

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

GERHARD HERZBERG

Transcript of an Interview
Conducted by

M. Christine King

at

The National Research Council of Canada

on

5 May 1986

BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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Gerhard Herzberg

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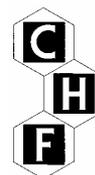
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GERHARD HERZBERG

1904 Born in Hamburg, Germany on 25 December

Education

1928 Dr. Ing., Darmstadt Technische Universität

Professional Experience

1928-1929 Post-doctoral fellow, University of Göttingen
1929-1930 Post-doctoral fellow, University of Bristol
1930-1935 Privatdozent, Darmstadt Technische Universität
1935-1945 Research Professor of Physics, University of Saskatchewan
1945-1948 Professor of Spectroscopy, Yerkes Observatory, University of Chicago
National Research Council of Canada
1948-1949 Principal Research Officer
1949-1955 Director, Division of Physics
1955-1969 Director, Division of Pure Physics
1969- Distinguished Research Scientist, Herzberg Institute of Astrophysics

Honors

Honorary Degrees

1953 LL.D., University of Saskatchewan
1954 D.Sc., McMaster University
1956 D.Sc., National University of Ireland
1958 LL.D., University of Toronto
1960 LL.D., Dalhousie University
1960 D.Sc., Oxford University
1961 LL.D., University of Alberta
1964 D.Sc., University of British Columbia
1965 D.Sc., Queen's University, Kingston
1966 D.Sc., University of New Brunswick
1966 Dr.fil.hed., University of Stockholm
1967 D.Sc., University of Chicago
1967 D.Sc., Carleton University
1968 Dr.rer.nat., University of Göttingen
1968 D.Sc., Memorial University, Newfoundland
1969 D.Sc., York University
1970 D.Sc., University of Windsor
1971 D.Sc., Royal Military College of Canada
1972 D.Sc., Drexel University
1972 LL.D., St. Francis Xavier University
1972 D.Sc., University of Montreal
1972 LL.D., Simon Fraser University
1972 D.Sc., Université de Sherbrooke
1972 D.Sc., Cambridge University
1972 D.Sc., McGill University

1973 D.Sc., University of Manitoba
 1974 Dr.rer.nat., University of Hamburg
 1975 D.Sc., University of Bristol
 1975 D.Sc., Andhra University
 1976 D.Sc., Osmania University
 1976 D.Sc., University of Delhi
 1976 D.Phil., Weizmann Institute of Science
 1976 D.Sc., University of Western Ontario
 1979 D.Sc., Laval University
 1983 Dr.phil.nat., University of Frankfurt
 1984 D.Phil., University of Toledo

Elected and Honorary Memberships

1939 Fellow, Royal Society of Canada
 1951 Fellow, Royal Society of London
 1954 Honorary Fellow, Indian Academy of Sciences
 1956-1957 President, Canadian Association of Physicists
 1957 Honorary Fellow, National Academy of Sciences,
 India
 1957-1963 Vice-President, International Union of Pure and
 Applied Physics
 1959-1960 Chair Francqui, Université de Liège
 1960 Corresponding Member, Société Royal des
 Sciences de Liège
 1964 Honorary Member, Hungarian Academy of Sciences
 1964 Academician, Pontifical Academy of Sciences
 1965 Honorary Foreign Member, American Academy of
 Arts and Sciences
 1968 Honorary Member, Optical Society of America
 1968 Honorary Fellow, Chemical Society of London
 (now Royal Society of Chemistry)
 1968 Foreign Associate, National Academy of Sciences,
 Washington
 1968 Companion of the Order of Canada
 1969 Honorary Member, Society for Applied Spectroscopy
 1970 Honorary Member, Royal Irish Academy
 1970 Honorary Fellow, Chemical Institute of Canada
 1970 Honorary Member, Spectroscopy Society of Canada
 1972 Foreign Member, American Philosophical Society
 1973-1980 Chancellor, Carleton University
 1973 Honorary Member, International Academy of Quantum
 Molecular Science
 1973 Honorary Fellow, Indian Chemical Society
 1974 Foreign Fellow, Indian National Science Academy
 1974 Honorary Member, La Asociacion de Quimicos
 Farmaceuticos de Columbia
 1974 Foreign Associate, Royal Academy of Belgium
 1976 ACS Centennial Foreign Fellow, American Chemical
 Society
 1976 Honorary Member, Japan Academy
 1978 Honorary Member, Chemical Society of Japan
 1978 Honorary Member, Real Sociedad Espanola de
 Fisica y Quimica
 1980 Member, European Academy of Arts, Sciences
 and Humanities

- 1981 Foreign Member (Physics), Royal Swedish Academy
of Sciences
1986 Korrespondierendes Mitglied, Bayerische Akademie der
Wissenschaften

Orders, Medals, Lectures and Prizes

- 1950 Médaille de l'Université de Liège
1953 Henry Marshall Tory Medal, Royal Society of
Canada
1954 Joy Kissen Mookerjee Gold Medal, Indian
Association for the Cultivation of Science
1957 Gold Medal, Canadian Association of Physicists
1959 Medal of the Society for Applied Spectroscopy
1960 Médaille de l'Université de Liège
1960 Bakerian Lecture, Royal Society of London
1962 Pittsburgh Spectroscopy Award, Spectroscopy
Society of Pittsburgh
1963 Twelfth Spiers Memorial Lecture, Faraday Society
1964 Frederic Ives Medal, Optical Society of America
1964 William Draper Harkins Lecture, University of
Chicago
1968 George Fisher Baker Non-Resident Lecturer in
Chemistry, Cornell University
1969 Willard Gibbs Medal, American Chemical Society
1969 Gold Medal, Professional Institute of the
Public Service of Canada
1970 Faraday Medal, Chemical Society of London
1971 Royal Medal, Royal Society of London
1971 Linus Pauling Medal, American Chemical Society
1971 Nobel Prize in Chemistry
1972 Chemical Institute of Canada Medal
1974 Madison Marshall Award, North Alabama Section,
American Chemical Society
1985 Earle K. Plyler Prize, American Physical Society
1987 Jan Marcus Marci Memorial Medal, Czechoslovak
Spectroscopy Society
1987 Minor Planet 3316=1984 CN1 named "Herzberg"

ABSTRACT

In this interview the late Christine King starts by asking Gerhard Herzberg to describe his schooling in Germany. An interest in science and mathematics was kindled at his school in Hamburg; indeed, Herzberg's first interest was astronomy. More practical considerations led him to follow the engineering physics course at Darmstadt, where he graduated with his doctoral degree in 1928. His introduction to spectroscopic studies was with Hans Rau, himself a student of Wien. A seminal year at Göttingen followed where Herzberg studied with both James Franck and Max Born; it was during this time that the basis for the well-known monographs was first established. A further postdoctoral year at Bristol with Lennard-Jones was followed by his return to Darmstadt as Privatdozent but the worsening political situation prompted Herzberg to seek a position abroad. He next describes his time at the University of Saskatchewan and how he was able to continue research, despite limited equipment. Analysis of cometary spectra led Herzberg into astrophysics which was further developed during the three year spell at the Yerkes Observatory. During the final section of the interview, Herzberg tells of his return to Canada and reflects on research direction at the National Research Council and the circumstances of the award of the Nobel Prize for Chemistry in 1971. Finally, Christine King learns of Herzberg's pastimes, in particular of his love of choral singing. As a coda, Herzberg is asked about his involvement with chemists, especially with those concerned with free radicals.

INTERVIEWER

Mary Christine King was born in China and educated in Ireland. She obtained a B.Sc. degree in chemistry from the University of London in 1968 which was followed by an M.Sc. in polymer and fiber science (1970) and a Ph.D. for a thesis on the hydrodynamic properties of paraffins in solution (1973), both from the University of Manchester Institute of Science and Technology. After working with Joseph Needham at Cambridge she received a Ph.D. degree in the history and philosophy of science from the Open University in 1980 and thereafter worked at the University of California at Berkeley and at the University of Ottawa, where she carried out research with Dr. Keith Laidler. Christine King died in an automobile accident in late 1987; her recent biography E. W. R. Steacie and Science in Canada (University of Toronto Press, 1989) was published posthumously.

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INTERVIEW: Dr. Gerhard Herzberg
INTERVIEWER: M. Christine King
LOCATION: National Research Council of Canada, Ottawa
DATE: 5 May 1986

KING: Dr. Herzberg, you were born in Hamburg on Christmas Day of 1904. Your father died when you were only eleven (1). Did your mother have any special scientific interests or ambitions for you or your brother?

HERZBERG: Not at all. My mother was not interested in science, let's put it that way. She didn't have the educational background to be interested in science.

KING: So your interest arose entirely from your own endeavors?

HERZBERG: And from my teachers.

KING: That was my next question. You spent all your school years in Hamburg, apart from a short stay in Frankfurt. At school you first became interested in atomic and molecular physics. Can you recall how this came about?

HERZBERG: We had a teacher at the school, which was the natural sciences branch of one of the oldest Hamburg schools, the Johanneum. One of the teachers in physics and mathematics was a man by the name of W. Hillers; he was a very competent physicist. He was co-editor of a textbook in physics, which is still in use by universities in Germany after more than fifty years. It is called Lehrbuch der Physik by E. Grimsehl (2). It was then in two volumes. Grimsehl was a teacher at another school in Hamburg but he was killed during the first World War. So he only prepared the first edition of this text book. Hillers and someone called Starke looked after a number of further editions. Hillers gave a series of lectures at the school ahead of the regular school time. Normally, we started at 8:00 a.m. but this was at 7:00 a.m. or something like that. He was not only a good lecturer but he also knew modern physics, as of that time. I certainly owe him a great deal for stimulating my interest in atomic and molecular physics.

KING: Do you remember any other courses during these years which influenced you to go into science, mathematics, chemistry?

