Candid Chemistry

by István Hargittai

Interviews with eminent scientists often provide for excellent oral history, a more engaging way to learn about science. As science has become increasingly impersonal, interviews allow readers to become personally acquainted with great scientists. Also, they provide a forum for these scientists to profess their opinions on various issues. Current chemical literature is often so terse and journal space is at such a premium that it is often impossible for authors to describe their unsuccessful attempts on the way to their discoveries. However, in an unhurried conversation, scientists are willing to reminisce about their failures as well as their successes. With this article, I want to share some of the experiences that I have gathered while conducting interviews.

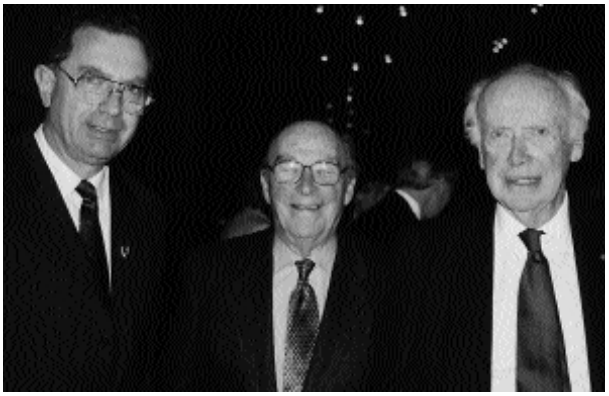
There is a plethora of questions that one may ask of great scientists. In addition to asking them about their family background, education, and their most important achievements in science, many other questions, such as the following come up: What turned them to science? What was the determining factor in their success? Did they recognize the importance of their discovery right away? Was it easy to publish their groundbreaking findings? And so on. I have found most scientists communicative with a few exceptions. In most cases, once we immerse ourselves into the conversation, I have to ask few questions to get a lot of information. In some other cases, a little prodding is needed. These are in-depth interviews, sometimes going on for hours.

A scientist is willing to reveal more about his inner self to an understanding colleague than to an aggressive investigating reporter.

My technique of interviewing is the following. I contact the scientist informing him (or her) that I will be in his neighborhood because of a conference or some other reason and would like to record a conversation with him. If we can arrange a meeting, we record a conversation. I live in Budapest, so this is almost always connected with a trip. Before the interview, I try to do as much homework as possible in my preparation for the interview. Usually, the more I know in advance, the more new information I can get during the encounter. However, for the sake of my future readers, I must try not to boast about my knowledge. To me the best interviewer is almost invisible. I would like to stress that no two interviews and no two sets of questions are ever the same. There are exceptional cases, when there is no possibility for any preparation.

In 1994, while I was attending an American Chemical Society meeting in Anaheim, California, I came across Glenn Seaborg, who was taking a leisurely walk alone. I introduced myself and asked him for an interview. The only available time was right then. Fortunately, my camera and miniature Dictaphone were with me, so we found a relatively quiet corner and recorded a conversation. The interview was brief but good, thanks to his gracious, cooperative nature. I later followed up with some additional questions in writing. I was lucky to have interviewed Seaborg when I did, because there was no other opportunity for me to meet with him before he passed away a few years later.

Upon my return home I prepare the transcript of the conversation, edit it slightly, and send the material to the interviewee for checking and changing. We repeat this process until the interviewee feels comfortable with the text. I consider the original recording merely a framework for the interview, which takes its final shape at a more leisurely pace. It may be argued that by doing so, some of the original spontaneity is lost. Also, when I feel during the conversation that a topic seems uncomfortable for the interviewee, I rather drop it than try to force getting more information. My experience is that it would not work anyway. A scientist is willing to reveal



István Hargittai (left) with Nobel Prize Laureates Arthur Kornberg a ('59 Physiology and Medicine) and James Watson ('62 Physiology and Medicine)

more about his inner self to an understanding colleague than to an aggressive investigating reporter. A famous American chemist wrote me after I had sent him the edited transcript of our conversation that, having read several of my previous interviews, he had decided to be on his guard and be reserved. When he received the transcripts, he was astonished that he had told me about things that he had not thought about for a long time and never disclosed, even to his wife. After that remark I would have expected him to delete substantial parts of the interview before publication, but he hardly touched the text.

I would like to sample here some of the answers to the example questions I mentioned above. What turned today's great chemists to chemistry? The most frequent answer is either Paul de Kruif's book *Microbe Hunters* or a chemistry set. It is interesting that de Kruif's book is about pioneers of science, but not so much about chemists. On the other hand, a chemistry set has been the original source of interest not only for many future chemists but for many future physicists and biologists as well. De Kruif's book first appeared in 1926 and has remained in print ever since. Its popularity has faded though. I suspect that nowadays the computer, let alone television, is a great competitor for books among youngsters (we are talking about early teenagers) and, besides, *Microbe Hunters* may be somewhat too romantic for the modern young adult. Chemistry sets may have also lost some of their luster. Today they are less popular than they used to be and part of the reason may be safety precautions that have excluded some of the most spectacular experiments from their repertoire.

