István Hargittai

CANDID SCIENCE

Conversations with Famous Chemists

与化学家同行

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Edited by Magdolna Hargittai

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Conversations with Famous Chemists

Foreword

This collection of interviews by Professor Hargittai is a timely celebration of the remarkable century of chemistry, which has just passed. Those interviewed are some forty chemists, who were fortunate enough to live during the momentous developments in chemistry, which have occurred during the last decades of the millennium.

These historic events will be retold for centuries to come, often with more understanding and appreciation than we can have today. But, in one respect, what is written here will never be repeated because it is the personal experiences of the chemists who took part, each of them a unique individual.

Scientists are sometimes seen as automata without human feeling or sentiment (unlike artists for example!). The scientists themselves, and those who live with scientists, know how false this view is. But, unfortunately, most people know very little of science or scientists personally and often see them as living in a different world from "ordinary" people.

I hope that the interviews recorded here will do something to dispel this increasing divide in our understanding. They tell how chemists of today have lived and worked, why they chose chemistry as a career and how they were led to make the discoveries for which they are best known. The informality of the interviews gives the impression that one is sharing in a conversation rather than listening to an autobiographical lecture.

Children are born scientists but, if they are to retain their love of science, they need role models to follow and encouragement from those who have been successful. This book will be enjoyed by all who have some interest in science and it will be of special value to the young people whom it may encourage to follow those, whose stories are told here.

London George Porter

Preface

At the dawn of the twenty-first century and the new millennium, it is fascinating to review the achievements of chemistry during the past decades of the twentieth century, so extraordinary in scientific progress. It is also fascinating to get acquainted with the stories behind these achievements directly from some of their most important participants. The subject matter in this collection of interviews covers a broad range of chemistry, with emphasis on the following loosely identified areas: structural chemistry, medicinal chemistry, natural products chemistry, stereochemistry, theoretical and computational chemistry, inorganic chemistry, physical organic chemistry, NMR spectroscopy, fullerenes, kinetics and reaction mechanisms, atmospheric chemistry. There is, of course, considerable overlap between these areas, and even more so in the research activities of most of the scientists interviewed. There are also important missing fields. Future collections, already under preparation, will considerably expand the scope of this volume.

This volume contains the first selection of my conversations with famous scientists. In addition to the interviews, we present here a few auxiliary entries. They include a brief chapter on Odd Hassel to whom I never posed questions in the way I did to the others, but my interactions with him made me think about him as one of my interviewees. I prepared two auxiliary entries of quotations by Erwin Chargaff and John Cornforth to augment their interviews. I did this because I wanted to share the intellectual pleasures I have experienced from their writings. Another entry augments the brief interview with Linus Pauling. It is about the controversy of the resonance theory in the Soviet Union at the beginning of the 1950s. I attended the Pauling memorial session of the ACS meeting in 1994 in Anaheim and was surprised by the scarce knowledge of this story among our colleagues who have been interested in Linus Pauling's life and works. Yet another entry is on the beginnings of the multiple metal-metal bond studies. The relevant systems were produced and examined by scientists in Moscow, although the multiple bonds in them were not recognized until Albert Cotton's studies. I compiled a brief segment on Buckminster Fuller because of his conspicuous, if indirect, role in the fullerene story. This was greatly helped by a conversation I had recorded with Ed Applewhite, Fuller's assistant in creating his opus magnum, *Synergetics*. Finally, there is a brief entry on Paul de Kruif because his book, *Microbe Hunters*, was at least as important as the chemistry set in turning interested children's attention to chemistry for the generations that are so prominently represented among my interviewees.

Although my first conversation with a famous chemist, Nikolai Semenov, was recorded back in 1965, launching the quarterly magazine, *The Chemical Intelligencer* by Springer-Verlag, New York in 1995 gave a great incentive for these conversations. I am grateful to Springer-Verlag for their gracious permission to reproduce published material here from *The Chemical Intelligencer*. Some interviews are reproduced in full, others with some changes, saving some space or extending the description of the circumstances of the interview. Quite a few of the interviews are presented for the first time in this volume. The relevant bibliographical data are given in each respective entry.

