Role Models in Chemistry

Linus Pauling

by Balazs Hargittai and István Hargittai

Linus Pauling (1901–1994) was one of the greatest scientists of the twentieth century. He received two unshared Nobel Prizes. The first was in 1954 in chemistry "for his research into the nature of the chemical bond and its application to the elucidation of the structure of complex substances." Less than 10 years later, he was awarded the Nobel Peace Prize 1962.

In 1984, Clarence and Jane Larson recorded an interview with Pauling as part of an extensive interview project with over 60 famous scientists. Clarence Larson (1909–1999) was a former chemistry professor who participated in the Manhattan Project, researched isotope separation, and served as commissioner of the U.S. Atomic Energy Commission (1969–1974). After his death, his widow Jane donated their tapes to us and encouraged us to disseminate the information they contained. With this article, we pay tribute to Linus Pauling on the tenth anniversary of his death by quoting some excerpts from that interview.

Quotes from Linus Pauling from his 1984 Interview

When I was 10 or 11, I became interested in insects, and I got books from the library about insects. When I was 12, I got interested in minerals, and again got books from the library, and I made tables for my own use. I made some efforts to collect some minerals, not very successfully because I didn't have transportation, and our valley was not an especially good place for finding minerals. Then, when I was 13, in my second year of high school, a boy of my own age, Lloyd Jeffress, said to me as we were walking home one day, "Would you like to see some chemical experiments?" I said yes, and he said, "Come on in," and I went to his home. He was an only child, and he carried out some experiments, which impressed me immensely. I became very enthusiastic about chemistry. That same day, I found a book that had belonged to my father about elementary chemistry, and I immediately repeated some experiments with materials around the house. And from there on I was a chemist.

. . . When we were 15, my grandmother in Oswego [Oregon] said to me, "What would you like to be when you grow up?" I said, "I'm going to be a chemical engineer," but Lloyd [Jeffress] immediately said, "No, he is going to be a professor."

I studied chemical engineering at Oregon State [College]. . . because not having any money, it was the cheapest school for me to go. There was Reed College only a couple of miles from where my mother lived, but I knew you had to pay tuition there and it didn't seem that there was much chance for me to go there. Also, I didn't know that there was any profession that would involve chemistry, except chemical engineering. At that time, 65 years ago, chemical engineering was to a much greater extent taught in a practical way. The first two years the chemical engineering students

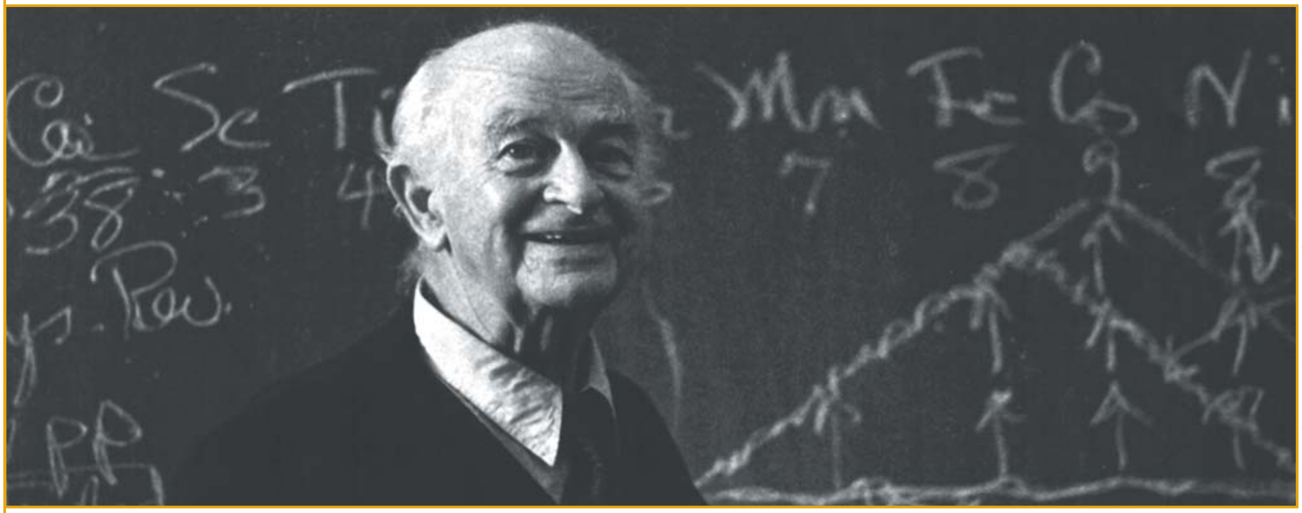
Linus Pauling lecturing at Moscow State University in 1983.