To the question about the determining factor in their success, most great scientists name one or two mentors. The determining period of their lives as scientists is their graduate studies in most cases. The venue where they happen to do their graduate work or where they start their independent research career is also a determining factor. The mentor effect may come as a result of a tight interaction with one's thesis supervisor, but it may also be just a casual remark by someone whose impact may then last for a whole research career. My favorite example is Frank Westheimer's story. Westheimer went to pursue his graduate studies at Harvard University because he wanted to work with James B. Conant, who, in the meantime, had become the president of Harvard. Thus, Westheimer had another supervisor, a rather indifferent one. When he finished and was about to leave, Conant called him into his office and asked him about his plans. Westheimer told Conant what he was planning to do as his research project.

Glenn Seaborg ('51 Nobel Laureate in Chemistry)

What turned today's great chemists to chemistry? The most frequent answer is either Paul de Kruif's book Microbe Hunters *or a chemistry set.*

Conant's reaction was devastating for Westheimer: "If you are successful with that project, it will be a footnote to a footnote in the history of chemistry." At that point Westheimer realized that he was supposed to do important things. For his long and successful career, Westheimer measured everything against Conant's words. Conant, in his turn, had a great career as a public servant. They met one more time when Conant had retired and Westheimer was a professor at Harvard. He was working in his office when someone knocked on his door. It was Conant. He looked at Westheimer and asked, "Do you remember me?" At this point in the interview, Westheimer became so moved that we could not continue for some time, but it was also a good finishing point for the conversation. Whenever I re-read this interview or tell others about it I can't help being deeply moved as well.

There are many examples of the importance of the venue for the start of research careers. I found Sidney Altman's description of his postdoctoral stint in the late 1960s at the Laboratory of Molecular Biology (LMB) in Cambridge, United Kingdom, especially interesting. Altman was a co-recipient with Thomas Cech of the chemistry Nobel Prize in 1989 "for their discovery of catalytic properties of RNA." He felt in Cambridge as he thinks young physicists must have felt in Copenhagen in the 1920s at the dawn of modern physics. Altman narrates how everyone went to tea at the LMB, according to the English custom, twice a day, and the "gods" of molecular biology were there. They were accessible in fact, eager to discuss things with everyone else in the lab. The scientists included Francis Crick, Sydney Brenner, Frederick Sanger, César Milstein, Max Perutz, Hugh Huxley, and Aaron Klug. It was a very formative atmosphere indeed.

The question about whether a discoverer recognizes the importance of his discovery right away is less trivial than it sounds. It does happen sometimes that the recognition comes much later after some other people had

done additional work in the field. The story of buckminsterfullerene provides a conspicuous example. Eiji Osawa in Japan proposed the C_{60} molecule of the truncated icosahedral shape 15 years before its discovery. However, he did not recognize its importance and restricted himself to publish about it in the Japanese language only, although he published his other works almost exclusively in English. A few years later two Russian authors, Gal'pern and the late Bochvar reported this structure from their quantum chemical computations. Although their Russian-language article had been translated into English, nobody had noticed it until after the actual discovery in 1985 by Kroto, Curl, Smalley, and their students. The original idea of the truncated icosahedral shape came from a colleague of Bochvar and Gal'pern, Ivan Stankevich, but they failed to include him among the authors because they, and Stankevich too, thought that this piece of work was of no particular interest. Stankevich used to play soccer and the shape of the soccer ball gave him the idea. The interviews with Osawa and with Gal'pern and Stankevich, along with those with the Nobel laureates Curl, Kroto, and Smalley, reflect a lot of human drama.

I have gradually warmed up to asking famous scientists whether it was easy to publish their groundbreaking findings. Originally, I thought that the best journals would be eager to publish Nobel Prize–level discoveries. However, the experience is different, although seldom can one read about it. For some, even for Nobel laureates, it is an uncomfortable topic to narrate about rejections by stern editors of studies that later would merit the highest recognition. Yet it is instructive to observe that perhaps mediocre papers have the easiest way onto the printed page. Very poor papers get filtered out, of course. However, real groundbreaking papers often have their hurdles because of their pioneering character.

Levi-Montalcini
('86 Nobel Laureate in Physiology and Medicine)

These are but a small sample of questions and the answers. There is great diversity among the fates and personalities of great scientists just as among the rest of us. It sounds commonplace, but the most succinct way to characterize great careers is for the right person to be there in the right place at the right time. In addition to being gifted though, many of the greatest scientists worked very hard to be there just when it was the right time and to move around until they "happened to be" in the right place.

