMillan is a Fellow of the American Physical Society and the American Academy of Arts and Sciences, a member of the National Academy of Sciences and the American Philosophical Society, and from 1954-1958 he served on the General Advisory Committee to the Atomic Energy Commission. In 1960 he was appointed to the Commission on High Energy Physics of the International Union of Pure and Applied Physics.  An honorary doctorate in science was awarded to him by the Rensselaer Polytechnic Institute in 1961, and by Gustavus Adolphus College in 1963.  While serving in the Faculty of Physics at Berkeley, McMillan married Elsie Walford Blumer, a daughter of Dr. George Blumer, Dean Emeritus of the Yale Medical School. There are three children of the marriage – Ann Bradford (1943), David Mattison (1945) and Stephen Walker (1949). |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0444 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0445** |
| **Biographical** | Glenn Theodore Seaborg was born in Ishpeming, Michigan, on April 19, 1912. At the age of 10 he moved with his family to California, in 1929 he graduated at David Starr Jordan High School in Los Angeles as valedictorian of his class.  He entered the University of California, Los Angeles, in 1929, and received the degree of Ph.D. in Chemistry from the University of California, Berkeley, in 1937. From 1937 to 1939 he was the personal laboratory assistant of the late G. N. Lewis, with whom he published a number of scientific papers. In 1939, Dr. Seaborg was appointed an instructor in chemistry at the University of California, Berkeley, where he was promoted to Assistant Professor in 1941, and to Professor of Chemistry in 1945. In 1946, he also took responsibility for direction of nuclear chemical research at the Lawrence Radiation Laboratory, operated for the Atomic Energy Commission by the University of California; from 1954 to 1961, he was Associate Director of LRL. In the same year, he was appointed by President Truman to be a member of the AEC’s first General Advisory Committee, a post he held until 1950. In 1958, he was appointed Chancellor of the University of California at Berkeley. In that capacity he served until his appointment by President Kennedy to the Atomic Energy Commission in 1961, when he was designated Chairman of the Commission. His term of office expires in 1968. From 1959 to 1961, he was also a member of the President’s Science Advisory Committee. Dr. Seaborg was given a leave of absence from the University of California from 1942-1946, during which period he headed the plutonium work of the Manhattan Project at the University of Chicago Metallurgical Laboratory. He was co-discoverer of plutonium and all further transuranium elements through element 102.  In addition to the discovery of transuranium elements, Dr. Seaborg and his colleagues are responsible for the identification of more than 100 isotopes of elements throughout the Periodic Table. He is also author of the actinide concept of heavy element electronic structure. In this connection, Dr. Seaborg demonstrated that the heavy elements form a “transition” series of actinide elements in a manner analogous to the rare-earth series of lanthanide elements. The concept demonstrated how the heavy elements fit into the Periodic Table and thus demonstrated their relationships to the other elements.  The body of information assembled in Dr. Seaborg’s laboratory has made it possible to predict the radioactive characteristics of many isotopes of elements still to be found. Under Dr. Seaborg’s leadership, also, whole new bodies of methodology and instrumentation have been developed and have become a cornerstone of modern nuclear chemistry. Dr. Seaborg is the author of approximately 200 scientific papers, including a number of comprehensive reviews and compilations in scientific publications. He is also author and co-author of several books on chemistry and the elements.  Honours include: in 1947 named as one of America’s 10 outstanding young men by the U.S. Junior Chamber of Commerce; 1947 recipient of the American Chemical Society’s Award in Pure Chemistry; 1948 John Ericsson Gold Medal by the American Society of Swedish Engineers; 1948 Nichols Medal of the New York Section of the American Chemical Society; 1953 John Scott Award and Medal of the City of Philadelphia; 1957 Perkin Medal of the American Section of the Society of Chemical Industry; 1959 Atomic Energy Commission’s Enrico Fermi Award for his outstanding work in the field of nuclear chemistry and for his leadership in scientific and educational affairs; in 1962, named Swedish American of the Year by Vasa Order of America, Stockholm; 1963 Franklin Medal of the Franklin Institute, Philadelphia.  Dr. Seaborg is an Honorary Fellow of the Chemical Society of London and of the Royal Society of Edinburgh. He is a Fellow of the American Institute of Chemists, the New York Academy of Sciences, the California Academy of Sciences, the American Physical Society and the American Association for the Advancement of Science. He is a Member of the National Academy of Sciences, the American Academy of Arts and Sciences, the Royal Society of Arts of England, and the Royal Swedish Academy of Engineering Sciences.  Honorary degrees awarded to Dr. Seaborg include Doctor of Science degrees from the University of Denver, 1951; Gustavus Adolphus College, 1954; Northwestern University, 1954; University of Notre Dame, 1961; Ohio State University, 1961; Florida State University, 1961; University of Maryland, 1961; Temple University, 1962; Tulane University, 1962; Drexel Institute of Technology, 1962; Georgetown University, 1962; University of the State of New York, 1962; Mundelein College, 1963; and Trinity College, 1963; the degree of Doctor of Laws from the University of Michigan, 1958; and University of Massachusetts, 1963; the degree of Doctor of Humane Letters from Northern Michigan College, 1962; the degree of Doctor of Public Service from George Washington University, 1962; and the degree of Doctor of Public Administration from the University of Puget Sound, 1963.  In 1942, Dr. Seaborg married Helen L. Griggs, then secretary to the late Dr. Ernest O. Lawrence (Nobel Laureate for Physics 1939). They have six children: Peter (b. 1946), Lynne (b. 1947), David (b. 1949), Stephen (b. 1951), John Eric (b. 1954), and Dianne (b. 1959). His chief hobby is golf, but he also follows other sports with interest. From 1953-1958 he served as the University of California (Berkeley) Faculty Athletic Representative. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0445 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0446** |
| **Biographical** | Otto Paul Hermann Diels was born in Hamburg, Germany, on January 23, 1876. When he was two years of age the family moved to Berlin, where his father was appointed to a professorship. His early education, from 1882 to 1895, was at the Joachimsthalsches Gymnasium, Berlin. In 1895 he went to Berlin University where he studied chemistry, together with other science subjects, under [Emil Fischer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1902/index.html), graduating in 1899. He was at once appointed assistant at the Institute of Chemistry at Berlin University, becoming a lecturer in 1904. Promotion to Professor followed in 1906, and he was appointed Head of Department in 1913. He became Professor at the University of Berlin in 1915 but, the following year, moved to the University of Kiel as Professor and Director of the Institute of Chemistry. There he remained until his retirement in 1945.  His earliest research was in the field of inorganic chemistry, where he was the discoverer of an oxide of carbon having some unusual properties – carbon suboxide. His subsequent work was in the domain of organic chemistry. He was responsible for introducing the use of selenium as a specific reagent for the dehydrogenation of hydroaromatic compounds (1927). This proved to be a very useful tool in elucidating chemical structures in the complicated steroid series, where his name is associated with the hydrocarbon 3′-methyl-1,2-cyclopentenophenanthrene. Diels obtained this skeletal steroid structure by dehydrogenating cholesterol, and other members of the series, with selenium.  His best known work was done in collaboration with Kurt Alder on the chemical reaction which bears their joint names (1928). This, sometimes known also as the diene synthesis, consists in the reaction of a diene with a second component which has carbonyl or carboxyl groups adjoining an ethylenic bond, to give unsaturated cyclic compounds. As the two reactants can be varied widely within the scope of this definition, a very large range of new compounds can be prepared. The reaction takes place without the need for forcing conditions and, by its use, many complicated natural products, and modifications thereof, may be synthesized.  Diels was the author of the *Einführung in die organische Chemie* (1907), which went through fifteen editions. He had many papers published, mostly in German scientific periodicals such as *Liebigs Annalen der Chemie*.  One of his earliest awards was in 1904, when he was awarded a Gold Medal at the St. Louis (USA) International Exhibition. He won the Adolf v. Baeyer Medallion in 1930 and the Grosskreuz des Verdienstordens der Bundesrepublik Deutschland in 1952. He held the honorary degree of Doctor of Medicine from the University of Kiel and was a member of the Academies of Halle, Munich, and Göttingen.  Diels married, in 1909, Paula Geyer. They had three sons and two daughters; two of their sons were killed in action during World War II. He was interested in reading, music, and travel for his recreation, and, in his younger days, he had been fond of mountaineering. He died on March 7, 1954. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0446 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | 0447 |
| **Biographical** | Kurt Alder was born in Königshütte, Upper Silesia, on the 10th of July 1902. His childhood and school years were spent in these industrial surroundings, but after the end of the First World War he was forced to leave his home, due to political circumstances.  He started reading chemistry at Berlin University in 1922, and later continued these studies at Kiel, where he obtained his degree of Ph.D. in 1926. The thesis for the doctorate, on which Alder worked under O. Diels, was entitled: *Über die Ursachen der Azoester-reaktion* (On the causes of the azoester reaction).  In 1930 Alder was appointed reader for chemistry by the Faculty of Philosophy at Kiel University; promotion to lecturer followed in 1934. Alder left Kiel in 1936 to take up the appointment as head of department in the science laboratories of the I. G. Farben-Industrie, at their works in Leverkusen, where he worked on the preparation and constitution of synthetic rubber (“Buna”). By this work some of his earlier interests were reawakened and stimulated.  In 1940 Alder was appointed to the Chair for Experimental Chemistry and Chemical Technology at Cologne University and also became Principal of the Institute of Chemistry. He received invitations from Berlin University in 1944 and from the University of Marburg in 1950, but declined both.  As early as 1927-1928, whilst at Kiel, Alder had studied problems of systematic organic chemistry in collaboration with his teacher O. Diels, and this lead to their joint discovery of the principle of the diene-synthesis, which they investigated and determined in all its aspects. At the same time Alder also worked in collaboration with younger colleagues on extensive stereochemical investigations, prompted by selection phenomena during organic chemical reactions, particularly in unsaturated systems. A series of other problems, such as the behaviour of double bonds in stressed carbon rings and the phenomena of intermolecular rearrangements, were investigated.  Although conditions in Cologne during the 1940’s were not favourable for scientific research, Alder was nevertheless able to continue his original work systematically and even discover relationships which were decisive for future developments. These are characterized by the transition from pure additive processes, of which the diene-synthesis is the most important, to processes of substitution. The purpose of these investigations was the analysis and the elimination of the dualism existing between addition and substitution. These studies covered a wide field and include also the reaction of molecular oxygen on unsaturated substrates.  Kurt Alder’s investigations have been described in about 150 papers, which were published mainly in *Justus Liebig’s Annalen der Chemie*, in the *Berichte der Deutschen Chemischen Gesellschaft* and in *Angewandte Chemie*.  In recognition of his work, Alder received the Emil Fischer Memorial Medal from the Association of German Chemists, in 1938. In the same year he became a member of the Kaiserlich Leopold.-Karol.-Deutsche Akademie der Naturforscher (Imperial Leopold.-Karol.-German Academy of Natural Philosophers) in Halle. The Medical Faculty of the University of Cologne conferred the honorary degree of M.D. on Alder in 1950, and in 1954 he received the honorary doctorate of the University of Salamanca. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0447 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0448** |
| **Biographical** | William Francis Giauque was born in Niagara Falls, Canada, of U.S. parentage, on May 12, 1895, the first of three children of William Tecumseh Sherman Giauque and Isabella Jane Duncan.  He attended public grammar schools principally in Michigan. Following the death of his father, in 1908, the family returned to Niagara Falls, Canada, where he received his secondary school education in the Niagara Falls Collegiate Institute. After graduation he sought employment in various power plants at Niagara Falls for financial reasons and because he had planned for many years to become an electrical engineer and wanted preliminary experience. He was unable to obtain this type of work. At this point chance entered decisively in the form of a newspaper advertisement of the Hooker Electro-Chemical Company in Niagara Falls, New York, which led him to accept employment in their laboratory. The well-organized operations in this chemical plant, together with problems which he saw in course of solution, captured his interest and caused him to decide to become a chemical engineer.  After two years employment he entered the College of Chemistry of the University of California, where he received the B.S. degree with highest honors in 1920, was a University Fellow for the year 1920-1921 and James M. Goewey Fellow 1921-1922. He received the Ph.D. degree in chemistry with a minor in physics in 1922.  Although his undergraduate work at the university was selected with the idea of an engineering career, he soon acquired a liking for fundamental research. The emphasis on scientific investigation by the group of faculty and students associated with Professor Gilbert N. Lewis was the major influence.  He was appointed Instructor of Chemistry in 1922 and after passing through the intermediate grades of professorship he became Professor of Chemistry in 1934.  His interest in the third law of thermodynamics as a field of research was aroused by the experimental work for his Ph.D. research under Professor G.E. Gibson. This work, which was concerned with the relative entropies of glycerine crystals and glass, had its origin in discussions of Professors Lewis and Gibson.  The principal objective of his researches has been to demonstrate through a considerable number and variety of accurate tests that the third law of thermodynamics is a basic natural law.  The researches of his students and himself have included a large number of accurate entropy determinations from low temperature measurements, particularly on condensed gases. The entropies and other thermodynamic properties of many gases have also been determined from quantum statistics and molecular energy levels available from band spectra and other sources.  Correlated investigations of the entropy of oxygen from its incompletely interpreted band spectrum and from low-temperature heat capacity measurements with Dr. H.L. Johnston, led to the discovery of oxygen isotopes 17 and 18 in the Earth’s atmosphere and the fact that chemists and physicists were unknowingly using different atomic weight scales.  Investigation of the effect of magnetic fields on the entropies of paramagnetic substances led to the invention of the adiabatic demagnetization method of producing temperatures considerably below 1° absolute. This method was first demonstrated in collaboration with Dr. D.P. MacDougall.  Professor Giauque’s scientific work has been described in some 75 papers.  He has received the Chandler Medal and the honorary degree of Sc.D. from Columbia University, an honorary LL.D. from the University of California, and the Elliott Cresson Medal from the Franklin Institute. In 1951 he received the Willard Gibbs Medal and in 1956 the Gilbert Newton Lewis Medal. He was Faculty Research Lecturer of the University of California in 1948. He is a member of the National Academy of Sciences, the American Philosophical Society, the American Chemical Society, Institut International du Froid, and is Fellow of the American Physical Society and of the American Academy of Arts and Sciences.  In 1932 he married Dr. Muriel Frances Ashley. They have two sons, William Francis Ashley G. and Robert David Ashley G. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0448 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | 0449 |
| **Biographical** | Arne Wilhelm Kaurin Tiselius was born August 10, 1902, in Stockholm. After the early loss of his father, the family moved to Gothenburg where he went to school, and after graduation at the local “Realgymnasium” in 1921, he studied at the University of Uppsala, specializing in chemistry. He became research assistant in [The Svedberg](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1926/index.html)‘s laboratory in 1925 and obtained his doctor’s degree in 1930 on a thesis “The moving-boundary method of studying the electrophoresis of proteins” (published in *Nova Acta Regiae Societatis Scientiarum Upsaliensis*, Ser. IV, Vol. 7, No. 4) and was appointed Docent (Assistant Professor) in Chemistry from 1930 on. During the years 1931-1935 Tiselius published a number of papers on diffusion and adsorption phenomena in naturally occurring base-exchanging zeolites, and these studies were continued during a year’s visit to H.S. Taylor’s laboratory in Princeton with support of a Rockefeller Foundation Fellowship. Stimulated by many contacts with American biochemists and physical chemists during this visit, Tiselius on his return to Uppsala resumed his interest in proteins, and the application of physical methods to biochemical problems in general. This led among other things to a much improved method of electrophoretic analysis, published in the *Transactions of the Faraday Society*, 33 (1937) 524. This method as applied to the study of serum proteins and to a number of other biochemical problems kept Tiselius and an increasing number of collaborators occupied for the following years. In 1938 a special research professorship was established for Tiselius through a donation to the University of Uppsala by Major Herbert Jacobsson and his wife. Some space was put at the disposal of the new professor in the Institute of Physical Chemistry (Prof. The Svedberg). In 1946 biochemistry was established as an independent department and in 1950-1952 obtained a new building, the present Institute of Biochemistry.  Under the leadership of Tiselius this institute has contributed to the development and improvement of a number of useful methods in biochemistry, such as electrophoresis, chromatography, phase partition, gel filtration, etc. These methods and others have been applied to studies of large molecular weight substances, chiefly proteins and enzymes, but also polysaccharides (dextran) and nucleic acids. There has always been a close contact between the methodological work and the research into special problems where the methods find their application. Tiselius took an active part in the reorganization of scientific research in Sweden in the years following World War II. Thus he was Chairman of the Swedish Natural Science Research Council 1946-1950, and Chairman of the Research Committee of the Swedish Cancer Society 1951-1955. He was President of the International Union of Pure and Applied Chemistry 1951-1955, became Vice President of the Nobel Foundation in 1947 and President since 1960. He has also served as member of the Nobel Committee for Chemistry since 1946.  He was married (1930) with Ingrid Margareta (Greta) Dalén, daughter of city judge Per Dalén of Gothenburg. They have two children: Eva (b. 1932), married to Dr. Torgny Bohlin, Lund; and Per (b. 1934), physician at the Academic Hospital, Uppsala. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0449 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | 0450 |
| **Biographical** | Sir Robert Robinson was born at Rufford, near Chesterfield, Derbyshire on September 13th, 1886, the son of William Bradbury Robinson, a surgical dressing manufacturer who invented his own machines for the production of lint, bandages, etc., and the cardboard boxes for packaging them. He was educated at the Chesterfield Grammar School, Fulneck School, near Leeds, and at Manchester University where he graduated B.Sc. in 1906 and D.Sc. in 1910.  In 1912, he was appointed the first Professor of Pure and Applied Organic Chemistry in the University of Sydney. He returned to Britain in 1915 to take the Chair in Organic Chemistry at the University of Liverpool until 1920 when he accepted an appointment as Director of Research at the British Dyestuffs Corporation. One year later, he became Professor of Chemistry at St. Andrews and in 1922 he took the Chair in Organic Chemistry at Manchester University until 1928 when he accepted a similar post in the University of London. In 1930, he was appointed Waynflete Professor of Chemistry, Oxford University, where he remained until his retirement in 1955 when he was appointed Emeritus Professor and Honorary Fellow of Magdalen College. He has been a Director of the Shell Chemical Company and a chemical consultant since 1955.  Sir Robert has been a member of over thirty Government Committees and chairman of some of them. He was a United Kingdom delegate to the first Conference of UNESCO in 1947. He was knighted in 1939 and appointed to the Order of Merit in 1949.  Robinson’s extensive researches in organic chemistry have dealt not only with the structure and synthesis of many organic bodies, but also with the electrochemical mechanism of organic reactions. His interest in the chemical constitution of plant dyestuffs (anthocyanins) soon extended to another group of vegetable bodies, the alkaloids, where the whole series of his researches are remarkable for their brilliant syntheses. He contributed greatly towards the definition of the arrangement of atoms within molecules of morphine, papaverine, narcotine, etc. These discoveries led to the successful production of certain antimalarial drugs (they are reported in numerous scientific papers, mainly in the Journal of the Chemical Society).  Sir Robert, a Fellow of the Royal Institute of Chemistry and of the Royal Society was President of The Chemical Society, 1939-1941; of the Royal Society, 1945-1950; of the British Association for the Advancement of Science, 1955; and of the Society for the Chemical Industry, 1958. He is a Commandeur de la Légion d’Honneur and holds Honorary Doctorates of over twenty British and foreign universities. He has been honoured by The Chemical Society (Longstaff, Faraday and Flintoff Medals), the Royal Society (Davy, Royal and Copley Medals) and the Swiss, American, French and German Chemical Societies; he has also been awarded the Franklin Medal of the Franklin Institute of Philadelphia, the Albert Gold Medal of the Royal Society of Arts and the Medal of Freedom (U.S. Government). Sir Robert is Corresponding Member, Honorary Fellow, Foreign Member, Associate or Correspondant of almost fifty British and foreign learned societies.  In 1962, The Chemical Society honoured Sir Robert by establishing a Robert Robinson Lectureship, to be delivered biennially *in lieu* of the usual Presidential Address.  In 1912 Sir Robert married Gertrude Maud Walsh, a fellow student at Manchester University. They collaborated in several fields of chemical research, notably in a survey of anthocyanins. She died in 1954; they had one son and one daughter. In 1957, he married Stearn Sylvia Hillstrom (*née* Hershey) of New York.  In his younger days, Sir Robert was a keen mountaineer, having climbed in the Alps, Pyrenees, Norway and New Zealand, and he is an ardent chess player being President of the British Chess Federation, 1950-1953. His hobbies also include photography and music. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0450 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | 0451 |
| **Biographical** | James Batcheller Sumner was born at Canton, Mass., on Nov. 19, 1887, as the son of Charles Sumner and Elizabeth Rand Kelly. His ancestors were Puritans who came from Bicester, England, in 1636 and settled in Boston. His father owned a large country estate, while his grandfather had a farm and also a cotton factory. Young Sumner attended the Eliot Grammar School for a few years and then was sent to Roxbury Latin school. At school he was bored by almost every subject except physics and chemistry. He was interested in fire-arms and often went hunting. While grouse hunting at the age of 17, he was accidentally shot in the left arm by a companion; as a consequence, his arm had to be amputated just below the elbow. Having been left-handed, he then had to learn to do things with his right hand. The loss of his arm made him exert every effort to excel in all sorts of athletic sports, such as tennis, skiing, skating, billiards, and clay-pigeon shooting.  In 1906 Sumner entered Harvard College; he graduated in 1910, having specialized in chemistry. After a short interval of working in the cotton knitting factory owned by his uncle, a type of work that did not interest him in the least, he accepted a teaching post at Mt. Allison College, Sackville, New Brunswick. This was followed by an assistantship in chemistry at Worcester Polytechnic Institute, Worcester, Mass., in 1911, from which he resigned in 1912 in order to study biochemistry with Professor Otto Folin at Harvard Medical School. Although Folin advised him to take up Law, since he thought that a one-armed man could never make a success of chemistry, Sumner persisted and obtained his Ph.D. degree in June, 1914. A few months later while travelling in Europe he was stranded in Switzerland for about a month by the outbreak of World War I. During this time he received a cable inviting him to be Assistant Professor of Biochemistry at Cornell Medical School, Ithaca, N.Y., a post which he held until 1929, when he was made full Professor of Biochemistry.  Sumner’s research work at Cornell first centered around analytical methods; despite hard work he was unable to obtain any interesting results. He then decided to isolate an enzyme in pure form, an ambitious aim never achieved by anyone up till then, but a type of research suited to his scanty apparatus and very small laboratory staff. In particular, he worked with urease.  For many years his work was unsuccessful, but in spite of discouragement from colleagues who doubted whether any enzyme could ever be isolated in pure form he continued. In 1921, when his research was still in its early stages, he had been granted an American-Belgian fellowship and decided to go to Brussels to work with Jean Effront, who had written several books on enzymes. The plan fell through, however, because Effront thought Sumner’s idea of isolating urease was ridiculous. Back in Ithaca, he resumed his work until finally, in 1926, he succeeded (“I went to the telephone and told my wife that I had crystallized the first enzyme”, he wrote in an autobiographical note). His isolation and crystallization of urease met with mixed response; it was ignored or disbelieved by most biochemists, but it brought him a full professorship in 1929.  Gradually, recognition came. In 1937, he was given a Guggenheim Fellowship; he went to Uppsala and worked in the laboratory of Professor [The Svedberg](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1926/index.html) for five months. He was awarded the Scheele Medal in Stockholm in the same year. When Northrop, of the Rockefeller Institute, obtained crystalline pepsin, and subsequently other enzymes, it became clear that Sumner had devised a general crystallization method for enzymes. The opponents gradually admitted Sumner’s and Northrop’s claims – [Willstätter](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1915/index.html) last of all – and the crowning recognition came in 1946 when the Nobel Prize was awarded to Sumner and Northrop. In 1948, Sumner was elected to the National Academy of Sciences (USA).  Sumner was married three times: in 1915 he married Bertha Louise Ricketts whom he later divorced. They had six children, one of whom died at an early age. In 1931 he married Agnes Paulina Lundkvist, and finally in 1943 Mary Morrison Beyer. He died of cancer on August 12, 1955. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0451 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0452** |
