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"I was always interested in literature, and therefore in clear expression."

Interview with Prof. Jack D. Dunitz

Prof. Dr. Jack D. Dunitz
- Born in 1923 in Glasgow (Scotland).
- In 1957 came to ETH Zurich, where he became Professor of Chemical Crystallography. Retired in 1990.
- Various distinguished awards, *e.g.* Paracelsus Prize (1986), Bijvoet Lecture and Medal (1989), Gregori Aminoff Prize (1990), Buerger Award (1991).
- Honorary Ph.D. from Technion (1990), Weizmann Institute (1992) and Glasgow University (1999). Awarded honorary membership of the Swiss Chemical Society in 2004.
- Became a chemist because "I had a school teacher who made chemistry interesting".

SCS: Coming from Scotland and having spent several years in the UK and USA, why did you settle down at ETH Zurich?
Jack Dunitz: (laughs) I suppose it came about through a series of accidents. I certainly did not start my career with the intention of becoming a professor in Zurich. After completing my Ph.D. at Glasgow University, I spent about a decade travelling. I first went to Oxford under Dorothy Hodgkin. In 1948, Linus Pauling came to Oxford as Visiting Professor, and when he returned to the California Institute of Technology (Caltech) I went there too.
One of the people I met at Caltech and with whom I shared an office for some time was Edgar Heilbronner, from the ETH Zurich. We kept in touch. In December 1956 while I was working at the Royal Institution in London I received a telephone call from Heilbronner. He told me that Leopold Ruzicka wanted to interview me. Two or three days later I flew to Zurich and met Ruzicka. He was planning to retire in October 1957, and wanted the technique of X-ray crystallography to be established in his laboratory at ETH. He offered me the opportunity to take this forward and gave me 14 days to think about it. So I went home and discussed it with my wife. At that time we were living in a small two-room flat. We were about to have a second baby, and didn't have enough money to move anywhere larger. After talking it over we decided that I should accept the offer. However, I had a five-year contract at the Royal Institution. Before I could accept, I had to ask the Director, Sir Lawrence Bragg, how he would feel about my leaving. He told me it could be a long time before I was offered such a great opportunity again and advised me to go. So I arrived at ETH in October 1957 and have spent the rest of my scientific career here…

What did your wife do at that time?
She was looking after our babies.

What was her education?
Barbara was born stateless in Berlin. She lost her mother when she was a two-year-old child. Her father, a viola player in the Berlin Radio Symphony Orchestra, was thrown out from his position because of his Jewish origin. He dragged out some kind of existence in Berlin during the first years of the Nazi era. He had friends in Holland from his music connections and arranged to take Barbara to the Dutch border, where she was met and looked after by a Dutch family in Amsterdam. From 1938 onwards my wife lived in Holland, where after the war she took Dutch nationality and obtained a diploma in book-selling and publishing. As part of her studies, she was sent to Blackwell's University Bookshop in Oxford; that's how we met.

Why did her father return to Berlin?
He was then not allowed to enter Holland. In January 1939, however, he did manage to come into Holland. Later, during the war, he had to go into hiding in Amsterdam. His daughter didn't see him for several years.

Did they reunite after the war?
They did!

> *"The intellectual atmosphere at Caltech was absolutely stupendous."*

You spent several extended periods at Caltech, the first being in 1948. Coming from English-speaking Europe, it must have been easy for you to go to the USA. Were there things that surprised you about California?
Firstly, there was the sun… (laughs) Secondly, there was the standard of living; remember that it took post-war England years to recover. But most of all, the intellectual atmosphere at Caltech was absolutely stupendous. Those of us who remember that time refer to it as the 'Golden Age'. On the day I arrived at the Faculty Club, the Atheneum, I went down to the restaurant in the early evening, sat down at a table, and was joined by two young men. One of them was Carlton Gajdusek, who won a Nobel Prize thirty years later by discovering kuru disease, the other was Gunther Stent, one of the pioneers in molecular biology. Imagine, the first two people I met there helped to make the science of the second half of the twentieth century…

Was there anything about American society, about the mentality there that surprised you?
I spent all my time with scientists. Caltech was tremendously international, with a wonderfully wide range of people from Europe. We were welcomed there. It is difficult to describe how many of the problems that were to influence science over the next fifty years were current topics of discussion at that time.
Since you mention mentality, there were of course other things too. I arrived at Caltech at about the same time as another postdoc, Ted Harrold, a biologist from England. Ted and I were fascinated by radio commercials. Ted was a very good piano player, an improviser. For Christmas 1948 we made up a sort of cabaret show based on radio commercials, with all the jingles, applied to academia. Our act was a great success. We had singing commercials for, say, Harvard, or Yale, or for scientific equipment. I would say that after a year or two, I was better known at Caltech for my comedy shows than for my contributions to science…