HERZBERG: My interest in science -- actually before I had the opportunity to listen to Hillers -- arose more from astronomy. I had a friend at school with whom I built a small and very primitive telescope and we read about astronomy and that was exciting. Even at that time, it was fairly clear that if I wanted to make any headway in astronomy, I had to know some physics and mathematics. As I was doing well in those subjects, astronomy was a natural interest. Of course, I did chemistry as well. We had a fairly good teacher in chemistry. Actually we didn't have W. Hillers as our classroom teacher. He taught other classes. Whether he taught only physics I don't remember. But we had some very poor teachers also. We had for most years a very good teacher in mathematics who had written several textbooks. But we had one person in physics who really didn't know very much. He had a background in meteorology. Even so, there was some degree of inspiration from him because he sometimes told us what some of his friends were doing in research, and that somehow seemed to appeal to me. I remember one particular instance when he told us about a friend of his who was working at the German analog of the National Bureau of Standards. He asked him at one time, "What are you working on?" and he said, "I'm calibrating a thermometer." After a year or so, he met this man again, "What are you doing?" and he replied, "I'm calibrating a thermometer." "Not the same one!" "Yes, the same one." And that impressed me.

KING: So you went to the same school during all these years. There's a sense of continuity?

HERZBERG: After leaving Frankfurt, yes, that's right; until I got to the equivalent of grade 12 or 13.

KING: By the time you finished school, you had decided to become an astronomer, but you were dissuaded from that ambition by financial factors. You then decided to study engineering physics at the Technical University in Darmstadt. First, what attracted you to astronomy? You said that you built a little telescope.

HERZBERG: At that time you could still see the stars as you walked the streets of Hamburg, nowadays you can't see the stars anymore [in cities]. Anyone who is "awake" must wonder -- What does it all mean? I couldn't escape that question. I would like to tell a little story. You said I was discouraged from going into astronomy; the question was more specific in that I went to a vocational guidance bureau, which existed even at that time in Hamburg. They took my question -- How do I go about becoming an astronomer? -- very seriously and they wrote to the then director of the Hamburg Observatory. He was a well-established scientist and, as I found later on, was well-known in astronomy. His name was Schorr. He wrote back to the vocational guidance bureau to the effect that if I were financially independent I should be

encouraged to go into astronomy. But if I weren't, then there was no way of making a living as an astronomer and it would be better if I did something else.

KING: Have you ever regretted that you didn't somehow try to obtain funds and pursue your ambition?

HERZBERG: Not really, no. Throughout my career I felt quite happy with what I was doing. It soon became pretty clear to me when I started as an engineering physics student that basic physics was more to my liking. But I did actually take some engineering subjects like descriptive geometry, for example, about which I learned a lot more than a physicist normally would. In that connection I did some drafting and things of that sort. It was an engineering school and both degrees that I had from that University are engineering degrees. It's only in more recent times that they have started to award non-engineering degrees in the natural sciences. But at the time I was there it was a doctor of engineering degree at Darmstadt.

KING: Do you think that this practical application helped you later when you needed to design apparatus?

HERZBERG: I think so, yes. Perhaps not on many occasions, but there were certainly times where my training in descriptive geometry and in the art of technical drawing were of considerable help.

KING: The other question I wanted to ask in connection with what we've been saying; was it then a relatively easy matter for you to be funded to study physics in Germany, as opposed to astronomy?

HERZBERG: No, that was the other thing. It was really quite difficult. I recall discussing what I should do when I finished school around the supper table at a friend's house when his father said, "Why don't you write to Stinnes?" I don't know whether this means anything to you, but after the first war, Stinnes was the biggest industrial firm in Germany. It was mainly a shipbuilding company. Knowing of Stinnes only as a big industrialist, I didn't really much appreciate this suggestion, but in desperation I did write a letter to Stinnes. And when the answer came back, not from Stinnes himself, but from some assistant of his, I got private support from Mr. Stinnes. On the strength of that, I was able to go to Darmstadt to start my studies. The trouble was that after two years, the big firm Stinnes went bankrupt and that was the end of my private fellowship. Fortunately by that time I had gotten to know the professor of physics in Darmstadt and other faculty there. Just

at that time, the German government had established a national fellowship scheme for the best students in the country, the Studienstiftung des Deutschen Volkes. I don't know how many fellowships they had at the beginning but I was able, on the strength of the recommendation of my professor, to get one of the first bursaries, and could therefore complete my studies. At the same time I was also employed as an assistant in the lab, so I earned some extra money that way.

KING: So by a combination of financial means you completed your university training, which began in 1924, and subsequently received your doctorate degree in engineering physics in 1928. You worked for Hans Rau, who had been a student of W. Wien. He gave you great freedom in choosing your line of research.

HERZBERG: That's right.

KING: It seems that after reading Sommerfeld's book Atomic Structure and Spectral Lines (3), you had some very good ideas. You said it was an obvious idea to try and produce the spectrum of Li^{2+} ; could you tell me if this idea seemed obvious to you at the time, or was it obvious in retrospect?

HERZBERG: Yes, it was very obvious. It was some time after Niels Bohr had developed his theory of atomic structure and it was formulated for the hydrogen atom. Very soon after that, Sommerfeld applied this theory to a similar system, except that the central charge was one unit higher. This was He^+ , which is the next element in periodic system, and He^+ is entirely similar to hydrogen except for a factor of 4 in the energy levels and, if you like, in the spectral lines. Not exactly 4, but very close to 4. If you extrapolate that, it's obvious that if you go to the next element Li and add one charge, leaving only one electron around the nucleus with charge 3, then you would again get a spectrum very similar to hydrogen, except that the factor instead of 4 is now 9, the square of the atomic charge. So that can't be considered as a very deep thought. In retrospect I found that many people had tried it, but I wasn't successful. I didn't really try very hard because I was sidetracked on to something else, in fact on something molecular. So I never got far enough with Li. It was much later that the spectrum of Li^{2+} was successfully studied.

KING: You said that this was the time Bohr's theory was being aired. Do you remember whether there was much excitement, were people very excited by new developments?

HERZBERG: Of course, you have to remember that Bohr's theory, at the time that I started to study in 1924, was already 12 years

old, and people were trying to understand the results of Bohr's theory with more theoretical background. There were certain unresolved riddles in the Bohr theory. Then in 1925 or 1926, they were solved by Heisenberg and Schrödinger (4). My own real excitement came later when Schrödinger and Heisenberg published their series of papers. I was more impressed with Schrödinger's formulation because it was easier to understand than Heisenberg's somewhat more abstract theories, but they actually amount to the same thing.

KING: This anticipates my next question. After you finished your doctoral work, you went to Göttingen for a year, in 1928. That was a glorious time to be there. If it's not too difficult a question, can you recall some of the people that you worked with and the general atmosphere of the day?

HERZBERG: Almost the first person that I met in Göttingen was a man only a few years older than I was. Walter Heitler was the initiator, with Fritz London, of the wave-mechanical treatment of the problem of chemical valence, in particular, the understanding of the formation of the H₂ molecule. I had found some experimental results while still in Darmstadt about certain molecules like CN and N₂⁺ and I discussed them with Heitler on the basis of his theory and we thought we had an interesting development (5). It turned out to be wrong, or, rather, not very significant, if you like. But it gave me a start to talk to people and of course the people I really wanted to work with -- James Franck, who was an experimentalist, and Max Born, who was a theoretician. I don't know whether I ever thought about becoming a theoretician, I don't think I did, but I spent officially, at any rate, the first six months in Max Born's institute [to which Heitler belonged], and the second six months in James Franck's institute.

Naturally, there were a number of outstanding people in both places and of course a stream of visitors from this continent and elsewhere that came in and so one met a lot of people. One person with whom I worked, while he was a guest in James Franck's laboratory, was G. Scheibe who at that time was a professor at the University of Erlangen in Bavaria. Later he was professor at the Technical University of Munich. We did one piece of work together which was the first study of the vacuum ultra-violet spectra of the methyl halides (6). People are still working on these spectra off and on. I think it was a fairly significant piece of research, nothing very record-breaking or fundamental, but after all, science doesn't proceed in big steps, it proceeds in very small steps, and this was one small step ahead. During my time in Göttingen a paper was published by Wigner and Witmer (7), which was very fundamental for molecular spectroscopy and I studied this paper very carefully and it stimulated me to develop a paper on the dissociation energy of the oxygen molecule, which is a very important molecule (8). I feel that that was a fairly important step.

KING: Had you met Wigner personally by then?

HERZBERG: Wigner was also a visitor in Göttingen (for a few days). He's still alive and still a great figure in physics, but he must be about 85 or something like that.

KING: But he travels a great deal just like you?

HERZBERG: Yes, he came originally from Budapest and he was a very extraordinary theoretician. Another person I met was a man by the name of Winans, who's also still around. He's retired from the University of Buffalo. During most of his life he was at the University of Wisconsin. [Winans died in January 1990] He did one important step right at the time when I was in Göttingen. Together with a Swiss theoretician by the name of Stueckelberg, who died only recently, and who was very well thought of as a theoretical physicist, he published a paper on the continuous spectrum of the hydrogen molecule (9). Now when I was in Darmstadt I had tried to interpret this spectrum and had an idea, which turned out to be quite childish in retrospect, but I was aware of the facts. Then I saw this manuscript. It was first circulated as a manuscript in Göttingen, and it was immediately clear that that was the explanation of the spectrum, and it has stood up in the course of time. That was done by Winans and Stueckelberg. Stueckelberg became a very prominent physicist, although not many lay people may know of his name. He was doing really extraordinarily good work, but he had trouble with illness all his life. Winans didn't quite live up to the promise, but he did this one important piece of work. I don't know who was the originator, but I would guess that Winans brought the problem to Stueckelberg and Stueckelberg solved the problem. Anyway, it's a very fine piece of work and I became aware of it as soon as I got to Göttingen because this manuscript was floating around there with various people. Those are two instances that I can remember.

There was one strange coincidence, among the visitors I met a Dutch physicist by the name of Druyvesteyn. He talked about his work, not about anything that was of particular interest to me at the time, but some 40 years later, I became aware of something that he had done at that time. He had found and described a spectrum in a mixture of He and Ne which however he couldn't explain and analyze (10). I became interested in this spectrum here in Ottawa about 15 years ago. We solved the riddle of that spectrum (11). That was an interesting experience. Again, nothing world-shaking, but a contribution that I think will remain and may be extended to other spectra.

KING: While you were at Göttingen you decided you would like to try and publish some of the lectures you had given in the form a book. This was the beginning of something very great, of course

(12-14). Can we skip chronology and could you talk a little bit about....