| **Biographical** | John Howard Northrop was born in Yonkers, New York, on July 5th, 1891. He is a direct descendant of Joseph Northrop who settled in New Milford, Connecticut in 1639, of Jonathan Edwards, President of Princeton University, 1758, and of Frederick C. Havemeyer, whose family presented Havemeyer Hall, the Chemical Laboratory, to Columbia University. His father, John I. Northrop, an instructor at Columbia, was fatally injured in a laboratory accident shortly before his birth. His mother, Alice R. Northrop, who formerly taught botany at Hunter College, New York City, returned to teaching and was responsible for the introduction of nature study into the curriculum of New York public schools. Her former country home in Massachusetts is maintained by the “Northrop Memorial” as a school of nature study for New York schoolchildren.  After public school education, Northrop entered Columbia University in 1908 to study zoology and chemistry under [T.H. Morgan](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1933/index.html) and J.M. Nelson. He graduated Bachelor of Science in 1912, Master of Arts in 1913 and received his Ph.D. in chemistry in 1915. He was appointed W.B. Cutting Travelling Fellow and spent the next year in Jacques Loeb’s laboratory at the Rockefeller Institute. The following year, he was appointed to the staff of the Institute and with the exception of his war service as a Captain in the Chemical Warfare Service (1917-1918), he has remained with the Institute ever since, becoming an Associate in 1917, Associate Member in 1920, and Member in 1924. In 1949 he was appointed Professor of Bacteriology, University of California, and later, Professor of Biophysics.  Northrop’s researches at Columbia were chiefly concerned with carbohydrates and his early work at the Rockefeller Institute was connected with theories of duration of life. Whilst in the service, he discovered a fermentation process for acetone, which he developed to the pilot-plant stage, and on demobilization and return to the Institute, he worked with Loeb on a kinetical study of enzymes essential to life processes. In 1929 he isolated pepsin in pure crystalline form by techniques which were later used by himself and other workers to crystallize trypsin, chymotrypsin, carboxypeptidase, and pepsinogen. He studied the proteins of viruses and antibodies, and succeeded in isolating a nucleoprotein which had bacteriophage activity: his suggestion that the activity was due to nucleic acid was later proved to be correct. During World War II, he was consultant to the National Defense Research Committee and he studied the mode of action of war gases and methods of detection, developing an apparatus for their automatic detection and analysis. His more recent researches have included work on the origin and relationship of viruses and the transforming principle.  Professor Northrop is the author of *Crystalline Enzymes*, published in 1939. He edited the *Journal of General Physiology* (Rockefeller Institute) for some years and he has written numerous papers on the physical chemistry of proteins, agglutination of bacteria, kinetics of enzyme reaction, and the chemical nature of enzymes.  He was awarded the Stevens Prize (Columbia) in 1931; Chandler Medal, 1936; Daniel Giraud Elliot Medal, 1939; the Certificate of Merit (U.S. Government), 1948; Alex. Hamilton Medal, 1961. He has received honorary Doctor of Science degrees from the Universities of Harvard, Columbia, Yale, Princeton, and Rutgers; and honorary Doctor of Law from the University of California. He is Honorary Fellow of the Chemical Society (London) and a member of many other scientific societies.  Professor Northrop married Louise Walker in 1917. Their only son, John, is an oceanographer and their only daughter, Alice, married Professor [Frederick C. Robbins](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1954/index.html), Nobel Laureate in Medicine, 1954. He is fond of riding and sailing, plays golf and tennis, but his chief hobbies are field shooting and salmon fishing. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0452 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0453** |
| **Biographical** | Wendell Meredith Stanley was born in Ridgeville, Indiana, on August 16th, 1904. He began his advanced education at Earlham College and graduated Bachelor of Science in 1926 when he entered the University of Illinois, gaining a Master of Science degree in 1927 and a Ph.D. in chemistry in 1929. He continued at Illinois as a research associate and later as an instructor before leaving, in the latter half of 1930, to do research at Munich as a National Research Council Fellow. In Munich he worked with [Heinrich Wieland](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1927/index.html) until late 1931 when he returned to the United States to take up a post as Assistant at the Rockefeller Institute. He remained with the Institute until 1948, becoming an Associate Member in 1937, and a Member in 1940. In 1948, he was appointed Professor of Biochemistry and Director of the Virus Laboratory, University of California; during 1948-1953 he was Chairman of the Department of Biochemistry, and in 1958 he became Professor of Virology and Chairman of the Department.  Stanley has been responsible for much important work on lepracidal compounds, diphenyl stereochemistry and the chemistry of the sterols. His researches on the virus which causes the mosaic disease in tobacco plants led to the isolation of a nucleoprotein which displayed tobacco mosaic virus activity. The virus appeared to act like an inanimate chemical but it presented evidence of being a living and growing organism. His more recent work on the preparation and investigation of influenza and similar viruses has led to his development of the centrifuge-type influenza vaccine. He is a recognized world authority on viruses and, as such, he has written over 150 papers on the topic and contributed chapters to several books.  Professor Stanley was awarded the American Association for the Advancement of Science Prize in 1937, and his many other honours and awards include the Rosenburger Medal (University of Chicago), Alder Prize (Harvard), and Scott Award (City of Philadelphia), 1938; Gold Medal of the American Institute of New York, 1941; Copernican Citation, 1943; Nichols Medal (American Chemical Society), 1946; Gibbs Medal (American Chemical Society), 1947; Franklin Medal and Presidential Certificate of Merit, 1948; the Modern Medicine Award, 1958; and the American Cancer Society’s Medal for Distinguished Service in Cancer Control, 1963. He has been awarded honorary Doctor of Science degrees by many universities and colleges, including Earlham, Harvard, Yale (1938), Princeton (1947) and Illinois (1959); honorary Doctor of Law degrees of the Universities of California (1946) and Indiana (1951), the Jewish Theological Seminary of America (1953) and Mills College (1960); and Doctor, *honoris causa*, University of Paris (1947).  He has served on the Councils of many academic, medical and other learned societies, and as consultant and advisor to the United States government and the World Health Organization. He continues as Director-at-large, American Cancer Society and as a Member of the Board of Scientific Counsellors of the National Cancer Institute. He is a member of many scientific societies.  Wendell M. Stanley married Marian Staples on June 15th, 1929. They have one son, Wendell M. Junior, and three daughters, Marjorie Jean, Dorothy Claire, and Janet Elizabeth. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0453 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0454** |
| **Biographical** | Artturi Ilmari Virtanen was born in Helsinki on the 15th of January, 1895, as the son of Kaarlo Virtanen and Serafiina Isotalo. He was educated at the Classical Lyceum at Viipuri, Finland. After finishing school, he studied chemistry, biology, and physics at the University of Helsinki, where he took his M.Sc. in 1916 and obtained his D.Sc. in 1919. Subsequently he studied physical chemistry in Zurich in 1920 under G. Wiegner, bacteriology in Stockholm in 1921 under Chr. Barthel, and enzymology in Stockholm during 1923-1924 under [H. von Euler](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1929/index.html). Since 1923, his interest turned to biochemistry.  He was first-assistant of the Central Laboratory of Industries at Helsinki during 1916-1917, and chemist in the Laboratory of Valio, Finnish Cooperative Dairies’ Association, during 1919-1920. In 1921 he became Director of this laboratory, and in 1931 of the Biochemical Research Institute at Helsinki. After having been docent in chemistry at the University of Helsinki since 1924, he was appointed Professor of Biochemistry at the Finland Institute of Technology at Helsinki in 1931, and at the University of Helsinki in 1939. Since 1948 he has been member and President of the State Academy of Science and Arts in Finland.  Professor Virtanen is a member of the Finnish, Norwegian, Swedish, Flemish, Bavarian, and Pontifical Academies of Science, and of the Swedish and Danish Academies of Engineering Sciences. He is an honorary member of learned societies in Finland, Sweden, Austria, Edinburgh, and the U.S.A., and holds honorary degrees of the Universities of Lund, Paris, Giessen, and Helsinki, the Royal Technical College at Stockholm, and the Finland Institute of Technology. Numerous medals and other distinctions have been conferred upon him from Sweden, Finland, Belgium, and Italy.  Virtanen established the indispensability of cozymase in lactic and propionic acid fermentations, as well as the phosphorylation of sugar (1924). In these works the similarity of different fermentation processes as to the first stages in the decomposition of sugar became apparent. Together with his collaborators he continued the fermentation experiments, special attention being paid to the mechanism of different bacterial fermentations. The fermentation of dioxyaceton to glycerol and glyceric acid in the presence of phosphates by the effect of *Coli* bacteria (1929) was the first sugar fermentation which was elucidated chemically from beginning to end. In this work attention was also paid to the adaptive formation of enzymes, which phenomenon his collaborator H. Karström treated in great detail in his doctor’s thesis (constitutive and adaptive enzymes). The phenomenon of adaptation, and in connection with it the uptake of nutrients by cells, is still subjected to investigations in his laboratory. The concept that almost all proteins in bacterial cells are enzyme proteins led to investigations on the relation between the protein content and enzymic activity of cells.  Since 1925, the biological nitrogen fixation which takes place in the root nodules of leguminous plants has been subjected to many-sided investigations in his laboratory. The importance of the red pigment, leghaemoglobin, in active root nodules for the fixation of nitrogen was proved.  The formation of vitamins in plants, as well as the ability of plants to utilize organic nitrogen compounds as their nitrogen source, have been treated in many publications from his laboratory.  Since the end of the 1940’s, the chemical composition of higher plants has been given special attention in his laboratory. A large number of new amino acids have been isolated from different plants, and have been characterized chemically. Numerous organic sulphur compounds, which may be of importance for the nutrition of man and domestic animals, have also been isolated from vegetables and fodder plants.  The application of biochemistry to agriculture and the dairy industry belongs to the practical activities of his laboratory. Among works performed in this field are the creation of a theoretical basis for the preservation of fresh fodder and the development of a practical method on this basis (the AIV method), with the aim to promote an effective utilization of protein-rich crops, and to produce milk of the same vitamin content in winter as that produced on summer pastures. Investigations aiming at the improvement of the quality of dairy products also have to be mentioned as belonging to the field of applied research.  Virtanen married Lilja Moisio in 1920; they have two sons, Kaarlo and Olavi. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0454 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0455** |
| **Biographical** | Otto Hahn was born on 8th March, 1879, at Frankfurt-on-Main. He attended the secondary high school there until he matriculated.  From 1897 Hahn studied chemistry at Marburg and Munich, taking his doctorate examination in 1901 at Marburg and submitting to Professor Theodor Zincke a thesis on organic chemistry.  He obtained a post as assistant in the Chemical Institute at Marburg, staying there two years, after which he worked under [Sir William Ramsay](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1904/index.html) at University College, London, from the autumn of 1904 to the following summer. His work here was rewarded by the discovery of a new radioactive substance, radiothorium, while working on the preparation of pure radium salts.  From the autumn of 1905 to the summer of the following year Hahn was at the Physical Institute of McGill University, Montreal (Canada) working under Professor [Ernest Rutherford.](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1908/index.html) Here he discovered radioactinium and conducted investigations with Rutherford on alpha-rays of radiothorium and radioactinium.  On his return to Europe Hahn moved to Berlin, to the Chemical Institute ([Emil Fischer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1902/index.html)) of the University and there he qualified as a university lecturer in the spring of 1907, which year also saw his discovery of mesothorium.  At the end of 1907, Dr. Lise Meitner came to Berlin from Vienna and then began more than thirty years’ collaboration. Their joint work embraced: investigations on beta-rays, their absorbability, magnetic spectra, etc.; use of the radioactive recoil, discovered shortly before by Hahn, to obtain new radioactive transformation products.  Between 1914 and 1918 Hahn’s work was interrupted by his service in the First World War, but he resumed his research with Professor Meitner in 1918, and discovered protactinium, the long-lived mother substance of the actinium series. Hahn’s own particular sphere was chemistry and he further discovered uranium Z, the first case of a nuclear isomerism of radioactive kinds of atoms. Using radioactive methods he investigated the absorption and precipitation of the smallest quantities of substances, normal and abnormal formation of crystals, etc. Hahn used the emanation method to test substances superficially rich or poor, and he elaborated the strontium method to determine the age of geological periods.  Following the discovery of artificial radioactivity by [M and Mme. Joliot-Curie](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1935/index.html) and the use of neutrons by [Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html) for atomic nuclear processes, Hahn again collaborated with Professor Meitner and afterwards with Dr. Strassmann on the processes of irradiating uranium and thorium with neutrons.  Hahn and Prof. Meitner had also worked together on the discovery of an artificially active uranium isotope, which represents the basic substance of the elements neptunium and plutonium, first revealed later in America.  Hahn’s work has won recognition in many learned circles. In 1912 he became scientific member of the Kaiser Wilhelm Institute for Chemistry and has been Director of this Institute since 1928. 1933 saw his appointment as Visiting Professor at Cornell University, Ithaca, New York. From 1st April, 1946, he has officiated as President of the Kaiser Wilhelm Society and from 28th February, 1948, has served as President of the Max Planck Society in Western Germany, being created Honorary President of the same Society in May, 1960.  His most spectacular discovery came at the end of 1938. While working jointly with Dr. Strassmann, Hahn discovered the fission of uranium and thorium in medium heavy atomic nuclei and his first work on these subjects appeared on 6th January and 10th February, 1939, in *Naturwissenschaften*. Since that time and until 1944 Hahn continued investigation on the proof and separation of many elements and kinds of atoms which arise through fission.  Hahn has been granted membership of the Academies of Berlin, Göttingen, Munich, Halle, Stockholm, Vienna, Boston, Madrid, Helsinki, Lisbon, Mainz, Rome (Vatican), Allahabad, Copenhagen, and the Indian Academy of Sciences.  In 1913 Hahn married Edith, *née* Junghans and they had one son, Hanno, born in 1922, killed by accident in 1960. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0455 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0456** |
| **Biographical** | George de Hevesy was born in Budapest on August 1st, 1885, the son of Louis de Hevesy, Court Counsellor and Eugénie, née Baroness Schosberger. After matriculating at the Gymnasium of the Piarist Order in 1903 he studied at Budapest University and Berlin Technical University and he gained his doctor’s degree at the University of Freiburg im Breisgau in 1908. He worked for two years as an assistant at the Institute of Physical Chemistry, Technical University of Switzerland before having a short spell with Professor [Fritz Haber](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1918/index.html) when he was able to witness much of the fundamental work of Haber and Rossignol on ammonia synthesis. He travelled to England in 1910 to study under Professor [Ernest Rutherford](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1908/index.html) at Manchester. He interrupted early in 1913 his studies to carry out jointly with Frederic Paneth the first radioactive-tracer experiment at the Vienna Institute of Radium Research. During his stay in Vienna he obtained the Venia Legendi in the University of Budapest. In 1915 he was drafted into the Austrian-Hungarian Army. After the end of the war he was teaching for 6 months in the University of Budapest and left the spring of 1919 for Copenhagen to discuss his future activities at [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html)‘s Institute which was to be erected. In 1920 he settled in Copenhagen.  Six years later he returned to Freiburg as Professor of Physical Chemistry. In 1930 he was appointed Baker Lecturer at Cornell University, Ithaca. Four years later he took up again his activities at Niels Bohr’s Institute which he terminated in 1952. He was domiciled in Stockholm since 1943 and was an Associate of the Institute of Research in Organic Chemistry. In 1949 he was elected Franqui Professor in the University of Ghent. In his retirement, he remains an active scientific associate of the University of Stockholm.  His early investigations involved a study of the chemical behaviour of molten salts and his introduction to practical radiochemistry came in Rutherford’s laboratories at Manchester. His work there, and later in Vienna and Budapest, mainly concerned the investigation and use of radium and lead isotopes.  In Copenhagen, de Hevesy’s researches were initially concerned with isotopic separations and in 1923, together with Coster, he discovered the element hafnium. He was responsible for pioneer work in the use of isotopic indicators both in inorganic and life sciences and later, in Freiburg, he was involved in the first clinical use of isotopes. On his return to Copenhagen, he demonstrated the formation of new artificially radioactive isotopes and subsequently introduced a method of activation analysis based on neutron bombardment of the element to be investigated. This method was to replace X-ray analysis with fluorescent X-rays which he introduced during his stay in Freiburg. The year 1934 saw the beginning of numerous investigations in the field of plant and animal physiology, using labelled atoms: these researches were supported by generous grants made by the Carlsberg Foundation, the Rask-Ørsted Foundation, the Rockefeller Foundation and others. His work in Sweden has continued on the same lines and he has studied, amongst other things, the effect of X-rays on the formation of nucleic acid in tumours and in normal organs, and iron transport in healthy and cancerous organisms; this work is supported by the Swedish State Research Council and the Wallenberg Foundation.  Professor de Hevesy is the author of several important books on radiochemistry and his many scientific papers are valuable and accurate records of devoted work. He was awarded the Cannizaro Prize (Academy of Sciences, Rome) in 1929, he was the Copley Medallist (Royal Society, London) in 1949, Faraday Medallist in 1950, Baily Medallist in 1951 and Silvanus Thompson Medallist in 1955. In 1959 he received the Ford Foundation’s Atoms for Peace Award Medal, in 1961 the Niels Bohr Medal and the Rosenberger Medal of the University of Chicago. Honorary degrees conferred upon Professor de Hevesy include Doctor of Philosophy, Uppsala, Freiburg, and Copenhagen; Doctor of Science, Ghent, Liège, London, and Capetown; and Doctor of Medicine, São Paulo, Rio de Janeiro, Turin, and Freiburg. He is a Fellow of the Royal Society (London), the [Swedish Academy of Sciences](http://www.kva.se/), Gothenburg Academy, and eleven other scientific academies. He is Honorary Fellow of the Chemical Society (London), the Royal Institution (London), the British Institute of Radiology, the Finnish Chemical Society, the German Bunsen Society, the German Physiological Society, the Chemical Society of Japan, and the American Society of Nuclear Medicine. In addition, he holds honorary memberships of many more learned societies.  Professor de Hevesy married Pia Riis in 1924. They have one son and three daughters. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0456 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0457** |
| **Biographical** | Adolf Frederick Johann Butenandt was born on March 24, 1903 at Bremerhaven-Wesermünde. The son of a business man Otto Butenandt of Hamburg, he went to school at Bremerhaven and studied chemistry at the Universities of Marburg and Göttingen. In 1927 he graduated at the University of Göttingen, where he had studied under [Adolf Windaus.](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1928/index.html)  From 1927 until 1930 he was Scientific Assistant at the Institute of Chemistry, Göttingen, and from 1931 until 1933 he was Privatdozent in the Department of Biological Chemistry at the University of Göttingen and acting Head of the laboratories for organic and inorganic chemistry. He then became Professor Ordinarius and Director of the Institute for Organic Chemistry at the Institute of Technology at Danzig, a post which he held until 1936.  From 1936 until 1960 he was Professor in the University of Berlin and Director of the Max Planck Institute for Biochemistry, Berlin-Dahlem, which later moved to Tübingen and then to Munich. From 1945 until 1956 he was Professor of Physiological Chemistry at Tübingen and in 1956 he became Professor of Physiological Chemistry in the University of Munich. From 1956 until 1960 he was Director of the Institute of Physiological Chemistry in the University of Munich.  Since 1960 he has been President of the Max Planck Society at Munich.  Butenandt’s name will always be associated with his work on sex hormones, for which he was awarded, jointly with Leopold Ruzicka, the Nobel Prize for Chemistry for 1939. In 1929 he isolated oestrone in pure, crystalline form, almost at the same time that [E.A. Doisy](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1943/index.html) did this in America. In 1931 he isolated androsterone in pure, crystalline form. From androsterone he as well as Ruzicka, independently of each other, obtained testosterone in 1939, a compound which had been obtained from the testes in 1935 by Ernst Laqueur. Progesterone was isolated by Butenandt from the corpus luteum in 1934.  In addition to these researches, Butenandt carried out much investigation of the interrelationships of the sex hormones and on the possible carcinogenic properties of some of them. His work on the sex hormones was largely responsible for the production of cortisone on a large scale.  A great number of honours and distinctions was bestowed upon him. He was awarded several medals and prizes from Germany, France, Sweden and England, he received the Grand Cross for Federal Services with Star (1959), he holds six honorary doctorates (Munich, Graz, Leeds, Madrid and two from Tübingen) and is Freeman of the city of Bremerhaven. He is corresponding member of the Academy of Sciences at Göttingen, honorary life member of the New York Academy of Sciences, and honorary member of the Japanese Biochemical Society, the Deutsche Akademie der Naturforscher Leopoldina, Halle, and the Austrian Academy of Sciences.  He married Erika Ziegner in 1931; they have seven children and live at Munich-Obermenzing. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0457 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0458** |
| **Biographical** | Leopold Stephen Ruzicka[\*](https://www.nobelprize.org/prizes/chemistry/1939/ruzicka/biographical/#anchor1164534) was born on September 13, 1887, in Vukovar, a small Croatian town on the Danube, somewhat east of its confluence with the Drava. His father, Stjepan Ruzicka, was a cooper; his mother’s maiden name was Ljubica Sever. His great-grandparents included a Czech, from whom the name Ruzicka stems, an Upper Austrian and his wife from Wurtemberg, the other five being Croats. His ancestors were artisans or farmers, who had enjoyed at most a few years of schooling. After the early death of his father in 1891, he returned with his mother to her birthplace, Osijek, on the Drava somewhat west of its junction with the Danube. There he attended the primary school and the classical gymnasium where the Croatian language was used. He was a fairly good pupil in a general way, but really interested only in physics and mathematics. The other subjects, including the purely descriptive sciences, left him cold. There was no chemistry in the curriculum but, nevertheless, he decided to study this subject out of his interest in the composition of natural products.  He wanted to study at the Zurich Polytechnic Institute but found to his dismay that an entrance examination was required not only in chemistry but also in “descriptive geometry”. He decided to go instead to Germany, where anyone with a completed secondary school education was acceptable as a student at a University or Technical Institute without having to undergo additional entrance examinations. He chose the Technische Hochschule at Karlsruhe, where he began his chemical studies in 1906. This step proved to be decisive for his future. Only later did he discover that in Zurich the curriculum, including practical work, was organized on a very rigid basis; still in 1906 attendance at the lectures was or could be checked. In Karlsruhe, on the other hand, there was considerable freedom. He completed his laboratory courses in 1 3 /4 years and then immediately started his doctoral work on ketenes with Professor [Staudinger](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1953/index.html), who was, at 27, only 6 1/2 years older. There were few bureaucratic formalities; he had attended the prescribed lectures neither in chemical technology nor, unfortunately, in physical chemistry and physics.  After two years of research work Ruzicka was a “Dipl. Ing”, and two weeks later “Dr. Ing”. Staudinger appointed him as his assistant, and they together entered the quite unexplored field of the active constituents – named by them pyrethrins – of Dalmatian insect powder, a plant product, toxic to insects and other coldblooded animals. They thus opened a new chapter of alicyclic chemistry, which was then as unfamiliar to Ruzicka as it was to Staudinger.  In October 1912 he followed Staudinger who became [Willstätter](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1915/index.html)‘s successor at the newly dubbed “Eidgenössische Technische Hochschule” (ETH, Swiss Federal Institute of Technology) at Zurich. In Switzerland he found not only a second homeland but also a peaceful and convivial environment that brought with it all the conditions for uninterrupted research. In 1917 he acquired Swiss citizenship.  In the previous year he had already started independent work and this decision initiated the most fruitful and happy decade of his life, when he could work at the laboratory bench from morning to night, on problems of his own choosing, with no teaching responsibilities, except one hour weekly from 1918 onward. During the years 1920-1924 he laid the fundaments of all his future work.  For the accomplishment of his Habilitation work (necessary to become a “Privatdozent”) in 1916-1917 he was glad of the support of the oldest perfume manufacturers in the world, Haarman & Reimer, of Holzminden in Germany. The starting-point for their collaboration was the Tiemann formula for drone; the results were the total synthesis of fenchone and the extension and interpretation of the Wagner rearrangement (this term was then introduced by him). After his habilitation in 1918 the firm of Ciba, Basle, became interested in his work on the preparation of quinine-like compounds. With various co-workers, the first synthesis of b-collidine and of linalool, the partial synthesis of pinene, and a series of investigations in the monoterpene field were carried out.  In 1921, the Geneva perfume manufacturers Chuit, Naef & Firmenich, asked him to collaborate with them. By this time the investigations that were to lead to the elucidation of the constitutional formulas of the higher terpenes has already been started. In the perfumery and sesquiterpene domain the total syntheses of nerolidol and farnesol were carried through. The structure of jasmone was established, Tiemann’s irone formula corrected, and synthetic work in these fields undertaken. But by far the most important fruits collected in the perfumery garden were the elucidations of the structures of the naturally occurring musk perfumes, civetone and muscone. Following these discoveries Ruzicka and his co-workers were able to prepare the whole series of alicyclic ketones with 9 to over 30 carbon atoms as ring members, compounds that had previously been believed to be incapable to existence.  Most of the years 1925-1926 he spent with his friends in Geneva. From October 1926 till 1929 he was Professor of Organic Chemistry at the University of Utrecht. Although he was very happy in Holland, he decided to accept the invitation of the ETH to return to Zurich. The main reason for this decision was the strength of the Swiss organic chemical industry, especially its pharmaceutical and perfumery branches, which required the skill and energy of a whole army of chemists with a thorough training in modern organic chemistry. This circumstance was not only a challenge to the teaching and research abilities of the professors; it also encouraged students to acquire the necessary understanding of the theories and methods to equip them for a career in these industries. The three-fold community of professors, students and industry was thus bound together by bonds of common interest.  In 1930, the Ciba renewed the contact with his laboratory. This association led in a few years to scientifically as well as industrially important successes in the field of the male sex hormones. From 1937 the Rockefeller Foundation generously provided financial backing for the research on natural compounds, especially the triterpenes and steroids, free from any special conditions. With the two industries, there was thus formed a strong group of constant supporters of his research team which had grown much in the meantime.  Professor Ruzicka holds eight honorary doctorates (4 Science, 2 Medicine, 1 Natural Sciences, 1 Law) 7 prizes and medals, 24 honorary memberships of chemical, biochemical and other scientific societies, 18 honorary, ordinary and foreign memberships of scientific academies. The circle of his friends is very wide, not only geographically but also spiritually, including the Vatican City as well as Moscow. He feels that the honours which he has won should be distributed among the whole team of his co-workers, and that, to mention only one example, the laudation of his 1936 honorary Doctor diploma of Harvard (tercentenary celebration of the oldest USA university) should more realistically be read in the plural form “… to the team of chemists, daring in their attacks, brilliant in their methods, successful in their interpretations of the architecture of nature’s baffling compounds”, since every member of the team helped to transform the youthful dreams of its oldest member into reality.  Ruzicka married Anna Hausmann in 1912, and Gertrud Acklin in 1951. He has no children, and lives in Zurich. His hobbies are old Dutch and Flemish paintings and alpine plants gardening.  \* The pronunciation can be best explained by the French transcription “Rougitchka”. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0458 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0459** |