Having arrived in Zurich, you stayed here for the rest of your career. Have you never felt tempted to leave?
I stayed here because I liked it, because I had good opportunities to work, and because I had a very fine group of colleagues. We got on very well together and supported one another in many ways. It really was a family atmosphere. Over the years I had offers to go elsewhere, to American, British or Dutch universities. In the mid-sixties, a professor in America was considerably better paid than here; this is no longer the case. At that time, when I was getting simultaneous offers from top American universities, I knew that if I turned them down they would not ask me again. I decided I'd rather stay here. From then on I had many Visiting Professorships, but I never had any serious flirtation with other universities.

"I had the good fortune to have had a series of top-class technical assistants."

Don't you think that moving on might have helped your career?
Not really. I never had a large research group; at most three or four Ph.D. students and three or four Postdocs. If my research group got to more than about ten I drew the line. With more students I could never have kept track of what they were all doing. I never wanted for equipment; if I needed anything new I got it. Also, I had the good fortune to have a series of technical assistants – three in succession – who could not have been better. This has to do with our being in Switzerland, by the way. There is a certain level of people here with a technical education that is of the highest quality.

Are you speaking of former 'Lehrlinge' (apprentices)?
Yes; these three were all 'Lehrlinge' who ended up in chemistry. I advised one of them to take a late Matura. He became a medical doctor. The Swiss educational system is very good in some ways, but all of these people failed in the middle school, perhaps because they were not good at one particular subject although they might have had great ability in another. Maybe things are better now, but back then that type of person tended to slip through the Swiss system. They sent them into technical education, and some of them proved to be exceptionally gifted. Anyway, I had the good fortune to have had a series of top-class technical assistants, which I could never have had anywhere else.

"When you analysed crystals with X-ray crystallography, the thing 'came out' so wonderfully definitely…"

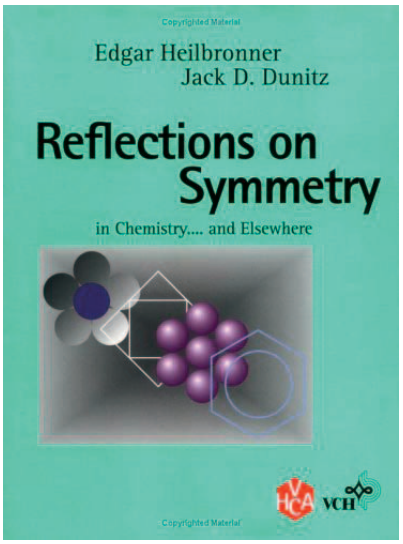
You have published over 350 scientific papers. There were years when you published well over ten papers a year. How can anyone manage that kind of workload?
In the old days, a paper was written literally with pencil and paper… I spent a considerable amount of time writing and trying to express things as clearly as possible. I was always interested in literature, and therefore in clear expression. People used to come to me with their own manuscripts and ask me to read them through. There was something they called the 'Dunitz rule' which says that in almost any scientific paper you can cross out the first sentence. After this is done, of course, there is another first sentence… I quite like writing.

Tell me what it is that so fascinates you about crystals.
It wasn't so much the crystal itself that fascinated me. When I became a graduate student I was given the job of determining the crystal structure of acetylenedicarboxylic acid dihydrate. This is a task that could be done in two hours with the equipment available today, but at that time it took me two years. Halfway through those two years, atoms started to appear – I could see them! I should remind you that at that time we had no NMR. Spectroscopic methods were in their infancy and were being applied only to very simple molecules. The method for determining molecule structure in organic chemistry consisted of taking a substance and applying a set of reactions. From the results of these reactions you tried to build up a model. The situation in the 1940s was that you could gain a doctorate by using some particular method to demonstrate the structure of, say, some particular alkaloid. Your successor gained a Ph.D. for demonstrating that this structure was incorrect, and was in fact such and such, and so on… I didn't enjoy that kind of work much. On the other hand, when you analysed crystals with X-ray crystallography, the thing 'came out' so wonderfully definitely… We got our results by analysing diffraction patterns. Of course, nowadays, with direct methods, high class diffractometers and computers, crystallographers can get results much more quickly. Thanks to this and to Linus Pauling's book 'The Nature of the Chemical Bond', chemical molecules gained a kind of definiteness for me. One could make statements about their structure, their stability, their reactivity *etc*. That appealed to me. It was the reason why I went to Caltech.