The general procedure of preparing an interview is the following. I contact the interviewee, we set up a date for our meeting, and have a fairly loose conversation recorded on audiotape. I then prepare and slightly edit the transcripts and send them to the interviewee for correction, deletion, and addition. This procedure is repeated until the interviewee feels absolutely comfortable with the material. Before publication, there is usually some additional light copyediting. The conversations are illustrated with snapshots made during the conversation and by photos provided by the interviewee. In a few cases, the interview was by correspondence.

In all the conversations I am trying to bring out some important aspects of chemistry as well as to learn about the life and thoughts of the interviewee. Since I am a fellow scientist rather than an investigative reporter, I never try to go after a particular problem with which the interviewee seems uncomfortable. From the start of our conversation I am asking my interviewee to ignore any of my questions that he or she does not care to discuss.

Thus the reader may at places feel that I should have pressed for more answers or more details and would never learn about my unanswered questions: I may and then, I may not have asked that particular question. My approach has limitations in this respect. On the other hand, I see benefit in my approach in that the interviewee may open up more to a friendly colleague than to an investigative reporter. In any case, the purpose of the interviews was to learn about chemistry and bring great scientists in human proximity. I have myself learned a great deal from these conversations, in terms of both chemistry and human behavior and personal philosophy. I am grateful to all my interviewees for their time, their contribution, and for their interest in and support for the project.

This book was edited by Magdolna Hargittai, herself a chemistry Ph.D. and Research Professor, who is my partner in my ventures in chemistry and all other aspects of my life, and who often knows my emphases and preferences before I could even articulate them.

For my doctoral work I did not have a supervisor, not even in a formal way. I never liked authority. Yet I always felt blessed when I had a good teacher and somehow I always had one at the right moment, although not necessarily in physical proximity. Having good teachers is the greatest thing that can happen in one's career. A good teacher need not be appointed to be your teacher; a good teacher is a good example, and that may come in a great variety of ways. I feel that my interviews have given me another experience of benefiting from good teaching, and my greatest hope is that they will be received as such by my readers, too. It is in this sense that I would like to dedicate this volume to the coming generations of students, for whom much of the material presented here will be science history.

Budapest, Fall of 1998 István Hargittai

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Linus Pauling (photo courtesy of the late Linus Pauling).

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Linus Pauling

The life and work of Linus Pauling (1901–1994) have been documented in great detail. His fame and broad interests are expressed by his being unique in having received two unshared Nobel Prizes. He received one in chemistry in 1954 "for his research into the nature of the chemical bond and its application to the elucidation of the structure of complex molecules." He then received the Nobel Peace Prize for 1962, by the decision of the Norwegian Parliament in 1963.

From time to time I had correspondence with Linus Pauling on a variety of issues, including symmetry and quasicrystals. I met him in person in the early 1980s at the University of Oslo. He came for a brief visit at the invitation of the rector of the University who was also my host and long-time friend, Otto Bastiansen. Pauling gave a lecture to a packed auditorium on structural chemistry, and in particular he discussed at length the hybridization of transition metals, a topic he had published on extensively. During the lecture he was deriving complicated expressions without as much as a scrap of paper. He was marching back and forth in front of a very long blackboard, and he covered it with his formulas. It was impressive and I felt sorry that the blackboard was to be erased after the lecture. Pauling kept his enthusiastic Norwegian audience in awe and he was visibly enjoying it. Only gradually did it dawn on me that the sophisticated derivations were superfluous to the understanding of the subject matter, that he could have just as well explained what he wanted to teach us with words. In any case, this was a rare exhausting lecture that I did not wish to end after one hour. During the luncheon following the talk, Pauling stayed fresher and more alert than any of us, he discussed disarmament and teaching, made publicity for his *General Chemistry* co-authored with his son Peter, and asked us about our research, too.