| **Biographical** | Richard Kuhn was born in Vienna on December 3, 1900. His father, Richard Clemens Kuhn, was an engineer and “Hofrat”; his mother, Angelika Rodler, was a teacher in elementary schools. He was educated at the “Gymnasium” (grammar school). He studied chemistry at Vienna University and later at Munich under [R. Willstätter](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1915/index.html). He obtained his degree (D.Ph.) in 1922 following his thesis *Über Spezifität der Enzyme* (On the specificity of enzymes).  In 1925 Munich University invited him to lecture on chemistry. Kuhn accepted an invitation by the Eidgenössische Technische Hochschule in Zurich, where he held the post of Professor for General and Analytical Chemistry from 1926 until 1929. He then became Principal of the Institute for Chemistry at the newly founded Kaiser-Wilhelm-Institut (since 1950, Max Planck Institut) for Medical Research in Heidelberg; in 1937 he also took over the administration of this Institute, in succession to L. von Krehl. Besides these duties he also filled the post of Professor of Biochemistry at the University of Heidelberg. For one year he was at the University of Pennsylvania, Philadelphia, as a Visiting Research Professor for Physiological Chemistry.  Kuhn investigated theoretical problems of organic chemistry (stereochemistry of aliphatic and aromatic compounds; syntheses of polyenes and cumulenes; constitution and colour; the acidity of hydrocarbons), as well as extensive fields in biochemistry (carotenoids; flavins; vitamins and enzymes).  For a period of twenty years Kuhn investigated compounds containing double bonds which proved to be of great interest in connection with the study of the chemical nature of the carotenoids. He discovered eight new types of these and was able to analyse their constitution. He also carried out important work on vitamin B2 and the antidermatitis vitamin B6. For his work in this important field Kuhn was awarded the Nobel Prize for Chemistry in 1938.  Kuhn received numerous other honours and awards in recognition of his work; he was awarded honorary degrees of the Technische Hochschule in Munich (1960), the University of Vienna (1960), the University of St. Maria, Brazil (1961). An impressive number of medals and prizes have been presented to him, as well as orders and the highest possible honours, in many countries. Kuhn is also a member of many scientific academies, societies and organizations in all parts of the world. He is the President of the Society of German Chemists, Vice-President of the Max Planck Society and Editor of *Liebigs Annalen der Chemie*. He belongs to the Board of the “Badische Anilin- und Soda-Fabrik”.  It may be mentioned that for a period of eight years (1910-1918) Richard Kuhn was a schoolmate of [Wolfgang Pauli](https://www.nobelprize.org/nobel_prizes/physics/laureates/1945/index.html) who was awarded the Nobel Prize in Physics for 1945.  Richard Kuhn married Daisy Hartmann in 1928 and has two sons and four daughters. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0459 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0460** |
| **Biographical** | Walter Norman Haworth was born at Chorley, Lancashire, on March 19, 1883. He attended the local school until the age of fourteen when he joined his father, Thomas Haworth, to learn linoleum design and manufacture. His interest in chemistry was aroused through the use of dyestuffs in his work and his thirst for further knowledge led him to seek private tuition in Preston. This coaching enabled him to pass the entrance examination of the University of Manchester and in 1903 he entered the Chemistry Department as a pupil of W.H. Perkin, Junior. He graduated with first class honours in 1906 and after three years research he went, on a scholarship, to [Wallach](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1910/index.html)‘s laboratory at Göttingen. He received his doctor’s degree in 1910 and returned to Manchester to be awarded his D.Sc. degree in 1911 – these qualifications were gained in the minimum time possible.  In 1911, Haworth took an appointment as a demonstrator at the Imperial College, London and in 1912 he moved to St. Andrews, Scotland, as Lecturer and Reader in Chemistry. In 1920, he was called to the Chair in Chemistry at the University of Durham and in the following year succeeded Phillips Bedson as Director. Haworth was appointed Professor and Director of the Department of Chemistry in the University of Birmingham in 1925 and he remained in this position until his retirement in 1948, becoming Dean of the Faculty of Science and acting as Vice-Principal during 1947-1948. Sir Norman was active in retirement, serving on many Boards and Committees; he represented the Royal Society at the Seventh Pacific Science Congress in New Zealand during February, 1949. He was knighted in 1947.  Haworth’s early researches, initially with Perkin, involved investigations on the constitution of terpenes and in 1912 he synthesized sylvestrene. At St. Andrews, in association with T. Purdie and J.C. Irvine, he turned his attention to carbohydrates, extending [Emil Fischer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1902/index.html)‘s method of reacting sugars with methanol to an elegant preparation of methylated derivatives which were, in turn, used to characterize the constitution of sugars.  During the First World War, Haworth organized the laboratories at St. Andrews for the production of fine chemicals and drugs; after the war he returned once more to his carbohydrate investigations. By 1928, he had evolved and confirmed, among others, the structures of maltose, cellobiose, lactose, gentiobiose, melibiose, gentianose, raffinose and the glucoside ring structure of normal sugars. He studied lactones from sugars and co-related structure with optical rotatory powers. His method for the determination of chain length in methylated polysaccharides, an important structural problem, helped to settle the basic features of the starch, cellulose, glycogen, inulin and xylan molecules.  Following the synthesis of ascorbic acid, which considerably cheapened commercial production, Haworth’s later researches contributed greatly towards further co-ordination of the chemical, physical and biological, problems concerned with bacterial polysaccharides.  Haworth wrote numerous scientific papers and contributed to *Advances in Carbobydrate Chemistry*. His book *The Constitution of Sugars* was published in 1929.  Haworth was President of the Chemical Society (1944-1946), and Fellow (1928), and Vice-President (1947-1948) of the Royal Society. He received honorary science degrees from the Universities of Belfast, Zurich and Oslo, honorary Doctor of Law, University of Manchester, and foreign memberships of nine foreign scientific academies. He was the Longstaff Medallist (Chemical Society), 1933; Davy Medallist (Royal Society), 1934, and Royal Medallist, 1942.  In 1922, Haworth married Violet Chilton, second daughter of Sir James Dobbie, LL.D., F.R.S. They had two sons. He died suddenly on March 19, 1950. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0460 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0461** |
| **Biographical** | Paul Karrer was born in Moscow on April 21, 1889. His parents, Paul Karrer and Julie Lerch, were Swiss nationals and in 1892 the family returned to Switzerland where he received his early education at Wildegg and at the grammar school in Lenzburg, Aarau, where he matriculated in 1908. He studied chemistry at University of Zurich under Professor [Alfred Werner](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1913/index.html) and after gaining his Ph.D. in 1911, he spent a further year as assistant in the Chemical Institute. In 1912 he took a post as chemist with [Paul Ehrlich](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1908/index.html) at the Georg Speyer Haus, Frankfurt-am-Main; he left Frankfurt six years later on his election as reader at University of Zurich. In 1919 he became Professor of Chemistry and Director of the Chemical Institute.  His early researches involved the preparation and investigation into the properties of complex metal compounds but his most important work has concerned plant pigments, particularly the yellow carotenoids. He was responsible for elucidating the chemical structure of the carotenoids and he also showed that some of these substances are transformed in the animal body into vitamin A. His work in this field led, in 1930, to the establishment of the correct constitutional formula for b-carotene, the chief precursor of vitamin A; this, the first time that the structure of a vitamin or provitamin had been established, in turn led to the clarification of the structure of vitamin A itself. Later, he confirmed the structure ascribed to ascorbic acid (vitamin C) by [Albert von Szent-Györgyi](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1937/index.html) and he extended his researches into the vitamin B2 and E fields. His important contributions to the chemistry of the flavins led to identification of lactoflavin as part of the complex originally thought to be vitamin B2.  Professor Karrer has published over 1,000 scientific papers in the various fields of organic chemistry, especially concerning vitamins A, B2, C, E and K, co-enzymes, carotenoids and other plant pigments, curare and other alkaloids, amino acids, carbohydrates and organo-arsenic compounds. His *Lehrbuch der Organischen Chemie* (1930) has passed through 13 editions and has been translated in full into English, Italian, Spanish, French, Polish and Japanese. His monograph on carotenoids (1948) has also been translated into English.  Karrer was President of the 14th International Congress on Pure and Applied Chemistry (Zurich, 1955). He has received honorary doctorate degrees from universities in Europe and America; they include Dr.med. Basle, Breslau, Lausanne and Zurich; Ph.D. Lyons, Paris, Sofia, London, Turin, Brussels and Rio de Janeiro; and Dr.Pharm. Madrid and Strasbourg. He has been awarded the Marcel Benoist Prize and the Cannizzaro Prize and he is a full member or honorary, corresponding or associate member of numerous chemical and biochemical societies throughout the world. These include the Academie des Sciences (Paris); the Royal Society (London); National Academy of Science (Washington); [Royal Academy of Sciences (Stockholm)](http://www.kva.se/); the National Academy (Rome); Royal Academy of Belgium; the Indian Academy of Science; the Royal Netherlands Academy of Sciences, and the Chemical Societies of Britain, France, Germany, Belgium, India and Austria.  Karrer married Helena Froelich in 1914. They have two sons. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0461 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0462** |
| **Biographical** | Petrus (Peter) Josephus Wilhelmus Debye was born March 24, 1884, at Maastricht, the Netherlands. He received his early education at the elementary and secondary schools in his home town and from then on his life has been devoted to a search for knowledge. He continued his studies at the Aachen Institute of Technology (Technische Hochschule) and gained a degree in electrical technology there, in 1905. This led to his appointment as Assistant in Technical Mechanics at the Aachen Technological Institute, where he worked for two years. In 1906 Debye obtained a similar position in Theoretical Physics at Munich University, where he qualified as a University lecturer in 1910 (having obtained this University’s Ph.D. in Physics in 1908).  In the following year, i.e. 1911, Debye became Professor of Theoretical Physics at Zurich University, where he remained for two years. He returned to The Netherlands in 1912 when he was appointed Professor of Theoretical Physics at Utrecht University, and in 1914 he moved to the University of Göttingen, to take charge of the Theoretical Department of the Physical Institute. Later, he became Director of the entire Institute and lectured on experimental physics until 1920.  In 1915 Professor Debye became Editor of *Physikalische Zeitschrift* and continued to act in this capacity until 1940.  Debye returned to Zurich in 1920, as Professor of Physics, and Principal of the Eidgenössische Technische Hochschule. In 1927 he held the same post at Leipzig and from 1934 to 1939 he was Director of the Max Planck Institute of the Kaiser Wilhelm Institute for Physics in Berlin-Dahlem and Professor of Physics at the University of Berlin.  This appointment terminated his work in Europe, and in 1940 he became Professor of Chemistry and Principal of the Chemistry Department of Cornell University, Ithaca, New York, taking American citizenship in 1946.  The year 1952 saw Debye’s resignation of his post of Head of the Chemistry Department at Cornell University and his appointment later as Emeritus Professor of Chemistry at Cornell University.  Professor Debye was wedded to Physics and Chemistry and his devotion to his work gained him many distinctions, and Honorary Doctorates have been conferred upon him by the following universities and learned institutes: Brussels and Liège; Oxford; Sofia; Mainz; Technische Hochschule, Aachen; Eidgenösissche Technische Hochschule, Switzerland; and in the United States: Harvard; St. Lawrence; Colgate; Notre Dame; Holy Cross; Brooklyn Polytechnic; Boston College; Providence College. He holds the Rumford Medal of the Royal Society, London, and the Franklin and Faraday Medals, the Lorentz Medal of the Royal Netherlands Academy, the Max Planck Medal (1950) awarded by the West Germany Physical Society, the Willard Gibbs Medal “Chicago (1949), the Nichols Medal (1961), the Kendall Award (Miami, 1957), and the Priestley Medal of the American Chemical Society (1963); and was appointed Kommandeur des Ordens Leopold II in 1956.  Debye was a Visiting Lecturer at many universities – Columbia, California, Paris, Liège, Oxford, Cambridge, Harvard, Michigan, South California – and has been associated with scientific academies in many countries: Washington, New York, Boston, and Philadelphia in the United States of America; The Netherlands; Great Britain (the Royal Institute of Great Britain and the Royal Society, London); Denmark; Berlin, Göttingen, and Munich (Germany); Brussels and Liège (Belgium); Royal Irish Academy, Dublin; the Papal Academy, Rome; the Indian Academy, Bangalore and the National Institute of Science (India); the Real Sociedad Española de Fisica y Quimica and Academia de Ciencias, Madrid (Spain); and the Academies of Science of the U.S.S.R., Hungary and Argentina. The year 1936 saw the award of the greatest honour possible to him – the Nobel Prize for Chemistry.  Professor Debye is married to Mathilde Alberer and has a son Peter Paul Rupprecht (b. 1916) and a daughter Mathilde Maria (b. 1921), both married. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0462 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0463** |
| **Biographical** | Jean Frédéric Joliot, born in Paris, March 19, 1900, was a graduate of the Ecole de Physique et Chimie of the city of Paris. His father was Henri Joliot, a merchant, and his mother was Emilie Roederer. In 1925 he became, at the Radium Institute, assistant to [Marie Curie](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1911/index.html), whose daughter Iréne he married in 1926. He obtained his Doctor of Science degree in 1930, having prepared a thesis on the electrochemistry of radio-elements, and became lecturer in the Paris Faculty of Science in 1935. At this time he carried out considerable research on the structure of the atom, generally in collaboration with his wife, Iréne Joliot-Curie. In particular they worked on the projection of nuclei, which was an essential step in the discovery of the neutron ([Chadwick](https://www.nobelprize.org/nobel_prizes/physics/laureates/1935/index.html), 1932) and the positron (Anderson, 1932). However, their greatest discovery was artificial radioactivity (1934). By bombardment of boron, aluminium, and magnesium with alpha particles, they produced the isotope 13 of nitrogen, the isotope 30 of phosphorus and, simultaneously, the isotopes 27 of silicon and 28 of aluminium. These elements, not found naturally, decompose spontaneously, with a more or less long period, by emission of positive or negative electrons. It was for this very important discovery that these two physicists received in 1935 the Nobel Prize for Chemistry. During this time F. Joliot, who had always taken an interest in social questions, joined the Socialist Party, the S.F.I.O. (1934), then the League for the Rights of Man (1936)  In 1937 he was nominated Professor at the Collège de France. He left the Radium Institute and had built for his new laboratory of nuclear chemistry the first cyclotron in Western Europe. After the discovery of the fission of the uranium nucleus, he produced a physical roof of the phenomenon; then with Hans Halban and Lev Kowarski, joined by Francis Perrin, he worked on chain reactions and the requirements for the successful construction of an atomic pile using uranium and heavy water; five patents were taken out in 1939 and 1940. On the advance of the German forces (1940), F. Joliot managed to get the documents and materials relating to this work transported to England. During the French occupation he took an active part in the Resistance; he was President of the National Front and formed the French Communist Party. After having been Director of the Centre National de la Recherche Scientifique (1945), he became the first High Commissioner for Atomic Energy (1946); he directed the construction of the first French atomic pile (1948). He was relieved of his duties in 1950 for political reasons. While still retaining the control of his laboratories, F. Joliot-Curie took a considerable part in politics and was elected President of the World Peace Council. On the death of Irene Joliot-Curie, in 1956, he became, while still retaining his professorship at the Collège de France, holder of the Chair of Nuclear Physics which she had held at the Sorbonne.  F. Joliot was a member of the French Academy of Sciences and of the Academy of Medicine. He was also a member of numerous foreign scientific academies and societies, and holder of an honorary doctor’s degree of several universities. He was a Commander of the Legion of Honour. His recreations show him as a man of wide attainments, among which piano playing, landscape painting and reading (particularly [Kipling](https://www.nobelprize.org/nobel_prizes/literature/laureates/1907/index.html)), were predominant.  Joliot devoted the last two years of his life to the inauguration and development of a large centre for nuclear physics at Orsay. He died in Paris in 1958.  Jean Frederic and Irene Joliot-Curie had one daughter, Helene, and one son, Pierre. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0463 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0464** |
| **Biographical** | Irène Curie, born in Paris, September 12, 1897, was the daughter of [Pierre and Marie Curie](https://www.nobelprize.org/nobel_prizes/physics/laureates/1903/index.html), and since 1926 the wife of Frédéric Joliot. After having started her studies at the Faculty of Science in Paris, she served as a nurse radiographer during the First World War. She became Doctor of Science in 1925, having prepared a thesis on the alpha rays of polonium. Either alone or in collaboration with her husband, she did important work on natural and artificial radioactivity, transmutation of elements, and nuclear physics; she shared the Nobel Prize in Chemistry for 1935 with him, in recognition of their synthesis of new radioactive elements, which work has been summarized in their joint paper *Production artificielle d’éléments radioactifs. Preuve chimique de la transmutation des éléments* (1934).  In 1938 her research on the action of neutrons on the heavy elements, was an important step in the discovery of uranium fission. Appointed lecturer in 1932, she became Professor in the Faculty of Science in Paris in 1937, and afterwards Director of the Radium Institute in 1946. Being a Commissioner for Atomic Energy for six years, Irène took part in its creation and in the construction of the first French atomic pile (1948). She was concerned in the inauguration of the large centre for nuclear physics at Orsay for which she worked out the plans. This centre was equipped with a synchro-cyclotron of 160 MeV, and its construction was continued after her death by F. Joliot. She took a keen interest in the social and intellectual advancement of women; she was a member of the Comité National de l’Union des Femmes Françaises and of the World Peace Council. In 1936 Irène Joliot-Curie was appointed Undersecretary of State for Scientific Research. She was a member of several foreign academies and of numerous scientific societies, had honorary doctor’s degrees of several universities, and was an Officer of the Legion of Honour. She died in Paris in 1956.  Jean Frédéric and Irene Joliot-Curie had one daughter, Helene, and one son, Pierre. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0464 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0465** |
| **Biographical** | Harold Clayton Urey was born in Walkerton, Indiana, on April 29, 1893, as the son of the Rev. Samuel Clayton Urey and Cora Rebecca Reinoehl, and grandson of pioneers who settled in Indiana. His early education in rural schools led to his graduation from high school in 1911 after which he taught for three years in country schools. In 1914 he entered the University of Montana and received his Bachelor of Science degree in Zoology in 1917. He spent two years as a research chemist in industry before returning to Montana as an instructor in Chemistry. In 1921 he entered the University of California to work under Professor Lewis and he was awarded the degree of Ph.D. in Chemistry in 1923. He spent the following year in Copenhagen at Professor [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html)‘s Institute for Theoretical Physics as American-Scandinavian Foundation Fellow to Denmark and on his return to the United States he became an Associate in Chemistry at Johns Hopkins University. In 1929 he was appointed Associate Professor in Chemistry at Columbia University and he became Professor in 1934; during the period 1940-1945 he was also Director of War Research, Atomic Bomb Project, Columbia University. He moved to the Institute for Nuclear Studies, University of Chicago in 1945 as Distinguished Service Professor of Chemistry and became Martin A. Ryerson Professor in 1952. He was George Eastman Visiting Professor, University of Oxford, during 1956-1957 and in 1958 he took his present post as Professor-at-Large, University of California.  Professor’s Urey’s early researches concerned the entropy of diatomic gases and problems of atomic structure, absorption spectra and the structure of molecules. In 1931 he devised a method for the concentration of any possible heavy hydrogen isotopes by the fractional distillation of liquid hydrogen: this led to the discovery of deuterium. Together with the late Dr. E.W. Washburn, he evolved the electrolytic method for the separation of hydrogen isotopes and he carried out thorough investigations of their properties, in particular the vapour pressure of hydrogen and deuterium, and the equilibrium constants of exchange reactions. He later worked on the separation of uranium isotopes and, more recently, he has been concerned with the measurement of paleotemperatures, investigations into the origin of the planets, and the chemical problems of the origin of the earth.  He is the author of the books *Atoms, Molecules and Quanta* (1930, with A.E. Ruark), and *The Planets* (1952). He was editor of the *Journal of Chemical Physics* during 1933-1940 and he has written numerous papers on the structure of atoms and molecules, the discovery of heavy hydrogen and its properties, separation of isotopes, measurement of paleotemperatures and the origin of planets. These have been published in many different chemical journals.  Professor Urey received the Willard Gibbs Medal (American Chemical Society) in 1934; Davy Medal (Royal Society, London), 1940; Franklin Medal, 1943; Medal for Merit, 1946; Cordoza Award, 1954; Honor Scroll Award (American Institute of Chemists), 1954; Joseph Priestley Award, 1955; Alexander Hamilton Award, 1961; and the J. Lawrence Smith Award (National Academy of Sciences), 1962. He has received honorary Doctor of Science degrees of Montana, Princeton, Newark, Columbia, Oxford, Washington and Lee, McMaster, Yale, Indiana, Birmingham Universities, and of the Universities of Athens, Durham, and Saskatchewan; also honorary Doctor of Law degree from Wayne University and the University of California. He is a member of many of the more important scientific societies of the world, and is Honorary Fellow of the Chemical Society (London), the National Institute of Sciences of India and the Weizmann Institute of Science (Israel).  In 1926 he married Frieda Daum. They have three daughters and one son. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0465 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0466** |
| **Biographical** | Irving Langmuir was born in Brooklyn, New York, on January 31, 1881, as the third of four sons of Charles Langmuir and Sadie, *neé* Comings. His early education was obtained in various schools and institutes in the USA, and in Paris (1892-1895). He graduated as a metallurgical engineer from the School of Mines at Columbia University in 1903. Postgraduate work in Physical Chemistry under [Nernst](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1920/index.html) in Göttingen earned him the degrees of M.A. and Ph.D. in 1906.  Returning to America, Dr. Langmuir became Instructor in Chemistry at Stevens Institute of Technology, Hoboken, New Jersey, where he taught until July 1909. He then entered the Research Laboratory of the General Electric Company at Schenectady where he eventually became Associate Director.  Langmuir’s studies embraced chemistry, physics, and engineering and were largely the outgrowth of studies of vacuum phenomena. In seeking the atomic and molecular mechanisms of these he investigated the properties of adsorbed films and the nature of electric discharges in high vacuum and in certain gases at low pressures.  His work on filaments in gases led directly to the invention of the gasfilled incandescent lamp and to the discovery of atomic hydrogen. He later used the latter in the development of the atomic hydrogen welding process.  He was the first to observe the very stable adsorbed monatomic films on tungsten and platinum filaments, and was able, after experiments with oil films on water, to formulate a general theory of adsorbed films. He also studied the catalytic properties of such films.  Langmuir’s work on space charge effects and related phenomena led to many important technical developments which have had a profound effect on later technology.  In chemistry, his interest in reaction mechanism caused him to study structure and valence, and he contributed to the development of the Lewis theory of shared electrons.  Among the awards made to him were: Nichols Medal, (1915 and 1920); Hughes Medal (1918); Rumford Medal (1921); Cannizzaro Prize (1925); Perkin Medal (1928); School of Mines Medal (Columbia University, 1929); Chardler Medal (1929); Willard Gibbs Medal (1930); Popular Science Monthly Award (1932); Nobel Prize in Chemistry (1932); Franklin Medal and Holly Medal (1934); John Scott Award (I937); “Modern Pioneer of Industry” (1940); Faraday Medal (1944); Mascart Medal (1950). In addition, he was a Foreign Member of the Royal Society of London, Fellow of the American Physical Society, Honorary Member of the British Institute of Metals, and of the Chemical Society (London). He had served as President of the American Chemical Society and as President of the American Association for the Advancement of Science.  Honorary degrees were bestowed upon Langmuir by the following colleges and universities: Northwestern, Union, Edinburgh (Scotland), Columbia, Kenyon, Princeton, Lehigh, Harvard, Oxford, Johns Hopkins, Rutgers, Queens (Canada), and Stevens Institute of Technology.  Dr. Langmuir’s hobbies were mountaineering, skiing, flying, and, most of all, to understand the mechanism of simple and familiar natural phenomena. He married Marion Mersereau in 1912. They had a son, Kenneth, and a daughter, Barbara. After a short illness, he died on August 16, 1957. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0466 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0467** |
