THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

KEITH J. LAIDLER

Transcript of an Interview
Conducted by

M. Christine King

at

The University of Ottawa

on

October 13, 14, and 18, 1983

CENTER FOR HISTORY OF CHEMISTRY ORAL HISTORY PROJECT

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Keith J. Laidler
(Signature)

12th March 1984
(Date)

KEITH J. LAIDLER

1916	Born in Liverpool, England, 3 January

Education

1937	B.A., chemistry, Oxford University
1940	Ph.D., physical chemistry, Princeton University
1955	M.A., physical chemistry, Oxford University
1956	D.Sc., physical chemistry, Oxford University

Employment

1940-1942	Research Chemist, National Research Council (Canada)
1942-1944	Science Officer, Canadian Armaments Research and Development Establishment
1944-1946	Chief Science Officer and Superintendent of Physics and Math Wing, Canadian Armaments Research and Development Establishment
	Chemistry Department, Catholic University of America
1946-1955	Assistant to associate professor
	Department of Chemistry, University of Ottawa
1955-1981	Professor
1961-1966	Chairman
1962-1966	Vice-Dean, Faculty of Pure and Applied Science
1966-1967	Commonwealth Visiting Professor, Sussex University
1981-	Emeritus Professor of Chemistry

Honors

1977	Queen's Jubilee Medal

ABSTRACT: In this interview, K. J. Laidler recalls his childhood, early education, and undergraduate days at Oxford University. He then speaks about his colleagues and teachers at Princeton University where he did graduate work. A consideration of the inception and development of the transition-state theory follows. Laidler then appraises the research that he did both with Steacie in Canada and independently at the Catholic University of America. He also comments upon the nitric oxide research of Hinshelwood and on his own recent work at the University of Ottawa. Finally, Laidler recollects the personal characteristics of several eminent chemists, among them Cyril Hinshelwood, Henry Eyring, and Hugh Stott Taylor.

Interviewer: M. Christine King was born in China and educated in Ireland. She studied chemistry at the University of London and has a Ph.D. degree in chemical physics from the University of Manchester. In 1981 she received a Ph.D. degree in the history and philosophy of science from the Open University, England.

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INTERVIEWER: M. Christine King

LOCATION: University of Ottawa

DATE: 13, 14, and 18 October 1983

King: Professor Laidler has worked on chemical kinetics in three countries. He first did research in England at Oxford with Hinshelwood. He then worked as a graduate student at Princeton in the United States with Henry Eyring and then with E. W. R. Steacie between 1940 and 1942 here in Canada. This is largely the order in which our conversation will take place. Let me begin by saying that you were born in Liverpool in England in 1916. Were either of your parents particularly interested in science?

Laidler: No. They weren't particularly interested in science but they were interested in scholarly matters. My father was a teacher throughout his career and ended up as headmaster of a very large school. My mother was a teacher before she was married and necessarily, in those days, had to retire when she got married; but she became a teacher again. They were therefore interested in teaching and in learning in general, but there was no particular interest in science on the part of my parents. They were more on the arts side, in fact.

King: What about your school days? Tell us a little bit about your school days and any special teacher that influenced you.

Laidler: Well, I had a fairly conventional education in England. When I was about fourteen or fifteen I began to realize that I was very interested in science and mathematics, more than in any other subject. I didn't do too badly in English and history and so forth, but I was clearly better in science. The school had a long tradition in classics and in history, and in fact my headmaster was a very distinguished classicist. He was very surprised when I told him at one stage that I intended to become a scientist. This was something that he didn't really quite understand. He urged me to change my mind and go into history. He realized I wouldn't be any good at classics and he tried to get me to go into history, but I was firm and he was very nice about it and supported me very strongly for many years afterward. But it was a surprise to him, simply because he thought there was no future in science.

It happened that there were two teachers in the school--one who taught physics and one who taught chemistry--who were both very inspiring teachers. This was a very great help to me. They encouraged me to continue with my interest in science.

I had difficulty deciding whether to be a physicist or a chemist. It's hard to know now why I decided to become a chemist and not a physicist. It didn't make very much difference in the end, but I know it was a bit of a struggle for me at the time to decide because I liked both of them almost equally well and was equally good at both of them. I might also mention that I had a magnificent mathematics master at school who also was a most inspiring teacher. His work contributed very much to my interest in physics and chemistry.

King: Well, you've answered my next one and a half questions. One thing I wanted to ask you was, we know that the attitude toward the role of science has changed a great deal in England between the beginning of the twentieth century and, say, thirty years later. What are your recollections of the place of science in English education as a whole when you began your serious interest in science?

Laidler: Well, certainly when I was at school, science was at something of a low ebb, I think, in the schools. I think in the universities it was another matter. The schools were a little behind the times. I think the schools in England certainly did tend to encourage people to go into the arts subjects, classics, English, history and so on, and not to go into science. There were, I'm sure, exceptions to that. There were some schools that were exceptional in that respect, but I think the average schools, both public and private in England, tended to direct people toward the humanities rather than toward the sciences. It was perhaps the individual students who decided themselves to go into science. Naturally, I'm thinking of the official policy of the schools.

There were some splendid teachers of physics and chemistry in the schools in those days. They were probably better than now, because in those days there wasn't so much industry and therefore lots of first-rate graduates from the universities necessarily had to become school teachers. Unless they became professors there wasn't much else for these graduates in physics and chemistry to do. Nowadays, of course, there are so many jobs outside the teaching professions for graduates in physics and chemistry. But then, certainly, the teaching of physics and chemistry was considered a very honorable pursuit. And some very superior people did go into the teaching of physics and chemistry. There was no biology taught in my school at all; it was never mentioned. But physics and chemistry were certainly taught very well. And naturally, of course, the teachers that I had and doubtless the teachers that many other people had, were very much interested in encouraging their students to go into the sciences.

King: I suppose this is a bit of a difficult question, but can you still remember the kind of physics and chemistry you were taught? Were you introduced to very recent or current topics by your teachers?