HERZBERG: Actually, this arose in connection with a series of informal lectures, perhaps half a dozen or so, which I was presenting. At the time there were three physics institutes at the University of Göttingen. I was attached to the second institute, headed by James Franck. The first institute of physics was under Professor R. W. Pohl. It was not Pohl himself but some of his younger colleagues who asked me to explain to them some of the elements of molecular theory, which I did. One of the people who came to listen to these lectures was Professor Scheibe, with whom I worked on the spectra of the methyl halides. He was the one who suggested that I should write up these lectures in the form of a book. He had a connection with a publisher by the name of Theodor Steinkopff, then located in Dresden. Thanks to Scheibe I got a request from this publisher to write a book on atomic and molecular spectra. That was to be a book of some 160 pages. Well, gradually I got busy and when I had written the part on atomic spectra, it was already more than 160 pages. So I split off the atomic part and it was published separately (12). I handed in the manuscript before I left Germany: it was published [in German] during my first year in Saskatchewan.

KING: In 1936.

HERZBERG: 1936, yes. Strangely, it is still being sold. On my way over from Germany to Saskatchewan, I visited Princeton, where I met Professor E. U. Condon, who had just started as the editor of a series of physics texts for the Prentice-Hall Company and he asked, "Could you translate this book? We'll publish it in this series." Of course, I jumped at this opportunity but I was a little leery about doing the translation myself because my English wasn't quite as firm as it might be and also because it was a lot of work. Dr. Spinks in Saskatchewan volunteered to do the translation. We then went over the manuscript together to iron out some points and it was published by Prentice-Hall in 1937. During the war when the book began to be sold out, Prentice-Hall wrote to me and said, "We cannot reprint because we need our paper for more profitable books." So they handed back the copyright and I then looked for another publisher. I got to Dover Publications, a reprint company, and they took it on. They have kept it in print ever since, and by now they have sold more than 100,000 copies. That is Prentice-Hall's loss.

KING: Would you like to say a little bit more about your other volumes now or should we talk about them later?

HERZBERG: This might be a suitable occasion because it fits

together. As I mentioned, after finding that atomic spectra would fill a volume of some 150-200 pages, actually it's a little over 200, I thought now the next thing for me to do is get the volume on molecular spectra out of the way. I worked on that but I found that I hadn't finished the spectra of diatomic molecules when I had already some 500 pages. So I had to divide again into diatomic and polyatomic molecules (13). The same happened once again when the polyatomic book was written. It turned out it would be a book of 1000 pages or more and so it was split again into a book on infra-red and Raman spectra (13b), which was published in 1945, and electronic spectra in 1966 (13c). One of my friends compared it to Richard Wagner's experience when he wrote *der Ring des Nibelungen*. He originally was going to write one opera about the death of Siegfried and eventually it became a short introductory opera, *Rheingold* and then three more. I had these three on molecular spectra and a preview on atomic spectra. That wasn't my idea, but it is quite amusing. [laughter]

KING: Going back to 1928 when you were at Göttingen. It seems to have been a really exceptional year for molecular spectra and structures. You were obviously doing the right subject at the right time and place.

HERZBERG: That's correct.

KING: You've already said a little bit about your experiences, is there anything else that you'd like to add to these developments?

HERZBERG: This was 1928, it was the year in which Friedrich Hund, who was a frequent visitor in Göttingen published his basic papers on molecular orbital theory. Of course, molecular orbital theory is usually connected with the name of Robert Mulliken, but...

KING: This is the Hund of Hund's Rule?

HERZBERG: That was even before. Hund's Rule refers originally to atoms. At that time, 1928, Hund published his papers applying what he had studied in connection with atoms to molecules and he had the concept of molecular orbitals (15), but he didn't use that word. It's a strange situation that Mulliken, who was well aware of Hund's work and used Hund's work, was the inventor of a good expression for this concept. Sometimes in science, it's useful to have such a word; it's easier to work on a subject if you can refer to it in brief terms. That is what happened to molecular orbital theory. Of course, Robert Mulliken then developed molecular orbital theory a great deal.

KING: Was he ever at Göttingen?

HERZBERG: I think he was at Göttingen at one time but I wasn't there then. I met him for the first time when I was in Bristol.

KING: This leads me on to the next question. Lennard-Jones visited Göttingen in 1929 and he invited you to spend a year at the University of Bristol. This was your first extended stay in an English-speaking country. Did you find the language a problem to your research?

HERZBERG: Yes and no. I had fairly good English instruction at school and in fact I thought I was doing fairly well with my English because there were many American and English visitors coming around. However a few days after I came to Bristol, there was a Faraday Society meeting. The first speaker was O. W. Richardson, a very distinguished physicist, and he also worked on molecular hydrogen, a subject I was very much involved in, but the trouble was that he mumbled, and I couldn't understand a word of what he was saying. I began to wonder whether I would ever be able to understand English. The second speaker at the same meeting was Professor C. V. Raman from India, who the following year received the Nobel Prize for his discovery of the Raman Effect, and I could understand every word he said, so I became a little more confident. Actually, there were never any great problems, and it did mean that when I had to leave Germany in 1935, I had sufficient background in English so that I had absolutely no difficulty. I could start lecturing the first day I was over here.

KING: The majority of the research papers published at that time -- were they largely German?

HERZBERG: I would say it was about fifty-fifty at that time.

KING: So in fact you had to read a great deal of English?

HERZBERG: Oh yes, already then. Of course, nowadays the situation is completely switched over to English and it's comparatively rare that I have to read a German paper. English is now 65 or 70% of the published literature in physics or chemistry.

KING: After all this time do you find it easier to think in English or in German?

HERZBERG: English. If I go to Germany now I try to avoid, not always successfully, lecturing in German. It somehow doesn't come out as fluently as in English.

KING: You wrote that in 1929 you found the Bristol physics department very well-equipped for your purposes (1). I thought this was very interesting because you had just gone from a very good university where the equipment must have been really excellent.

HERZBERG: You mean in Göttingen?

KING: Yes.

HERZBERG: Yes, although I would say that certainly at that time the Bristol physics department was much better supported financially than the Göttingen physics department. But on the other hand, there were some very powerful minds working in Göttingen. And I don't think the Bristol people could quite match that. You were asking about Lennard-Jones, shall I answer that now?

KING: Yes, please do.

HERZBERG: Lennard-Jones was another one of those visitors that came along at the time of my stay in Göttingen. He was a theoretician and interested in molecular orbital theory. I was just then in the process of writing a lengthy paper on molecular orbital theory from a more experimental point of view. We had lots and lots of discussions at the time and indeed Lennard-Jones was preparing a summary paper on the subject for this very meeting on molecular structure that I was attending during my first few days in Bristol. If you compare our two papers there's quite a similarity in their points of view (16,17). Certainly Lennard-Jones had some good ideas. He was a very clear writer. I enjoyed the contacts with him but still I didn't pursue my theoretical work all that much. I did have one opportunity to talk with Hund, whom I mentioned before, and he was aware of my work, but in a way he wasn't because in his next paper (18), he omitted a reference to my paper which was published by then. When it was pointed out that one particular idea that he had stressed was actually in my paper he was very strong in his apologies and in his next paper made very generous amends of his oversight (19). I have just written to Hund on the occasion of his 90th birthday. He's still active in Germany.

[END OF TAPE, SIDE 1]

A friend who came to visit us last week had recently been to

Copenhagen to celebrate Bohr's centenary and Hund was there. He was sitting right next to him and he was amazed at his vitality. So Hund was certainly still around and still very much interested in physics. He had become interested in the history of physics.

KING: That's not surprising. There was something I wanted to ask you which is somewhat connected to what we were talking about. This is about advances in instruments. How has this altered or affected your work? Would you like to talk about this?

HERZBERG: Yes, I don't know whether I can be very explicit, but it is obvious that advances in instrumentation are very important for the progress of science -- physics, chemistry, or any other science for that matter. In my own field, when I was in Darmstadt as a graduate student and later as a junior member of the faculty, until about 1932 or so we had only prism instruments to do our spectral work. These are certainly not as good or have the resolution of grating instruments. It was only then, in 1932 or thereabouts, that we finally got grating instruments and it made a great deal of difference to my work because from that time on I was able to study the fine structure, the so-called rotational fine structure, of molecular spectra and that was a first step. Then gratings were improved, photographic plates were improved; that was important for my work, particularly in connection with the study of the absorption spectrum of our atmosphere and the discovery of so-called forbidden transitions in oxygen. Also together with Dr. Spinks, who was in Darmstadt throughout 1934 studying the rotational-vibration spectra in the photographic infra-red, where I think we made some fairly nice progress. Nowadays, of course, everything, not everything, but many things are done by lasers. I haven't myself done very much directly with lasers, but I'm using modern infra-red instruments which use laser techniques. I think it's fair to say that the improvement of instrumentation is an important factor in the development of science. Of course, the development of science by itself improves instrumentation. It's an interplay between the two. Many physicists and chemists have contributed to the development of modern techniques, so it's a very important interrelation between these two things.

KING: Have you at any stage of your work found that you were prevented from pursuing an idea because of lack of a certain item that either was not available or just did not exist?

HERZBERG: Well, I might say that when I first came to Saskatoon there was very little equipment there and there was only one reasonable prism instrument, a Hilger instrument, available. During the first two or three years I was there I couldn't begin to do the kind of work I wanted to do, until, in about 1937 or so, I got a grant from the American Philosophical Society in

Philadelphia. The grant was the magnificent sum of \$1500 and that allowed me to get a grating, and to build a grating spectrograph with it. From then on I could handle in Saskatoon spectra that allowed me to discuss rotation of molecules and things of that sort. That was one occasion. Here in Ottawa we haven't really had this problem. Up to now, up to now I emphasize, funding for basic science has been quite generous. Instrumental problems haven't really held us up, except for those occasions where the instrument didn't yet exist and it had to be developed and thought about.

KING: Could we go back to the chronology of the events in your life now You returned to Darmstadt at the end of 1929 and got married.

HERZBERG: I went to Darmstadt in 1930. It was while I was in Bristol that I got married.

KING: I see. Your wife [Louise Oettinger] was also a physicist and I believe had a Ph.D. in spectroscopy?