As I was preparing to launch *The Chemical Intelligencer*, I had some correspondence about it with Linus Pauling, in the fall of 1993. He was very enthusiastic about a magazine devoted to the culture of chemistry. This would have sufficed for support, but he added that his other works prevented him from writing a contribution for the magazine. This gave me the idea of posing some questions to him that would not need too much of an effort to answer. I suspected that he was rather ill by then.

Given that his views had been expressed copiously about a wide range of issues, I decided to focus on four questions that I personally found most interesting. They referred to the long-standing validity of his teachings about structural chemistry; the controversies his ideas on resonance and electronegativity had generated in the Soviet Union; the discoveries of quasicrystals and fullerenes; and the ever growing importance of computations in chemical research.

In addition to what he said, I am quoting two paragraphs from his paper that he referred to in his answer to my last question. I am also giving a brief account of the 1951 Moscow meeting on the resonance theory followed by a few comments in the next chapter. Regarding quasicrystals, I had some correspondence with Pauling at the time of his contributing an article to one of my edited symmetry books. The article had a very telling title, "Interpretation of So-called Icosahedral and Decagonal Quasicrystals of Alloys Showing Apparent Icosahedral Symmetry Elements as Twins of an 820-Atom Cubic Crystal".[1] I was curious whether he had changed his rather rigid opposition to the notion of quasicrystals and, as it turned out, he did not. Since then, *The Chemical Intelligencer* has carried a set of interviews with the major players of that discovery.[2] As for the fullerenes, there is a set of interviews in this volume with the major participants of their discovery, including those who had predicted the stability of C_{60}. Apparently, Pauling was not aware of such predictions.

Now the questions and answers:*

*This interview was originally published in *The Chemical Intelligencer* **1995**, *1*(1), 5

It has been estimated recently[3] *that at the time of the first edition of your* The Nature of the Chemical Bond, *only about 0.01% of today's structural information was available. Nevertheless, your observations and conclusions concerning molecular structure and bonding have withstood the test of time. Would you care to comment on this and, in particular, on the value of comparative methods in chemistry?*

I think that the structural information available in 1939, when my book *The Nature of the Chemical Bond* was first published, was enough to permit reliable general conclusions about chemical bonds to be made, and I am not surprised that the generalizations have withstood the test of time. I think that my own crystal-structure work and that of R. W. G. Wyckoff and Roscoe G. Dickinson involved a good selection of crystals to answer questions that at that time needed to be answered. Also, our electron diffraction work in the 1930s involved many simple molecules of such a sort as to permit further general conclusions to be drawn.

In the early fifties, at the time of your difficulties with the U.S. State Department, because of your allegedly leftist views, you were also sharply criticized in the Soviet Union because of the resonance theory, which was found to be ideologically hostile. How closely did you follow the debates in the Soviet Union? What was your reaction about a decade later to the nonacceptance of the electronegativity concept by official Soviet chemistry?

It took several years, from about 1949 to 1955, for the chemists in the Soviet Union to get a proper understanding of the resonance theory. I may say that I do not know about the nonacceptance of the electronegativity concept in official Soviet chemistry.

Recent discoveries such as the quasicrystals and the fullerenes seem to have caught the solid state and chemical communities by surprise. Were these greatly exceptional events or should we be getting prepared to seeing more of these kinds of unexpected findings in the future?