Laidler: Yes. I could tell, perhaps, a little story that will illustrate that point. Perhaps I was very lucky but I was, I think, introduced to a number of very recent concepts in science by these two masters who were exceptionally good. I remember when I first went to Oxford and talked to Hinshelwood, he recommended that I should read Sigdwick's Electronic Theory of Valency.* I said to him, "Well, I have already read that; I read it at school." He was most surprised and very impressed, and said, "Really, really." He said "Are the schools really teaching that now?" Now I think that was perhaps unusual, because Sidgwick's book had only just come out, but my chemistry master was a great admirer of Sidgwick; he was an Oxford graduate himself. He remembered Sidgwick very well and was a great admirer of him. He was very impressed by his famous book The Electronic Theory of Valency. He did have me read it as a schoolboy. I think perhaps that's a little exceptional.

King: What about your physics?

Laidler: In physics similarly. I think we were introduced to quite modern concepts in physics. When I became an undergraduate I felt I had a good background. Of course, one has to remember that in 1934 at school not too much was understood about quantum mechanics. And, in fact, even when I was an undergraduate I took one course in quantum mechanics which wasn't really very good. I learned my quantum mechanics, actually, after I went to Princeton in 1938.

King: Just for the record, perhaps we should note the names of your masters.

Laidler: Oh, yes. One was Mr. C. T. Parton, he taught physics. The other was Mr. W. P. Dickinson, spelt with an i--he always insisted he wasn't connected in any way with Charles Dickens. He taught chemistry. And Mr. Prestwick taught mathematics.

King: Between leaving school and going to Oxford did you know whether you were going to be, very decidedly, on the science side?

Laidler: Oh yes. Very definitely by that time; in fact, before that. I was brought up in a system that contained a sixth form in the school, where we specialized very considerably. This is, of course, not so today. But there was considerable specialization at school, so that when I went into the sixth form, which I did at the age of fifteen, I took from then on nothing but physics, chemistry, mathematics, English and Divinity, which was compulsory for everyone. The examinations I took at the end of those years were Oxford and Cambridge school certificate examinations, and I took them in those subjects. We had to do that. English was also required

*Nevil V. Sidgwick, The Electronic Theory of Valency (Oxford: The Clarendon Press, 1927).

for everyone. But after the age of fifteen, I had no teaching in history or geography or, of course, classics. I had had a certain amount of Latin. Of course, Latin in any case was required for admission to Oxford in those days, so I had to have enough Latin to get into Oxford, but I had had that by the time I was fifteen. I had a little Greek, but very little. I have forgotten all of it. I was three years in the sixth form. At the end of those three years, if I remember, I took higher school certificate examinations. But in the later years I took the higher level papers and I took them, as I say, in chemistry, physics, mathematics and English. I think there was a paper in Divinity each year--I'm not quite sure of that. Certainly there was one the first year.

King: What about modern languages?

Laidler: We had a lot of that before I got into the sixth form. I had a very good French teacher who taught us French grammar extremely well, so that I was able to read and write French. Unfortunately, as I discovered years afterward when I talked to this particular master, he himself couldn't really speak French. He couldn't conduct a conversation in French at all, although he had a good accent. He would have managed in France, but he wasn't at all fluent in French. I think this was rather unfortunate, that we did learn French in a very academic way. German, again--I can't remember exactly how I learned German--but I did have to take some lessons in German in school. I think they were on the side, because German in my time was required for a degree at Oxford in science, so I knew that I was going to have to take an examination in German.

King: So when you went up to Oxford to do science you were expected already to be able to read scientific papers in both French and German?

Laidler: That's right, yes. In those days, I am not sure what it is now, but in those days one had to satisfy the examiners at the beginning of one's undergraduate career that one could read French and German adequately.

King: You went up to Oxford on an open scholarship in 1934. I'd like to know what your first impressions were. Before you answer that, however, I want to ask you what pulled you towards Oxford as opposed to, say, Cambridge or other major schools of science in England. Was there any influence from your school?

Laidler: Yes, I think it was Mr. Dickinson, who perhaps had a slightly stronger influence than Mr. Parton. Mr. Dickinson was the chemistry master and Mr. Parton was the physics master. Mr. Dickinson had a slightly stronger influence, though I admired both of them very much. I got to know Mr. Parton extremely well later; he died only rather recently as a matter of fact. I kept in touch with him. He was a very fine person. Mr. Dickinson was perhaps a more volatile person, perhaps a little more persuasive. As a young man I was, I think, persuaded by him because of his personality. He was quite enthusiastic. He was an Oxford graduate himself. He went to Oriel College--I'm sorry, he went to Corpus Christi College. He was always telling me about life at Oxford and how splendid Oxford was for chemistry. This was true, of course, and it was he who told me about the work of Sidgwick on the one hand and Hinshelwood on the other. He knew Sidgwick well when he was an undergraduate. Hinshelwood, of course, was much younger. He was, I think, thirty-nine when I became an undergraduate, so that when Dickinson was an undergraduate, Hinshelwood was probably only in his twenties. But he knew of him as a great man, one who was going to make great advances in chemistry. I remember him telling me at school that much was going to come from Hinshelwood. Of course, he was absolutely right. I think perhaps it was some of these personal matters that inspired me. Mr. Parton was a Cambridge man, a physicist. He talked mildly about Cambridge; perhaps, because of his quieter personality. He didn't particularly inspire me to go to Cambridge. I think it was really as simple as that.

King: So, coming back to your first impressions...

Laidler: My first impressions of when I became an undergraduate?

King: Yes, when you first went up to Oxford.

Laidler: Well, of course I was impressed by the people I came in touch with. Hinshelwood in particular was always extremely nice, extremely pleasant as a tutor. It was always well known that he was an extremely kindly tutor. He wasn't, perhaps, as kindly towards people with whom he had other kinds of relationships. He was considered to be a very difficult man by many of his colleagues, and he became more so in later life. I must say for him that he was always extremely courteous and helpful towards his students. I never heard any complaints of any kind.