What did you like most about Edgar Heilbronner?
(meditates) He had a tremendous sense of humour and imagination. He loved to make up stories, poems. He was also a wonderful cartoonist; he certainly could have had a career as a cartoonist… We were next door neighbours for a few years in Rüschlikon, where our children grew up together.

And what was it about his chemistry that appealed to you?
Apart from being a great theoretical chemist, he was a wonderful teacher. His lectures on theoretical chemistry and quantum mechanics were among the best I've ever heard. Scientifically our interests were not the same, but they overlapped, especially in theoretical chemistry. After his retirement, we wrote a book together on symmetry for anyone interested (see box).

What was Heilbronner like as a teacher?
He always tried to generalize the familiar, to take something familiar to you and expand it. This is the opposite of a teacher who sets out from a very abstract starting point. With his way of his



Edgar Heilbronner and **Jack D. Dunitz**, **Reflections on Symmetry in Chemistry... and Elsewhere!**
Wiley-VCH 1992, 160 pages.
What, another book on symmetry! Aren't there enough already? Yes, but this book is different from the others. It is neither a textbook loaded with mathematical jargon, nor a bird's-eye view of the various examples of symmetry that can be found in art, science, and everyday life. Not too serious, not totally frivolous – it is somewhere in between. A special feature of the book is the illustrations, many of historical interest, taken from early sources, while others were prepared specifically to clarify and illuminate the text.

explaining things, you learned something you hadn't known before, in a way that seemed obvious.

You once mentioned that you had "tremendous hero-worship admiration" for Linus Pauling. What role does personal admiration play in academic life?
I am not sure that your question is framed in the right way.

How would you frame it?
I think that role depends on the circumstances in which you meet extraordinary people. When you have the good fortune of meeting such people you admire them in a certain way. I had the good luck, partly through Oxford and Caltech, of meeting them when I was young and impressionable. I met a number of people who turned out to be leading figures in science. Anybody meeting people like Linus Pauling, Francis Crick, Sydney Brenner or Leslie Orgel would have been impressed.

How can one learn from such people? One can be impressed or even awestruck…
Certainly you mustn't be awestruck. It's largely a matter of self-confidence. On the other hand, I have met people whose imaginative and intellectual powers were so much greater that my own that I knew I could not emulate them, but I could still admire them and perhaps try to copy them a bit.

How did you learn from impressive people? Did they just inspire you? Did you want to become better than them?
I don't think I wanted to become better than them. I enjoyed the experience very much. I didn't do any of this in a spirit of competition, but rather in a spirit of celebration of their wonderful ideas.

It can inspire your work, can't it?
I think in my own work it's not so much a matter of being inspired by ideas but more of having questions that you can't quite solve, and then walking round chewing on the problem for a number of years, bit by bit. Very often in my life, what I needed were more facts. I am an experimentalist. I have always had a certain propensity for looking at errors people make and then trying to find out why something went wrong. The other thing I would say is that I'm a bit of an opportunist. I discovered problems and then worked on them; I didn't start out with theories.

When I watched a video recording of one of your public talks I was impressed by its pedagogic quality, and in particular by the simplicity and clarity of the language. Are you aware of this?
I think it is one of our duties to try to be as easy to understand as possible. I know there are some authors who cloak their language in such a mysterious, closed-up way that it's difficult to know what they are talking about. I have always tried to avoid this and make my prose as clear as possible.

> *"I discovered problems and then worked on them; I didn't start out with theories."*

Why?
I have a certain amount of experience of talking to a general audience and trying to explain to them what I was doing. I don't enjoy listening to talks where the speaker is trying to mystify me; I feel cheated…

Is it perhaps easier to be precise in the English language, compared to, say, German?
There is no question that reading the works of English-speaking philosophers, David Hume for example, and reading German thinkers, like Immanuel Kant, are two different experiences. Hume is not difficult to understand. On the other hand, people used to say that French is a very precise language. I have great admiration for a French mathematician, Lazare Carnot, the author of 'Géometrie de Position'. One of the greatest books about general science in the 20th century is 'La Statue intérieure' by the French biologist and Nobel Prize winner François Jacob.

German can be precise too. It doesn't lack the elements; it is more a question of how it is used…
Right.