HERZBERG: Her degree came just shortly before we left on account of the Nazis and was based on work that was done in Darmstadt under my direction, but since we were then married, I wasn't the official supervisor. The Ph.D. examination was actually held at the University of Frankfurt, where there was also a very distinguished spectroscopist [K. W. Meissner], but interested entirely in atomic spectroscopy. Later on he became a good friend, who came to this continent also because he had the same trouble as I; a Jewish wife. So my wife's degree was from the University of Frankfurt but if she had postponed her submission just a few months she wouldn't have obtained her degree because of the Nazis.

KING: I wish there was more time to talk about her work. You've often said that her work helped a lot in your early days.

HERZBERG: She helped both in the scientific work and I did many joint papers with her, but also she helped a great deal with the first three volumes of the series on spectra by drafting some of the figures and tables and things of this sort.

KING: At the end of your time in England, what were the things that had most influenced you? Had you traveled outside Germany before then?

HERZBERG: No, that was my first trip outside Germany.

KING: Were you surprised by anything?

HERZBERG: I was certainly taken by the many great differences between the English attitude and the German attitude to things in general. Whether it's politics or polite manners, most everything is different. On the whole, the differences are not all that great. For example, at that time, we never had afternoon tea in Germany, but in England that was a very consistent routine, having this tea. At Bristol, they had a mail clerk look after the tea. English tea is something very special when you're not used to it. That was one of the lighter things. [laughter]

KING: I guess the custom of afternoon tea was one of the better English customs.

HERZBERG: It is, yes, if it doesn't go too far. I heard of one difficulty that arose with a German refugee and his wife. This German refugee married an English girl -- the mother-in-law was not very well and they brought her up tea with various things, but they bought the wrong things and that just wouldn't do -- it broke the good relations between mother-in-law and son-in-law. [laughter]

KING: I think it's not so serious now.

HERZBERG: No, it's not so serious now. It's strange how some customs are so firmly established.

KING: I believe that Rutherford had some of his best ideas during these afternoon tea sessions. So in 1930 you returned to Darmstadt and began teaching. Was this a good experience, looking back?

HERZBERG: I think so, yes. I worked fairly hard to prepare my lectures. It didn't come easy to me but once I had prepared them I enjoyed giving them and I learned a great deal about the subjects that I was teaching. Of course at first I was a Privatdozent, which means I was a private lecturer. As such, I could give lectures of my own choosing but no regular lectures. Then when the Nazis came, the first year, one of the older members of the staff of the physics department who was Jewish, who had been teaching theoretical physics was relieved of this teaching and I was asked to substitute for him. That was certainly good training for me because I hadn't really studied theoretical physics all that well during my years as a student in Darmstadt because there was either nothing or there was only this old Jewish professor who was a little stuck in the early years of

physics and he didn't follow contemporary advances. He was sent to a concentration camp eventually but Professor Rau got him out of that under the condition that he would immediately leave the country and he went to England. At one stage his permission to stay in England wasn't extended and when he got the word for that he had a heart attack and died.

KING: What was his name?

HERZBERG: Baerwald. He was a good soul but he was not a great scientist.

KING: I guess now I'm repeating a question that I asked you earlier about your student days at Darmstadt. Now you're a lecturer and you're more mature and you've met people like Schrödinger, Born, Franck, and Wigner; do you recall this as something of a golden age in physics or do you think all this glow and excitement that people associate with that era was observed only in retrospect?

HERZBERG: I think it's quite true that in retrospect it seems more golden than it actually was. But still it was an exciting time, there's no question about it. Particularly the years 1926 to 1930. It was only in 1932, if I remember correctly, that heavy hydrogen was discovered by Urey. That was a major discovery for both chemistry and physics. When Dr. Spinks came over to work with me in 1933 to 1934, heavy hydrogen had just been separated and we set up an apparatus to produce our own heavy water. We were only partially successful. We got some, but by that time you could buy it, even though at a price, and it was much less troublesome to buy it than to make it oneself. All this went on and Spinks went around Germany and Austria to visit some labs and came across a physical chemist in Vienna by the name of Patat who was very good at preparing compounds containing heavy hydrogen and on that basis we produced work that we would otherwise not have done. So everything worked together and it was an exciting time, there's no question about it.

KING: You also met Edward Teller at this time.

HERZBERG: It must have been around 1932. Teller was a very bright young man at the time -- he's still bright but he uses his brilliance for purposes that I don't quite approve of. I had quite a lot of contact with him after first meeting him at a meeting in Leipzig, where he was just getting his Ph.D. under Heisenberg. He was interested in molecular problems and I had some experimental knowledge on molecular problems and we exchanged those two, and eventually, in 1934, wrote a paper (20). This was all Teller's and all I did was to be the midwife in

getting it out of him -- in writing it down and so forth. But he insisted that the authors would be named in alphabetical order, which put me first. I wanted him to be the first author, but he didn't want to hear of it, an indication that he was in many ways a very modest man. I met him again a good deal during our first years on this continent -- he also came about the same time and went to George Washington University in Washington, DC. At one time I actually stayed with him there. He often, in discussions with other people, had a very bright idea about what to try but he would never insist that he should be a co-author. I don't think personal ambition is what drives Edward Teller. I certainly have the highest respect for his scientific ability. I spent half an hour with him three years ago in Livermore and we didn't talk about politics, so we got along very well.
[laughter]

KING: Have you ever discussed politics with him?

HERZBERG: I did at one time and this is the strange point. When I was in Chicago between 1945 and 1948, Teller was in Chicago and he, as well as Mulliken, wanted to persuade me to stay at the University of Chicago. The point I want to make is at that time, I don't exactly remember whether it was 1945, 1946, or 1947, Teller was traveling throughout the country lecturing on world government; he knew all the answers to all the objections on world government and all that and was a very convincing speaker. He traveled on that theme so much that the faculty was a little worried that he was neglecting his duties as a professor at the University of Chicago. So he was in a way quite a different man from now.

KING: Did you ever talk to him during...

HERZBERG: During that time I talked to him, yes. He convinced me that world government is our only way out.

KING: Very interesting. I wanted to ask you; during the time that you were in Darmstadt as a lecturer, did you meet some of the chemists who were working in Germany at that time -- Nernst, Bodenstein, Haber, Polanyi...?

HERZBERG: Yes, Polanyi senior you mean?

KING: Yes, Michael.

HERZBERG: Yes, indeed I did. All those that you mentioned. The person who made the greatest personal impression on me was Karl

Friedrich Bonhoeffer. I met him at a meeting in around 1928, when I was still in Darmstadt. I had written a paper about a subject that he had also written about (21,22) and I had criticized him, like eager young men do, but he didn't hold it against me at all and he was willing to discuss the problem.

KING: Do you remember what that paper was about?

HERZBERG: The afterglow of nitrogen, which at the time was quite a puzzle and for many years remained a puzzle. It's a very striking phenomenon and whoever sees it is greatly tempted to investigate it further. When you send a discharge through nitrogen you find when you turn it off that it glows with a beautiful yellow-golden color, but that's a different matter. That goes back to about the year 1900, when it was studied by Lewis (23) and then studied further by Strutt (24). Anyway, it's a striking phenomenon and both Bonhoeffer and I were interested in it. At this first meeting I found him to be one of the most genuine people that I have ever met. I continued my acquaintance with him when he became a professor at the University of Frankfurt, which is close to Darmstadt. So all through those years between 1930 and 1935, I would see Bonhoeffer certainly once a year, if not more often. He not only was a very fine scientist -- the discoverer of ortho and para-hydrogen -- but he also was an extraordinarily fine person and he was certainly one who stood up to the Nazis as well as anybody. Of course, you are familiar with his brother Dietrich Bonhoeffer, who was a minister and who was executed by the Nazis.

KING: Did you ever meet him?

HERZBERG: No, I never met Dietrich.

KING: Once before when you were talking about this, you said that when you later met Bonhoeffer here in Ottawa he didn't talk about his experiences in Germany.

HERZBERG: No. He came to Ottawa... In fact, was there anything about that in the letter that Steacie wrote? I'm not quite sure but anyway, when I looked for these letters that you had asked for, I found a letter in which Steacie mentioned the fact that Bonhoeffer might be coming here and that he would send me a copy of the letter to Bonhoeffer indicating that I would be very much interested in seeing him. He did come and he actually stayed with us.

KING: So you remembered him to have changed very much physically, but he didn't talk about his experiences?

HERZBERG: No, we didn't. But I knew, I don't know whether he told me that himself, but he was always aware of what was happening. His brother Dietrich was involved with this putsch to get rid of Hitler and Karl Friedrich was aware all along what was happening. A very fine family, the father was a very famous psychiatrist at the University of Berlin. Karl Friedrich was one of three or four people whom I consider as the most genuine people and for whom I have the most affectionate regard.

KING: This makes me want to ask you at this stage about the work that chemists were doing at about the same time that we're talking about -- your years at Darmstadt. They were having a very difficult time with this whole concept of free radicals -- proving their existence and establishing their role in reaction mechanisms. Do you remember much interaction between chemists and physicists generally?

HERZBERG: I think there was always a good deal of interaction between physical chemists and the physicists who worked in the molecular field. The physicists pretty soon went off to nuclear physics and elementary particles and left the field of molecular studies. Not completely, but after that the interaction didn't need to be very strong because the physical chemists had taken over. Of course, that is one reason why I didn't get the [Nobel] prize in physics, but in chemistry. But the interaction was always there. I do recall, as a student in Darmstadt in 1925 or thereabouts, there was an international meeting in physical chemistry, I don't know what it was called, but two or three of the most prominent physical chemists were there. One of them was Arrhenius, who I think was one of the first to get the Nobel Prize in chemistry, and the other was Paneth, who was the first to produce the methyl radical. They gave lectures but I have no very clear recollection of the content. I seem to remember having met Arrhenius, just very briefly. I do remember that Paneth gave a lecture, but I don't remember actually having met him at that time. Of course, I remembered the lecture when I worked on the spectrum of the methyl radical.

KING: I would like to pursue this a little bit later on. If we go back now to the chronology of your life. Two events which were oddly connected occurred in 1933. You had a visit from a young Canadian physical chemist, John Spinks, whom you've already mentioned, and who played a role in bringing you to Canada, and also in that year the Nazis came to power. Could you say a little bit about your life at this time and decision to leave Germany?