King: Well I was hoping to come on to Cyril Hinshelwood a little bit later, because I think it would be nice to talk about him in some detail, going back to your earlier impressions. I believe, in fact, your first choice was not Trinity where you ended up?

Laidler: Yes.

King: How did this come about?

Laidler: Mr. Dickinson actually recommended that I should take a scholarship in a group of colleges that included Lincoln College and Trinity College. Now, the tutor at Lincoln College was Sidgwick and the tutor at Trinity was

Hinshelwood. On his recommendation, I put Sidgwick, that is Lincoln, down as first choice and Trinity as second choice. But of course, there's a certain amount of sorting out. I took a scholarship examination that covered those two colleges plus a number of others including Balliol and Christ Church. Of course, the examiners get together, look at the papers and sort it all out. Hinshelwood apparently saw something good in what I had written and he said he'd like to have me. Sidgwick didn't quarrel with that. So that's the way it worked out.

King: Before we start talking about your work with Hinshelwood it would be nice if you told us a little bit about other teachers. Oh, first of all, were there any strange rules that you remember from your Oxford days, your undergraduate days?

Laidler: Well, when one went to Oxford in those days, and to some extent now, one had to learn that there were certain conventions that had to be followed. Some of them may seem rather absurd. We had to wear gowns under certain circumstances, of course when we had dinner in hall. We had to wear gowns when we saw fellows of the college. There was an exception to that, in my case and that of my colleague of the same year, Hobbs--who also was a scholar of Trinity in chemistry. He and I had joint tutorials under Hinshelwood who taught the two of us together. When we both first dutifully appeared at Hinshelwood's room wearing our gowns, Hinshelwood raised his eyebrows and said, "Oh, don't bother with those things." So we didn't wear them from then on. That was his style; he didn't mind about that. But, of course, even if one knew that, one had to do it the first time and have him suggest that we not bother from then on. But when one went to see any other fellow one would be required to wear one's gown.

Then, of course, there were certain restrictions about coming in and going out of the college gate. The college gates were closed at midnight and unless one climbed over the wall, which at Trinity was a bit difficult, one couldn't get in after midnight. Furthermore, violating the gate restrictions might lead to serious trouble because the president of the college was rather a stern old gentleman. It would be more than one's career was worth to get into trouble of that kind. So one was always dutiful about being in by midnight. Another curious thing was that one couldn't go <u>out</u> of the College after, I think, ten o'clock. I don't know quite why. If you wanted to go out in the evening you had to be out before ten. I think it was ten; it may have been nine. So one had to contrive to be out by ten if one wanted to go out at all. A curious rule. I don't know the reason for it. Apart from that, I don't think there were many difficulties. It was a free and easy life in many ways. It was very pleasant, very happy. One got to know people very quickly. I knew quite a few people from my own school, and I quickly got to know other people. I never had any problems as an undergraduate. I enjoyed it very much.

King: What did you do during the long vacations?

Laidler: Well, the joke at Oxford is that one does all one's work in the vacations, and doesn't do much in term time. There are people who don't do much either in term time or during vacation. I was fairly conscientious so I did in fact do work during vacation. This was prescribed by tutors.

I'll never forget my surprise, when at the end of my first term, that is before Christmas of 1934, Hinshelwood, at the last tutorial said, "Oh, I must give you some work to do over the vacation. Just read Sommerfeld's Atombau und Spektrallinien."* I must say I was slightly taken aback by this because this book, which had only recently come out, had not been translated. It was a question of reading it in German, and it was a fairly high-powered text. It was a bit of a struggle to get through that book; it was a fairly good size. When we came back after the vacation, we had a little examination on the book, on what we had learned from Sommerfeld's book. That was the usual pattern. At the end of each term Hinshelwood would assign some reading and then give an examination on it at the beginning of the following term.

One thing I should mention that's a little amusing was that we dutifully wrote the examinations and Hinshelwood never on any occasion ever made any reference to the examination. Whether we were to conclude that we had done it satisfactorily or whether he was disgusted I don't know. He never said anything. I must conclude, of course, that he was fairly satisfied that we had done something about it. Now, there was another undergraduate in my year who never did a stroke of work and he got along with Hinshelwood quite well. He also took these examinations and presumably did very badly. He had the same treatment; Hinshelwood never said anything. So undoubtedly Hinshelwood had it in his mind to remember all this and so when he came to write recommendations later on he made some distinctions between us and a person who did not work at all. That was Hinshelwood's style. Some tutors, I gather, were--I won't say more conscientious--but at least did mark the examinations and did discuss them. Hinshelwood didn't; for all I know he didn't read them.

King: Would you tell us a little more about how the teaching was actually carried out at Oxford?

Laidler: Yes. Well, the lectures were not compulsory, nor were laboratories, amazingly enough, and for many years that continued. I'm not sure whether they are now compulsory. But neither lectures nor laboratories were compulsory. The procedure was that there was a schedule of lectures, of course, that the tutors had and that we saw. Hinshelwood at the beginning of each year would say, "Well, So And So is giving lectures on such and such; I think you should take them." Or sometimes, he would say, "So And So is giving

*Arnold Sommerfeld, Atombau und Spektrallinien (Braunschweig: F. Vieweg, 1919).

lectures and I don't think they'd be worth your while going to them." I can remember his saying that on one or two occasions. We dutifully went to the lectures that Hinshelwood recommended, which didn't amount to very many. They were all given in the mornings. In the afternoons the laboratories were open and the conscientious students tried to go. They went to the laboratories in the afternoon, let's say from two to five or something of that kind, and then worked on experiments. There were cards in the laboratory. You picked out a card for the experiment you wanted to do and you did it. People who were called demonstrators, who were actually very distinguished people in many cases like R. P. Bell, would wander around. If they saw you in any trouble they would ask if they could help; but if they thought you were happy they would ignore you. So you could go for days and days without anyone saying anything. Sometimes they just stopped to chat and to discuss the experiment or discuss other things, so it was all very informal. They obviously made sure people didn't do anything dangerous, but we were very much left to ourselves.