How did you acquire your oratorical skills?
I think I acquired such as I may have by imitation. Pauling's 'The Nature of the Chemical Bond' was a book that impressed me enormously. It is written in very simple English.

Jews have been more successful in science across the ages than other races. You are Jewish yourself. How do you explain the remarkable success of Jews in intellectual life?
I don't have an explanation for that. I have heard one suggestion, which I mention here without necessarily saying that I completely believe it. For hundreds of years, when the majority of the world's Jewish population lived in Western Russia and in what is now Poland, Jews were barred from certain occupations such as agriculture, and were more or less pushed into activities that involved buying and selling things, and later on into banking. In the small Jewish communities, the most respected person was the Rabbi, especially if he was a learned scholar. Unlike as in the Catholic Church, Rabbis were allowed to and were even expected to marry and have children. Because the Rabbi didn't earn much money, it became a sort of tradition that the daughter of the richest man in the community was given to be the Rabbi's bride, so that a wealthy family would look after him to some extent. Thus, what you might call a tendency towards learning and intellectual argument was passed on from generation to generation. This tradition may have favoured the tendency towards intellectual and spiritual thinking. There is no question that in the 20th century, when the Jews of Eastern Europe were liberated, they have produced an enormous number of top class scientists, philosophers and writers — as well as many great musicians, of course.

You mentioned the regions in which Jews were concentrated. What about those in the rest of the world, the Diaspora, where Jews were clearly in a minority?
I suspect that this situation produced mainly a first-generation phenomenon. There was a pressure to learn. For example, among

those who left Europe, were a considerable number of clever Jewish boys whose parents had immigrated to the United States. Think of physicists such as Richard P. Feynman and Steven Weinberg, and practically all of the famous American physicists. The same can be observed among Chinese immigrants today, for example. One Chinese cosmologist I met talked about the work ethic that he received from his parents. I asked him, what about your children? He said, my children are not interested.

You once said your father was a dreamer. Aren't all scientists dreamers, in a way?
Some are, some aren't.

If you take away the negative aspects of dreaming, such as the lack of realism, it is basically an act of expanding one's inner world, imagining things, constructing ideas…
Nowadays, particularly in physics and quantum theory, there is so much scope for imagination that it can be taken too far. I am thinking, for example, of the suggestion that there may be parallel universes. One aspect of scientific theories is that you are supposed to make statements that are in principle falsifiable through experiment. How can you falsify the statement that there may be other universes?

Is representing a molecule in your mind not similar to dreaming? It's in your head and you can't touch it.
Yes, but there is also a lot of nonsense in our heads.

The art is to sort out the things of value from the nonsense…
There I agree. The greatest burst of intellectual originality and depth in the last century is unquestionably the six papers written by Albert Einstein in 1905. At that time nobody had ever heard of Einstein. He did not teach, and was working as a patent inspector in Berne. Nevertheless, he found the time to write six papers, at least three of which – the ones about the special theory of relativity, about light quanta, and about turbulence and random systems – are absolutely unique. This came from a young man who was practically unknown. However, this kind of burst of intellectual power is unparalleled in science. It is certainly not typical of the way science goes about its business.

"I still enjoy talking to the Ph.D. students and postdocs."

You still go in to work regularly at ETH Zurich. What qualities do researchers gain when they become older, and what qualities do they lose?
That's a difficult question. Obviously what you gain is experience. What I most enjoy about still coming in to ETH Zurich is the opportunity to meet and talk to colleagues and younger people. I try to talk to the Ph.D. students and postdocs.

Do you attend their meetings? Technically you are a Postdoc of Professor Diederich…
I attend François Diederich's weekly research meetings, where his students talk about their work. I listen to them and sometimes interrupt, sometimes make suggestions and sometimes just listen. I find it very useful to keep in touch with how young people today think.

Do you still publish?
Yes. Last year we published a paper that got a certain amount of publicity. It so happened that one day Duilio Arigoni asked me what is the crystal structure of ribose. It turned out that nobody knew. I couldn't believe this, and found out that ribose crystals are of very poor quality and had somehow been neglected in the half-million crystal structures that were known. I talked to my younger colleagues, obtained some ribose, gave samples to some of them who were working with modern techniques, and within a few weeks we knew the crystal structure of ribose and published it under the title 'The Crystal Structure of D-Ribose — At Last!'

Prof. Dunitz was talking to Lukas Weber, Executive Director of the Swiss Chemical Society.