| **Biographical** | Carl Bosch was born at Cologne on August 27, 1874, and grew up there. From 1894 to 1896 he studied metallurgy and mechanical engineering at the Technische Hochschule in Charlottenburg, but started reading chemistry at Leipzig University in 1896. He graduated under Professor Wislicenus with a paper on organic chemistry in 1898. He entered the employ of the Badische Anilin- und Sodafabrik, Ludwigshafen, Rhine as a chemist in April 1899 and participated actively in the development of the then new industry of synthetic indigo under the guidance of Dr. Rudolf Knietsch.  At the turn of the century Bosch became interested in the problem of the fixing of nitrogen and his first experiments in this field were done with metal cyanides and nitrides; in 1907 he started a pilot plant for the production of barium cyanide.  Bosch’s opportunity for really large-scale work came when in 1908 the Badische Anilin- und Sodafabrik acquired the process of high-pressure synthesis of ammonia, which had been developed by [Fritz Haber](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1918/index.html) at the Technische Hochschule in Karlsruhe. Bosch was given the task of developing this process on a large industrial scale. This task involved the construction of plant and apparatus which would stand up to working at high gas pressure and high reaction temperatures. Haber’s catalysts, osmium and uranium had to be replaced by a contact substance which would be both cheaper and more easily available. Bosch and his collaborators found the solution by using pure iron with certain additives. Further problems which had to be solved were the construction of safe high-pressurized blast furnaces, a cheap way of producing and cleaning the gases necessary for the synthesis of ammonia. Step by step Bosch went on to using increasingly larger manufacturing units and thus created the industry which deals with the production of synthetic ammonia according to the high-pressure process.  From this work resulted the second task of making the thus won ammonia available for use in industry and agriculture. Bosch succeeded in working out methods for the industrial production of nitrogen fertilizers, thus providing practically every country in the world with sufficient fertilizers for agricultural purposes. The Stickstoffwerke (Nitrogen works) in Oppau were opened in 1913, followed by the even larger Leunawerke near Merseburg in 1917, where the synthesis of methanol and the hydrogenation of oil were added to the production programme. Bosch was appointed Managing Director of the Badische Anilin- und Sodafabrik in 1919 and in 1925 was made Principal of the I.G. Farbenindustrie Aktiengesellschaft, which was created by the merger of the German coal-tar dye works. In 1935 Bosch was appointed Chairman of the Board of Directors of the I.G. Farbenindustrie A.G.  Bosch was honoured in many ways and not only for his achievements and inventions in the field of industry, but also for his research in pure science, which he considered to be his duty. He received the honorary doctorate of the Technische Hochschule in Karlsruhe (1918), of the Landwirtschaftliche Hochschule (Agriculture College), Berlin (1921), the Technische Hochschule in Munich (1922), of Halle University (1927), the Technische Hochschule in Darmstadt (1928). The distinctions of Honorary Senator of the Universities of Heidelberg (1922) and Leipzig (1939), and of Honorary Citizen of Frankfurt (1939) were conferred upon him.  He received the Liebig Memorial Medal of the Association of German Chemists, the Bunsen Medal of the German Bunsen Society, the Siemens Ring, the Golden Grashof Memorial Medal of the VDI (Association of German Engineers), the Exner Medal from the Austrian Trade Association, and the Carl Lueg Memorial Medal from the Association of German Metallurgists. In 1931 he was awarded the highest international honour, the Nobel Prize for Chemistry, jointly with Friedrich Bergius, for their contributions to the invention and development of chemical high pressure methods.  Bosch particularly enjoyed his membership of various German and foreign scientific academies, and his chairmanship of the Kaiser Wilhelm Society of which he became its President in 1937.  He died after a prolonged illness on April 26, 1940. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0467 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0468** |
| **Biographical** | Friedrich Bergius was born on October 11, 1884, in Goldschmieden near Breslau, Silesia. He belonged to an old respected family of scientists, theologians, civil servants, army officers, and business men. His grandfather was Professor of Economics in Breslau and his father owned a chemical factory in Goldschmieden.  Bergius was educated in Breslau and whilst still at school took great interest in his father’s factory where he was able to study various working methods under the guidance of his father and thus became acquainted with chemicotechnical processes. The time spent in the laboratories and the works there later stood him in good stead, as already at a very early age he obtained considerable insight into industrial as well as scientific matters.  Before entering University, Bergius was sent to the Ruhr for six months by his father, where he studied the practical aspects of a large metallurgical plant and profited greatly by this experience.  In 1903 he entered Breslau University to read chemistry under Ladenburg, Abegg and [Herz](https://www.nobelprize.org/nobel_prizes/physics/laureates/1925/index.html); after doing one year’s military service he proceeded to Leipzig University in 1905 and worked under Hantzsch on his thesis *Über absolute Schwefelsäure als Lösungsmittel* (On absolute sulphuric acid as a solvent). This work was completed in Breslau under Abegg, and Bergius received his degree at Leipzig in 1907. The lively scientific atmosphere in the laboratories of Hantzsch and Abegg induced Bergius to devote himself to a career in scientific research; for this reason he worked for two further terms at [Nernst](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1920/index.html)‘s Institute in Berlin and then went to Karlsruhe to study under [Haber](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1918/index.html) for one term in 1909.  The work which was being carried out there on the chemical equilibrium in gas reactions, in particular that on the synthesis of ammonia, prompted his own research in Hanover in 1909. He started with a detailed investigation on the dissociation of calcium peroxide and developed a practical method for laboratory work at pressures up to 300 atmospheres.  The equipment available in the laboratories of the Technische Hochschule in Hanover soon proved insufficient and in1910 Bergius established his own private laboratory in Hanover, which was gradually extended by several workshops and plants, and where he employed a number of collaborators. The most important result of his research was the hydrogenating effect of hydrogen on coal and heavy oils under high pressure, in 1912 and 1913.  It was not easy to obtain the means for maintaining this laboratory, especially as the scale of the experiments had to be enlarged in order to apply the laboratory methods to a small industrial scale. Therefore in 1914 Bergius accepted an offer made already previously by Dr. Karl Goldschmidt to transfer his laboratory to the Essen works of the firm Th. Goldschmidt A.G.; shortly afterwards he also accepted an executive position in this firm.  For a short time in 1911 Bergius lectured on technical gas reactions, equilibrium theory, and metallurgy at the Technische Hochschule in Hanover. However, the outbreak of the First World War and the increased rate of work on the problem of liquefaction of coal made it impossible for him to continue with his teaching activity. From 1914 to 1921 he lived in Berlin.  A comparatively large industrial plant for the technical development of the hydrogenating process was set up in Rheinau near Mannheim. It was soon found that the scope of this research work was too large for one firm alone and after the end of the war Bergius endeavoured to find firms suitable for collaborating in the development of hydrogenation. Apart from groups of German companies, he also induced the Shell Trust and a number of British enterprises, in particular the coal industry, to collaborate with him. However, in critical times he still had to bear the whole responsibility and risk for the further development of the process. Finally, in 1927, he was able to conclude his own work on the liquefaction of coal, after the practical possibilities had been proved on a large scale. The I.G. Farbenindustrie and Imperial Chemical Industries then took up the work on an industrial scale.  From that time onwards Bergius devoted himself to a process of obtaining sugar from cellulose in wood, on which he had already worked during the First World War. He succeeded after 15 years’ work and an industrial plant was set up, also in the Rheinau works. It is amazing with what intensity Bergius took up the second part of his life’s work, namely this hydrolysis of cellulose in wood and similar substances to sugar. It seems as if the well-known difficulties of working with highly concentrated hydrochloric acid had presented a special challenge to Bergius. Initially the process was taken up only in England and only during the thirties did Bergius manage to continue these experiments in Germany; his main concern was to rationalize the process and to ensure complete recovery of the hydrochloric acid used by constructing intricate devices. In 1921 he moved to Heidelberg, in order to be near his technical work in Mannheim-Rheinau, and at the same time to be in contact with Heidelberg University.  His home was international, and always full of mentally alive people. He had done great work and had received most of the honours possible during the eventful epoch of his lifetime. He received the degree of Dr. Phil. from the University of Heidelberg and the honorary doctorate from the University of Hanover; he was awarded the Liebig Medal and was elected to the Board of Directors of many associations and companies interested in coal and oil. In 1931 he shared the Nobel Prize with Carl Bosch for their contributions to the invention and development of chemical high-pressure methods.  After the last war it was impossible for Bergius to find a field of work in Germany which would have done justice to his abilities. He emigrated to the Argentine, where death put an end to his eventful career in Buenos Aires in 1949. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0468 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0469** |
| **Biographical** | Hans Fischer was born in July 27, 1881 at Hoechst, on the river Main, in Germany. His father was Dr. Eugen Fischer, Director of the firm of Kalle & Co, Wiesbaden, and Privatdozent at the Technical High School, Stuttgart; his mother was Anna Herdegen. He went to a primary school in Stuttgart, and later to the “humanistische Gymnasium” (grammar school with emphasis on the classics) in Wiesbaden, matriculating in 1899. He read chemistry and medicine simultaneously, first at the University of Lausanne and then at Marburg. He obtained his chemistry degree under T. Zincke at Marburg in 1904; two years later, in 1906, a licence for medicine was conferred on him at Munich. In 1908 he qualified for his M.D. under F. von Müller, also at Munich.  Fischer spent his first working years at the Second Medical Clinic in Munich and at the First Berlin Chemical Institute under [Emil Fischer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1902/index.html). He returned to Munich in 1911 and qualified as lecturer on internal medicine one year later. In 1913 he was appointed E.F. Weinland’s successor at the Physiological Institute (O. Frank) in Munich, where he became a lecturer on physiology. In 1916 Fischer followed the invitation of the University of Innsbruck to succeed [Windaus](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1928/index.html) as Professor of Medical Chemistry; from there he went to the University of Vienna in 1918.  From April 1, 1921 until his death he held the position of Professor of Organic Chemistry at the Technische Hochschule (Technical University) in Munich, as successor to [Wieland](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1927/index.html).  Fischer’s scientific work was mostly concerned with the investigation of the constitutive properties of the pigments in blood, bile, and also leaves, as well as with the chemistry of pyrrole. The main reason for the latter investigation was the synthesis of these natural pyrrole pigments. Of special importance was his synthesis of bilirubin. His numerous papers were mostly published in *Liebigs Annalen der Chemie* and *Hoppe-Seylers Zeitschrift für physiologische Chemie.*  In recognition of his work in these fields, the title of a “Geheimer Regierungsrat” (Privy Councillor) was conferred upon Hans Fischer in 1925; in 1929 he was awarded the Liebig Memorial Medal; Harvard University bestowed the title of honorary doctor on him in 1936; he received the Davy Medal in 1937. Fischer received the greatest honour of all, the Nobel Prize for 1930, for his work on the chemistry of pyrrole and the synthesis of haemin. In 1935, Professor Fischer married Wiltrud Haufe.  He died on March 31, 1945 in Munich. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0469 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0470** |
| **Biographical** | Arthur Harden was born in Manchester, England, on October 12, 1865; his father was Albert Tyas Harden and his mother Eliza Macalister. He was educated at a private school in Victoria Park (1873-1877) and at Tettenhall College, Staffordshire (1877-1881). He entered The Owens College in the University of Manchester in 1882, studying under Sir H.E. Roscoe, and graduated in 1885 with first-class honours in chemistry. In 1886 he was awarded the Dalton Scholarship in Chemistry and he spent twelve months during 1887-1888 working with [Otto Fischer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1973/index.html) at Erlangen. He returned to Manchester as lecturer and demonstrator, and remained until 1897 when he was appointed chemist to the newly founded British Institute of Preventive Medicine, which later became the Lister Institute. In 1907 he was appointed Head of the Biochemical Department, a position which he held until his retirement in 1930, and in 1912 he received the title of Emeritus Professor of Chemistry, University of London. He continued his scientific work at the Institute after his retirement.  Before he went to London, Harden had studied the action of light on mixtures of carbon dioxide and chlorine. He entered the Institute with an excellent chemical background and applied his methods to the investigation of biological phenomena (among others: the chemical action of bacteria and alcoholic fermentation). He studied the breakdown products of glucose but he was soon to be drawn to the problem of the chemistry of the yeast cell. His classic study of the chemistry of the fermentation of sugar by yeast juice, which have advanced the knowledge of the processes of intermediary metabolism in all living forms, lasted for many years and provided a stepping-stone for many research workers in allied fields. Harden himself contributed to the vitamin field with a series of papers on the antiscorbutic and antineuritic vitamins and their occurrence in food and drinks.  Harden was Joint Editor (with W.M. Bayliss) of *The Biochemical Journal* from 1913 to 1938, and, in addition wrote numerous papers in scientific journals. He is the author of *Alcoholic Fermentation*, *A New View of the Origin of Dalton’s Atomic Theory*, and *Chemistry for Advanced Studies* (with Sir H.E. Roscoe), *Inorganic Chemistry for Advanced Students*, and *An Elementary Course of Practical Organic Chemistry* (with F.C. Garrett).  Harden was knighted in 1926. He was honorary Doctor of Science, University of Athens and honorary Doctor of Laws, Universities of Manchester and Liverpool. A Fellow of the Royal Society (1909), he received the Davy Medal in 1935.  He married Georgina Sydney Bridge, daughter of C. Wynyard Bridge of Christchurch, New Zealand, in 1900. They had no children. His wife died in January, 1928, and Sir Arthur on June 17, 1940, at his home in Bourne End, Buckinghamshire. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0470 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0471** |
| **Biographical** | Hans von Euler-Chelpin was born on February 15, 1873, at Augsburg. His father, who was then a captain in the Royal Bavarian Regiment, was transferred to Munich and von Euler-Chelpin spent most of his childhood not far away with his grandmother at Wasserburg. After going to school at Munich, Wurzburg and Ulm, he studied art from 1891 until 1893 at the Munich Academy of Painting, first under Schmid-Reutte and later under Lenbach, whose gifted and powerful personality greatly influenced him. His desire to study problems of colour and especially the colour of the spectrum led von Euler-Chelpin to begin, in 1893, the study of science.  He therefore went to the University of Berlin to study chemistry under [Emil Fischer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1902/index.html) and A. Rosenheim, and physics under E. Warburg and [Max Planck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1918/index.html); and in 1895 he took his doctorate at the University of Berlin.  He then took a short course in physical chemistry in Berlin and went, after this, to the University of Göttingen to work from 1896 until 1897 under [W. Nernst](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1920/index.html). In the summer of 1897 he went to work in the laboratory of [Svante Arrhenius](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1903/index.html) in Stockholm and was appointed assistant there. In 1898 he qualified as Privatdozent in physical chemistry in the Royal University at Stockholm, and in 1899 he was appointed as such in that University. Between 1899 and 1900 he visited the laboratory of [van ‘t Hoff](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1901/index.html). Both van ‘t Hoff and Nernst greatly influenced his scientific development.  Subsequently, visits to the laboratories of A. Hantzsch and J. Thiele confirmed his interest in organic chemistry and he then began to work on this subject, partly in collaboration with Astrid Cleve, who was his wife at that time. During this period he visited the laboratories of [E. Buchner](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1907/index.html) in Berlin and G. Bertrand at the Pasteur Institute in Paris.  In 1906 he was appointed Professor of General and Organic Chemistry in the Royal University, Stockholm. In 1929 the Knut and Alice Wallenberg Foundation and the International Education Board of the Rockefeller Foundation established in Stockholm the Vitamin Institute and Institute of Biochemistry, and von Euler-Chelpin was appointed as its Director. In 1941 he retired from teaching, but continued his researches.  Von Euler-Chelpin’s first biochemical work was published in 1904. Its subject was the action of enzymes considered in relation to his earlier work on catalysis. This earlier work had won him, in 1898 the Lindblom Prize of the German Academy of Sciences for a paper on the catalytic hydrolysis of substrates by salt formation with the catalyst, work which he later developed further in collaboration with E. Rudberg and A. Ölander. From 1906 onwards, von Euler-Chelpin was chiefly concerned with physico-chemical and biochemical problems, publishing, from 1908 to 1909, the results of his work on the chemistry of plants, and in 1915 the results of his collaboration with P. Lindner on the chemistry of the fungi. From 1925 until 1930 he worked on the chemistry of enzymes.  An important part of von Euler-Chelpin’s work was done in collaboration with K. Josephson on the enzymes saccharase and catalase. The work he had begun in 1905 on fermentation was specialized in his Institute to phosphorylation, and the first phases of fermentation and their catalysis were studied, with special attention to co-zymase and also related activators. In this latter work K. Myrback played a great part. An important result of his work with his collaborators on co-zymase was his clarification of its structure, other conclusions being confirmed by [Lord Todd](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1957/index.html) in 1956.  In 1924 von Euler-Chelpin began his numerous studies of the vitamins in collaboration with B. von Euler, Paul Karrer and Margareta Rydbom, with whom he carried out, in 1928 a study of the marked vitamin A activity of carotene. His work on fermentation and general enzyme chemistry was continued in the Institute established in Stockholm by the Wallenberg and Rockefeller Foundations, special attention being given to the use of enzyme chemistry for the study of heredity and the blood serum.  In 1929 von Euler-Chelpin was awarded jointly with Arthur Harden, the Nobel Prize for Chemistry for his work on alcoholic fermentation. In 1914 he had published a book on the chemistry of yeast and alcoholic fermentation. Between 1925 and 1934 he embodied the results of his very extensive research in enzymology in his monograph entitled *Chemie der Enzyme,* which was the first modern monograph on this subject. In 1957 he published with B. Eistert a book on the chemistry and biochemistry of reductones. With C. Martius he had succeeded in preparing triose-reductone. In 1958 he visited Japan and published a monograph on the reductones in collaboration with Professor K. Yamafuji and Drs. Namura and Adachi.  Later in 1935 von Euler-Chelpin investigated the biochemistry of tumours and especially studied the nucleic acids in tumours by means of labelled compounds by a technique he invented in collaboration with [G. de Hevesy](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1943/index.html). On tumours he published two important monographs, one entitled the *Biochemistry of Tumours*, written in collaboration with Boleslaw Skarzynski of Cracow and published in 1942 and the other entitled *The Chemotherapy and Prophylaxis of Cancer*, published in 1962.  A careful teacher and a friend to all who worked with him, von Euler-Chelpin has inspired and exerted great influence on many workers in the field of biochemistry.  He was a Fellow of the Academies of Sciences of Bangalore, Berlin, Munich, Rome, Paris, Vienna, Copenhagen, Helsinki, Moscow, Leningrad, Halle, Göttingen, Tokio and New Delhi. He was a Member of the Royal Swedish Academy of Sciences, the Royal Swedish Academy of Engineering Sciences, the Royal Institution, London, the Finnish Academy of Sciences, and the Academy of Sciences of New Delhi, a Foreign Member of the Max Planck Society, a Corresponding Member of the Academy of Sciences, Paris, and an honorary member of the Academies of Sciences of Helsinki and Japan, the Indian Academy of Sciences, the Chemical Societies of France, Italy and Berlin, and of the Japanese Cancer Association, Tokio. He held honorary doctorates of the Universities of Stockholm, Zurich, Athens, Kiel, Berne, Turin, and Rutgers and New Brunswick Universities. In 1959, he was awarded the Grand Cross for Federal Services with Star, Germany.  Von Euler-Chelpin married twice. His first wife, Astrid Cleve, is mentioned above. They had five children. In 1913 he married Elisabeth Baroness af Ugglas, who collaborated with him in his work. There were four children by this marriage.  He died in Stockholm on November 6, 1964, at the age of 91. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0471 |
| **Interview** |  |
|  |  |
| **Chemistry 1999-** |  |
| **ID** | **0472** |
| **Biographical** | Adolf Windaus was born in Berlin on December 25, 1876, the son of Adolf Windaus and Margarete Elster. His ancestors had for generations mostly been artisans (from his father’s side, drapery manufacturers).  After attending the renowned “Französisches Gymnasium” (French grammar school) in Berlin, where his interests were mainly focussed on literature, he took up medicine in 1895 (in Freiburg i.Br. and in Berlin), passing his preliminary medical examinations (“Physikum”) in 1897.  He had been particularly fascinated by [Emil Fischer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1902/index.html)‘s lectures during his stay in Berlin and in consequence he began studying chemistry at Freiburg i.Br. under Kiliani, at the same time continuing his medical studies. In the winter of 1899-1900 he obtained his Dr. phil. degree, the subject of his thesis dealing with the cardiac poisons of the Digitalis plant.  Windaus chose zoology as his subsidiary subject on account of the deep impression which Weismann’s lectures had made on him in Freiburg. After graduating Windaus moved to Berlin to work with Emil Fischer. It was there that he met [Otto Diels](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1950/index.html), with whom he formed a life-long friendship. He returned to Freiburg i.Br. in 1901 and on Kiliani’s suggestion began his work on cholesterol and related sterols. Windaus’s decision to study these substances was based on the fact that nothing was known about the structure of cholesterol at the time and he expected that investigations in this field would yield new and unexpected results. From the very start he correctly believed that sterols, which occur in every cell, must be considered as the parent substance of other groups of natural substances. Based on this work, briefly entitled *On Cholesterol*, Windaus “habilitated” as lecturer in 1903. In 1919 he succeeded in transforming cholesterol into cholanic acid, which had previously been isolated from the bile acids by [Wieland](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1927/index.html) – another close friend of his who received the Nobel Prize for 1927 for his studies of the constitution of the bile acids and related substances – Windaus thus demonstrated that the bile acids are closely related to the sterols.  Another rich field of investigation from biological viewpoint was that of imidazole derivatives. This work, carried out in collaboration with Knoop, resulted from his attempt to prepare natural amino acids through the action of ammonia on sugar, and thus to establish conversion of sugar into proteins. The production of imidazole derivatives as a result of this reaction induced him to demonstrate the presence of a prototype of the imidazole ring in proteins. This in turn led to his demonstration that histidine, a building-stone of proteins, is an imidazole alanine, and to his discovery of histamine (imidazole-ethylalanine), a hormone of great importance in physiology and pharmacology, which was the reason for the interest shown by German chemical industry in his work.  As the foremost expert on sterols he was in 1925 invited by the American physiologist Alfred Hess to come to New York to cooperate in the study of the antirachitic vitamin.  It was also his interest in the imidazole derivatives that prompted him to examine the structure of the antineuritic vitamin. Cooperating with the I.G. Farbenindustry, which supplied the precious raw material for the work, he was able to prove that not an imidazole-ring (as suggested by Jansen and Donath), but a thiazole- and a pyrimidine-ring are present in vitamin B1.  Other research taken up by Windaus was into the stereochemical problems of the *cis-* and *trans-*linking of hydrogenated ring systems. In all these investigations his starting-point was always that of experimental observation, and not of theoretical deduction; the inductive method was the most suitable for his talents. During the last years of his life he also investigated the possibility of chemotherapy in cancer research.  Windaus was appointed Assistant Professor (1906), and Professor of Applied Medical Chemistry at Innsbruck University (1913), where he remained for two years. He moved to Göttingen in 1915 as Professor of Chemistry, succeeding Otto Wallach, where he stayed until his retirement in 1944 as Director of the Laboratory for General Chemistry, formerly the Wöhler Institute. Among his pupils may be mentioned [Adolf Butenandt](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1939/index.html), Nobel Prize winner 1939 for his work on sex hormones – which are closely related to the sterols – and Hans Brockmann.  Windaus received the Nobel Prize in Chemistry in 1928, on account of his work on the constitution of sterols and their connection with vitamins. Among the many honours he was awarded were the Pasteur Medal (1938), the Goethe Medal (1941), also the “Groszes Verdienstireuz” (Grand Order of Merit) in 1951, and the “Groszes Verdienstkreuz mit Stern” (Grand Order of Merit with Star) in 1956. In addition he was recipient of the Order pour le Merite, Peace Class, in 1952. He was also honorary doctor of the Universities of Göttingen, Munich, Freiburg, and Hanover.  Professor Windaus married Elisabeth Resau in 1915. There were three children by his marriage: Günter (b. 1916), Gustav (b. 1918), and Margarete (b. 1921)  He died on June 9, 1959. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0472 |
| **Interview** |  |
|  |  |
| **ID** | **0473** |