One might wonder why did one bother to go to laboratories if they were not compulsory. The catch was that in the examination system at Oxford there was a practical examination which was quite demanding, so that if one had not gone through a lot of these experiments, one would have considerable difficulty with the practical examination. One might then fail to get the degree, or to get a good degree. Now another point was that the demonstrators did report to the tutors on the presence or absence of the students, so that Hinshelwood knew what laboratories we had been to and so on. In our case, Hobbs and myself--I'm saying <u>we</u> because we tended to work together throughout my undergraduate career--we were rather similar in background. In our case Hinshelwood never said anything to us because we were very conscientious. A person who neglected his laboratory work might get a reprimand from his tutor, so he'd do better next time.

King: So, if you conscientiously worked through these cards given to you in the laboratory, you could be sure to pass the exam at the end?

Laidler: Well, not sure of course, but one could be reasonably confident if one understood the experiments, had done them, and had gained the necessary skills. If one did not it would be very difficult to pass the examination because of the nature of the examination. The examination did require some technical skill, of course--knowledge of how to set up the experiment.

King: Could you elaborate a little bit about your other lectures when you were an undergraduate?

Laidler: Yes. Well, R. P. Bell stands out as a very good lecturer. He lectured on solutions and acids and bases and so on; specifically on acid/base catalysis, the theory of solutions, and the thermodynamics of solutions. He was extremely good. Wolfenden, who is now at Dartmouth College in the States, lectured on electrochemistry and was excellent. He was the electrochemist there at the time. And then of course, there was Bowen, E. J. Bowen, on photochemistry. There was A. G. Ogston, who later became a physiologist, and who later, in fact, became president of my college, Trinity. He was professor of biochemistry in Australia later in life. At that time he was a young man who also taught solution theory. That's in physical chemistry.

I should mention, of course, the other people. Sidgwick, of course, stands out as a magnificent lecturer. In my second year, I attended his lectures on inorganic chemistry. He was then writing his famous book on inorganic chemistry.* He was lecturing from the manuscript obviously. It was a superb set of lectures, so well and beautifully organized, covering the material in such a comprehensive way. He's the inorganic lecturer who stands out; he was quite extraordinary.

In organic chemistry, there was Sir Robert Robinson, who was then working on the structure of strychnine, if I remember. I attended two separate courses of lectures that he gave. One was concerned with the basic organic techniques and so on, and then he went on to describe his own work on the structure of strychnine. Of course, in those days it was all done by classical organic methods. There were no mass spectrometers; many of the modern techniques were then, of course, not available. The work was done in the most laborious way, but it was a great education to see how the structure of a complicated molecule was determined by those classical methods. Also he gave a course of lectures on the electronic theories of chemical reactions. Of course, I was particularly interested in that because that was very much related to kinetics. At that time he had developed his electronic theories, of inductive and electrometric effects, somewhat similar to Ingold's theories. Ingold's theories are much better known today, I think. But Robinson had done very much that same thing, roughly at the same time. So those are the ones that stand out.

I did take a course on radioactivity from Professor Soddy in my first year. Soddy was the professor of physical and inorganic chemistry at the time and he was, as is well-known now, a bit of a disappointment to Oxford. He'd been appointed as professor at a time shortly after he'd done very famous work. He was rather a disgruntled and unhappy man, and he quarreled with the university authorities. He wasn't really doing very much chemistry at that time, but he did conscientiously give a course on radioactivity which I remember as being perfectly adequate,if not very inspiring.

I remember lectures from D. L. Chapman on the halogens. That, too, was a bit disappointing because it was really more on the inorganic chemistry of the halogens, which wasn't his

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*Nevil V. Sidgwick, Chemical Elements and Their Compounds (Oxford: Clarendon Press, 1950).

field. I don't know why he was teaching that. It would have been more interesting if he had talked about his kinetic work on the hydrogen-chlorine reaction. But there wasn't much of that. It was more about the regular inorganic chemistry of the halogens, which I didn't find particularly exciting and which I don't think he did particularly well--perhaps because he didn't find it exciting either.

King: Do you remember your interest already beginning to focus on one of the three major chemical topics?

Laidler: Yes. I think that because I had this difficulty at school deciding whether to be a physicist or a physical chemist, I had no difficulty in deciding that if I was going to be a chemist I was going to be a physical chemist. I wanted to be on the physical side of things. If I had become a physicist it probably would have been as a chemical physicist. In other words, I was interested really in the field that lies between physics and chemistry itself. There was no difficulty about that.

For that reason I think perhaps it was a blessing in disguise that I went to work with Hinshelwood and not with Sidgwick. Sidgwick, of course, was a great man, but he was not really a physical chemist, although he had done good kinetics in his early days. His interest was really in inorganic chemistry. I don't think I would have found that particularly inspiring. It's hard to judge. I think I would not have found working in inorganic chemistry as inspiring as working in kinetics. So, of course, when I found myself with Hinshelwood as a tutor, who clearly emphasized the physical side of chemistry and certainly emphasized kinetics, then, of course, I went along with him. I might add, however, that Hinshelwood as a tutor was remarkably well-rounded. He did, in fact, tutor us in organic and inorganic chemistry. By way of explanation I should say that his tutoring consisted of the rather old-fashioned method of having us write essays and read them to him. He would assign an essay on a particular topic at the end of each tutorial and we would work on it for a week and read him the essay. I remember, for example, writing one on the anthocyanins which certainly was far from Hinshelwood's own interests. He felt that that was something that had to be covered in the tutorial, and I read the essay to him and learned something from what he said. I don't recall that he commented very much on the essay. On the other hand if we wrote him an essay on kinetics, of course, he would be apt to get into an interesting discussion of it afterward.