Photo by and courtesy of Larissa A. Zassourskaya, Moscow were combined with the mining engineering students. I had four years of mathematics at Washington High School [Portland, Oregon] and time went by without me getting additional training in mathematics. So I got some instruction in mining engineering, blacksmithing, and making of drills, too. After my sophomore year, I was working as a paving plant inspector in the summer in southern Oregon.

. . . I was 18 years old in 1919. A very interesting event occurred during this year when I was teaching. I had a desk in the chemistry library. No one else came into the chemistry library, but the journals arrived and I read. I had a little spare time despite the heavy teaching load and I read the journals. The *Journal of the American Chemical Society* came with a couple of articles by Irving Langmuir on the shared electron pair theory of the chemical bond. He referred back to 1916, to G.N. Lewis, so I got out the 1916 copy of the journal with G.N. Lewis's paper, and I gave a seminar on chemical bond theory of the shared electron pair. It was the only seminar that was given that year. A chemistry seminar was not a very common thing, and I continued to be interested in the chemical bond ever since.

> *. . . I have been very fortunate during my life in that several times something has happened that, in retrospect, I see, turned out to have been just the right thing to have happened.*

. . . I have been very fortunate during my life in that several times something has happened that, in retrospect, I see, turned out to have been just the right thing to have happened. For me to have gone to [Caltech in] Pasadena [California] in 1922 was really most fortunate. I don't believe I could have gotten better training or to work under better circumstances anywhere in the world than there, in Pasadena. . . . There were remarkable teachers in Pasadena and it was a small place [back in 1922], a total of 300 undergraduate students, 30 or 40 graduate students, and 50 faculty members. The man with whom I did my doctoral work, Roscoe Gilkey Dickinson, was the first person to get a Ph.D. from the California Institute of Technology. He got it in 1920; then there were a couple every year until 1925 when quite a number got it in physics and chemistry.

. . . My first scientific paper was published in 1923 on a crystal structure. By 1925, I was publishing papers on the old quantum theory. Richard C. Tolman and I published a paper in 1925 on the entropy of crystals and supercooled liquids; this was a publication in quantum mechanics.

. . . My work with nucleic acids came about through the natural outgrowth of my interests in molecular structure. . . . I thought here is an interesting substance—hemoglobin. I didn't know much about biology, but I knew about hemoglobin. It had been found a few years earlier, in 1927. The molecule contains four iron atoms in the heme groups. I [had] heard about [the] sigmoid equilibrium curve of oxygen (O_2), so I applied physical chemistry and structural chemistry to that [and] I worked out a theory of the oxygen equilibrium curve. That was my first paper on proteins.

Then I thought, nobody knows how the oxygen molecules stick to the hemoglobin molecules. Some people say it is sort of an adsorption onto this large molecule. Other people say there is a chemical bond formed. Oxygen has two unpaired electrons, it is paramagnetic. You can pick up liquid oxygen by a magnet–liquid oxygen will hang between the poles of the magnet. I knew that. I knew that G.N. Lewis, back in the 1920s, interpreted measurements of the magnetic susceptibility of solutions of liquid oxygen and liquid nitrogen to show that there is an equilibrium between the paramagnetic O_2 and diamagnetic O_4. He had determined the equilibrium constant, the standard free energy and standard free enthalpy of the reaction. Very clever of G.N. Lewis to have done that. He discovered O_4, the dimer of O_2. So, I thought, why don't we measure the magnetic susceptibility of oxy-hemoglobin? It will be paramagnetic due to the oxygen molecules or at least there will be a paramagnetic component.