| **Biographical** | Heinrich Otto Wieland was born as the son of Württemberger parents, Dr. Theodor Wieland and Elise Blum, on June 4, 1877, in Pforzheim where his father was a pharmaceutical chemist. He studied at the Universities of Munich, Berlin and Stuttgart, and then returned to the Baeyer Laboratory in Munich where, in 1901, he received his doctorate under Johannes Thiele. In Munich, where he had chosen to make his home, he received in 1904 the *venia legendi* and in 1913 a senior lectureship in the University Chemical Laboratory. In 1917 he transferred his activities to the Technical College nearby as a full Professor. From 1917-1918 he was busy at the Kaiser Wilhelm Institute in Berlin-Dahlem working on Defence. In 1921 he accepted a call to Freiburg and in 1925 returned to Munich at [Willstätter’s](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1915/index.html) request to succeed him in his Chair in the University of Munich. For twenty-seven years the destiny of the Munich laboratory lay in his hands.  Wieland’s scientific work, recorded in four hundred publications, covers a wide field in the realm of Organic Chemistry and Biochemistry.  For several decades Wieland’s attention was claimed by the organic nitrogenous compounds. His brilliant studies of the reaction of nitrogen oxides with orefins and aromatics, the clarification of furoxanes as well as the classical experiments with fulminic acid and its polymerization may be chosen as examples. In the same connection, Wieland succeeded in the first production of stable organic nitrogen radicals, of diphenyl nitrogen and its N-oxide. The proof of short-lived radicals in solution reactions involved him in an extensive series of experiments whose importance for the modern development of organic radical chemistry can hardly be overestimated.  In later years Wieland was entirely devoted to the chemistry of natural substances. His contributions to the clarification of the structure of morphine and strychnine, the constitution and synthesis of the lobelia alkaloid and the research into the curare alkaloid were masterpieces. Work on the poisonous agent in the “death cap” mushroom led to the isolation of the crystalline cyclopeptides phalloidine and amanitine; work on the pigment of butterflies led to the discovery of the biologically important class of pterin compounds. The publications which began in 1912 on the subject of bile acids culminated in 1932 in the clarification of the carbon framework of the steroids, whose general importance in Nature Wieland recognized.  Much of Wieland’s life work was occupied by the investigations into the oxidation processes in living cells, which enabled him to recognize dehydrogenation as a universal oxidation principle in Nature. This work restored the unity of Organic Chemistry and Biochemistry which had been lost since the time of Liebig.  It is not surprising that Wieland was accorded all the honours that the scientific world has to offer. He was a member of the great learned societies of the world, he was awarded a Nobel Prize in 1927, he received the Order of Merit and the [Otto Hahn](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1945/index.html) Prize, and for twenty years he was Editor of *Justus Liebigs Annalen der Chemie*. Wieland’s great school is evidence of his importance as an academic teacher.  In an age of increasing specialization, Wieland was among the last of those who were able to enjoy an encyclopaedic knowledge of the whole of Chemistry.  In 1908, he married Josephine Bartmann of Munich. They had three sons, Wolfgang, a doctor of pharmaceutical chemistry; Theodor, professor of chemistry in the University of Frankfurt; and Otto, professor of medicine in the University of Munich, and one daughter, Eva, who is married to Professor [Feodor Lynen](https://www.nobelprize.org/nobel_prizes/medicine/laureates/1964/index.html), professor of biochemistry in the University of Munich, Nobel Laureate in Physiology or Medicine, 1964.  The great scholar’s life was ruled by hard work, but also by love and kindness both to his pupils and to his family; it ended shortly after his 80th birthday, on August 5, 1957, in Starnberg. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0473 |
| **Interview** |  |
|  |  |
| **ID** | **0474** |
| **Biographical** | Theodor (The) Svedberg was born at Fleräng, Valbo, in the county of Gävleborg, Sweden on August 30, 1884, as the son of Elias Svedberg, a works manager, and his wife, Augusta, *née* Alstermark. He attended the Köping School, the Örebro High School and Gothenburg Modern School, where he passed his student examination in December 1903, before matriculating and beginning his lifelong association with Uppsala University in January 1904. He gained his Bachelor of Arts degree in 1905, his Master’s degree in 1907 and his Doctor of Philosophy in 1908.  Svedberg accepted a post as assistant in the Chemical Institute at Uppsala in 1905 and in 1907 he was given the additional position of lecturer in chemistry in the university. He obtained a special appointment in 1909 as lecturer and demonstrator of physical chemistry and in 1912 he was elected Professor of Physical Chemistry, University of Uppsala. He was made emeritus in 1949, since when he has been Director of the Gustaf Werner Institute for Nuclear Chemistry in the University.  Svedberg’s work has mainly been concerned with colloids and macromolecular compounds. In his doctor’s thesis *Studien zur Lehre von den kolloiden Lösungen* (1908), now considered as a classic, he described a new method of producing colloid particles and gave convincing evidence of the validity of the theory founded by [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/index.html) and von Smoluchowski on the Brownian movements, thereby providing definite proof of the existence of molecules. With numerous collaborators he has studied the physical properties of colloids, such as their diffusion, light absorption, and sedimentation, from which could be concluded that the gas laws could be applied to disperse systems. For the study of sedimentation he constructed his well-known ultracentrifuge, where large molecules in solution, such as proteins, carbohydrates, and high polymers have been investigated. In the ultracentrifuge the large molecules are put under the influence of centrifugal forces up to about 106 gravity. The findings were related to molecular size and shape. He showed that the molecules of certain pure proteins are all of one size and demonstrated the use of the ultracentrifuge to show the presence of contaminants. Later, he turned his attention to the problems of nuclear chemistry and radiation biology. He has also studied photographic processing, and assisted in photographing the famous *Codex argenteus* in colour, which made this source of the Gothic language available for research. During the Second World War he worked out a method for making synthetic rubber.  Svedberg has contributed a large number of scientific papers to Swedish and foreign journals on colloid and high-molecular solutions, nuclear chemistry and radiation biology.  Among his publications the following books should be mentioned: *Die Methoden zur Herstellung kolloider Lösungen anorganischer Stoffe* (1909), *Die Existenz der Moleküle* (1912), *Colloid Chemistry* (1914; sec. enl. ed. with [A. Tiselius](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1948/index.html) 1928 ), *The Ultracentrifuge* (with K.O. Pedersen 1940; also in German). He has written several excellent popular books in Swedish, namely *Materien* (1912; sec. ed. 1919; also German ed. 1914), *Arbetets dekadens* (1915, 1920; German ed. 1923) and *Forskning och Industri* (1918).  He has been awarded the John Ericsson Medal (1942), the Berzelius Medal (1944), and the Medal of the Franklin Institute (1949). For his work on the disperse systems he received the Nobel Prize in 1926.  To extend his knowledge Svedberg has visited numerous laboratories in foreign countries and cities: Germany (1908), Holland and France (1912), Berlin (1913), Vienna (1916), London and Paris (1920), England and Denmark (1922), the United States and Canada (1922-1923).  He has been awarded honorary doctor’s degrees in science and medicine, and, amongst many other academies and learned societies, he is a member of the [Swedish Academy of Sciences](http://www.kva.se/), the Halle Academy, The Chemical Society (London), the Indian Academy of Science, the American Philosophical Society (Philadelphia), the New York Academy of Sciences, the Royal Society (London) and the National Academy of Science (Washington).  Svedberg has married four times: in 1909, Andrea Andreen, a medical graduate; in 1916, Jane Frodi; Ingrid Blomquist in 1938; and in 1948, Margit Hallén, M.A. He has twelve children, six sons and six daughters.  His hobbies are painting and botany. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0474 |
| **Interview** |  |
|  |  |
| **ID** | **0475** |
| **Biographical** | Richard Adolf Zsigmondy was born in Vienna on April 1, 1865. His father, doctor Adolf Zsigmondy, had done a great deal to promote dentistry in Austria, had invented several surgical instruments and devices, and had published several scientific and medical works. He encouraged his four children’s interest in the natural sciences. He died when Zsigmondy was only 15 years old. Zsigmondy’s mother, Irma von Szakmary, encouraged her children to lead an outdoor life, to take an interest in the arts and to follow their own inclinations. Zsigmondy and his brothers spent much of their time climbing, mountaineering, swimming and diving.  Zsigmondy’s interest in chemistry and physics developed at an early age; he studied Stoeckhardt’s textbook *Schule der Chemie* and carried out many of the experiments mentioned there in his own small laboratory in his home. He was also influenced by Roscoe-Schorlemmer’s and Berzelius’s textbooks on chemistry.  Under the guidance of Professor E. Ludwig of the Medical Faculty in Vienna he learnt the basic facts about quantitative analysis. He then studied at the Technische Hochschule in Vienna and in 1887 went to Munich to read organic chemistry under Professor W. von Miller. After taking his doctorate, he remained as Professor von Miller’s assistant and later accepted a similar position as assistant to the physicist Professor Kundt in Berlin. In 1893 he qualified as lecturer at the Technische Hochschule in Graz and also accepted a teaching post there. His work on lustre colours for glass and china induced him to study more closely the chemistry of colloids, and led to an appointment with the glass works Schott und Genossen in Jena, where he remained until 1900. He left in order to exclusively pursue scientific research. During this period he discovered how to prepare reproducibly gold hydrosols and also developed the slit-ultramicroscope in joint collaboration with Siedentopf.  In 1907 Zsigmondy was appointed Professor and Director of the Institute of Inorganic Chemistry at the University of Göttingen, where he remained until his retirement in February 1929. Following the First World War, especially in 1922 and 1923, the Institute suffered severe shortages of the most simple chemical materials and scientific research work became very difficult. In 1925 Zsigmondy was awarded the Nobel Prize for Chemistry for his work on the heterogeneous nature of colloidal solutions. This made it possible for him to overcome, with deep gratitude, most of the difficulties he had encountered in the previous years. Apart from his *Lehrbuch der Kolloidchemie*, Zsigmondy published a book *Über das kolloide Gold* in collaboration with P.A. Thiessen. His son-in-law, Dr. Erich Huckel, who was also one of his co-workers, contributed a book on the theories of adsorption for Zsigmondy’s collected papers *Kolloidforschung in Einzeldarstellungen*.  Zsigmondy married Laura Luise, *née* Müller, the daughter of Professor Wilhelm Müller, lecturer in pathological anatomy in Jena, in 1903. Two daughters, Annemarie and Käthe, resulted from this marriage. In 1925 Annemarie married Dr. Erich Huckel of Göttingen, who in Zurich was assistant to Professor [Debye](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1936/index.html), and later became lecturer there.  He died in Göttingen on September 24, 1929. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0475 |
| **Interview** |  |
|  |  |
| **ID** | **0476** |
| **Biographical** | Fritz Pregl was born in Laibach[\*](https://www.nobelprize.org/prizes/chemistry/1923/pregl/biographical/#not) on September 3, 1869, and attended the local “Gymnasium” (grammar school), from where he proceeded to the University of Graz to study medicine. He received his M.D. in 1894, but even prior to graduation he became assistant lecturer for physiology and histology under Alexander Rollett, taking over the chair when Rollett died in 1903. During this time Pregl also acquired a thorough knowledge of all branches of chemistry under the guidance of Professor Skraup.  In 1904 he went to Germany, where he studied for short periods under Gustav v. Hüfner in Tübingen, [W. Ostwald](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1909/index.html) in Leipzig and [Emil Fischer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1902/index.html) in Berlin. On his return to Graz in 1905 Pregl worked at the Medico-Chemical Institute under K.B. Hofmann and was appointed forensic chemist for the Graz circuit in 1907. At that time he started investigating the components of albuminous bodies and the analysis of bile acids. His work, however, was handicapped by the lack of sufficient starting materials and this fact impelled him to look for methods requiring smaller amounts when making quantitative analyses of elements in compounds.  The years 1910-1913, whilst professor at Innsbruck University, were almost entirely devoted to developing the method of quantitative organic micro-analysis. Pregl continued with this work when he was recalled to Graz University in 1913; he was appointed Dean of the Medical Faculty for the year 1916-1917 and Vice-Chancellor of Graz University for 1920-1921.  Initially Pregl’s scientific work had been mainly in the fields of physiology and physiological chemistry; later he turned to the study of the constitution of chemical compounds, in particular the investigation of bile acids. By 1912 he was able, by using his own methods of quantitative micro-analysis, to make measurements of carbon, hydrogen, nitrogen, sulphur, and halogen, using only 5-13 mg of starting materials with results as accurate as those obtained by macro-analysis. Later he perfected his techniques so that as little as 3-5 mg were adequate. Pregl also contributed a number of micromethods for measuring atomic groups and developed a series of apparatus, including a sensitive microbalance, necessary for his work.  Recognition for his work was first accorded with the Lieben Prize for Chemistry from the Imperial Academy of Science in Vienna (1914), an honorary doctorate in philosophy from the University of Göttingen (1920 ); in 1921 he was elected Corresponding Member by the Academy of Sciences in Vienna. The greatest and most unexpected honour was the award of the Nobel Prize for Chemistry by the [Swedish Academy of Sciences](http://www.kva.se/) in 1923. O. Hammarsten, the Chairman of the Nobel Committee at the time, pointed out that it was not for a discovery, but for modifying and improving existing methods that Pregl was awarded the prize.  Pregl had, in the early stages of his investigations, avoided publishing individual reports on his experiments, until he had convinced himself that his methods did not only work in his own, but also in other laboratories. He then, in 1917, set down his findings in a monograph entitled *Die quantitative Microanalyse* (published by J. Springer, Berlin). A second edition was published in 1923 and a third revised and enlarged edition (256 pages) appeared in 1930. Later editions were revised by Dr. H. Roth. The seventh edition was published in 1958 by Springer in Vienna. Pregl’s monograph has also been translated into French and English.  Following the award of the Nobel Prize for Chemistry in 1923, chemists from all over the world came to the Medico-Chemical Institute in Graz to study Pregl’s techniques of quantitative organic micro-analysis under his guidance.  Pregl never married, and died after a short illness at the age of 61 at Graz on December 13, 1930. Shortly before his death he put a considerable amount of money at the disposal of the Vienna Academy of Sciences for the promotion of micro-chemical research, stipulating that the interest from this fund was to be used each year to award a prize for outstanding work to Austrian micro-chemists. Since then, the Vienna Academy of Sciences has awarded this prize as the “Fritz Pregl Prize”.  \* Now Ljubljana, Slovenia. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0476 |
| **Interview** |  |
|  |  |
| **ID** | **0477** |
| **Biographical** | Francis William Aston was born in September 1877 at Harborne, Birmingham, England, the third of a family of seven children. He was educated at Harborne Vicarage School and Malvern College where his interest in science was aroused. In 1894 he entered Mason College, Birmingham (later to become the University of Birmingham) where he studied chemistry under Frankland and Tilden, and Physics under Poynting. His winning of the Forster Scholarship in 1898 enabled him to work on the optical properties of tartaric acid derivatives; the results of this work were published in 1901.  Leaving academic life for a time, he worked for three years as a chemist in the laboratory of a brewery. At about this time, however, his interest in physics, rather than chemistry, began to predominate; his aptitude for mechanical contrivance showed itself in his design and construction of new types of pumps for evacuating vessels. From this stemmed his interest in gas discharge phenomena in evacuated tubes.  In 1903 he obtained a scholarship to Birmingham University (as it had now become) to work on the properties of the Crookes Dark Space in discharge tubes. Within a short time he had discovered the phenomenon which is known as the Aston Dark Space. At the end of 1909 he accepted the invitation of [Sir J.J.Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1906/index.html) to work as his assistant at the Cavendish Laboratory, Cambridge, on studies of positive rays. It was during this period that he obtained definite evidence for the existence of two isotopes of the inert gas neon.  This research was interrupted by the War of 1914-1918, during which time Aston worked at the Royal Aircraft Establishment, Farnborough, where he studied the effect of atmospheric conditions on aeroplane fabrics and dopes (i.e. synthetic coatings).  Returning to the Cavendish Laboratory in 1919, he again attacked the problem of the separation of the isotopes of neon. He quickly achieved success in this by his invention of the mass spectrograph, an apparatus in which the ingenious use of electromagnetic focusing enabled him to utilize the very slight differences in mass of the two isotopes to effect their separation. Extending this principle to other chemical elements, he discovered, in a series of measurements, no less than 212 of the naturally occurring isotopes. From the results of this work he was able to formulate the so-called Whole Number Rule which states that, the mass of the oxygen isotope being defined, all the other isotopes have masses that are very nearly whole numbers.  Aston continued to make measurements, using an improved instrument, with ever-increasing refinement and precision. He observed and was able to measure those deviations from the Whole Number Rule which were to become so important in the field of atomic energy.  The results of his work were published in the *Proceedings of the Royal Society* and in the *Philosophical Magazine.* He was also the author of the books *Isotopes* (1922; revised edition 1941) and of *Structural Units of the Material Universe* (1923).  Aston was elected to a Fellowship at Trinity College in 1920, in which year he also received the Mackenzie Davidson Medal of the Röntgen Society. In 1921 he was made a Fellow of the Royal Society and was awarded the Society’s Hughes Medal the following year, the same year that he received the Nobel Prize. The John Scott and the Paterno medals were given to him in 1923, the Royal medal in 1938, and he was Duddell medalist of the Physical Society in 1941.  He was an honorary member of the Russian Academy of Sciences and of the Accademia dei Lincei, and held honorary doctorates of the Universities of Birmingham and Dublin.  Aston, a bachelor, was an enthusiastic sportsman; skiing, rock climbing, tennis and swimming were among the sports in which he excelled. He was also keen musician, playing the piano, violin and the cello.  He died at Cambridge on November 20, 1945. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0477 |
| **Interview** |  |
|  |  |
| **ID** | **0478** |
| **Biographical** | Frederick Soddy, the son of Benjamin Soddy, a London merchant, was born at Eastbourne, Sussex, England, on September 2, 1877. He was educated at Eastbourne College and the University College of Wales, Aberystwyth.  In 1895 he obtained a scholarship at Merton College, Oxford, from which University he graduated in 1898 with first class honours in chemistry. After two years of research at Oxford he went to Canada and from 1900 to 1902 was Demonstrator in the Chemistry Department of McGill University, Montreal. Here he worked with Professor [Sir Ernest Rutherford](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1908/index.html) on problems of radioactivity. Together they published a series of papers on radioactivity and concluded that it was a phenomenon involving atomic disintegration with the formation of new kinds of matter. They also investigated the gaseous emanation of radium.  Leaving Canada, Soddy then worked with [Sir William Ramsay](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1904/index.html) at University College, London where he continued the study of radium emanation. Here, Soddy and Ramsay were able to demonstrate, by spectroscopic means, that the element helium was produced in the radioactive decay of a sample of radium bromide and that helium was evolved in the decay of emanation.  From 1904 to 1914 Soddy was lecturer in physical chemistry and radioactivity in the University of Glasgow. Here he did much practical chemical work on radioactive materials. During this period he evolved the so-called “Displacement Law”, namely that emission of an alpha-particle from an element causes that element to move back two places in the Periodic Table. His peak was reached in 1913 with his formulation of the concept of isotopes, which stated that certain elements exist in two or more forms which have different atomic weights but which are indistinguishable chemically.  In 1914 he was appointed Professor of Chemistry at the University of Aberdeen, but plans for research were hampered by the war. In 1919 he became Dr. Lees Professor of Chemistry at Oxford University, a post he held until 1937 when he retired, on the death of his wife.  After his period at Glasgow he did no further work in radioactivity. His interest changed to economic, social and political theories which gained no general acceptance at the time, and to unusual mathematical and mechanical problems.  His books include *Radioactivity* (1904), *The Interpretation of Radium* (1909), *The Chemistry of the Radioactive Elements* (1912-1914), *Matter and Energy* (1912), *Science and Life* (1920), *The Interpretation of the Atom* (1932), *The Story of Atomic Energy* (1949), and *Atomic Transmutation* (1953)*.*  Soddy was elected a Fellow of the Royal Society in 1910 and Oxford awarded him an honorary degree. He was awarded the Albert Medal in 1951.  He was a man of strong principles and obstinate views, friendly with students and prickly with colleagues.  ln 1908, he married Winifred Beilby. He died on September 22, 1956 at Brighton. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0478 |
| **Interview** |  |
|  |  |
| **ID** | **0479** |
| **Biographical** | Walther Hermann Nernst was born in Briesen, West Prussia, on June 25, 1864. His father, Gustav Nernst, was a district judge. He spent his early school years at Graudentz, and subsequently went to the Universities of Zurich, Berlin and Graz (Ludwig Boltzmann and Albert von Ettinghausen), studying physics and mathematics, before proceeding to Wurzburg (Friedrich Kohlrausch), where he graduated in 1887 with a thesis on electromotive forces produced by magnetism in heated metal plates. He joined [Wilhelm Ostwald](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1909/index.html) at Leipzig University, where [van ‘t Hoff](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1901/index.html) and [Arrhenius](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1903/index.html) were already established, and it was in this distinguished company of physical chemists that Nernst began his important researches.  In 1894 he received invitations to the Physics Chairs in Munich and in Berlin, as well as to the Physical Chemistry Chair in Göttingen. He accepted this latter invitation, and in Göttingen founded the Institute for Physical Chemistry and Electrochemistry and became its Director. In 1905 he was appointed Professor of Chemistry, later of Physics, in the University of Berlin, becoming Director of the newly-founded “Physikalisch-Chemisches Institut” in 1924. He remained in this position until his retirement in 1933.  Nernst’s early studies in electrochemistry were inspired by Arrhenius’ dissociation theory which first recognized the importance of ions in solution. In 1889 he elucidated the theory of galvanic cells by assuming an “electrolytic pressure of dissolution” which forces ions from electrodes into solution and which was opposed to the osmotic pressure of the dissolved ions. In the same year he derived equations which defined the conditions by which solids precipitate from saturated solutions. His heat theorem, known as the Third Law of Thermodynamics, was developed in 1906. It demonstrated that the maximum work obtainable from a process could be calculated from the heat evolved at temperatures close to absolute zero – earlier ideas had not considered the effects of temperature – and conditions of equilibrium in many chemical reactions could now be precisely worked out. In addition to its theoretical implications, the theorem was soon applied to industrial problems, induding calculations in ammonia synthesis.  Nernst and his students in Berlin proceeded to make many important physico-chemical measurements, particularly determinations of specific heats of solids at very low temperatures and of vapour densities at high temperatures. All these were considered from the point of view of quantum theory.  In 1918 his studies of photochemistry led him to his atom chain reaction theory. This assumed that once the energy of a quantum has initiated a reaction in which free atoms are formed, these formed atoms can themselves decompose other molecules with the liberation of more free atoms and so on. The reaction can thus continue for long periods without further outside initiations.  Nernst was mechanically minded and he was always to the forefront in considering ways of applying the results of scientific research to industry. His improved electric light, the Nernst Lamp, used a ceramic body and it might have assumed importance had not tantalum and tungsten filaments been developed. His electrical piano, which replaced the sounding board with radio amplifiers, did not gain acceptance among musicians. In later years, he occupied himself with astrophysical theories, a field in which the heat theorem had important applications.  For his work in thermochemistry he received the Nobel Prize in Chemistry for 1920. Many other distinctions and awards were bestowed upon him for his contributions to science.  Walther Nernst’s fundamental contributions to electrochemistry, the theory of solutions, thermodynamics, solid state chemistry and photochemistry are recorded in a series of monographs, and in his many papers to learned societies, etc. His book *Theoretische Chemie vom Standpunkte der Avogadro’schen Regel und der Thermodynamik* (Theoretical chemistry from the standpoint of Avogadro’s rule and thermodynamics) was first published in 1893 and the tenth edition appeared in 1921 (the fifth English edition in 1923). Together with A. Schonflies he wrote a textbook *Einführung in die mathematische Behandlung der Naturwissenschaften* (Introduction to the mathematical study of the natural sciences), which reached its tenth edition in 1923. Of his other books, his monograph *Die theoretischen und experimentellen Grundlagen des neuen Wärmesatzes* (1918, second edition 1923) was also published in English (The New Heat Theorem, 1926).  Nernst married Emma Lohmeyer in 1892. They had two sons, who were both killed in the First World War, and three daughters. His favourite pastimes were hunting and fishing. He died on November 18, 1941. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0479 |
| **Interview** |  |
|  |  |
| **ID** | **0480** |