King: Well, I really wanted to ask you to talk a little bit now about Sir Cyril Hinshelwood, but you've already indicated quite a lot. I realize that since you have been involved with kinetics during most of your career, you probably came to know Hinshelwood a lot better later, after your student days. So, we could talk about Hinshelwood as a personality at this stage of your life and perhaps ignore the chronology.

Laidler: Yes. It is not quite so that I knew Hinshelwood well in his later career. I left England in 1938, was in Princeton, of course, until 1940, and then was in Canada during the war with some time in England also during the war. I didn't see much of Hinshelwood at that time. Whenever I visited England later on I always went to see Hinshelwood, and he has been here in Ottawa and I've seen him here. But, I can't say, of course, that I knew him personally very much after my undergraduate days. I only knew him just for occasional meetings, perhaps only a dozen or so in all. Of course, I kept in touch with him; I wrote to him from time to time. He was always very good about replying. He would always reply immediately to any letter I wrote to him. My knowledge of Hinshelwood in his later life however was perhaps more second hand. Although I know about his later personality from discussions with people who knew him well, I can't really say that I know that of my own knowledge. Of course, I have read most of his papers and I know a lot about his work.

King: Hinshelwood had some unusual interests, other interests, as a linguist and a classicist.

Laidler: Yes. He was very proficient at various languages. When I was an undergraduate, we were aware of that fact. He could read several languages and, surprisingly enough, although he didn't travel a great deal, he did seem to pick up pronunciation of languages very well. I might mention that not so long before his death, when the University of Ottawa gave him an honorary degree, he gave his speech of acceptance in French. My French-speaking colleagues here were amazed with the quality of the French and the accent. They said that if they had not known otherwise they would have thought he was a Frenchman. Yet, I don't believe he spent much time in France; I heard that he did use the Berlitz records. Perhaps he could mimic an accent fairly well. He did seem to be able to pick up a good French accent, and I've heard the same about German. He spoke German quite fluently, and I believe with a very good accent. He could manage Chinese, I believe, but I was told that that wasn't perhaps too good. His Italian, I understand, was very good. I believe in Russian he was competent. He didn't know much Greek, perhaps because his educational background was somewhat similar to my own. I had to learn Latin and didn't have much Greek. Now that I think of it, when he went to Oxford I think Greek was compulsory for admission. Nonetheless, I don't think he knew an awful lot of Greek. This fluency with languages was, of course, of great use to him in his activities as president of the Royal Society. He was able to greet foreign visitors very fluently in their own languages.

King: Well, we might come back to Hinshelwood a little bit later in connection with kinetics in the 'thirties. Coming back to the end of your training at Oxford, the final year of undergraduate studies is a crucial time when students have to decide on an area of specialization. How did you choose yours? Was there a specific decision?

Laidler: Yes. At Oxford one takes the examination for the Bachelor of Arts degree after three years. I'm speaking of the course in chemistry, of course. So, at the end of three years, having gone to lectures and tutorials and so on for that period of time, one takes the final examinations--Part I, as it is called. They then mark the examinations. They give one the degree, but they don't give one the class of the degree; that is, they don't say how well one has done. To obtain the class one has to do what is called Part II, which consists of research with someone. Now, normally one works with one's tutor, and in my case there was no question about that; I continued to work with Hinshelwood. I did my research, in other words, with Hinshelwood, under his direction and submitted a thesis at the end of the fourth year. Of course that was on a problem assigned by Hinshelwood after discussion with Hobbs and myself about what we wanted to do. He had me do reactions in solution; my colleague Hobbs did reactions in the gas phase.

King: You graduated from Oxford in 1938 and then went to Princeton to work with Henry Eyring. Can you tell us how this came about?

Laidler: Yes. During my last year at Oxford, of course, I had to give some thought to the question of what I was going to do afterwards. I could just have taken a job at that time. Hobbs did, in fact, do that; he went into industry right away. A number of students did that, but I didn't particularly want to go into industry right away. I did want to continue to take a higher degree and I applied for a fellowship to remain at Oxford to remain working with Hinshelwood.

I also applied for a Commonwealth Fund Fellowship to take me over to the United States. I think part of the reason that I did that was that it happened that a member of my family had obtained a Commonwealth Fund Fellowship a few years previously, so I did know all about them. He had talked to me about his own happy experiences in an American university; he went to MIT. I, in fact, suggested to Hinshelwood that I should apply for a Commonwealth Fund Fellowship. He thought that it was a reasonable idea and so I did apply. He wrote a letter of recommendation and I assume it was a reasonable one because I was awarded the fellowship.

Now an interesting point about that is that my first choice was to work with Pauling at the California Institute of Technology and my second choice was to work with Eyring at Princeton. When I went for the interview in London those who interviewed me asked me if I would consider going to Princeton instead of to Caltech. They did so because so many of the fellows were going to Caltech and to other universities in California that they wanted to spread them around a little. I had given Princeton as a strong second choice, and they had never had anyone before who had worked with Eyring. They were very pleased therefore with the idea that someone from England was going to work with Eyring on the Commonwealth Fellowship. They asked me if that was acceptable, and of course I said indeed it was. So they appointed me the fellowship to work at Princeton. Of course, as it turned out I was very pleased at the fact that I was able to do that. I'm sure that I would have enjoyed working with Pauling, but the work with Eyring was rather more in my line as it turned out.

King: Which brings me beautifully to the next point. We were all sad at Professor Eyring's death in 1981. So it makes your memories of him even more precious. Could you describe him? His personality? His methods of working?

Laidler: Yes. I knew little about Eyring when I first went to Princeton. I had seen him, because he did give a seminar at Oxford when I was an undergraduate. I did not, however, have a chance to speak to him at that time. I was only, I think, in my third year. I saw him and I found him a very lively lecturer. Hinshelwood had always spoken very highly of him. At the end of his lecture, I might mention, Professor Lindemann, who was a professor of physics at Oxford at that time, asked some rather aggressive and unpleasantly worded questions of Eyring which caused some annoyance and embarrassment to the people present. This was, of course, somewhat Lindemann's style. I remember Hinshelwood afterwards commenting on what Lindemann had done, saying that it was in very bad taste to treat a visiting professor in that way. So that was my only previous knowledge of Henry Eyring. When I arrived at Princeton I first went to see H. S. Taylor, chairman of the department, and he was very pleasant. I had corresponded with him a little about the department, telling him that I was coming and so forth. He wrote a very nice letter in reply to that, I remember. Then I went to see Henry Eyring.