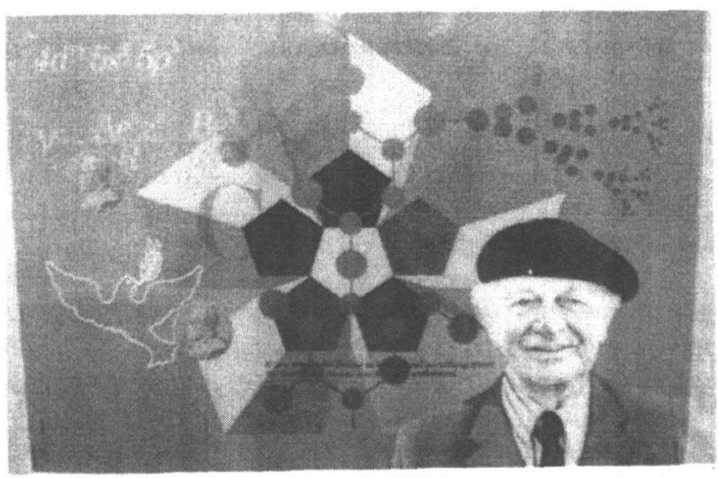
I am rather surprised that no one had predicted the stability of C_{60}. I might have done so, especially since I knew about the 60-atom structure with icosahedral symmetry, which occurs in intermetallic compounds. It seems to be difficult for people to formulate new ideas. An example is that from 1873 to 1914 nobody, knowing about the tetrahedral nature of the bonds of the carbon atom, predicted that diamond has the diamond structure.

As to the quasicrystals, you know that I contend that icosahedral quasicrystals are icosahedral twins of cubic crystals containing very large icosahedral complexes of atoms. It is not surprising that these crystals exist. The first one to be discovered was the MgZnAl compound reported by my associates and me in 1952. We did not observe quasicrystals of this compound, but they have been observed since then.

According to some, the ever improving computational techniques are overtaking the physical experimental techniques, at least in structural chemistry. What's your expectation of the near future in this respect?

I do not think that quantum mechanical calculations of molecular structure or crystal structure will ever make the sort of chemical arguments about structure presented in my book obsolete. The quantum mechanical calculations are made for one substance, and perhaps then for another somewhat similar substance. My arguments about the chemical structure are very general and pertain to all molecules and crystals.

Simple quantum mechanical calculations have great value, as pointed out in the early chapters of my book and in a paper that I published in *Foundations of Physics* a couple of years ago.



Poster on the wall of the Linus Pauling Institute of Science and Medicine in Palo Alto, California. I gave a seminar at the Institute on February 28, Linus Pauling's birthday, in 1996, not long before the Institute was dissolved. The symbolism of the poster refers to Pauling's two Nobel Prizes and displays conspicuously a pentagonal structure. Both the quasicrystal and fullerene discoveries were intimately related to pentagonal symmetry.

Two paragraphs are quoted here from Linus Pauling's paper that he referred to in his response to the last question. The paper was titled "The Value of Rough Quantum Mechanical Calculations."[4]

"In thinking about the history of science in the period around 60 years ago, I have come to the conclusion that much of the progress was the result of carrying out approximate quantum mechanical calculations. It is my impression that in recent years the effort has been made to carry out quantum mechanical calculations that are as quantitatively accurate as possible. Instead of making calculations of energy levels and other properties of a system with use of a simple approximate wave function corresponding to some simple model, the effort of many physicists is to formulate as complicated a wave function as can be handled by the computers. I remember reading a paper in which the author reported that his wave function contained a million terms. There is little doubt that such calculations will give results in agreement with experiment. With a complicated wave function, however, it is essentially impossible to formulate an interpretation in terms of a model of the system." (p. 830)

...

"Chemistry has probably been fortunate in that a great amount of empirical knowledge about the properties of chemical substances had been obtained before the development of quantum mechanics. Chemists strove to understand these properties, and as a result the classical structure theory of chemistry was developed. If the accumulation of large amounts of information about the properties of substances had not been gathered before quantum mechanics was formulated, it may well be that chemical structure theory would not have developed. In fact, at the present time, in 1991, little use is made by chemists of quantum mechanics, except to the extent that the principles of chemical bond formation that were formulated on the basis of quantum mechanical principles are extensively used. Some use is made by chemists of accurate quantum mechanical treatments of molecular structure, but much more use is made of the chemical structure-theory model." (p. 834)

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