| **Biographical** | Fritz Haber was born on December 9, 1868 in Breslau, Germany, in one of the oldest families of the town, as the son of Siegfried Haber, a merchant. He went to school at the St. Elizabeth classical school at Breslau and he did, even while he was at school, many chemical experiments.  From 1886 until 1891 he studied chemistry at the University of Heidelberg under Bunsen, at the University of Berlin under A.W. Hoffmann, and at the Technical School at Charlottenburg under Liebermann. After completing his University studies he voluntarily worked for a time in his father’s chemical business and, being interested in chemical technology, he also worked for a while under Professor Georg Lunge at the Institute of Technology at Zurich. He then finally decided to take up a scientific career and went for one and a half years to work with Ludwig Knorr at Jena, publishing with him a joint paper on diacetosuccinic ester. Still uncertain whether to devote himself to chemistry or physics, he was offered in 1894, and accepted, an assistantship at Karlsruhe by the Professor of Chemical Technology there, Hans Bunte. Here he remained until 1911. Bunte was especially interested in combustion chemistry and Carl Engler, who was also there, introduced Haber to the study of petroleum and Haber’s subsequent work was greatly influenced by these two colleagues.  In 1896 Haber qualified as a Privatdozent with a thesis on his experimental studies of the decomposition and combustion of hydrocarbons and in 1906 he was appointed Professor of Physical Chemistry and Electrochemistry and Director of the Institute established at Karlsruhe to study these subjects.  In 1911 he was appointed to succeed Engler as Director of the Institute for Physical and Electrochemistry at Berlin-Dahlem, where he remained until, in 1933, the Nazi race laws compelled nearly all his staff to resign and Haber, rather than agree to this, himself resigned. He was then invited by Sir William Pope to go to Cambridge, England and there he remained for a while. He had, however, been suffering for some time from heart disease and, fearing the English winter, he moved to Switzerland.  Haber’s early work on the decomposition and combustion of hydrocarbons has already been mentioned.  In 1898 Haber published his textbook on Electrochemistry, which was based on the lectures he gave at Karlsruhe. In the preface to his book he expressed his intention to relate chemical research to industrial processes and in the same year he reported the results of his work on electrolytic oxidation and reduction, in which he showed that definite reduction products can result if the potential at the cathode is kept constant. In 1898 he explained the reduction of nitrobenzene in stages at the cathode and this became the model for other similar reduction processes.  There followed, during the next ten years, many other electrochemical researches. Among these was his work on the electrolysis of solid salts (1904), on the establishment of the quinone-hydroquinone equilibrium at the cathode, which laid the foundations for Biilmann’s quinhydrone electrode for determining the acidity of a liquid; but Haber invented, in collaboration with Cremer, the glass electrode for the same purposes which is now widely used. This led Haber to make the first experimental investigations of the potential differences that occur between solid electrolytes and their aqueous solutions, which were of great interest to physiologists.  During this period Haber also studied the loss of energy by steam engines, turbines and motors driven by fuels, and sought methods of limiting their loss by electrochemical means. He did not succeed in finding a solution of this problem that was commercially applicable, but he did succeed in finding a fundamental solution for the laboratory combustion of carbon monoxide and hydrogen.  He then turned to the study of flames and did fundamental researches on the Bunsen flame, showing that, in the luminous inner cone of this flame, a thermodynamic water-gas equilibrium is established and that, in its outer mantle, there is combustion of water-gas. This led to a chemical method of determining flame temperatures.  Haber then undertook the work on the fixation of nitrogen from the air for which he was given the Nobel Prize in Chemistry for 1918 (awarded in 1919).  In 1905 he had published his book on the thermodynamics of technical gas reactions, in which he recorded the production of small amounts of ammonia from N2 and H2 at a temperature of 1000° C with the help of iron as a catalyst. Later he decided to attempt the synthesis of ammonia and this he accomplished after searches for suitable catalysts, by circulating nitrogen and hydrogen over the catalyst at a pressure of 150-200 atmospheres at a temperature of about 500° C. This resulted in the establishment, with the cooperation of Bosch and Mittasch, of the Oppau and Leuna Ammonia Works, which enabled Germany to prolong the First World War when, in 1914, her supplies of nitrates for making explosives had failed. Modifications of this Haber process also provided ammonium sulphate for use as a fertilizer for the soil. The principle used for this process and the subsequent development of the control of catalytic reactions at high pressures and temperatures, led to the synthesis of methyl alcohol by Alwin Mittasch and to the hydrogenation of coal by the method of Bergius and the production of nitric acid.  During the years between the two World Wars Haber produced his firedamp whistle for the protection of miners, his quartz thread manometer for low gas pressures and his observation that adsorption powers can be due to unsaturated valence forces of a solid body, on which [Langmuir](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1932/index.html) founded his theory of adsorption. When the First World War broke out he was appointed a consultant to the German War Office and organised gas attacks and defences against them. This and other work undermined his health and for some time he was engaged in administrative work. He helped to create the German Relief Organisation and served on the League of Nations Committee on Chemical Warfare.  From 1920 until 1926 he experimented on the recovery of gold from sea water, his idea being to enable Germany to meet her war reparations. Greatly depressed by the failure of this project, which he attributed to his own deficiency, he devoted himself to the reorganisation of his Institute, to which he appointed sectional directors with complete freedom in their work. Among these were James Franck, Herbert Freundlich, Michael Polanyi and Rudolf Ladenburg; from the Institute came much work on colloid chemistry and atomic physics. Haber himself, at this time, made great efforts to re-establish the scientific relationships of Germany with other countries and the colloquia which he held every fortnight did much to establish the international repute of his Institute. During his last years he worked on chain reactions and on mechanisms of oxidation and on hydrogen peroxide catalysis.  Haber lived for science, both for its own sake and also for the influence it has in moulding human life and human culture and civilization. Versatile in his talents, he possessed an astonishing knowledge of politics, history, economics, science and industry and he might have succeeded equally well in other fields. The hesitation with which he finally decided to be a chemist has already been mentioned. He welcomed administrative responsibilities in addition to research work. Always approachable and courteous, he was interested in every kind of problem. His ability to clarify, in a few sentences, the obscurities of a scientific discussion, was a valuable feature of the colloquia he held at his Institute and his organising talent made him a model Director of a large establishment in which he allowed complete freedom, to the workers under him, maintaining, nevertheless, a remarkable control over the activities of the Institute as a whole. A man of forceful personality, he left a lasting impression on the minds of all his associates.  Apart from the Nobel Prize, Haber received many honours during his life. At Max von Laue’s instigation, the Institute for Physical and Electrochemistry at Berlin-Dahlem was renamed the Fritz Haber Institute after his death.  After a grave illness, Haber died on January 29, 1934, at Basle, on his way from England to convalesce in Switzerland, his spirit broken by his rejection by the Germany he had served so well. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0480 |
| **Interview** |  |
|  |  |
| **ID** | **0481** |
| **Biographical** | Richard Martin Willstätter was born in Karlsruhe in Baden on August 13, 1872, and went to school first in his home town and then, when his parents moved house, at the Technical School in Nuremberg. When he was 18 he went to the University of Munich where he studied Science, entered the Department of Chemistry under [Baeyer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1905/index.html) and stayed there for the following fifteen years, first as a student, from 1896 as a lecturer – pursuing his scientific work independently – until in early 1902 he became J. Thiele’s successor as Extraordinary Professor.  As a young man he studied principally the structure and synthesis of plant alkaloids such as atropine and cocaine. In this, as in his later work on quinone and quinone type compounds which are the basis of many dyestuffs, he sought to acquire skill in chemical methods in order to prepare himself for the extensive and more difficult work of investigating plant and animal pigments. For this undertaking the working facilities which the Munich laboratory afforded him were too limited and he was glad to accept the first offer of a Professorial Chair which he received in the summer of 1905. It was thus that he came to Zurich to the Federal Technical College.  These seven years in Switzerland were for him the best and most significant. But while research and teaching brought him great satisfaction, at the same time he suffered personal misfortune and soon became lonely. He enjoyed his work in Zurich so much that he did not think of those years as a waiting period until he was called back to Germany in 1912. For the Jubilee of the University of Berlin, Kaiser Wilhelm had established a Society for the Promotion of Scientific Knowledge and this body had founded an Institute of Chemistry in Berlin/Dahlem. He was offered a Research Laboratory here in conjunction with an honorary professorship at the University of Berlin.  In the two short years before the outbreak of the first World War he was able with a team of collaborators to round off his investigations into chlorophyll and, in connexion with that, to complete some work on haemoglobin and, in rapid succession, to carry out his studies of anthocyanes, the colouring matter of flowers and fruits. These investigations into plant pigments, especially the work on chlorophyll, were honoured by the award of the Nobel Prize for Chemistry (1915), just at the time when he had decided to accept a call to the University of Munich and again, as successor to his old teacher Adolf von Baeyer, take an active part in university teaching; for, as things were then, the even tenour of scientific life at Dahlem was gone.  In the period that followed Willstätter continued on lines of fundamental importance, and his brilliant and fruitful work is regarded today as a pioneering achievement. The investigations into photosynthesis and into the nature and activity of the enzymes were precursory of modern Biochemistry. At that time the method so far developed of concentrating enzymes through adsorption did not make it possible to attain to the crystallized enzymes. In this connexion, Willstätter carried out important studies of adsorbents, metal hydroxides, hydrogels and silicic acids. In addition he was quick to give his attention also to problems of theoretical chemistry. Thus he achieved the first synthesis of cyclo-octatetraene, and went on to compare it with benzene; so also he set up experiments to produce cyclobutadiene.  Willstätter’s career came to a tragic end when, as a gesture against increasing antisemitism, he announced his retirement in 1924. Expressions of confidence by the Faculty, by his students and by the Minister failed to shake the fifty-three year old scientist in his decision to resign. He lived on in retirement in Munich, maintaining contact only with those of his pupils who remained in the Institute and with his successor, [Heinrich Wieland](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1927/index.html), whom he had nominated. Dazzling offers both at home and abroad were alike rejected by him. In 1938 he fled from the Gestapo with the help of his pupil A. Stoll and managed to emigrate to Switzerland, losing all but a meagre part of his belongings.  Willstätter was married to Sophie Leser, the daughter of a Heidelberg University professor. They had one son, Ludwig, and one daughter, Ida Margarete.  The old man passed the last three years of his life in Muroalto near Locarno writing his Biography (*Aus meinem Leben*, edited by A. Stoll, Verlag Chemie, Weinheim, 1949; English edition *From my Life*, Benjamin, New York, 1965) until he died on 3rd August, 1942, of a heart attack.  In 1956 a memorial to Richard Willstätter was unveiled in Muroalto.  In an epilogue written by A. Stoll to Willstätter’s Biography the list of honours and distinctions accorded to this great scholar in every part of the civilized world alone occupies no less than three pages. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0481 |
| **Interview** |  |
|  |  |
| **ID** | **0482** |
| **Biographical** | Theodore William Richards was born in Germantown, Pennsylvania, USA on January 31, 1868. His father, William T. Richards was a well-known painter of landscapes and seascapes: his mother, Anna, *née* Matlack, won fame for her poetical works.  During his childhood, Richards travelled to England and France and, up to the age of fourteen, he was educated by his mother. In 1883 he entered Haverford College, Pennsylvania, to graduate in science in 1885 and enter Harvard University. He received the degrees, B.A. in 1886; M.A. and Ph.D. in 1888. The following twelve months were spent in Germany where he studied under Victor Meyer, P. Jannasch, G. Kruss and W. Hempel; on his return to Harvard he was appointed Assistant in Chemistry. He successively became Instructor (1891), Assistant Professor (1894) and Professor (1901); in 1901 he also declined an offer of a full professorship in the University of Göttingen. In 1903 he became Chairman of the Department of Chemistry at Harvard and in 1912 he was appointed Erving Professor of Chemistry and Director of the Wolcott Gibbs Memorial Laboratory.  About half of Richards’ original work has concerned atomic weights, starting in 1886 with work on oxygen and copper. He quickly developed a new technique for the determination of halide ratios and did much towards improving methods of weighing. He invented the nephelometer and demonstrated the insidious effect of occluded moisture in gases and solids. By 1912 he had redetermined, with the highest accuracy, the atomic weights of over thirty important elements and in later years he was to play his part, by his work on the determination of the atomic weight of isotopes, in the modern concept of the atom. During his initial work he was guided by J.P. Cooke.  Richards also studied atomic and molecular volume and he formulated a hypothesis of compressible atoms. He carried out a series of measurements of compressibilities of many elements and compounds in support of his theory, developing, applying and testing new methods and techniques. He introduced the use of transition temperatures of pure hydrated salts as fixed points in the standardization of thermometers, and the fundamentals of adiabatic calorimetry were developed under his guidance. His researches are recorded in some three hundred technical papers published mainly in the *Proceedings of the American Academy of Arts and Sciences,* the *Journal of the American Chemical Society* and the publications of the Carnegie Institution of Washington.  Professor Richards received honorary doctorate degrees in science from Yale (1905), Harvard (1910), Cambridge, Oxford and Manchester (1911) and Princeton (1923); in philosophy from Prague (1909) and Christiania (1911); in law from Haverton (1908), Pittsburgh (1915) and Pennsylvania (1920); in chemistry from Clark (1909); and in medicine from Berlin (1910). He was President of the American Chemical Society (1914), the American Association for the Advancement of Science (1917) and the American Academy of Arts and Sciences (1919-21). He received the Davy Medal (Royal Society), 1910; the Faraday Medal, 1911, and Willard Gibbs Medal (American Chemical Society), 1912; the Franklin Medal (Franklin Institute), 1916; and the Le Blanc and Lavoisier Medal in 1922. He was appointed Officier de la Lègion d’Honneur in 1925 and he held fellowships or memberships of academies and learned societies in the United States, the British Isles, France, Germany and Scandinavia.  Richards married Miriam Stuart Thayer, daughter of Professor Joseph H. Thayer, in 1896; they had one daughter and two sons. His favourite recreations were sketching, golf and sailing.  He died at Cambridge, Massachusetts, on April 2, 1928. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0482 |
| **Interview** |  |
|  |  |
| **ID** | **0483** |
| **Biographical** | Alfred Werner, son of factory foreman J.A. Werner and his wife Jeanne, *née* Tesche, was born on December 12, 1866, at Mülhausen in Alsace, where he went to school. While he was at school he showed an interest in chemistry and did, when he was only 18, his first independent chemical research.  From 1885 until 1886 he did his military service in Karlsruhe and, during this, attended the lectures of Engler at the Technical High School in that city. In 1886 he attended lectures at the Federal Technical High School at Zurich and in 1889 obtained there the Diploma in Technical Chemistry. During his studies there he was much influenced by Professor A. Hantzsch.  In 1889 he was appointed Assistant in Professor Lunge’s laboratory at the Zurich Technical High School and he then began to cooperate with Professor Hantzsch in research. In 1890 he took his degree in the University of Zurich with a thesis on the spatial arrangements of the atoms in molecules containing nitrogen.  From 1890 until 1891 he did further work on this subject and visited Paris, where he worked under Professor Berthelot at the Collège de France. In 1892 he returned to Zurich as a lecturer in the Technical High School, and in 1893 he was appointed Associate Professor in the University of Zurich, to succeed Victor Merz and then gave the University lectures on organic chemistry.  In 1895 he became, when he was only 29 years old, Professor of Chemistry in the University, giving the lectures on organic chemistry until, in 1902, he took over the lectures on inorganic chemistry as well. In 1895 he was acquired Swiss nationality and though he was offered posts at Vienna, Basle and Wurzburg, he declined these, preferring to remain in Zurich.  Werner’s name will always be associated with the theory of coordination which he established and with his work on the spatial relationships of atoms in the molecule, the foundations of which were laid in the work he did, when he was only 24, for his doctorate thesis in 1892. In this work he formulated the idea that, in the numerous compounds of tervalent nitrogen, the three valence bonds of the nitrogen atom are directed towards the three corners of a tetrahedron, the fourth corner of this being occupied by the nitrogen atom. In 1891 he had published a paper on the theory of affinity and valence, in which he substituted for Kekulé’s conception of constant valence, the idea that affnity is an attractive force exerted from the centre of the atom which acts uniformly towards all parts of the surface of the atom.  In 1893 he stated, in a paper on mineral compounds, his theory of variable valence, according to which inorganic molecular compounds contain single atoms which act as central nuclei around which are arranged a definite number of other atoms, radicals or other molecules in a simple, spatial, geometric pattern. The figure which expresses the number of atoms thus grouped round a central nucleus was called by Werner the coordination number, the most important of these coordination numbers being 3, 4, 6 and 8, the number 6 occurring especially often. Thousands of molecular compounds correspond to the number 6 type, and in the majority of these there is a central atom with coordinated atoms at the corners of an octahedron.  For the next 20 years Werner and his collaborators studied and prepared new series of molecular compounds and studied their configurations, publishing many papers on them, 150 of which were by himself. Finally, his work culminated in the discovery of optically-active isomers of the complexes studied, the existence of which had been forecast by his hypothesis. More than 40 series of optically-active complexes with octahedral symmetry were separated in optically-active forms, with the result that the spatial configuration of the complexes to the coordination number 6 was established as firmly as that of the tetrahedral carbon atom of [van ‘t Hoff](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1901/index.html) and Le Bel.  Werner also worked on complexes with other coordination numbers, especially 4, for which the form can be tetrahedral or a plane square. As Paul Pfeiffer, in his account of Werner’s work published in *Great Chemists* (1961, Edited by Eduard Farber, Interscience, New York) remarks the coordination theory of Werner extended throughout the whole range of systematic inorganic chemistry and into organic chemistry as well. For his work on it Werner was awarded the Nobel Prize for Chemistry for 1913.  Werner was corresponding member of the Royal Society of Sciences (Königliche Gesellschaft der Wissenschaften) at Göttingen and of the Physico-Medical Society (Physikalisch-medizinische Sozietät) of Erlangen. He held an honorary doctorate of the University of Geneva, and was an Honorary Member of the Society of Physics and Natural History in the same town, of the Physical Association (Physikalischer Verein) of Frankfurt/Main, of the German Bunsengesellschaft, of the Société Vaudoise des Sciences Naturelles at Lausanne, and of the Chemical Society of London. He was also a permanent member of the Imperial Society of Friends of Natural History, Anthropology and Ethnography of Moscow. France conferred upon him the Leblanc Medal of its Societe Chimique and the distinction of Officier de l’Instruction Publique.  Werner was a very sociable man, whose recreations were billiards, chess and the Swiss card game, Jass. He spent his holidays among the mountains and travelled much to attend scientific meetings outside Switzerland. As a lecturer he was a convincing and enthusiastic speaker with a gift for clear explanations of difficult problems. Among his pupils were such distinguished men as Jantsch, [Karrer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1937/index.html) and Pfeiffer. Valuable for others were his books *Neuere Anschauungen auf dem Gebiete der anorganischen Chemie* (New ideas in inorganic chemistry) and *Lehrbuch der Stereochemie* (Textbook of stereochemistry), both published in 1904.  In 1894, Werner married Emma Giesker of Zurich, a member of a German family. They had one son, Alfred, and one daughter, Charlotte.  In 1913, the year in which he received the Nobel Prize for Chemistry, he was already suffering from arteriosclerosis and by 1915 this had compelled him to give up his general lectures on chemistry and in 1919 he had to give up his Professorship. On November 15, 1919, he died at the early age of 53. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0483 |
| **Interview** |  |
|  |  |
| **ID** | **0484** |
| **Biographical** | François Auguste Victor Grignard was born in Cherbourg on May 6, 1871. He attended local schools during 1883-87 and in 1889 he won a scholarship to the École Normale Spécial at Cluny. After two years, the school, which was intended to produce teachers for modern secondary schools, was closed because of a dispute between supporters of the “classic” and “modern” methods of secondary education. Grignard and his classmates were transferred to other establishments in order to finish the entitlement of their scholarships and Grignard himself had the good fortune to join the University of Lyons, where he was attached to the Faculté des Sciences. He was unsuccessful in the licentiate examination in mathematics and in 1892 he left to fulfil his military service. Towards the end of 1893 he was demobilized and returned to Lyons to gain the degree Licencié ès Sciences Mathématiques in 1894.  In December, 1894, after some persuasion, he accepted a junior post in the Faculté des Sciences, working with Louis Bouveault: he was later promoted to préparateur and it was then that he began his long association with Philippe Barbier. He obtained the degree Licencié-ès-Sciences Physiques and in 1898 he became chef des travaux pratiques and also wrote his first paper, jointly with Barbier. In 1901 he submitted his brilliant thesis on organic magnesium compounds *Sur les Combinaisons organomagnésiennes mixtes*, and was awarded the degree Docteur ès Sciences de Lyons.  He was appointed Maître de Conférences, University of Besançon in 1905 but he returned to Lyons in the following year, occupying a similar position until his election as Professeur-adjoint de Chimie Générale in 1908. In 1909 he took charge of the Department of Organic Chemistry at Nancy, in succession to Blaise who had moved to Paris, and in the following year he became Professor of Organic Chemistry. At the beginning of the First World War, Grignard was mobilized in his former rank of corporal, but he was soon to be commissioned to study, at Nancy, the cracking of benzols and, later, to work on problems of chemical warfare in Paris. He visited the United States during 1917-18 as the chemical representative on the Tardieu Committee and he delivered a lecture at the Mellon Institute. After the war he returned to Nancy and in 1919 he succeeded Barbier as Professor of General Chemistry at Lyons. In 1921 he took an additional post as Director of l’École de Chimie Industrielle de Lyons, becoming a member of the University Council, and in 1929 he became Dean of the Faculty of Sciences.  Grignard’s first investigations concerned “ethyl b-isopropylacetobutyrate and the stereoisomeric diisopropylbutenedicarboxylic acids” and studies of branched unsaturated hydrocarbons. In 1899, on Barbier’s recommendation, he studied organomagnesium compounds and his discovery of the classic preparation of magnesium alkyl halides was first communicated by [Henri Moissan](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1906/index.html) to the Académie des Sciences on May 11, 1900. He quickly developed the immediate applications of these elegant and simple reagents, which were destined to play such an important part in organic synthesis that, at the time of his death in 1935, there were over 6,000 references to them in the literature. He used the agents to prepare and study the more exotic alcohols, ketones, keto-esters, nitriles and terpene compounds and he developed a method for the synthesis of fulvenes. He has also been concerned with work on the constitution of unsaturated compounds by quantitative ozonization, condensation of aldehydes and ketones, ketone splitting of tertiary alcohols, the cracking of hydrocarbons in presence of aluminium chloride and catalytic hydrogenation and dehydrogenation processes under reduced pressures.  Grignard was the author of some 170 publications on his researches and, at his death, he was working to fulfil his ambition to see a great chemical reference work in the French language. Two volumes of his *Traité de Chimie Organique* (Treatise on organic chemistry) had already been published, two more were ready for the press and the editorial work for another two was well advanced: it was later to be finished by his collaborators. In 1937, two of his students, Jean Cologne and Roger Grignard, published *Précis de Chimie Organique* (Survey of organic chemistry) which is based on Grignard’s lecture course in organic chemistry.  Grignard shared the Cahours Prize (Institut de France) in 1901 and again in 1902, when he also won the Berthelot Medal. In 1905 he was awarded the Prix Jecker and in 1912 the Lavoisier Medal. In 1912 the Nobel Prize for Chemistry was conferred upon him, on account of his discovery of the so called Grignard reagent, sharing the prize with Paul Sabatier who received it because of his method of hydrogenating organic compounds by means of finely divided metals. In the order of the Légion d’Honneur, he was appointed Chevalier (1912), Officier (1920) and Commandeur (1933). He was also Honorary Professor, University of Nancy (1931); he held the honorary doctorate of the Universities of Brussels and Louvain, and he was Honorary Fellow of the Chemical Society (London) and foreign member of the [Royal Swedish Academy of Sciences](http://www.kva.se/).  Grignard married Augustine Marie Boulant in 1910 and their only son, Roger, followed in the academic footsteps of his father: they also had one daughter. He died on December 13, 1935. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0484 |
| **Interview** |  |
|  |  |
| **ID** | **0485** |
| **Biographical** | Paul Sabatier was born at Carcassonne in Southern France on November 5, 1854. He was educated at the local Lycée and then prepared at Toulouse for the entrance examinations to the École Polytechnique and the École Normale Supérieure. He was accepted for both and chose the latter which he entered in 1874; three years later he graduated, first in his class. He taught physics for a year in a local school at Nîmes before going to the Collège de France in 1878 as assistant to Berthelot. He received the degree of Doctor of Science in 1880.  Sabatier took charge of courses in physics in the Faculty of Sciences at Bordeaux until January, 1882, when he accepted a similar post at the University of Toulouse. He additionally became responsible for courses in chemistry in 1883 and was elected Professor of Chemistry in 1884, a post which he retained until his retirement in 1930. He became Dean of the Faculty of Science in 1905 and continued to lecture after his retirement until his death in 1941. He was ever faithful to Toulouse and turned down many offers of attractive positions elsewhere, notably the succession to [Moissan](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1906/index.html) at the Sorbonne in 1908.  Sabatier’s earliest researches concerned the thermochemistry of sulphur and metallic sulphates, the subject for his thesis leading to his doctorate, and, in Toulouse, he continued his physico-chemical investigations to sulphides, chlorides, chromates and copper compounds. He also studied the oxides of nitrogen and nitrosodisulphonic acid and its salts and carried out fundamental research on partition coefficients and absorption spectra.  When he started his investigations into the phenomenon of catalysis, Sabatier soon pointed out anomalies in Faraday’s physical theory and he in turn formulated his chemical theory which postulated the formation of unstable intermediaries. His subsequent painstaking work and discovery of the use of finely-divided metal hydrogenation catalysts subsequently formed the bases of the margarine, oil hydrogenation, and synthetic methanol industries. He demonstrated the selectivity of catalytic action and also the selectivity of catalysts to poisons, as well as introducing the use of supports and showing the resultant enhanced activity. He also made a close study of catalytic hydration and dehydration, examining carefully the feasibility of specific reactions and the general activity of various catalysts.  Sabatier’s work is accurately recorded in the publications of learned societies and his most important book, *La Catalyse en Chimie Organique* (Catalysis in organic chemistry), was first published in 1913, with a second edition in 1920, of which an English translation by E.E. Reid was published in 1923.  Sabatier was a Member of the French Academy of Sciences and Commander of the Légion d’Honneur. He was Doctor of Science, *honoris causa*, of the University of Philadelphia and honorary member of the Royal Society of London, of the Academy of Madrid, the Royal Netherlands Academy of Sciences, the American Chemical Society, and many other foreign institutions. He was awarded the Prix Lacate (1897); the Prix Jecker (1905); the Davy Medal (1915) and Royal Medal (1918) of the Royal Society; and the Franklin Medal of the Franklin Institute (1933). For his method of hydrogenating organic compounds in the presence of finely divided metals, he was awarded the Nobel Prize in Chemistry for 1912, sharing the prize with Victor Grignard, who received it on account of his discovery of the so-called Grignard reagent.  Paul Sabatier was a very reserved man. He married Mlle. Herail and they had four daughters, one of whom married the Italian chemist, Emilio Pomilio. He was fond of art and gardening.  He died on August 14, 1941. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0485 |
| **Interview** |  |
|  |  |
| **ID** | **0486** |
| **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](https://www.nobelprize.org/nobel_prizes/physics/laureates/1903/index.html) 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, Iréne, 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* (Investigations on radioactive substances) (1904), *L’Isotopie et les Eléments Isotopes* (Isotopy and isotopic elements) and the classic *Traité de radioactivité* (Treatise on radioactivity) (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](https://www.nobelprize.org/nobel_prizes/physics/laureates/1903/index.html), 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, 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.  The Curie’s elder daughter, Iréne, married Frédéric Joliot in 1926 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.  Mme. Curie died in Savoy, France, after a short illness, on July 4, 1934. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0486 |