I was taken with him right away. What struck me as quite remarkable--coming from Oxford perhaps, the contrast was so great--was his extraordinary friendliness. He treated me as a friend from the very first time I saw him. Indeed, he just said, "Everybody calls me Henry; you call me Henry, won't you." He called me "Keith." So that was it, within five minutes. Throughout the time, he was a friend as well as a research director. I was in an office next to his. I remember we were along a corridor and he was in one office and I think the office I was in was just as big as his. It was not a particularly large office, but everyone seemed to be treated in the same way. His door was always open and people wandered in all through the day. If Henry ever found himself alone he would walk along the corridor and invite somebody in, or perhaps just go into somebody else's office and sit down and talk.

He did his research very largely just by talking to people and working out things on the blackboard. He was always producing ideas. Many of them were quite unreasonable and wrong. In some cases we were able to show him that they were wrong, or that he was saying things that were, in fact, well known. He always took this very well. But a certain proportion of the ideas that he threw out were very stimulating. Some of them needed further work. Sometimes this further work indicated that they weren't worth pursuing any more, but in a small proportion of the cases his ideas did turn out to be extremely fruitful. The way he operated was quite an extraordinary revelation to me. It was quite different from anything I had ever experienced before. Hinshelwood was a complete contrast. Hinshelwood would never really say anything unless he'd worked it all out carefully in his mind; he wouldn't blurt out an idea. Everything he said he'd worked out extremely carefully. Eyring was just at the other extreme. He blurted just anything that came into his mind, and it was the duty of his graduate students and other research assistants to sort them out, work on them, and to tell him if they were wrong. If they were right, they were to develop them in some way. So that really was the way the work proceeded throughout, I would say.

When I first arrived he assigned me a problem on constructing a partition function for water, which was not kinetics at all. He was quite keen to pursue that because he knew my interest in reactions in solution. Well, we didn't get anywhere with that. It soon became apparent after a short time that the time was not ripe for developing a partition function for water. I'm not sure that the time is still ripe; it's a very, very complicated problem. So it didn't lead to any publishable results. But of course, it was very useful training for me because up until then I didn't know much about partition functions. Although I'd had a course in statistical mechanics at Oxford, I didn't know a great deal about it, so I had to learn it as I went along.

When he saw that my attempt to develop a partition function wasn't working, and he heard the New York Academy of Sciences wanted a paper on reactions in solutions, he asked me to collaborate with him on the latter topic. I did that. Also, we got working on reactions on surfaces. I don't remember how that came about; maybe H. S. Taylor had suggested to Eyring that that would be a useful problem. So we applied the theory--what is now called transition-state theory, which he called then absolute reaction-rate theory-- we applied that to reactions on surfaces.

A year after I arrived, this would be the summer of 1939, Samuel Glasstone came to Princeton. Glasstone was an electrochemist who had been at Sheffield University in England. He decided to leave England and go to the United States. He arrived at Princeton without any particular position, although he did get an appointment at Princeton fairly quickly. He arrived and was interested in collaborating with Eyring. Since he was an electrochemist he pointed out to us that the problem of overvoltage had not been satisfactorily dealt with by reaction-rate theories. Now that was another problem that I worked on with Glasstone as well as with Eyring. We wrote a couple of papers on which all three of us were authors.*

King: I'd like to come back to these two people that you've been talking about, Eyring and Glasstone, a little bit later. But let's get back to the general scene at Princeton. Hirschfelder has described the 1930's as a golden age for theoretical chemistry and you, of course, just overlapped with this period. Who were your colleagues and other teachers at Princeton and how was the graduate course run?

Laidler: Well, the system at Princeton was in some ways not unlike that at Oxford, in the sense that lectures were not compulsory. At that time one was not required to take graduate courses. Undergraduates, I believe, had to take courses, but graduate students did not have compulsory courses. On the other hand it was, of course, considered desirable to take lecture courses if one was going to obtain a Ph.D. degree.

I might say that when I first went to Princeton, it was not my intention to take the Ph.D.; I really just intended to work with Eyring on research. I didn't think I'd have time to take the Ph.D. degree as well. I realized later that probably I could manage it and did in the end manage it, because of the fact that the lecture courses were not demanding. There were no assignments. One pretty well learned by one's self and took a comprehensive examination at the end and submitted a thesis. I was well able to do that in the two years that I had, which would probably not have been possible if there had been demanding courses to take at the same time with lots of assignments. That was all to the good as far as I was concerned.

I did attend a number of lectures at Princeton. I certainly took H. S. Taylor's lectures on kinetics, which were extremely well organized. He was an extremely good lecturer. That was a very profitable experience. I took Eyring's courses. He gave a course one term in quantum mechanics and another term in statistical mechanics. I certainly took those. These were a great experience. I might say that Eyring was probably one of the world's worst lecturers.

King: You anticipate me.

Laidler: Well, that's going too far. No. Onsager and Bohr were probably the worst lecturers that I ever heard. But Eyring was not good. He was so informal that it didn't really matter very much if one wanted to learn, because he didn't mind if one interrupted him in the middle of a lecture. Of

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*Henry Eyring, Samuel Glasstone, and Keith J. Laidler, "Application of the Theory of Absolute Reaction Rates to Overvoltage," Journal of Chemical Physics, 7 (1939): 1053-65.

Henry Eyring, Samuel Glasstone, and Keith J. Laidler, "A New Theory of Overvoltage," Transactions of the Electrochemical Society, 76 (1939): 1-8.

course, the class was small. I remember probably about fewer than a dozen people. The class was small and he was so pleasant and informal that if you didn't understand anything, you would just say, "Henry you're going too fast," or "You're getting us all muddled could you go over this again." He'd take it in perfectly good part and go over it again. So with the help of interruptions of that kind he'd get the subject matter across.