. . . I had a student, Charles Coryell, he had [received] his Ph.D. and came to me as a postdoc fellow. He and I set up an apparatus, got some blood, and measured oxy-hemoglobin. It was diamagnetic, which showed that you had chemical bonding, but the hemoglobin without the oxygen was strongly paramagnetic, and I hadn't predicted that. This was one of those rare occasions when something has come along due to an experiment that I carried out that was a surprise to me. But the change in the magnetic properties of the iron atom permitted us to gain great insight into the arrangement of the other atoms around the iron atoms in hemoglobin. Moreover, this technique of measuring magnetic susceptibility permitted us to measure equilibrium constants and rates of reactions, so over the next five years my students and I published 15 to 20 papers on hemoglobin and hemoglobin derivatives. The method was also then used in Sweden to study heme compounds and iron proteins.

Then I thought, what about the rest of the hemoglobin molecule? [William] Astbury in England was making X-ray diffraction photographs of hair and finger nails. And other people, too, starting in Japan and Germany, had made photographs of silk and wool. I took some of these photographs in 1937 and tried then to find the structure in way of coiling the polypeptide chain. Other people were trying, too, but without success.

I thought, "I know a lot about these atoms and how they combine with one another, but the structures that I have been predicting don't seem to be the right ones, so there must be something that I don't know about proteins." Nobody has ever determined the structure of an amino acid or a dipeptide, a simple peptide. So why don't we go ahead and do that. The Rockefeller Foundation gave us money and Robert Corey had just come that summer, in 1937, to work with me. I talked with him about this problem, which interested him. We decided to go ahead, and for 10 years at our institute, with a good number of different people involved in it, we determined these structures for about 10 amino acids and several simple peptides. Nobody else in the whole world had turned out a single structure for any of these fundamental substances during this whole period.

Later on my wife said to me "If that was such an important problem, why didn't you work harder at it?"

. . . Ten years later when I was an Eastman professor at Oxford, I thought, "I better think about that problem again. I failed in 1937, and here it is in 1948." . . . There was nothing surprising about the amino acids or the simple peptides. They all had just the structures that I had designed to them back in 1937, but I thought I would try again and I would forget about the X-ray diffraction photographs. First, I [didn't] have them [there]. But they weren't any good anyway—these fiber-diagrams [Pauling points to his hair]. Second, I'll just forget about them.

Suppose, I assume the residues are equivalent to one-another. Back in 1928 I had written a paper about structural principles involving silicates and such substances. One of the principles was that the different kinds of units are to be as few as possible in number. So I'll assume that all the amino acids in the polypeptide chain are equivalent. In a course that I had from Bateman in 1927, it was shown that the most general symmetry operation that converts an asymmetric object into an identical object is rotation around some line in space coupled with translation along it. If you repeat this operation you get a helix. So I said that I haven't looked at any helical structures; I know other people have. I am not sure if I knew that then, but other people have looked at the helical structures for the polypeptide chains, but haven't found them. So I'll look at them. I took a sheet of paper, made a sketch on it, then folded the paper to get those bond angles of the α-carbon correct, and kept folding it parallel, until it came around again and I tried to form a hydrogen bond from this turn to the next turn and couldn't do it. I tried again, putting the folds in a different way, and finally got this hydrogen bond. And that was the α-helix.

So I predicted the properties of this α-helix and the X-ray diagram. This showed the repeat in 5.4 Å. Actually that was the pitch of this helix, 5.4 Å . The X-ray diagram showed 5.1 Å and there you have about 5% error and I couldn't see how that was possible. I waited more than a year before publishing anything about it, and in 1950, a paper was published in the *Proceedings of the Royal Society* by Bragg, Kendrew, and Perutz on the structure of the polypeptide chain of α-keratin. They described about 20 structures, all of which were wrong. I said to Corey that we better publish about the α-helix and the γ-helix, so we sent off a short note to be printed and started writing a longer paper. . . .

. . . And then, of course, I thought that I would work out the structure of DNA and started to work on it, rather desultorily, I suppose. Later on my wife said to me "If that was such an important problem, why didn't you work harder at it?" . . .

The full interview will be published in *Candid Science V: Conversations with Famous Scientists*, written by B. Hargittai and I. Hargittai (Imperial College Press, London, 2005).

www.icpress.co.uk/books/popsci/p366.html

Dr. Balazs Hargittai is at St. Francis University in Loretto, Pennsylvania, and Dr. István Hargittai is at the Budapest University of Technology and Economics.