| **Interview** |  |
|  |  |
| **ID** | **0487** |
| **Biographical** | Otto Wallach was born on March 27, 1847, in Königsberg, Germany, the son of Gerhard Wallach and his wife, *née* Otillie Thoma. His father was a high-ranking civil servant, who later became Auditor General at Potsdam.  During his early school years at the humanistic “Gymnasium” at Potsdam, Wallach had a profound liking for history and art – in those days subjects like chemistry were hardly taught at secondary-school level.  In 1867 he went to Göttingen to study chemistry with Wöhler, Fittig and Hübner but soon left for Berlin to study for one semester under A.W. Hofmann and G. Magnus. After his return to Göttingen he worked so hard that he managed to obtain his doctor’s degree – in 1869 under Hübner – after studying for only five semesters. (At that time working hours at the Wöhler laboratory were from 7 a.m. till 5 p.m., after which gas was turned off and some work had to be rounded-off under the light of privately bought candles.) His thesis dealt with the position isomers in the toluene series.  In 1869 and 1870 he was assistant to H. Wichelhaus in Berlin, with whom he worked on the nitration of b-naphthol. Easter 1870 found him in Bonn with Kekulé. The latter, himself an artist at heart and who once seriously considered making architecture his profession, had written to Wallach: “It will not hurt you to come to Bonn. Here we are leading a scientific artist life.” That same year, however, Wallach had to leave Bonn for military service in the Franco-Prussian war.  After the war he tried for the third time to establish himself in Berlin, working with a newly founded firm “Aktien-Gesellschaft für Anilin-Fabrikation” (later “Agfa”), but his fragile health could not stand the noxious fumes of the factory, and in 1872 he returned to Bonn, where he stayed for 19 years. He first became assistant in the organic laboratory, and later was appointed Privatdozent. In 1876 came his appointment as Professor Extraordinary. When in 1879 the Chair of Pharmacology became vacant he was obliged to occupy it, which forced him to specialize in this direction. It was during this period that he discovered the iminochlorides by the action of phosphorus pentachloride on the acid amides. But when Kekulé drew his attention to the existence of an old forgotten cupboard full of bottles containing essential oils, and invited him to make a study of the contents, he became absorbed in the matter, thus entering a field of study in which he was to be the eminent pioneer for more than a decade, and which was to be his main life-work, crowned with the highest possible distinction.  Already in his first publication (1884) he raised the question of the diversity of the various members of the C10H16 group, which in current practice at that time came under a multitude of names ranging from terpene to camphene, citrene, carvene, cinene, cajuputene, eucalyptine, hesperidine, etc. Utilizing common reagents such as hydrogen chloride and hydrogen bromide, he succeeded in characterizing the differences between the structure of these compounds. A year later he could establish that many of them were indeed identical. In 1909 he published the results of his extensive studies in his book *Terpene und Campher*, a volume of 600 pages dedicated to his pupils.  Mention should also be made of his other investigations; the conversion of chloral into dichloroacetic acid, the series of studies on the amide chlorides, imide chlorides, amidines, glyoxalines, etc., his work on azo dyes and diazo compounds, and many others. They all denote his practical skill: like [Emil Fischer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1902/index.html) and [Adolf von Baeyer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1905/index.html), he relied more on carefully performed experiments than on theoretical deliberations.  In 1889 he was appointed Victor Meyer’s successor in Wöhler’s Chair, which made him at the same time Director of the Chemical Institute at Göttingen. He retired in 1915 from these posts when at the start of World War I six of his assistants were killed in action.  Wallach received the Nobel Prize in 1910 for his work on alicyclic compounds. His other honours included Honorary Fellowships of the Chemical Society (1908), Honorary Doctorates of the Universities of Manchester, Leipzig and the Technological Institute of Braunschweig. In 1912 he became Honorary Member of the Verein Deutscher Chemiker. He received the Kaiserlicher Adlerorden III. Klasse (Imperial Order of the Eagle) in 1911, the Davy Medal in Gold and Silver in 1912, and in 1915 the Königlicher Kronorden II. Klasse (Royal Order of the Crown).  Wallach remained a bachelor throughout his life, and died on February 26, 1931. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0487 |
| **Interview** |  |
|  |  |
| **ID** | **0488** |
| **Biographical** | Wilhelm Ostwald was born on September 2, 1853, in Riga, Latvia, as the son of master-cooper Gottfried Wilhelm Ostwald and Elisabith Leuckel.  He was educated at the “Real gymnasium” there and in 1872 was admitted to Dorpat University to read chemistry. After taking his final examinations three years later, he obtained the post of assistant at the Physics Institute under Professor Arthur von Oettingen, and subsequently took a similar position in the Chemistry Laboratory under Carl Schmidt. Ostwald himself declared that he was most indebted to these two teachers for his scientific training. In 1877 he was admitted as unpaid academic lecturer at Dorpat University, and this was followed (1881) by the appointment of full time Professor of Chemistry at the Polytechnicum in Riga. Six years later he accepted an invitation as Professor of Physical Chemistry at Leipzig University. Among his later famous pupils are [Arrhenius](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1903/index.html) (Nobel Prize 1903), [Van ‘t Hoff](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1901/index.html) (Nobel Prize 1901), [Nernst](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1920/index.html) (Nobel Prize 1920), Tammann and Wislicenus. Ostwald remained in Leipzig until he retired in 1906, with the short interruption for one term as first “Exchange Professor” at Harvard University, Cambridge (Mass.) in 1904-1905.  Ostwald started his experimental work in 1875, with an investigation on the law of mass action of water in relation to the problems of chemical affinity, with special emphasis on electrochemistry and chemical dynamics.  In consequence of his pioneering work especially in the field of electrochemistry, which also led to the discovery of the law of dilution named after him, his activities as a writer and his gift for organization, Ostwald became one of the founders of classical physical chemistry. He published numerous textbooks, starting with the *Lehrbuch der Allgemeinen Chemie* (Textbook of general chemistry) in 1884. This was followed by *Grundriss der Allgemeinen Chemie* (Outline of general chemistry) in 1889 and *Hand- und Hilfsbuch zur Ausführung physikalisch-chemischer Messungen* (Handbook and manual for physicochemical measurements) in 1893. Numerous other scientific works on analytical chemistry, electrochemistry, inorganic chemistry followed.  Ostwald also founded and edited the *Zeitschrift für physikalische Chemie* in 1887; Ostwald himself edited 100 volumes, up to 1922.  He was also in charge of the organization of the Department of Physical Chemistry at Leipzig University, and in 1894 he founded the “Deutsche Elektrochemische Gesellschaft” (German Electrochemical Society) which in 1902 expanded to become the “Deutsche Bunsen-Gesellschaft für angewandte physikalische Chemie” (German Bunsen-Society for Applied Physical Chemistry).  In 1909 Ostwald was awarded the Nobel Prize for Chemistry for his work on catalysis, chemical equilibria and reaction velocities. He received honorary doctorates from several universities in Germany, Great Britain and the USA, and was made an honorary member of learned societies in Germany, Sweden, Norway, the Netherlands, Russia, Great Britain and the USA. In 1899 he was made a “Geheimrat ” by the King of Saxony.  After his retirement in 1906, Ostwald found a new sphere for his scientific and organizatorial talents. Besides continuing his studies and publication on philosophy, such as *Der energetische Imperativ* (The energetic imperative), *Moderne Naturphilosophie* (Modern natural philosophy), *Die Pyramide der Wissenschaften* (The pyramid of the sciences), he also took an active part in public life. He supported the middle-class pacifist movement, was interested in educational reforms and in monism. He believed that in view of his position he could decisively fight the Church’s claim to power in the field of natural sciences and to spread a modern scientific ideology. This aim he pursued in his writings *Monistische Sonntagspredigten* (Monistic Sunday sermons) and *Arbeiten zum Monismus* (Works on monism).  In 1902 Ostwald founded *Annalen der Naturphilosophie* and edited 14 volumes up to 1921. He also founded *Klassiker der exakten Wissenschaften* in 1889, of which some 250 volumes have been published.  Right up to the end of his life Ostwald studied colours and shapes, in the endeavour to find a scientific standardization for colours. His main works in this field are *Die Farbenfibel* (The coulour primer), *Die Farbenlehre* (Colour theory), *Die Harmonie der Farben* (Harmony of the colours). He also published a periodical *Die Farbe* (Colour).  Ostwald was married to Helene von Reyher in 1880. They had two daughters and three sons, one of whom, Dr. Karl Wilhelm Wolfgang (known in the scientific literature as Wo. Ostwald), was Lecturer in the University of Leipzig, and Editor of the *Zeitschrift für Chemie und Industrie der Kolloide*, the forerunner of the *Kolloid-Zeitschrift*.  After an extremely active life, Ostwald died at his country home near Leipzig on April 4, 1932. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0488 |
| **Interview** |  |
|  |  |
| **ID** | **0489** |
| **Biographical** | Ernest Rutherford was born on August 30, 1871, in Nelson, New Zealand, the fourth child and second son in a family of seven sons and five daughters. His father James Rutherford, a Scottish wheelwright, immigrated to New Zealand with Ernest’s grandfather and the whole family in 1842. His mother, née Martha Thompson, was an English schoolteacher, who, with her widowed mother, also went to live there in 1855.  Ernest received his early education in Government schools and at the age of 16 entered Nelson Collegiate School. In 1889 he was awarded a University scholarship and he proceeded to the University of New Zealand, Wellington, where he entered Canterbury College[\*](https://www.nobelprize.org/prizes/chemistry/1908/rutherford/biographical/#not). He graduated M.A. in 1893 with a double first in Mathematics and Physical Science and he continued with research work at the College for a short time, receiving the B.Sc. degree the following year. That same year, 1894, he was awarded an 1851 Exhibition Science Scholarship, enabling him to go to Trinity College, Cambridge, as a research student at the Cavendish Laboratory under [J.J. Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1906/index.html). In 1897 he was awarded the B.A. Research Degree and the Coutts-Trotter Studentship of Trinity College. An opportunity came when the Macdonald Chair of Physics at McGill University, Montreal, became vacant, and in 1898 he left for Canada to take up the post.  Rutherford returned to England in 1907 to become Langworthy Professor of Physics in the University of Manchester, succeeding Sir Arthur Schuster, and in 1919 he accepted an invitation to succeed Sir Joseph Thomson as Cavendish Professor of Physics at Cambridge. He also became Chairman of the Advisory Council, H.M. Government, Department of Scientific and Industrial Research; Professor of Natural Philosophy, Royal Institution, London; and Director of the Royal Society Mond Laboratory, Cambridge.  Rutherford’s first researches, in New Zealand, were concerned with the magnetic properties of iron exposed to high-frequency oscillations, and his thesis was entitled *Magnetization of Iron by High-Frequency Discharges*. He was one of the first to design highly original experiments with high-frequency, alternating currents. His second paper, *Magnetic Viscosity*, was published in the *Transactions of the New Zealand Institute* (1896) and contains a description of a time-apparatus capable of measuring time intervals of a hundred-thousandth of a second.  On his arrival at Cambridge his talents were quickly recognized by Professor Thomson. During his first spell at the Cavendish Laboratory, he invented a detector for electromagnetic waves, an essential feature being an ingenious magnetizing coil containing tiny bundles of magnetized iron wire. He worked jointly with Thomson on the behaviour of the ions observed in gases which had been treated with X-rays, and also, in 1897, on the mobility of ions in relation to the strength of the electric field, and on related topics such as the photoelectric effect. In 1898 he reported the existence of alpha and beta rays in uranium radiation and indicated some of their properties.  In Montreal, there were ample opportunities for research at McGill, and his work on radioactive bodies, particularly on the emission of alpha rays, was continued in the Macdonald Laboratory. With R.B. Owens he studied the “emanation” of thorium and discovered a new noble gas, an isotope of radon, which was later to be known as thoron. [Frederick Soddy](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1921/index.html) arrived at McGill in 1900 from Oxford, and he collaborated with Rutherford in creating the “disintegration theory” of radioactivity which regards radioactive phenomena as atomic – not molecular – processes. The theory was supported by a large amount of experimental evidence, a number of new radioactive substances were discovered and their position in the series of transformations was fixed. [Otto Hahn](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1944/index.html), who later discovered atomic fission, worked under Rutherford at the Montreal Laboratory in 1905-06.  At Manchester, Rutherford continued his research on the properties of the radium emanation and of the alpha rays and, in conjunction with H. Geiger, a method of detecting a single alpha particle and counting the number emitted from radium was devised. In 1910, his investigations into the scattering of alpha rays and the nature of the inner structure of the atom which caused such scattering led to the postulation of his concept of the “nucleus”, his greatest contribution to physics. According to him practically the whole mass of the atom and at the same time all positive charge of the atom is concentrated in a minute space at the centre. In 1912 [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html) joined him at Manchester and he adapted Rutherford’s nuclear structure to [Max Planck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1918/index.html)‘s quantum theory and so obtained a theory of atomic structure which, with later improvements, mainly as a result of Heisenberg’s concepts, remains valid to this day. In 1913, together with H. G. Moseley, he used cathode rays to bombard atoms of various elements and showed that the inner structures correspond with a group of lines which characterize the elements. Each element could then be assigned an atomic number and, more important, the properties of each element could be defined by this number. In 1919, during his last year at Manchester, he discovered that the nuclei of certain light elements, such as nitrogen, could be “disintegrated” by the impact of energetic alpha particles coming from some radioactive source, and that during this process fast protons were emitted. [Blackett](https://www.nobelprize.org/nobel_prizes/physics/laureates/1948/index.html) later proved, with the cloud chamber, that the nitrogen in this process actually could be transformed into an oxygen isotope. [G. de Hevesy](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1943/index.html) was also one of Rutherford’s collaborators at Manchester.  An inspiring leader of the Cavendish Laboratory, he steered numerous future Nobel Prize winners towards their great achievements: [Chadwick](https://www.nobelprize.org/nobel_prizes/physics/laureates/1935/index.html), Blackett, [Cockcroft and Walton](https://www.nobelprize.org/nobel_prizes/physics/laureates/1951/index.html); while other laureates worked with him at the Cavendish for shorter or longer periods: [G.P. Thomson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1937/index.html), [Appleton](https://www.nobelprize.org/nobel_prizes/physics/laureates/1947/index.html), [Powell](https://www.nobelprize.org/nobel_prizes/physics/laureates/1950/index.html), and [Aston](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1922/index.html). C.D. Ellis, his co-author in 1919 and 1930, pointed out “that the majority of the experiments at the Cavendish were really started by Rutherford’s direct or indirect suggestion”. He remained active and working to the very end of his life.  Rutherford published several books: *Radioactivity* (1904); *Radioactive Transformations* (1906), being his Silliman Lectures at Yale University; *Radiation from Radioactive Substances*, with James Chadwick and C.D. Ellis (1919, 1930) – a thoroughly documented book which serves as a chronological list of his many papers to learned societies, etc.; *The Electrical Structure of Matter* (1926); *The Artificial Transmutation of the Elements* (1933); *The Newer Alchemy* (1937).  Rutherford was knighted in 1914; he was appointed to the Order of Merit in 1925, and in 1931 he was created First Baron Rutherford of Nelson, New Zealand, and Cambridge. He was elected Fellow of the Royal Society in 1903 and was its President from 1925 to 1930. Amongst his many honours, he was awarded the Rumford Medal (1905) and the Copley Medal (1922) of the Royal Society, the Bressa Prize (1910) of the Turin Academy of Science, the Albert Medal (1928) of the Royal Society of Arts, the Faraday Medal (1930) of the Institution of Electrical Engineers, the D.Sc. degree of the University of New Zealand, and honorary doctorates from the Universities of Pennsylvania, Wisconsin, McGill, Birmingham, Edinburgh, Melbourne, Yale, Glasgow, Giessen, Copenhagen, Cambridge, Dublin, Durham, Oxford, Liverpool, Toronto, Bristol, Cape Town, London and Leeds.  Rutherford married Mary Newton, only daughter of Arthur and Mary de Renzy Newton, in 1900. Their only child, Eileen, married the physicist R.H. Fowler. Rutherford’s chief recreations were golf and motoring.  He died in Cambridge on October 19, 1937. His ashes were buried in the nave of Westminster Abbey, just west of Sir Isaac Newton’s tomb and by that of Lord Kelvin.  \* Canterbury College (now Canterbury University) was located in Christchurch, but was administered from the University of New Zealand, Wellington. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0489 |
| **Interview** |  |
|  |  |
| **ID** | **0490** |
| **Biographical** | Eduard Buchner was born in Munich on May 20, 1860, the son of Dr. Ernst Buchner, Professor Extraordinary of Forensic Medicine and physician at the University, and Friederike *née* Martin.  He was originally destined for a commercial career but, after the early death of his father in 1872, his older brother Hans, ten years his senior, made it possible for him to take a more general education. He matriculated at the Grammar School in his birth-place and after a short period of study at the Munich Polytechnic in the chemical laboratory of E. Erlenmeyer senior, he started work in a preserve and canning factory, with which he later moved to Mombach on Mainz.  The problems of chemistry had greatly attracted him at the Polytechnic and in 1884 he turned afresh to new studies in pure science, mainly in chemistry with [Adolf von Baeyer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1905/index.html) and in botany with Professor C. von Naegeli at the Botanic Institute, Munich.  It was at the latter, where he studied under the special supervision of his brother Hans (who later became well-known as a bacteriologist), that his first publication, *Der Einfluss des Sauerstoffs auf Gärungen* (The influence of oxygen on fermentations) saw the light in 1885. In the course of his research in organic chemistry he received special assistance and stimulation from T. Curtius and H. von Pechmann, who were assistants in the laboratory in those days.  The Lamont Scholarship awarded by the Philosophical Faculty for three years made it possible for him to continue his studies.  After one term in Erlangen in the laboratory of Otto Fischer, where meanwhile Curtius had been appointed director of the analytical department, he took his doctor’s degree in the University of Munich in 1888. The following year saw his appointment as Assistant Lecturer in the organic laboratory of A. von Baeyer, and in 1891 Lecturer at the University.  By means of a special monetary grant from [von Baeyer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1905/index.html), it was possible for Buchner to establish a small laboratory for the chemistry of fermentation and to give lectures and perform experiments on chemical fermentations. In 1893 the first experiments were made on the rupture of yeast cells; but because the Board of the Laboratory was of the opinion that “nothing will be achieved by this” – the grinding of the yeast cells had already been described during the past 40 years, which latter statement was confirmed by accurate study of the literature – the studies on the contents of yeast cells were set aside for three years.  In the autumn of 1893 Buchner took over the supervision of the analytical department in T. Curtius’ laboratory in the University of Kiel and established himself there, being granted the title of Professor in 1895.  In 1896 he was called as Professor Extraordinary for Analytical and Pharmaceutical Chemistry in the chemical laboratory of H. von Pechmann at the University of Tübingen.  During the autumn vacation in the same year his researches into the contents of the yeast cell were successfully recommenced in the Hygienic Institute in Munich, where his brother was on the Board of Directors. He was now able to work on a larger scale as the necessary facilities and funds were available.  On January 9, 1897, it was possible to send his first paper, *Über alkoholische Gärung ohne Hefezellen* (On alcoholic fermentation without yeast cells), to the editors of the *Berichte der Deutschen Chemischen Gesellschaft*.  In October, 1898, he was appointed to the Chair of General Chemistry in the Agricultural College in Berlin and he also held lectureships on agricultural chemistry and agricultural chemical experiments as well as on the fermentation questions of the sugar industry. In order to obtain adequate assistance for scientific research, and to be able to fully train his assistants himself, he became habilitated at the University of Berlin in 1900.  In 1909 he was transferred to the University of Breslau and from there, in 1911, to Würzburg. The results of Buchner’s discoveries on the alcoholic fermentation of sugar were set forth in the book *Die Zymasegärung* (Zymosis), 1903, in collaboration with his brother Professor Hans Buchner and Martin Hahn. He was awarded the Nobel Prize in 1907 for his biochemical investigations and his discovery of non-cellular fermentation.  Buchner married Lotte Stahl in 1900. When serving as a major in a field hospital at Folkschani in Roumania, he was wounded on August 3, 1917. Of these wounds received in action at the front, he died on the 13th of the same month. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0490 |
| **Interview** |  |
|  |  |
| **ID** | **0491** |
| **Biographical** | Henri Moissan was born in Paris on September 28, 1852. His advanced education began in the Collège de Meaux and later in Edmond Frémy’s laboratory at the Musée d’Histoire Naturelle, where he attended lectures by E.H. Sainte-Claire Deville and Henri Debray. A year later, he removed to Dehérain’s laboratory in the École Pratique des Haute Études and subsequently he directed a small laboratory of his own before joining Debray and Troost in the laboratories of the Sorbonne. He was appointed to a junior position in the Agronomic Institute in 1879 and he gained his doctoral degree in 1880 with a thesis on the cyanogen series. He became assistant lecturer and senior demonstrator at the School of Pharmacy and in 1886 he was elected Professor of Toxicology. In 1899 he took the Chair of Inorganic Chemistry and in 1900 he was appointed Assessor to the Director of that School. In the same year, he succeeded Troost as Professor of Inorganic Chemistry, University of Paris.  Moissan’s first researches concerned the interchange of oxygen and carbon dioxide in the leaves of plants. He was soon to leave biology for the field of inorganic chemistry, where his early work was on the oxides of iron-group metals and chromium and a study of the chromous salts. In 1884 he turned his attention to fluorine chemistry, preparing some organic and phosphorus derivatives of that element. The following year he discovered that solutions of potassium fluoride in hydrogen fluoride at certain strengths remained liquid and conducted electrolytically at sub-zero temperatures: a year later he successfully electrolysed these solutions to isolate fluorine for the first time. He made a full study of the properties of the gas and its reactions with other elements.  In 1892, Moissan theorized that diamonds could be synthesized by crystallizing carbon under pressure from molten iron. He designed and developed the electric-arc furnace, attaining temperatures up to 3,500°C, to assist him in work which led to the production of tiny artificial stones. He subsequently used the furnace to volatilize many substances which had been regarded as infusible and to prepare many new compounds, particularly carbides, silicides and borides; in 1891 he discovered carborundum. His close study of the carbides and their reaction with water led to his theory that, in some cases, petroleum may be formed by subterraneous reaction between certain carbides and water. He prepared the hydrides of calcium, sodium and potassium and showed them to be non-conductive and, using the electric furnace, isolated many metals.  Moissan is credited with over three hundred publications, his greatest works being *Le Four Électrique* (The electric-arc furnace) (1897), *Le Fluor et ses Composés* (Fluorine and its compounds) (1900) and *Traité de Chimie Minerale* (Treatise on inorganic chemistry) (five volumes 1904-1906). He was an excellent lecturer and a meticulous and patient experimentalist.  Moissan, a Commandeur de la Légion d’Honneur, was elected member of the Académie de Médecine (1888), Académie des Sciences (1891), Conseil d’Hygiène de la Seine (1895 ), and the Comité Consultatif des Arts et Manufactures (1898). In 1887 he was awarded the Prix Lacaze; he was Davy medallist in 1896 and Hofmann medallist in 1903. He was honoured by the Franklin Institute of Philadelphia and awarded Fellowships of the Royal Society of London and The Chemical Society (London). He held honorary memberships of many other learned societies.  He died suddenly in Paris on February 20, 1907, shortly after his return from the prize-giving ceremony in Stockholm. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0491 |
| **Interview** |  |
|  |  |
| **ID** | **0492** |
| **Biographical** | Johann Friedrich Wilhelm Adolf von Baeyer was born on October 31, 1835, in Berlin, as the son of Johann Jakob Baeyer and Eugenie née Hitzig. He came from a family distinguished both in literature and the natural sciences. His father, a lieutenant-general, was the originator of the European system of geodetic measurement. Even as a child Baeyer was interested in chemical experiments and at the age of twelve found a new double salt of copper.  Baeyer devoted his first two years as a student at the University of Berlin (1853-1855) chiefly to physics and mathematics. By 1856, however, his old love for chemistry re-awakened and drew him to Bunsen’s laboratory in Heidelberg. His studies here on methyl chloride resulted in his first published work which came out in 1857. During the next year he worked in Kekulé’s private laboratory in Heidelberg and was associated with his ingenious structure theory. Baeyer’s life work was soon to bring this indeed most brilliant of chemical theories much resounding success. In 1858, in Berlin, he received his doctorate for his work on cacodyl compounds which had been done in Kekulé’s laboratory.  For the next year or two Baeyer was again working with Kekulé who had meanwhile become Professor at Ghent. A study of uric acid, which also led him to the discovery of barbituric acid, provided the thesis by which he qualified as a university teacher in 1860. In the same year he became a lecturer in organic chemistry at the “Gewerbe-Akademie” (Trade Academy) in Berlin. He received little money but was given a spacious laboratory. In 1866 the University of Berlin, at the suggestion of A.W. Hofmann, conferred on him a senior lectureship, which, however, was unpaid.  It was during the Berlin period that Baeyer began most of the work that was to bring him fame later. In 1865 he started his work on indigo – the blue dye had fascinated him since his youth-and this soon led to the discovery of indole and to the partial synthesis of indigotin. His pupils Graebe and Liebermann, with the help of the zinc-dust distillation developed by Baeyer, clarified the structure of alizarin and worked out the synthesis used industrially. Studies were initiated on condensation reactions which, after Baeyer had gone to Strassburg as Professor in the newly established University (1871) brought to light that important category of dyestuffs – the phthaleins. Baeyer’s theory of carbon-dioxide assimilation in formaldehyde also belongs to this period.  On the death of Justus von Liebig in 1873, Baeyer was called to his Chair in the University of Munich and there, over many years, built up an excellent new chemical laboratory. With his tenure at Munich came elegant total syntheses of indigo, as well as work on acetylene and polyacetylene, and from this derived the famous Baeyer strain theory of the carbon rings; there were studies of the constitution of benzene as well as comprehensive investigations into cyclic terpene. In this connexion the Baeyer-Villiger oxidation of ketones by means of per-acids was discovered. Especial interest was aroused theoretically by his work on organic peroxides and oxonium compounds and on the connexion between constitution and colour.  Von Baeyer’s work was at once pioneering and many-sided. With admirable penetration and extraordinary experimental skill he combined dogged perseverance and, even at 70 years old, a youthful buoyancy in his work. He was careful never to overestimate the value of a theory. While Kekulé sometimes approached Nature with preconceived opinions, von Baeyer would say: “I have never set up an experiment to see whether I was right, but to see how the materials behave”. Even in old age his views did not become fixed, and his mind remained open to new developments in chemical science.  Like Berzelius and Liebig, von Baeyer distinguished himself by forming a school which alone nurtured fifty future university teachers. Honours were heaped upon him, including the Nobel Prize for Chemistry in 1905. On his fiftieth birthday he was raised to the hereditary nobility.  Adolf von Baeyer married Adelheid (Lida) Bendemann in 1868. They had one daughter, who became the wife of the chemist Oskar Piloty, and two sons, both university lecturers, Hans in medicine at Munich, and Otto in physics at Berlin. He was still young in spirit when he succumbed to a seizure at his country house at Starnberger See on August 20, 1917. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0492 |