I had taken a course in quantum mechanics at Oxford, but I didn't really feel I understood the subject at all. Of course, the subject was then still somewhat ill-digested. There weren't many books on quantum mechanics then, if any. Dirac's book, I suppose, was out; that was rather formidable.* Pauling and Wilson, of course, had come out and that was a great help.** I think it only came out at about the time I went to Princeton; I don't think I had studied Pauling and Wilson before. At any rate, with the aid of Pauling and Wilson plus Eyring's lectures I did manage to get a good grasp of the subject.

The statistical mechanics was the same. I never felt before I had a great grasp of it. But Eyring did it in a very informal way. He didn't go through formal proofs very much, but he gave us an insight into what one can do with statistical mechanics. Of course that's terribly important. You can get formal treatments from the text books, but the insights into what the subject can do are more important in many ways, and Eyring was very good at instilling these insights into people. So those were the courses in physical chemistry.

I remember I took courses in organic and inorganic chemistry; I had to do some of that. I didn't do as much of that, but I knew I was going to be examined in these topics. I had a pretty good background from my undergraduate days. I didn't have to work perhaps tremendously hard on the other two subjects. In the end I wrote up my thesis. Since we'd written the book <u>The Theory of Rate Processes</u> and it was in manuscript, my thesis was pretty largely taken from <u>The Theory of Rate Processes</u>.*** Princeton had a very easy way of allowing one to write a thesis, you see. It was permitted then simply to put one's reprints together as a thesis. I do remember that the reprint of the paper on overvoltage was part of my thesis. Also, parts of chapters that we had written on the quantum mechanics and on reactions in solution and reactions on surfaces were included. This, of course, was slightly anomalous because I wasn't the sole author of any of

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*Paul A.M. Dirac, <u>The Principles of Quantum Mechanics</u> (Oxford: The Clarendon Press, 1930).

**Linus Pauling and E. Bright Wilson, <u>Introduction to Quantum Mechanics; with Applications to Chemistry</u> (New York: McGraw-Hill, 1935).

***Samuel Glasstone, Keith J. Laidler, and Henry Eyring, The Theory of Rate Processes <u>(New York: McGraw-Hill, 1941).</u>

this, but Princeton had a very liberal attitude towards this and it was quite acceptable. In fact, Glasstone and Eyring, particularly Glasstone, had done a substantial amount of the writing. I had written parts of these sections but Glasstone, an extremely good writer, had certainly written quite a lot so that I couldn't really claim that the thesis was entirely my own. That didn't seem to bother the Princeton authorities. It was a relief because that might have presented a difficulty--I didn't really want to rewrite everything. It would have been rather a waste of time, if I had to take it and write it all over in my own words, especially since some of it was already in my own words.

King: You've already mentioned H. S. Taylor. Taylor was, of course, British. Could you backtrack a little and talk a bit about H. S. Taylor?

Laidler: Yes. Taylor as you say was born in England. He went to Liverpool University. He got his Ph.D. degree--or perhaps it was called a Doctor of Science degree, I'm not sure --at Liverpool University. He came under the influence of Donnan, and then went to work with Max Bodenstein, a German kineticist. Shortly before the First World War, I believe it was, he came over to the United States. I'm not sure whether he went to Princeton first, but he had been at Princeton for a very long period of time when I arrived there and was then known to be a very distinguished kineticist. He was chairman of the department when I was a graduate student.

King: You attended a course of lectures in kinetics given by H. S. Taylor. I've seen your lecture notes, and it's interesting how closely they follow his textbook on physical chemistry.* Could you tell us a little bit about his lectures?

Laidler: Yes. I remember them quite clearly and as you say I still have retained my notes of his lectures. We do know, of course, what fields he covered. I was impressed and still am impressed as to how modern his approach was. Of course, I must say that Hinshelwood was also extremely modern. Taylor was perhaps more enthusiastic about transition-state theory as we call it now, about Eyring's theories. Taylor had been instrumental in getting Eyring's 1935 paper published in the Journal of Chemical Physics.** Eyring had difficulties with referees, but Taylor intervened strongly and the paper was published early in 1935. So Taylor was very, very familiar, of course, with transition-state theory and with the related work of Polanyi and Wigner which came out just a few years

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*Hugh S. Taylor, Elementary Physical Chemistry (New York: D. Van Nostrand, 1927).

**Henry Eyring, "Activated Complex in Chemical Reactions," Journal of Chemical Physics, 3 (1935): 107-15.

before.* He therefore covered these matters in his lectures. I remember he spent quite a bit of time on Polanyi's experimental studies in the 1930's: on reactions of sodium atoms with chlorine and so on, the atomic reactions. Polanyi wrote a book called Atomic Reactions and Taylor was obviously much impressed by this particular study; he emphasized that work very much.** Of course, he also covered in very considerable detail catalyzed reactions on surfaces. He, himself, did a great deal of important work on catalysis. He was responsible for the idea of active centers of surfaces and for the idea of activated adsorption; there would be an activation energy associated with adsorption processes. This was, of course, of very great importance in clarifying the ideas of different kinds of adsorption in chemistry, chemisorption, and physical adsorption. All these things Taylor brought out in his lectures. He covered the different orders of reaction in kinetics and reactions on surfaces. Those are the things that stand out most in my mind as to the things that he covered in his lectures.

King: I'd like to come back in a little while to some of the points you've raised in connection with the transition-state theory, but to round off what we've been saying about colleagues and teachers at Princeton, whom else do you recall?

Laidler: Oh, I should have mentioned R. N. Pease, who also was doing research in kinetics. He was a gas kineticist. He was working on free radical reactions at that time. I don't remember that I went to a course of his lectures; he may not in fact have given a course of lectures. I don't remember going to his lectures, but I remember him very well as a person. I very often spoke to him.