HERZBERG: To most intellectual people, the advent of the Nazis was a terrible experience in view of their fantastic ideas about the Jews and all that. I remember, for example, at the beginning of the Nazi regime in April 1933, it was just the time when I was

collaborating with Edward Teller on this joint paper. Then it appeared in the newspapers that James Franck had resigned, although he was not directly affected by the Nazi legislation because he had served in the first war on the front, and that was a condition that he would be exempted from being fired, but he said that he didn't want to make use of this exemption. What about his children? They would be considered as second class citizens in the country, so he resigned and soon afterwards he left the country. I remember writing a very strong letter to Edward Teller about what I felt about this. Edward Teller was also affected, but for a while I myself was not affected more than by what had happened at the University. The anti-Jewish laws of the Nazis became worse and worse as time went on and in 1934 it became pretty obvious that people who had married Jews would also be in trouble. So I began to look out for some opportunity but didn't really get very far.

The point was that when the first wave of refugees came out, they naturally got most of the jobs that were at all acceptable and the people who left later had to put up with what remained, which wasn't very much. I was lucky from two points of view. First of all, John Spinks was working with me. He had come to Darmstadt to work with me for a year, and when he left to return to Canada, to Saskatoon, I had impressed upon him the need for me to leave and that therefore he should try to see if there were any opportunities. He tried, for example, to persuade the people in Toronto that they should give me a place to work and live, but their position was that they had many people who had taken their Ph.D. in Toronto who didn't have jobs and they were their first responsibility so they couldn't find any job for me. When Spinks came back to Saskatoon, at the end of 1934 that must have been, he made a little bit of propoganda on my behalf there. Then it happened, and that is the second piece of good luck, that the Carnegie Foundation of New York established a fund specifically for refugees who would be able to go to Commonwealth Universities, that is Australia, Canada, or a few other places, and that they would offer two year's salary to the university who was willing to take them. The salary was \$2250, if I remember correctly. It was with the understanding that the university at the end of those two years would seriously consider whether they couldn't offer this man a more permanent job. I pointed this out to Dr. Spinks and he pointed it out to the president of the university [Walter C. Murray]. The president of the university had tried to get me into Toronto, where there was a lot of spectroscopy going on, when this didn't have any result, he was willing to take me on. So I could go to Saskatoon for two years.

When I came there one of the staff members was on leave, and it turned out after a couple of months that he was resigning. He had taken a job in Glasgow in Scotland, where he came from, and so there was a position open. They had tried me out for three months and the president offered me the job. So I was well set, I didn't have to worry about the future any more. With these pieces of luck, I was all set. Of course, there was not much in the way of equipment in Saskatoon. We didn't even give a Ph.D.

degree at the time at the University. Students were limited; they left after they got their Master's degree, if they stayed that long, so there was not much help from the students in doing research. Anyway, everything turned out rather well and I look back with considerable nostalgia to those days in Saskatoon.

KING: I'm interested to know something. You've just been talking about how you came to Canada. During your year in Bristol you visited a number of other universities and made a number of contacts with British scientists like Blackett, Oliphant and Cockcroft. Many German scientists ended up in England during the war. Did you ever consider going to Cambridge, for example, or...

HERZBERG: Well, I would have loved to go to Cambridge but no offer was forthcoming. At the time I visited Cambridge, that was in 1929, there was no reason for me to angle for a job in Cambridge and after spending a year in Bristol I was pretty sure they wouldn't want a German physicist or spectroscopist in Cambridge -- it didn't seem worth trying. Then, I didn't really intend to leave Germany.

KING: In fact, when the time came in 1933-1934, the connections really which loomed very large were with Canada.

HERZBERG: I had one or two other possibilities because there were two organizations that looked after refugees. There was one in England -- the International Academic Assistance Council... I can't remember the exact name, and there was a group in Zürich called Notgemeinschaft Deutscher Wissenschaftler im Ausland, which was a very similar organization. During the Nazi time I didn't go to England but I did go to Zürich to find out what could be done and they had been in touch with me. Indeed, this information about the possibility of a grant from the Carnegie Foundation in New York came to me via one of these two organizations, I can't remember which one, but they also offered me two other jobs. One job was in the Soviet Union, and I'm quite happy that I didn't bite for that one. During my Göttingen year I met a Russian physicist by the name of Rumer, a very interesting person; he was a theoretical physicist and a friend of Heitler, they wrote a couple of papers together (25). Through this connection I was invited to go to Kharkov in the Ukraine in about 1932. I was to go in 1933 to Kharkov but when the Nazis came this immediately fell by the wayside. There was the possibility of a job in Kharkov as a refugee, but not many of the refugees really lived through those times; although some of the refugees went there and I remember one very distinguished spectroscopist who went to the Soviet Union and was never heard of again. So, that was one. The other one was in Ghent, Belgium which was a small university. I went there to be interviewed and I found it quite nice, but when this affair in Canada arose, it

seemed...

I don't know whether I realized then that Belgium would be invaded by the Nazis, but at any rate, the opportunity was certainly better. In Ghent, I would have had a very low assistantship, so I didn't consider it further when the Carnegie award came through.

KING: You decided to take up the Canadian offer and in 1935 you and your wife left Germany to go to the University of Saskatchewan in Saskatoon. You managed to take some spectroscopic equipment with you. This was very far-sighted, please tell me some more.

HERZBERG: At the time, it was impossible to take more than ten marks, which at that time was \$2.50 per person. But you could take furniture or other things. We didn't have any furniture because we had been living in rooms but I had saved some money and also had got some money from my father-in-law, who was still in Germany at the time, and we could buy some equipment, which we did. That was used to build an instrument in Saskatoon. Not a very big instrument, but, still, it was useful to have at the time. The total value of what we took out was perhaps something of the order of certainly not more than \$500, 1935 dollars. I think it was less because we really didn't have that much money. I'm not sure what happened to the instrument that we built, it must still be in Saskatoon.

KING: In 1935 did you have to leave and pack in a great hurry?

HERZBERG: No. I didn't perhaps say explicitly that in 1934 the University told me that my contract wouldn't be renewed. I was earning a living as an assistant, I was not earning much as a Privatdozent, that was not worth talking about. This assistantship was discontinued as of October 1935, so I had to find something. But when this Carnegie Foundation guest professorship, (because it was not a permanent job they called it a guest professorship), so when I told the authorities I was going to Canada as a guest professor they thought it was good for Germany.

[END OF TAPE, SIDE 2]

HERZBERG: Of course, all our baggage had to be investigated but the particular customs officer, or whatever he was, took it very easy. There was no comment about taking a few pieces of scientific equipment along, particularly since I was going on a "guest professorship" and spreading German culture, German habits, German science to other countries. [laughter]

KING: At that time there were no restrictions?

HERZBERG: Except that we were tied by this constraint of ten marks. Fortunately, the university in Saskatoon had arranged that we would get some money when we landed. Naturally we crossed by boat.

KING: Who paid the fare?

HERZBERG: We could pay the fare. Indeed, we paid for a return fare. We may even have thought that in one or two years this whole thing would be over, but also to have something that we could possibly sell later. So we paid for the return fare. That was possible still.

KING: Did your brother stay in Germany?

HERZBERG: My brother stayed in Germany, yes. He was not affected. It didn't go that far; a sister-in-law wouldn't count as an argument against you.

KING: On your way from Germany to Saskatoon, you stopped off at Princeton?

HERZBERG: Oh, yes. We paid in advance not only for the boat trip to New York but also the whole trip from New York to Saskatoon. That much we could do with the German money we had.

KING: You stopped off in New York and then visited Princeton?

HERZBERG: Yes. I visited Princeton, Chicago, and on my way I also visited McGill University. Oh, yes; I also visited the University of Illinois because there was a very prominent molecular physicist by the name of F. W. Loomis who had invited me to stop by. I don't really remember all the places I visited but there must have been about a dozen places on the way, because I didn't think I'd be able to come back that soon. With a modest salary it wouldn't be so easy to travel such long distances.

KING: This was your first trip to the United States?

HERZBERG: Yes.

KING: Can you remember any of your first impressions?

HERZBERG: Not really. Everything was so different. Again, it's different in Canada from what it is in the United States.

KING: So now, you've arrived in Saskatoon, Saskatchewan. Your years there were very productive, despite the obvious lack of equipment. Is there anything now, looking back, that you would like specifically to recall?

HERZBERG: The university, which was a very small university at that time -- there were only some 1200 students and a hundred faculty members. Of course, you met all the faculty members very shortly after you arrived, so it was a very personal thing. They were very friendly and tried to make things easy for us. I look back on the days in Saskatoon, as I say, with great pleasure and it was a nice time; if only the war hadn't come.

KING: Was it difficult to be a German scientist during the war?

HERZBERG: Not in Saskatoon, no. Not at all. At first, I had to report to the police once a month, but it was very friendly. Later on, I actually did some war research work on explosives, for which I had to travel to the United States. For that of course I needed a passport or something like it and my German passport wasn't very useful, so they provided eventually a special document in lieu of a passport and then I could travel to the United States and go to various meetings. There's one interesting occurrence before the United States entered the war. I was at a meeting in Chicago, I think it was, traveling by rail, of course. Then when I wanted to return to Saskatoon, the Nazis had just invaded Holland and Belgium and the American government had decided to close the boundaries of the country against all aliens. When I tried to go back to Saskatoon I was taken off the train on the way between Minneapolis and Winnipeg as not being eligible to leave the country. The particular immigration officer was a very kind old man. It took about three days to get this straightened out. This old man said that he wouldn't want to restrict me by putting me in proper cell, but if I were to escape and cross the border, by just walking across, he would lose his job, so naturally I didn't attempt to.

KING: So where did they put you during those three days?

HERZBERG: They had two or three rooms in the border office of Immigration and Customs Service where they could lock up people, but they didn't lock me up. [laughter]

KING: So, during the war you were not forbidden to make contacts with other scientists anywhere in the United States.

HERZBERG: No, not at all.

KING: In effect, you carried on with your work at the university -- teaching, research and so on.

HERZBERG: I had more of a teaching load at that time because some of the regular faculty members were involved in teaching younger military people radio and things of that sort. Indeed, I was also used because of my knowledge of German. During the later part of the war, they had to have people to send over to Europe who knew some German. The strange thing was that there were many people of German origin in Western Canada, who wanted to go into this kind of job and I had to examine them. I found that some of these people of German origin had German that was not as good as some of the others who had really studied the language. So I had something to do with that also.