| **Interview** |  |
|  |  |
| **ID** | **0493** |
| **Biographical** | William Ramsay was born in Glasgow on October 2, 1852, the son of William Ramsay, C.E. and Catherine, née Robertson. He was a nephew of the geologist, Sir Andrew Ramsay.  Until 1870 he studied in his native town, following this with a period in Fittig’s laboratory at Tübingen until 1872. While there his thesis on orthotoluic acid and its derivatives earned him the degree of doctor of philosophy.  On his return to Scotland in 1872 he became assistant in chemistry at the Anderson College in Glasgow and two years later secured a similar position at the University there. In 1880 he was appointed Principal and Professor of Chemistry at University College, Bristol, and moved on in 1887 to the Chair of Inorganic Chemistry at University College, London, a post which he held until his retirement in 1913.  Ramsay’s earliest works were in the field of organic chemistry. Besides his doctor’s dissertation, about this period he published work on picoline and, in conjunction with Dobbie, on the decomposition products of the quinine alkaloids (1878-1879). From the commencement of the eighties he was chiefly active in physical chemistry, his many contributions to this branch of chemistry being mostly on stoichiometry and thermodynamics. To these must be added his investigations carried on with Sidney Young on evaporation and dissociation (1886-1889) and his work on solutions of metals (1889).  It was however in inorganic chemistry that his most celebrated discoveries were made. As early as 1885-1890 he published several notable papers on the oxides of nitrogen and followed those up with the discovery of argon, helium, neon, krypton, and xenon. Led to the conclusion by different paths and, at first, without working together, both [Lord Rayleigh](https://www.nobelprize.org/nobel_prizes/physics/laureates/1904/index.html) and Sir William Ramsay succeeded in proving that there must exist a previously unknown gas in the atmosphere. They subsequently worked in their separate laboratories on this problem but communicated the results of their labours almost daily. At the meeting of the British Association in August 1894, they announced the discovery of argon.  While seeking sources of argon in the mineral kingdom, Ramsay discovered helium in 1895. Guided by theoretical considerations founded on Mendeleev’s periodic system, he then methodically sought the missing links in the new group of elements and found neon, krypton, and xenon (1898).  Yet another discovery of Ramsay (in conjunction with [Soddy](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1921/index.html)), the importance of which it was impossible to foresee, was the detection of helium in the emanations of radium (1903).  With regard to the scientific honours which – besides the Nobel Prize have been awarded to Ramsay, mention can be made of a great number of honorary memberships, viz. of the Institut de France, the Royal Academies of Ireland, Berlin, Bohemia, The Netherlands, Rome, Petrograd, Turin, Roumania, Vienna, Norway and [Sweden](http://www.kva.se/en/); the Academies of Geneva, Frankfurt and Mexico; the German Chemical Society; the Royal Medical and Chirurgical Society of London; the Académie de Médecine de Paris; the Pharmaceutical Society, and the Philosophical Societies of Manchester, Philadelphia and Rotterdam. He also received the Davy and Longstaff Medals, honorary doctorate of Dublin University, the Barnardo Medal and a prize of $ 5,000 from the Smithsonian Institution, a prize of Fr. 25,000 from France (together with [Moissan](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1906/index.html)), and the A.W. Hoffmann Medal in gold (Berlin, 1903). He was created K.C.B.(Knight Commander of the Order of Bath) in 1902, and was also a Knight of the Prussian order “Pour le mérite”, Commander of the Crown of Italy, and Officer of the Legion d’Honneur of France.  In 1881 Ramsay married Margaret, the daughter of George Stevenson Buchanan. They had one son and one daughter. His recreations were languages and travelling.  Sir William Ramsay died at High Wycombe, Buckinghamshire, on July 23, 1916. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0493 |
| **Interview** |  |
|  |  |
| **ID** | **0494** |
| **Biographical** | Svante August Arrhenius was born on February 19, 1859, the son of Svante Gustaf Arrhenius and Carolina Christina Thunberg. His ancestors were farmers; his uncle became Professor of Botany and Rector of the Agricultural High School at Ultuna near Uppsala and later Secretary of The Swedish Academy of Agriculture. His father was a land surveyor employed by the University of Uppsala and in charge of its estates at Vik, where Svante was born. The family moved to Uppsala in 1860. The boy was educated at the Cathedral school where the rector was a good physics teacher. From an early age Svante had shown an aptitude for arithmetical calculations, and at school he was greatly interested in mathematics and physics. In 1876 he entered the University of Uppsala, studying mathematics, chemistry and physics. The practical instruction in physics was not of the best, and in 1881 he went to Stockholm to work under Professor E. Edlund at the Academy of Sciences.  Here, Arrhenius began by assisting Edlund in his work on electromotive force measurements in spark discharges but soon moved to an interest of his own. This resulted in his thesis (1884) *Recherches sur la conductibilité galvanique des électrolytes* (Investigations on the galvanic conductivity of electrolytes). From his results the author concluded that electrolytes, when dissolved in water, become to varying degrees split or dissociated into electrically opposite positive and negative ions. The degree to which this dissociation occurred depended above all on the nature of the substance and its concentration in the solution – being more developed the greater the dilution. The ions were supposed to be the carriers of the electric current, e.g. in electrolysis, but also of the chemical activity. The relation between the actual number of ions and their number at great dilution (when all the molecules were dissociated) gave a quantity of special interest (“activity constant”).  The idea of a connection between electricity and chemical affinity, once advocated by Berzelius, had, however, so completely vanished from the general consciousness of scientists that the value of Arrhenius’ publication was not well understood by the science faculty at Uppsala, where the dissertation took place. On the other hand, Otto Pettersson, Professor of Chemistry at Stockholms Högskola, emphasized the originality of the dissertation[\*](https://www.nobelprize.org/prizes/chemistry/1903/arrhenius/biographical/#fotnot), and [Wi. Ostwald](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1909/index.html) travelled to Uppsala to make the acquaintance of the young author. The fundamental importance of Arrhenius’ work was thus made clear, and at the end of 1884 he got a docentship at Uppsala in physical chemistry – the first in Sweden in this new branch of science. Through Edlund’s influence he was awarded a travelling fellowship from the Academy of Sciences which enabled him to work in 1886 with Ostwald in Riga and with Kohlrausch in Würzburg. In 1887 he was with Boltzmann in Graz and in 1888 he worked with [van ‘t Hoff](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1901/index.html) in Amsterdam. During these years Arrhenius was able to prove the influence of the electrolytic dissociation on the osmotic pressure, the lowering of the freezing point and increase of the boiling point of solutions containing electrolytes. Later on he studied its importance in connection with biological problems such as the relationship between toxins and antitoxins, serum therapy, its role for digestion and absorption as well as for the gastric and pancreatic juices. The paramount importance of the electrolytic dissociation theory is today universally acknowledged, even if certain modifications have been found necessary.  In 1891, Arrhenius declined a professorship offered to him from Giessen, Germany, and soon afterwards he obtained a lectureship in physics at Stockholms Högskola. In 1895 he became Professor of Physics there. He was in addition Rector from 1897 to 1905, when he retired from the professorship. He had got an invitation to a professorship in Berlin, and the Academy of Sciences then decided (1905) to start a Nobel Institute for Physical Chemistry with Arrhenius as its chief. Initially he had to work in a rented flat, but a new building was inaugurated in 1909. A large number of collaborators came to him from Sweden and from other countries, and helped to give his ideas wider currency.  In 1900 Arrhenius published his *Lärobok i teoretisk elektrokemi* (Textbook of theoretical electrochemistry), in 1906 followed *Theorien der Chemie* (Theories of Chemistry) and *Immunochemistry* and in 1918 the Silliman lectures *Theories of solutions*. He took a lively interest in various branches of physics, as illustrated by his theory of the importance of the CO2-content of the atmosphere for the climate, his discussion of the possibility that radiation pressure might enable the spreading of living spores through the universe (panspermy) and by his various contributions to our knowledge of the northern lights. In 1903 appeared his *Lehrbuch der kosmischen Physik* (Textbook of cosmic physics).  Many lectures and short publications gave witness of his interest and capacity for writing for the general public. Especially during the last decades of his life he published a number of popular books, which were usually translated into several languages and appeared in numerous editions. To these belong *Världarnas utveckling* (1906, Worlds in the Making), *Stjärnornas Öden* (1915, Destiny of the Stars) and others. In 1913 appeared *Smittkopporna och deras bekämpande* (Smallpox and its combating) and in 1919 *Kemien och det moderna livet* (Chemistry and modern Life).  Arrhenius was elected a Foreign member of the Royal Society in 1911, and was awarded the Society’s Davy medal and also the Faraday Medal of the Chemical Society (1914). Among the many tokens of distinction that he received were honorary degrees from the Universities of Birmingham, Cambridge, Edinburgh, Greifswald, Groningen, Heidelberg, Leipzig and Oxford.  Arrhenius was a contented man, happy in his work and in his family life. During the First World War, he made successful efforts to release and repatriate German and Austrian scientists who had been made prisoners of war.  He was twice married – in 1894 to Sofia Rudbeck, by whom he had one son, and in 1905 to Maria Johansson by whom he had one son and two daughters.  He died at Stockholm on October 2, 1927, and is buried at Uppsala.  \* The Stockholms Högskola (High School of Stockholm) corresponded in those days to a science faculty of the universities, but it was a private foundation and did not have the right to hold examinations for degrees or to judge the value of theses. It was not until 1960 that the University of Stockholm, with its many faculties, was established by the State. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0494 |
| **Interview** |  |
|  |  |
| **ID** | **0495** |
| **Biographical** | Hermann Emil Fischer was born on October 9, 1852, at Euskirchen, in the Cologne district. His father was a successful business man. After three years with a private tutor, Emil went to the local school and then spent two years at school at Wetzlar, and two more at Bonn where he passed his final examination in 1869 with great distinction. His father wished him to enter the family lumber business, but Emil wished to study the natural sciences, especially physics and, after an unsuccessful trial of Emil in the business, his father – who, according to the laureate’s autobiography, said that Emil was too stupid to be a business man and had better be a student – sent him in 1871 to the University of Bonn to study chemistry. There he attended the lectures of Kekulé, Engelbach and Zincke, and also those of August Kundt on physics, and of Paul Groth on mineralogy.  In 1872, however, Emil, who still wished to study physics, was persuaded by his cousin Otto Fischer, to go with him to the newly established University of Strasbourg, where Professor Rose was working on the Bunsen method of analysis. Here Fischer met [Adolf von Baeyer](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1905/index.html), under whose influence he finally decided to devote his life to chemistry. Studying under von Baeyer, Fischer worked on the phthalein dyes which Rose had discovered and in 1874 he took his Ph.D. at Strasbourg with a thesis on fluoresceine and orcin-phthalein. In the same year he was appointed assistant instructor at Strasbourg University and here he discovered the first hydrazine base, phenylhydrazine and demonstrated its relationship to hydrazobenzene and to a sulphonic acid described by Strecker and Römer. The discovery of phenylhydrazine, reputed to have been accidental, was related to much of Fischer’s later work.  In 1875 von Baeyer was asked to succeed Liebig at the University of Munich and Fischer went there with him to become an assistant in organic chemistry.  In 1878 Fischer qualified as a Privatdozent at Munich, where he was appointed Associate Professor of Analytical Chemistry in 1879. In the same year he was offered, but refused, the Chair of Chemistry at Aix-la-Chapelle. In 1881 he was appointed Professor of Chemistry at the University of Erlangen and in 1883 he was asked by the Badische Anilin- und Soda-Fabrik to direct its scientific laboratory. Fischer, however, whose father had now made him financially independent, preferred academic work.  In 1888 he was asked to become Professor of Chemistry at the University of Würzburg and here he remained until 1892, when he was asked to succeed A. W. Hofmann in the Chair of Chemistry at the University of Berlin. Here he remained until his death in 1919.  Fischer’s early discovery of phenylhydrazine and its influence on his later work have already been mentioned. While he was at Munich, Fisher continued to work on the hydrazines and, working there with his cousin Otto Fischer, who had followed him to Munich, he and Otto worked out a new theory of the constitution of the dyes derived from triphenylmethane, proving this by experimental work to be correct.  At Erlangen Fischer studied the active principles of tea, coffee and cocoa, namely, caffeine and theobromine, and established the constitution of a series of compounds in this field, eventually synthesizing them.  The work, however, on which Fischer’s fame chiefly rests, was his studies of the purines and the sugars. This work, carried out between 1882 and 1906 showed that various substances, little known at that time, such as adenine, xanthine, in vegetable substances, caffeine and, in animal excrete, uric acid and guanine, all belonged to one homogeneous family and could be derived from one another and that they corresponded to different hydroxyl and amino derivatives of the same fundamental system formed by a bicyclic nitrogenous structure into which the characteristic urea group entered. This parent substance, which at first he regarded as being hypothetical, he called purine in 1884, and he synthesized it in 1898. Numerous artificial derivatives, more or less analogous to the naturally-occurring substances, came from his laboratory between 1882 and 1896.  In 1884 Fischer began his great work on the sugars, which transformed the knowledge of these compounds and welded the new knowledge obtained into a coherent whole. Even before 1880 the aldehyde formula of glucose had been indicated, but Fischer established it by a series of transformations such as oxidation into aldonic acid and the action of phenylhydrazine which he had discovered and which made possible the formation of the phenylhydrazones and the osazones. By passage to a common osazone, he established the relation between glucose, fructose and mannose, which he discovered in 1888. In 1890, by epimerization between gluconic and mannonic acids, he established the stereochemical nature and isomery of the sugars, and between 1891 and 1894 he established the stereochemical configuration of all the known sugars and exactly foretold the possible isomers, by an ingenious application of the theory of the asymmetrical carbon atom of [Van’t Hoff](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1901/index.html) and Le Bel, published in 1874. Reciprocal syntheses between different hexoses by isomerization and then between pentoses, hexoses, and heptoses by reaction of degradation and synthesis proved the value of the systematics he had established. His greatest success was his synthesis of glucose, fructose and mannose in 1890, starting from glycerol.  This monumental work on the sugars, carried out between 1884 and 1894, was extended by other work, the most important being his studies of the glucosides.  Between 1899 and 1908 Fischer made his great contributions to knowledge of the proteins. He sought by analysis effective methods of separating and identifying the individual amino acids, discovering a new type of them, the cyclic amino acids: proline and oxyproline. He also studied the synthesis of proteins by obtaining the various amino acids in an optically-active form in order to unite them. He was able to establish the type of bond that would connect them together in chains, namely, the peptide bond, and by means of this he obtained the dipeptides and later the tripeptides and polypeptides. In 1901 he discovered, in collaboration with Fourneau, the synthesis of the dipeptide, glycyl-glycine and in that year he also published his work on the hydrolysis of casein. Amino acids occurring in nature were prepared in the laboratory and new ones were discovered. His synthesis of the oligopeptides culminated in an octodecapeptide, which had many characteristics of natural proteins. This and his subsequent work led to a better understanding of the proteins and laid the foundations for later studies of them.  In addition to his great work in the fields already mentioned, Fischer also studied the enzymes and the chemical substances in the lichens which he found during his frequent holidays in the Black Forest, and also substances used in tanning and, during the final years of his life, the fats.  Fischer was made a Prussian Geheimrat (Excellenz), and held honorary doctorates of the Universities of Christiania, Cambridge (England), Manchester and Brussels. He was also awarded the Prussian Order of Merit and the Maximilian Order for Arts and Sciences. In 1902 he was awarded the Nobel Prize in Chemistry for his work on sugar and purine syntheses.  At the age of 18, before he went to the University of Bonn, Fischer suffered from gastritis, which attacked him again towards the end of his tenure of the Chair at Erlangen and caused him to refuse a tempting offer to follow Victor Meyer at the Federal Technical University at Zurich and to take a year’s leave of absence before he went, in 1888, to Würzburg. Possibly this affliction was the forerunner of the cancer from which he died.  Throughout his life he was well served by his excellent memory, which enabled him, although he was not a naturally good speaker, to memorize manuscripts of lectures that he had written.  He was particularly happy at Würzburg where he enjoyed walks among the hills and he also made frequent visits to the Black Forest. His administrative work, especially when he went to Berlin, revealed him as a tenacious campaigner for the establishment of scientific foundations, not only in chemistry, but in other fields of work as well. His keen understanding of scientific problems, his intuition and love of truth and his insistence on experimental proof of hypotheses, marked him as one of the truly great scientists of all time.  In 1888 Fischer married Agnes Gerlach, daughter of J. von Gerlach, Professor of Anatomy at Erlangen. Unhappily his wife died seven years after their marriage. They had three sons, one of whom was killed in the First World War; another took his own life at the age of 25 as a result of compulsory military training. The third son, Hermann Otto Laurenz Fischer, who died in 1960, was Professor of Biochemistry in the University of California at Berkeley.  When Fischer died in 1919, the Emil Fischer Memorial Medal was instituted by the German Chemical Society. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0495 |
| **Interview** |  |
|  |  |
| **ID** | **0496** |
| **Biographical** | Jacobus Henricus van ‘t Hoff was born in Rotterdam, The Netherlands, on August 30, 1852. He was the third child in a family of seven children of Jacobus Henricus van ‘t Hoff, a physician, and Alida Jacoba Kolff.  In 1869 he entered the Polytechnic School at Delft and obtained his technology diploma in 1871. His decision to follow a purely scientific career, however, came soon afterwards during vacation-work at a sugar factory when he anticipated for himself a dreary profession as a technologist. After having spent a year at Leiden, mainly for mathematics, he went to Bonn to work with A.F. Kekulé from autumn 1872 to spring 1873; this period was followed by another in Paris with A. Wurtz, when he attended a large part of the curriculum for 1873-1874. He returned to Holland in 1874 and obtained his doctor’s degree that same year under E. Mulder in Utrecht.  In 1876 he became lecturer at the Veterinary College at Utrecht, but left this post for a similar position at the University of Amsterdam the following year. In 1878 came his appointment as Professor of Chemistry, Mineralogy, and Geology at the same university. After having occupied this chair for 18 years he accepted an invitation to go to Berlin as Honorary Professor, connected with a membership of the Royal Prussian Academy of Sciences. The principal reason for this change was the fact that he was overburdened with obligations to give elementary lectures and to examine large numbers of students, including even those for medical propaedeutics, leaving him with too little time to do his own research work. He was an ardent advocate for the creation of a special class of scientific workers. At his new post he remained till the end of his life.  van ‘t Hoff has acquired fame particularly by his epoch-making publications. His doctor’s thesis (1874) was entitled *Bijdrage tot de Kennis van Cyaanazijnzuren en Malonzuur* (Contribution to the knowledge of cyanoacetic acids and malonic acid). Of far greater weight, however, was his publication which appeared several months before: *Voorstel tot Uitbreiding der Tegenwoordige in de Scheikunde gebruikte Structuurformules in de Ruimte*, etc. (Proposal for the development of 3-dimensional chemical structural formulae). This small pamphlet, consisting of twelve pages text and one page diagrams, gave the impetus to the development of stereochemistry. The concept of the “asymmetrical carbon atom”, dealt with in this publication, supplied an explanation of the occurrence of numerous isomers, inexplicable by means of the then current structural formulae. At the same time he pointed out the existence of relationship between optical activity and the presence of an asymmetrical carbon atom.  His revolutionary ideas, however, only found acceptance after the publication, in 1875, of his *Chimie dans l’Espace*; especially when two years later the German translation appeared, with an introduction by J. Wislicenus. (The English translation: *Chemistry in Space* did not appear until 1891.) In his *Dix Années dans l’Histoire d’une Théorie* (Ten years in the history of a theory) he drew attention to the fact that J.A. Le Bel had independently arrived at the same ideas, though in a more abstract form.  In 1884 his book *Études de Dynamique chimique* (Studies in dynamic chemistry) appeared, in which he entered for the first time the field of physical chemistry. Of great importance was his development of the general thermodynamic relationship between the heat of conversion and the displacement of the equilibrium as a result of temperature variation. At constant volume, the equilibrium in a system will tend to shift in such a direction as to oppose the temperature change which is imposed upon the system. Thus, lowering the temperature results in heat development while increasing the temperature results in heat absorption. This principle of mobile equilibrium was subsequently (1885) put in a general form by Le Chatelier, who extended the principle to include compensation, by change of volume, for imposed pressure changes – it is now known as the van ‘t Hoff-Le Chatelier principle.  The following year, in 1885, followed *L’Équilibre chimique dans les Systèmes gazeux ou dissous à I’État dilué* (Chemical equilibria in gaseous systems or strongly diluted solutions), which dealt with this theory of dilute solutions. Here he demonstrated that the “osmotic pressure” in solutions which are sufficiently dilute is proportionate to the concentration and the absolute temperature so that this pressure can be represented by a formula which only deviates from the formula for gas pressure by a coefficient *i*. He also determined the value of *i* by various methods, for example by means of the vapour pressure and Raoult’s results on the lowering of the freezing point. Thus van ‘t Hoff was able to prove that thermodynamic laws are not only valid for gases, but also for dilute solutions. His pressure laws, given general validity by the electrolytic dissociation theory of [Arrhenius](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1903/index.html) (1884-1887) – the first foreigner who came to work with him in Amsterdam (1888) – are considered the most comprehensive and important in the realm of natural sciences.  During his Berlin period he was from 1896 to 1905 continuously engaged on the problem of the origin of oceanic deposits, with special reference to those formed at Stassfurt. In this extensive work he was especially assisted by W. Meyerhoffer, who had previously worked with him in Amsterdam for a number of years. He was probably the first to apply small-scale results, obtained in the laboratory, to phenomena occurring on a large scale in Nature. The results of this ambitious investigation, mostly published in the Proceedings of the Prussian Academy of Sciences, were summarized by him in a two-volumes work *Zur Bildung ozeanischer Salzablagerungen,* 1905-1909.  van ‘t Hoff greatly valued the power of imagination in scientific work, as is apparent from his inaugural address on taking up his professorship in Amsterdam: *Verbeeldingskracht in de Wetenschap* (The power of imagination in Science), in which, after a rather elaborate study of biographies, he arrived at the conclusion that the most prominent scientists have possessed this quality in a high degree. [Wilhelm Ostwald](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1909/index.html), who together with him established the *Zeitschrift für physikalische Chemie* in Leipzig, and can be regarded as founders of physical chemistry.  Of the numerous distinctions he himself mentioned the award of the first Nobel Prize in Chemistry (1901) to him as the culmination-point of his career. In 1885 he was appointed member of the Royal Netherlands Academy of Sciences, after his nomination had been withheld in 1880 because of an insufficient number of votes – a proof that his ideas initially found little acceptance in his own country. Among his other distinctions were the honorary doctorates of Harvard and Yale (1901), Victoria University, Manchester(1903), Heidelberg (1908); the Davy Medal of the Royal Society (1893), Helmholtz Medal of the Prussian Academy of Sciences (1911); he was also appointed Chevalier de la Legion d’Honneur (1894), Senator der Kaiser-Wilhelm-Gesellschaft (1911). He was also member or honorary member of the Chemical Society, London (1898), Royal Academy of Sciences, Gottingen (1892), American Chemical Society (1898), Académie des Sciences, Paris (1905).  van ‘t Hoff was a lover of nature; as a student in Leyden he frequently took part in the botanical excursions, and later in Bonn he fully enjoyed the mountains in the vicinity, taking long walks in company or alone. His quite detailed description of his journey to the United States, resulting from an invitation to lecture at Chicago University, amply shows his love of travel. His receptiveness for philosophy and his predilection for poetry were already apparent in his early school years – Lord Byron was his idol.  In 1878 he married Johanna Francina Mees. They had two daughters, Johanna Francina (b. 1880) and Aleida Jacoba (b. 1882) and two sons, Jacobus Hendricus (b. 1883) and Govert Jacob (b. 1889).  van ‘t Hoff died on March 1, 1911, at Steglitz near Berlin. |
| **Autobiographical** |  |
| **Podcast** |  |
| **Telephone**  **interview** | 0496 |
| **Interview** |  |
|  |  |
|  |  |
|  |  |
|  |  |
|  |  |
|  |  |
|  |  |
|  |  |
|  |  |
|  |  |
|  |  |
|  |  |
|  |  |
|  |  |