Reiko Kuroda
University of Tokyo

Recently, Alan MacDiarmid of the University of Pennsylvania and co-recipient of the chemistry Nobel Prize in 2000 summed up his philosophy in the guise of a Chinese proverb: "I am a very lucky person and the harder I work the luckier I seem to be."

So far, two volumes of my Candid Science series have appeared, *Candid Science: Conversations with Famous Chemists* (2000) and *Candid Science II: Conversations with Famous Biomedical Scientists* (2002). The third volume is now coming out, *Candid Science III: More Conversations with Famous Chemists*. Each volume contains 36 interviews, and more than half are Nobel Laureates. The Candid Science series published by Imperial College Press London is open ended, and one more volume has been contracted (*Candid Science IV: Conversations with Famous Physicists*). Beyond that, I already have material for yet another volume. I have been doing this interviewing mostly during the past half a dozen years.

It was the interaction with Linus Pauling that initiated this project in 1993. However, my very first interview with a famous scientist was 1965 with Nikolai Semenov (1896-1986), the Russian Nobel laureate of 1956. I was asked to do this interview by the science section of Radio Budapest. Semenov came to Budapest to receive an honorary doctorate from the University of Technology. The interview was not only broadcast on radio, later it was also printed in the *Radio and Television Yearbook*, a volume of the best programs. Recently, I purchased a copy of the original tape from the Archives of Radio Budapest and, after more than 35 years, I still found it interesting. I was lucky that Semenov was as experienced in such matters as he was gracious. I was as inexperienced as one

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could be, but had the self-assurance of an ignorant beginner. The Radio supplied me with a tape recorder of the size of a trunk and a technician who operated it. One of the interesting features of the interview was that I asked Semenov to prognosticate about science from the perspective of the mid–1960s, and he did. From today's perspective, he did not say anything extraordinary, but this is what makes his prognostication so valid. I was happy to include Semenov in my first interviews volume.

For six years (1995-2000) I published most of my interviews in The *Chemical Intelligencer*, a now defunct magazine. I have now an interview in each issue of the magazine *Chemical Heritage* published by the Chemical Heritage Foundation. These interviews have been my second university education and I am happy to share all that I have learned from them with everyone.

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Photos: I. Hargittai.

www.tki.aak.bme.hu/hargittai/hargittai.htm

Pesticide Residues

IUPAC Representative's Report on the 34th Codex Committee Session

The Codex Committee on Pesticide Residues (CCPR) convened at The Hague this past May to continue its mission of promulgating recommendations on international standards for maximum residue level (MRL) of pesticides on internationally traded agricultural commodities. In this report, Kenneth Racke, the IUPAC representative on Codex, offers a brief summary and major highlights of the past meeting. The CCPR serves as a forum for discussion and decisions regarding aspects of MRL process risk management. Actual technical recommendations regarding MRLs and the acceptable daily intake (ADI) and acute reference dose (ARfD) toxicological endpoints arise from the annual Joint Meeting on Pesticide Residues (JMPR) of the Food and Agricultural Organization of the United Nations/World Health Organization (FAO/WHO). MRLs recommended by CCPR are subject to formal approval by the biennial Codex Alimentarius Commission (CAC) as Codex MRLs.

by Kenneth D. Racke

Big Group, Small Steps

Two-hundred-fifty delegates representing 51 countries and 15 international organizations attended but, as is typical, achieved few definitive outcomes or concrete decisions. The CCPR enthusiastically debates particular topics and position papers, appoints working groups to prepare new or revised position papers for future discussion, and refers technical matters to the JMPR for learned consideration. Eventually, the CCPR recommends finalized policies and MRLs that have proceeded through an eight-step process to formal adoption by the CAC.

In addition to the frequent interventions of national delegations, opinions aplenty were forthcoming from the manufacturers' representative, CropLife International, and the self-appointed committee conscience, Consumers International. With CropLife International anxious to see reasonable and pragmatic progress in promulgation of MRLs and Consumers International most concerned that the precautionary principle be observed with every action, these delegations found little common ground.

Referrals from CAC and JMPR

Acting on an early agenda item, the committee initiated a comprehensive review of the standard-setting and technical-evaluation processes employed by all Codex-sponsored bodies, including that of CCPR and JMPR. An independent panel of experts' report will be available for discussion at the July 2003 CAC meeting and could yield recommendations for process changes or further study. Included in the ongoing review will be an overall consideration of harmonized approaches across all Codex bodies toward food standards establishment and the associated risk analysis and management.

Several general considerations from the 2001 JMPR were briefly discussed. Among them, the WHO will be developing a guidance document on ARfD establishment. A pilot program for sharing chemical reviews from national regulatory authorities with the JMPR will be advanced later. The JMPR also noted impending availability of a new guidance document, "FAO Plant