KING: In the meantime, did you get any news from home?

HERZBERG: No, I didn't get any word from my brother and his family during all those years.

KING: Just a very brief mention of some of the work you did at Saskatoon during these years. I was very interested in the discovery of bond shortening in methyl acetylene.

HERZBERG: Oh, yes.

KING: And Pauling evidently didn't believe you.

HERZBERG: That's right. This was really some work that was done just before I left Germany. I think John Spinks was involved also, and Patat I mentioned earlier. I contributed this paper to a meeting in Princeton (26), actually.

KING: Pauling was there?

HERZBERG: No, Pauling was not there, so there was no argument about this paper. Later on I found out that Pauling hadn't believed this result and had someone whom I knew from Göttingen, R. M. Badger, to repeat the experiment. Sure enough, he got the

same results. Then Pauling believed! [laughter]

KING: Did you correspond with him on this?

HERZBERG: I didn't ever correspond with Pauling over this, no. I corresponded with Pauling more recently in connection with some of the Soviet dissidents and all that.

KING: I guess Pauling's book hadn't appeared then (27). Another point which comes to mind is that your work around this time appeared to concentrate more and more on the application of spectroscopy to astrophysics. In a way, your earlier interests were now being combined. Did you engineer this or was this a natural development?

HERZBERG: I think it was a natural development because I was interested in astrophysical problems. People knew that and they sometimes came to me with spectroscopic problems. One of the most significant was when Professor Swings of the University of Liège in Belgium asked me what I thought of a spectrum (the 4050Å group) that appears in comets (28). I looked at the spectrum a good deal and tried to find what it could be. At that time I was writing volume II (13), so I thought I was clever in saying that this 4050Å group was due to CH₂ (29), which in fact it wasn't. It was only much later here in Ottawa that I observed the real spectrum of CH₂ (30). The spectrum to which Swings had called my attention was later identified as being due to triatomic carbon (31). But the other astronomical result, which was at that time, at any rate, more important was the identification of a number of sharp lines observed in interstellar space. We had a meeting actually at the Yerkes Observatory, this was 1937, and I attended this meeting. Edward Teller and Robert Mulliken were there; Struve, of course, and I think Swings himself was also there. We debated what these lines could be and Mulliken had suggested CH₂, which was incorrect. Teller and I got together in discussing this and we decided that it must be CH⁺. At the time when I came back from this trip to the United States I had a very good graduate student, Alex Douglas, and he had just the right kind of apparatus ready. He introduced some benzene vapor, and within two days we had the proof that the spectrum was really due to CH⁺. For many years that was the only molecular ion that had been observed in interstellar space. Since at the time, only two other molecules had been seen, CN and CH, it was a fairly significant contribution to the development of the subject of the interstellar medium. It was on that score that the director of the Yerkes Observatory offered me a job there.

KING: This is in 1943?

HERZBERG: Yes, the offer came in 1943, but at that time I couldn't leave the country because of manpower regulations -- scientific people couldn't leave the country. When the war ended I got permission to leave, so I took on that job.

KING: So that was in 1945. You weren't there very long, 1945-48. What do you think, looking back, was the most productive outcome of your stay there?

HERZBERG: For the three years? The most important undoubtedly was the observation of the quadrupole spectrum of hydrogen. In the Yerkes Observatory I had built a long absorption tube of a type that had not been built before. I had one great advantage. At the Yerkes Observatory there was an optical technician, Fred Pearson, who formerly had worked for Michelson, the famous optical scientist. Pearson prepared a set of mirrors for me that allowed me, in a tube that was 75 feet long, to send light back and forth up to 250 times. So I had a very long absorbing path. With that I studied a number of other molecules, but the most important one from my point of view was the study of molecular hydrogen. I still feel proud of the fact that before I left Yerkes, I did eventually observe a number of quadrupole lines of hydrogen. This became rather important much later in astronomy.

It was an astronomical problem that had stimulated me to try to find these quadrupole lines. That was the detection of hydrogen in planetary atmospheres, the atmospheres of Jupiter and the outer planets; it had long been suggested by astronomers that there was a lot of hydrogen, but nobody had been able to prove it. My work was going to prove it but it took ten years after my observation of the quadrupole spectrum in the laboratory before the astronomers obtained some of the quadrupole lines in the atmosphere of Jupiter. In addition to that, many years later, quite unexpectedly you might say, the quadrupole emission spectrum of hydrogen was found in interstellar space, for example in the Orion molecular cloud, because in this cloud there are shock waves which excite hydrogen. The density is so low that the time between collisions becomes of the order of the lifetime of the molecules in the excited state, not quite that but at any rate, long enough so that one can see this quadrupole spectrum. The first time it was observed was about 1975. It gave me a great deal of pleasure even though I had nothing to do with observation; I was still the first to have observed, in the laboratory, the quadrupole spectrum of molecular hydrogen.

KING: I'm very familiar with the next event in your life which was that in 1947 you received an invitation from the National Research Council in Ottawa, specifically from E. W. R. Steacie, to set up a spectroscopy lab with all kinds of enticing promises regarding equipment and promises of minimal administrative duties. How did you feel about returning to Canada? Were you pleased at the prospect?

HERZBERG: Yes, I was certainly pleased at the prospect. There were a number of things that I didn't particularly like in the United States. You might say that one item that gave additional impetus to my desire to go back was when the Office of Naval Research [ONR] was formed soon after the war and applications were invited from university people for grants for research. The Director of the Observatory suggested that I should apply but it turned out that my application was turned down. Many other people did get research grants from the Office of Naval Research and still do. Well, I began to ask myself whether I was in the wrong place. This was not the decisive point by any means but generally I felt that, while I originally wanted to be an astronomer, to live only among astronomers was not really quite what I had expected. I needed the contact of chemists and physicists. Of course I could have gone to the campus of the University of Chicago. [Yerkes Observatory, part of the University of Chicago, is situated in Wisconsin.] I was invited to do so, but life in Chicago didn't seem to me very attractive - it still doesn't. In fact I still wonder why my friend Takeshi Oka, who was a member of our staff for ten years, finally chose to go to the University of Chicago. The point of course is that there are some very distinguished people at the campus, very bright people to be associated with, and that's all very nice, but living in Chicago is not what I would like to do. Of course, my American friends think, "How could I possibly go back to living in an igloo?" [laughter] It was almost expressed in that way.

KING: We should also mention that your two children were born in Canada, so were in effect Canadian.

HERZBERG: That had an effect also, but perhaps the strongest impression that contributed our going back to Canada [was the following]. When we lived in Saskatoon throughout the war, there was no black market, absolutely none that we could detect. The moment we came down to Williams Bay where the observatory is sited, a small, small place, there was a black market all over the place. That somehow upset us and that had a fair amount to do with going back to Canada.

KING: What about the kind of equipment you had at the observatory, did you have everything you wanted?

HERZBERG: That was in a way another problem of course, everything had to be obtained -- the observatory budget was not all that strong, but I did build up a spectrograph and a spectroscopic laboratory, and I did fairly well, but there were limits. That's why I applied for funds from the Office of Naval Research which didn't work out, so I would now have to try some other place to get funds. One of the really attractive things at

NRC at that time was that I didn't have to make any application for research funds, they were already there.

KING: We come to the next point about your return to Canada. You came to the National Research Council in Ottawa, and Dr. Steacie, who subsequently became president of the NRC was a great believer in the freedom of research and a minimum amount of bureaucracy. Just for those who have never heard about Dr. Steacie's methods achieving these in a government laboratory, could you say a little about how this philosophy affected your own methods of working after you became Director of the Physics Division, which was in 1949?

HERZBERG: It certainly did because he was, as you said, a great believer in the freedom of the research worker to do what he wants to do, to come at hours that he wants to. If someone insisted on working at night, that was perfectly all right with Dr. Steacie. The one thing I learned from him very, very strongly was that if I wanted to run a good physics division, I had to give my staff the freedom to do what they wanted. A director of such a division is not there to direct the people; as Steacie said, "To find good scientists and then let them do what they think is best." This philosophy is very hard to sell to our politicians but it's the only philosophy that really gives all the possibilities to a creative individual that could lead to very striking developments. The moment that the government or the organization prescribes what a good, creative person should do, then he takes the next chance at leaving. We had one such person -- Dr. Oka, one of our most creative scientists. Well, nobody told him what to do. There were other reasons, he wanted contact with students and this sort of thing, at least that's what he told me. The leaving of one creative person from a lab is a very serious affair; if you make life unpleasant for people working on basic research then you lose the background and the initiative. Creative work in a subject is the only kind of work that will do the later applications any good. I'm not suggesting that you should do basic research only on account of the probable applications, I think that's wrong. You do basic research for the same reason you write poems, compose music, paint paintings.

KING: This prompts me to ask you something which is connected about events that you hear at the NRC. Originally the physics division had both a pure and an applied section, which in 1955 were deliberately separated into two divisions, again as a direct result of Dr. Steacie's personal philosophy. I remember you saying that you were personally against this break between pure and applied research. Would you care to elaborate?

HERZBERG: My feeling was that there was enough exchange between the people in pure and applied physics and that was something that should be maintained. What actually happened was that plans were drawn up for a new building for applied physics only and I

wasn't told about it and I was supposed to be director of the whole physics division. At that point I went to Dr. Steacie and told him, "Well, this can't go on, either I am the director of the division or I'm not." He immediately saw the point. I don't know whether I should blame it on Dr. Steacie but even great administrators make mistakes. I feel this is one mistake that Dr. Steacie made. He let this happen without informing me. It shouldn't have happened at all. There's enough contact and that contact would be lessened if the two subdivisions were separated in space.

KING: That's certainly a puzzling aspect of Dr. Steacie's presidency. The other thing that I wanted to ask; Dr. Steacie instigated the tradition of having postdoctoral fellows here at the NRC, in 1948. This brought fellows from all over the world to Ottawa, so instead of the graduate students one would have had in an academic institution, more senior workers, already qualified, came. How did this affect your own work?

HERZBERG: I think it was an extremely good idea. When I took over the physics division we immediately started with the same system. I would imagine we had something like a hundred post-doctoral fellows in the course of these years. They certainly contributed a great deal to the work of the division or the group with which they were associated. I would certainly do it the same way over again. But you could argue that students are more useful to a professor than postdoctoral fellows are to a research director. If a research director is sensible, he will not necessarily direct those postdoctoral fellows. If they are really good, let them do what they want to do; that's very good for the postdoc fellows but not for the director, he doesn't get any work done that way. But I still think, as I say, I would do the same thing over again because I feel it's more important that really good people are supported and that the not-so-good people are taken on and taught how to do good work. So I'm all for it but I want to point out that there are some aspects that are not all that good for the person who still wants to maintain, in spite of administrative responsibilities, his own scientific work. He needs a helper. I decided I needed somebody who works directly with me, and I chose one of our postdoctoral fellows, he was a very good man -- he was too good for the job. When I suggested a topic, he went ahead and did it. Well, it was no longer my work. I wanted to have some work of my own that I can say, "This, I did." That's the problem.

KING: This is an interesting point. When you had an idea for a specific problem, how did you overcome that obstacle?

HERZBERG: Something that helped me greatly was through Dr. Steacie again. He came in one day and said, "I have here a letter from Professor Allmand of King's College, University of

London. He has a technician who for family reasons wants to come to Canada. Allmand considers him one of the best technicians and asks if I would be interested." Dr. Steacie asked me, "Are you interested?" I was, and this man turned out to be a real winner. He replaced my hands, I never got my hands dirty anymore in the lab, unfortunately, I had to give up the pleasures of actually doing the experiment, but it was still my experiment because I did all the thinking about it. Of course, he had to do some thinking about how to carry it out, but it was still my experiment and I could, with good conscience, put my name to it.

KING: Just out of interest, how would you go about planning an experiment together?

HERZBERG: Together with another scientist?

KING: With your technician Mr. Shoosmith?

HERZBERG: I had some idea of what I wanted to do and how I wanted to do it and I presented this thought to him. He would have suggestions saying, "Well, this surely won't work, this might be better. Couldn't we do it this way."

KING: But he was not trained in spectroscopy?

HERZBERG: No, but he brought a lot of experience. I have a person like that now.

KING: So basically you would come up with an idea and you would present him with perhaps a rough drawing or something of what you wanted.

HERZBERG: And he would see how to get it through the instrument shop. He might have to make up a detailed drawing, this kind of thing. He would be the actual experimenter. I still remember the most important -- I don't remember whether I told you this story before -- about when I finally found the spectrum of methylene, which was one of the citations in the Nobel award. We had been searching for fourteen years and one day Mr. Shoosmith came up to me and said, "I have another spectrum downstairs in the darkroom, do you want to see it?" I said, "I'm terribly busy and I don't know whether I can." He said to me, "You must see it!" And that was it! He had realized that it was something. I had one look at it and I knew that that was it. That's how close we were.

KING: How many years did you work together?

HERZBERG: For twenty years. His time for retirement had come and he didn't want an extension. That was wise for him because he died of cancer soon after.

KING: To recall, now that you are here in Ottawa, the war was over. Did you find making contacts again with the scientists in Europe or anywhere around the world, did you find this an easy matter? Did things settle down quickly in peacetime?

HERZBERG: I think that was very easy as far as Europe was concerned, but a little more difficult with the eastern bloc countries. Even that became possible; I went to the Soviet Union a number of times.

KING: What was the greatest difference that you noticed after the war in your field? Did you notice a very distinct shift of workers from what was the center of science in Germany to the United States? Did you find a very definite change after the war, as a consequence of events that had taken place during the war?

HERZBERG: You mean that something had happened during the war that made them shift in their field or in their actual attitude?

KING: Well, both. What were the greatest changes that you noticed in science because of the war?

HERZBERG: I don't quite know what to say to that question. There were people who had been affected by the Nazis in some way concerned, I don't think there was any real difference except that in those five or six years science had changed. The first trip I made to Europe was in 1950, that is after two years here. I can't clearly remember all the places I visited. Certainly I was in England, Germany, and Sweden I think. Sweden at the time, and even now, is very strong in molecular spectroscopy so I had a very interesting time there. There were also meetings of the International Union [of Pure and Applied Physics], and I served on some of their committees. That was interesting.

KING: I didn't phrase the question very well. You said science had changed, and this is what I meant to ask. What were the changes that you noted? Was it in terms of equipment or people's attitudes toward things?

HERZBERG: You might say in terms of equipment. Just to take one example, during the war microwaves were developed. It was only after the war that the whole field of microwave spectroscopy was developed. People like Bright Wilson at Harvard and Charlie Townes and many others, who during the war were doing war research on microwave communications, now used their knowledge to do scientific work with it. I think that there was a tremendous stimulus to everybody in the field. There was only one paper that you could call microwave spectroscopy before the war, one single paper. All the rest of microwave spectroscopy, which is such a wide field now, was done after the war. Indeed, the development of the maser and laser originated from this. Anyone can see that applied techniques that are extremely useful, like the laser originate out of pure research. It originated if you like from microwave communications. Microwave spectroscopy was something nobody thought had practical application. Still something practical did developed.

KING: May I skip a few years. It was 1971 during your visit to the Soviet Union that you were informed that you had been awarded the Nobel Prize for chemistry. I've been told that when you heard this news, you said, "Oh, there must be a mistake because I'm a physicist." Is this an apocryphal tale?

HERZBERG: It's the other way around.

KING: Oh.

HERZBERG: When I was first told about it, I was told it was in physics, and that surprised me. I didn't explicitly say to the person who told me, "You must be mistaken." I didn't go that far. But I was surprised because if I had done anything of sufficient interest to be worth a Nobel Prize, it was on the borderline between physics and chemistry, and something that chemists were more interested in than physicists. I went all by myself in a day coach from Leningrad to Moscow that afternoon; it was the 2nd of November. It was puzzling; how could it be that the physics committee would give the prize? That just didn't seem possible. When I came to the end of the trip there was a message from the President of NRC congratulating me on the prize in chemistry. Then everything became clear.

KING: I'm glad we cleared that one up. Your association with the NRC has been very long and very happy. Am I right in saying that?

HERZBERG: Yes. Certainly.

KING: They created the Herzberg Institute in your honor. In 1984 for your 80th birthday, they brought together quite a galaxy of scientists to lecture in your honor. I'm glad they invited a few chemists like Henry Taube and Melvin Calvin in addition to all the physicists. Was this occasion kept a secret from you or did you know about it beforehand? Did you have a hand in choosing the speakers?

HERZBERG: I did not have a hand in choosing the speakers, no.

KING: Did you know about the occasion before?

HERZBERG: Yes, it was not sprung as a big surprise event, but I learned about it at a fairly late stage. I didn't have to worry about whom to invite. I think I was asked in one or two cases -- did I know this person, would I like to see this one -- but only one or two.

KING: Was this a very pleasant occasion?

HERZBERG: It was a most pleasant affair. The only trouble was the very day of the dinner, that was the second day of the meeting, the cuts for NRC were announced. That was not quite so pleasant.

[END OF TAPE, SIDE 3]

KING: Could we just mention a few of your other interests? You're very interested in music and also in mountaineering, not something you can do very easily around Ottawa.

HERZBERG: No. [laughter] I shouldn't perhaps call it mountaineering. Mountain hiking is a better expression for it. In the sixties, I had climbed three mountains in the immediate neighborhood of the place we go to on summer vacation, where we'll be going this coming July, which are more than 4000 meters high. So they're not trivial mountains, there was some climbing, some mountaineering in that you put on crampons and this kind of stuff. I don't do that anymore, but I'm still doing mountain hiking.

KING: Tell us a little bit more about your interest in music.

HERZBERG: I've always been interested in music. In fact, my brother was a musician. One of my friends in high school was quite a good piano player. They often played four-hand arrangement of Beethoven's symphonies. I kept my interest in

music and I learned to play the violin when I was a youngster, but I had to give that up when I started university work. It was only in 1945, after I finished volume II that I decided that I needed something that I can really do for pleasure, and that it should not be a chore, as writing a book is. I decided to take lessons in singing. The more I did so the more I enjoyed it, the more I enjoyed Schubert and Brahms lieder. Brahms I started fairly late in the game. I enjoy Mozart and Verdi, so I've done a fair amount without addressing the public that way. I've only taken part in one concert where people had to pay. [laughter]

KING: Really?

HERZBERG: That was here at the Jewish Community Center where we performed the Handel oratorio, Judas Maccabaeus.

KING: Was this here in Ottawa?

HERZBERG: Yes.

KING: Now that you've appeared as a public performer, did you enjoy the experience?

HERZBERG: Yes, I did. I performed also at a physics meeting in Vancouver where they wanted to give a concert by physicists and I took part in that together, with my friend the late Harry Welsh, who was a pianist, and I did some singing. I enjoyed that. The people from the Fifth Estate when they interviewed me insisted that there should also be some singing and I obliged them.

KING: Splendid.

HERZBERG: So that was public if you like.

KING: I won't keep you long enough for that tonight. [laughter]

HERZBERG: Earlier this afternoon I had a session with an accompanist: I skipped my lunch for that. [laughter]

KING: You never ceased your research after you retired.

HERZBERG: I never retired.

KING: Oh, you never officially retired?

HERZBERG: Not yet. I'm still on the full payroll of NRC.

KING: But you have a special title now?

HERZBERG: Yes, I have a special title.

KING: Which, in fact, dates from the time when you were supposed to retire.

HERZBERG: It goes back to the time when I would have retired if I had followed the rule. But this is one thing you might say that I got out of getting the Nobel Prize, that they never wanted me to retire; so far.

KING: Do you allow yourself a little bit more leisure time now?

HERZBERG: Slightly, perhaps. I don't work quite as hard for reasons of health.

KING: But you come to the lab every day?

HERZBERG: Yes.

KING: Something I've always wanted to ask people born on Christmas day; do you always end up just getting one set of presents?

HERZBERG: I was very upset when I was a small boy about this particular problem. It doesn't bother me now. [laughter]

KING: But it still happens?

HERZBERG: No, people are now more aware of it I think. As a boy it happened all too many times. I was really upset.

KING: As a scientist, you've had a very unusual role. Sometimes you've taken on the role of debating with senators and government ministers who are more inclined towards science policy than to science. This borderline between politics and science has made

your name very well known to people who are not perhaps interested in science as such. You've also been on the borders of political statement when you have spoken in support of Soviet dissidents and Soviet scientists.

HERZBERG: Yes.

KING: This has probably taken up a lot of time.

HERZBERG: More time than you might think because I'm not sufficiently articulate to express it succinctly and it's really necessary and it doesn't come easy.

KING: Have these developments surprised you?

HERZBERG: No, not really. I was always aware that our politicians and the general public have really no understanding of basic research and its importance by itself. The best that you can get out of them is support of basic research because eventually it always turned out to be useful. Well, they do support astronomy, but even that is not so certain with the present government. Just as they don't want to support the Arts Center Orchestra, and these kinds of things, at least there is talk of that. To me that would be terrible thing to demolish, something that has been first rate, just because of some preconceived ideas by people who don't know. At any rate, I was aware that when I talked to non-scientists and the impression they had of science is, "Only if it's useful is it any good." That's the wrong attitude to science.

KING: I guess the audience that we might have in the United States will not be familiar with some of the developments that you've been concerned with. In the period from about 1968 onwards, there's no time really to talk much about it, except merely to say that from about that time, there have been problems funding for pure research in Canada and you have been a very prominent speaker on behalf of science.

HERZBERG: Yes, but not a very efficient one, or successful one I should say. The previous government didn't understand basic science and the present government doesn't understand basic science, so what have I done? [laughter]

KING: Well, you've made your stand.

HERZBERG: Yes, I've made my stand but they never come and ask

me, "What do you think about this?"

KING: I'm sure they get to hear about it from the press.

HERZBERG: I've been interviewed by the press any number of times.

KING: Do you believe that freedom of science will survive?

HERZBERG: I'm not sure of that, but I hope so. Bureaucracy is increasing all the time. Eventually, I fear that it will completely negate freedom of science. Then there are these people who say that scientists are responsible for this and that. It might be true in some instances, but most of the time what scientists develop is something that may or may not be applicable, and nobody knows the answer. When microwaves were studied and the maser was invented by Charlie Townes. He didn't know that it would be something useful. He invented it because he wanted to study a basic problem in the interaction of light and matter. And the same with the laser. As long as people don't realize that, they won't get the real fruit from science...

KING: I guess this is an international situation.

HERZBERG: It is an international situation, yes. But it's more or less so in different countries. In Germany, the Minister of Science emphasizes at every turn that they want to support basic science. A far higher proportion of basic science is supported by the German government than we have, far higher. And the same in Japan, in spite of evidence to the contrary.

KING: Turning back to more scientific matters, two questions I wanted to ask you that we have probably covered to some degree, during our conversation largely about the progress of instruments. Unless there is something else you'd like to say?

HERZBERG: No.

KING: The other thing I was going to say, we don't have to review your work because this is all in the literature, but I guess the best known discoveries associated with your work has been the CH₃ and CH₂ radicals and perhaps the ammonium radical. I wondered which of your own discoveries have given you personally the most satisfaction?

HERZBERG: That's like asking me which of your children do you like best? [laughter]

KING: In effect you're excited by all of them?

HERZBERG: At the time I was excited. I would say in my later years the discovery of triatomic hydrogen was giving me most of the pleasure because it was something rather unexpected and something that could have been done thirty years earlier and nobody did. It could have been done when some people were involved, very much more than I was, in the study of the spectrum of hydrogen, but they didn't find it. We did.

KING: That was in...?

HERZBERG: That's when I was 75 years old. So I feel rather proud of that. I certainly don't want to belittle the discovery of CH₂, I'm proud of that too, but that was a long time ago.

KING: Finally, I think, I have to come back to the question of interaction between your subject and chemists. I mentioned this earlier on; chemists in the thirties were having a terrible problem with free radicals. Did you ever meet F. O. Rice?

HERZBERG: Yes, I think I did.

KING: Of course, the link is with Dr. Steacie here at the NRC, he was very interested. His subject was free radicals. The chemists were having a difficult time throughout the thirties just trying to convince people like Rice and other chemists that free radicals existed. People like yourself, the physicists, had no problems at all, you were just carrying on with your work. Did you interact? During this period did you ever have a chemist come up to you and cry on your shoulder and say they were having these terrible problems and could you help?

HERZBERG: Not really, no. I don't think I could say that. For me, many of the diatomic molecules that I was dealing with, like other spectroscopist, are free radicals. OH, CH, NH; they are free radicals. It was a perfectly every day experience to see a spectrum. Under conditions where OH, CH, and NH occur, as in a flame, the flame is a chemical phenomenon, and you see these radicals there. So the radicals are there and there's no question. The only question is can we find a spectrum that can reveal their structure? We know the spectrum of OH, CH, NH, and other similar ones -- SH, and there's no problem, but the problem was with radicals where the spectrum had not been seen, like CH₂

and CH₃, or NH₂...

KING: Well, that of course would make it very late, in the fifties.

HERZBERG: That's right.

KING: There were people like Rice who were having problems back in the thirties.

HERZBERG: That's right.

KING: Their problem was not merely accepting the existence of radicals, but they had to know the concentration and then determine mechanisms.

HERZBERG: And the concentration can't be determined if you don't know the spectrum. The indirect chemical methods are somewhat problematic, at least they appear so to a physicist. [laughter]

KING: Do you remember ever having a conversation about this with any chemists? Perhaps with Dr. Steacie.

HERZBERG: I had many conversations with Dr. Steacie.

KING: What were the things that, as a chemist, he was most bothered with?

HERZBERG: I don't know whether I can answer that. When I knew him he was convinced that CH₃ existed and he determined the time for it to react with another CH₃ to form ethane and this sort of thing. I don't really think he was really terribly concerned about that. He had chemical methods to do that and I think on the whole, they turned out to be quite satisfactory.

KING: They went through these different periods because in the thirties, that was the period when they were trying to convince, not Steacie, but Rice, who was trying to convince chemists of the existence of free radicals. Later on he came up with the Rice-Herzfeld mechanism (32). By the forties and fifties, when you came to Ottawa, they believed...

HERZBERG: I began to be interested in free radicals as such

after I had this idea about the cometary spectrum, the so-called 4050Å group being due to CH₂, which it turned out not to be. At that time I really started to look up some papers by Paneth and later papers on the same subject who were trying to find a spectrum of these things. At least they first tried to establish that they really exist, by moving a mirror from one place to another and this kind of [technique] so my interest started I would say, around 1940. At that time I thought the physical chemists, the kineticists, would detect free radicals by the reactions that were taking place. It was often a little somewhat hypothetical, so it seemed to me, but there it was. I was interested in the spectrum, I was trying to show them that here is the CH₂ molecule, and here is a CH₃, the radical I should say.

KING: What about Bonhoeffer? Bonhoeffer's work is extremely interesting because he appears to be extremely advanced when you look back at it. What about your conversations with him, did he raise any specific problem?

HERZBERG: [Herzberg gets up to get a book]... This is in German -- The Chemical Reactions is the title of a series of books (33), edited by Herman Mark, who's still around, and Michael Polanyi, father of John Polanyi.

KING: What year was this?

HERZBERG: This was published in 1933. I helped Bonhoeffer with some of the pictures in his volume. I supplied this [showing King a photographic print], not that I took it, and a number of others that I see here. He was a photochemist, essentially; this [book] is the foundation of photochemistry (33), and photochemistry, of course, is one way of dealing with free radicals and perhaps the easiest way, because it does mean that when a light quantum hits a molecule, it will split it. What does it split into? Two free radicals, normally. Conceivably, it could split into two saturated molecules, but what usually happens is two free radicals and I think this book is on that basis. My scientific discussions with Bonhoeffer were usually on that kind of topic. He would ask me what did I think of this or that paper in which certain free radicals [were postulated] and my answer would normally be, "Well, we have to find the spectrum of that radical." That's not so easy! It was only made possible when the flash photolysis method was developed.

KING: I should reverse the question: instead of chemists coming to you begging for help, did you ever receive inspiration from any papers published by a chemist?

HERZBERG: Oh, I did, yes. For example, about CH₂, I found a

paper written by a chemist in which the lifetime of CH₂ was reported to be of the order of a tenth of a second. Well, that appealed to me. They had done mirror experiments, eating up the mirror by free radicals.

KING: This was Paneth?

HERZBERG: The Paneth method, yes. While I was still in Saskatoon I thought I would try to build an apparatus to find CH₂. But I was way out; I didn't have a chance and didn't know it. I didn't know the concentration, so I was an optimist then. Eventually, after 14 years, I did manage to get it.

KING: Did you discuss these problems with Paneth at the time? Did you meet him?

HERZBERG: I never met Paneth, no.

KING: But he was here in Canada during the war, in Montreal.

HERZBERG: Oh, yes, he was, but I never ran into him.

KING: During the war you didn't come to Ottawa or Montreal?

HERZBERG: Toward the end of the war I did, yes. To Ottawa, not to Montreal. I never met Paneth, I don't know why that is. We never exchanged reminiscences about free radicals. [laughter]

KING: I've kept you a very long time. Are there any other points that you would like to mention about your very long career?

HERZBERG: [laughter] I think I've told you all my stories. The only thing one could add perhaps is that one meets interesting people, very fine scientists, and sometimes even politicians. The closer one gets up to Nobel Prize [status] the more chances one has to meet very prominent people, and that includes meeting with the Pope, as a member of the Pontifical Academy.

KING: The present Pope?

HERZBERG: The present Pope and his predecessor. In all the occasions when one meets very prominent people, sometimes it's

not interesting, but sometimes it is.

KING: Does the present Pope stand out as one of them?

HERZBERG: I think he would. Of course I didn't get the chance to have a discussion with him. He impresses one certainly as a remarkable person. I don't believe in his ideas about birth control; I think that's all wrong because he doesn't know mathematics, he doesn't know what an exponential function is. But he's a very impressive person, very impressive, there's no question about that.

KING: I'm sure he'd say the same about you, that you don't know about Catholic doctrine.

HERZBERG: That's right. [laughter]

KING: Dr. Herzberg, thank you very much for sharing your memories with us.

HERZBERG: You're most welcome.

[END OF INTERVIEW]

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