The Princeton chemistry department was quite small at that time. In my day the Princeton graduate school was restricted to 250 graduate students in all subjects. So there were probably not more than 20 graduate students in chemistry. There were a few postdoctorates, but it was very, very small and the result was that one could easily get to know everyone. So, I knew many of the professors personally, just by meeting them in the corridor.

Everyone was always extremely friendly. I certainly remember Pease as being extremely friendly. Another person I remember very well is Smyth, C. P. Smyth, who worked on dipole moments. He was an extremely pleasant man and very able. He did very distinguished work on dipole moments and he wrote a

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*Michael Polanyi and Eugene Wigner, "Uber die Interferenz von Eigenschwingungen als Ursache von Energieschwankungen und chemischer Umsetzungen," Zeitschrift fur Physikalische Chemie, 139 (1928): 439-52.

**Michael Polanyi, Atomic Reactions (London: Williams & Norgate, Ltd., 1932).

book on dipole moments.* Then there was one person who was not really in my field at all but who was exceptionally kind to me, as was his wife. That was A. W. C. Menzies, spelled Menzies but pronounced "Mengies", a Scottish name. He was a Scotsman. Although he was in his sixties when I was a graduate student, he still had such a strong Scottish accent that the Princeton undergraduates had a little difficulty with his lectures at first until they got used to him. He and his wife were delightful people and they made me most welcome at their house. In fact, almost every Saturday evening I would have dinner with them and spend the evening there. That was a very pleasant experience.

Menzies worked on osmotic pressure; that was one of the things he had done early. He was a somewhat classical chemist --somewhat old-fashioned one might say. So I didn't actually attend any of his lectures, but I was aware of his earlier work; he did some rather pioneering work on the measurement of osmotic pressures, probably about the turn of the century. He wasn't doing a great deal of research at that time, but was an extremely strong influence in the department.

One other person I should mention is Hubert Alyea, H. N. Alyea. Alyea is still at Princeton as far as I know. He was young, probably an assistant professor when I was there. He had done some kinetic work in his time. He worked with Backstrom in Stockholm, I think. He decided he didn't want to continue to do research, so he, with the full approval of his department, went into the field of education, education in chemistry. He devoted himself to the teaching of chemistry, particularly to undergraduates--in fact, almost exclusively to undergraduates. His first year course in chemistry was quite famous and it was remarkable for the fact that he did an extraordinarily large number of demonstrations in his lectures. He spent a great deal of time preparing each lecture. Each lecture must have taken several hours to prepare, and he had a great many demonstrations which were very interesting and striking. Also he was a most entertaining person, he ought to have been on the stage. Well, in a sense he was on the stage, because his lectures were stage performances. Before Christmas, the final lecture of the session was what he called a "review lecture" and the only trouble about that was that it was very difficult to get a seat. Not only did the undergraduates all want to go, but a lot of the professors in all fields in the university wanted to go, and their wives and children and all sorts of people who had no connection with the course all piled into the lecture room to hear and see this remarkable lecture. I think he claimed, and this was probably true, that he did sixty demonstrations in sixty minutes. It really was a most extraordinary performance because of the way the stage was set The lecture bench was covered with apparatus and he went from one to the other with no delay at all. He did explosion after

*C.P. Smyth, Dielectric Behavior and Structure; Dielectric Constant and Loss, Dipole Moment and Molecular Structure (New York: McGraw-Hill, 1955).

explosion--flashes of flame and all sorts of things. It was really entertainment and really no review but a stage performance. That brought him a lot of publicity. He has been going round for many many years in the United States and Canada and to some extent maybe other countries, giving these remarkable demonstrations. He has done a great deal for the teaching of chemistry. He's devised kits to help chemistry teachers in schools teach chemistry. He's devised kits with a minimum of equipment to do a maximum of experiments.
King: That's really interesting.

Laidler: Those are the professors at Princeton I remember. There may be some others I've forgotten. But that's quite a group.

King: I'd like to bring these two personalities you've already mentioned, H. S. Taylor and Henry Eyring, together. As you've already said, Taylor played a role in the eventual publication of Eyring's 1935 paper on the transition-state theory.* Taylor himself has described these years at Princeton with Henry Eyring as his golden years. He has said that he was inspired by Eyring's personality. The transition-state theory took place a little bit before you arrived in Princeton, but could you fill in a little bit more about it, before you arrived and after? What state was it at?

Laidler: The paper appeared in 1935 and I arrived in 1938, so there had been a three year gap. During that period a great deal had been done, certainly on developing potential energy surfaces for chemical reactions and also developing the dynamical treatment. Those procedures that were used in the 1930s are considerably out of date now, although they paved the way for later work. But no one does the calculations in the way that they were done at that time.

King: Hirschfelder had left by this time?

Laidler: Hirschfelder had left, I think, the year before I arrived. He and Eyring had done a lot of very important work on potential energy surfaces and dynamics of surfaces. Nothing had been done, however, on extending transition-state theory to apply to different kinds of reactions. We were the first to look at overvoltage, which was at Glasstone's suggestion. He saw that the theory could be applied to the phenomenon of overvoltage. That, I think, was a very useful contribution; one still sees that work quoted. Also the work that I did with Eyring on reactions on surfaces was quite new. No one had looked at surface reactions from the point of view of transition-state theory. We were able to develop it for different kinds of reactions. At the same time we were working on diffusion processes from the point of view of Eyring's theory. He was at that time extending his basic theory into different kinds of processes as well as developing

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*See note on p. 17.

the basic theory. I should mention, in addition, that shortly before I arrived at Princeton, Eyring, in collaboration with Taylor and with Hirschfelder, had worked on the theory of radiation-induced chemical reactions. That was another application of the theory to another kind of process.

King: One of the most interesting outcomes of your Princeton years was the writing of the textbook, known to every student of chemical kinetics, The Theory of Rate Processes, which appeared in 1941.* You've already mentioned Professor Eyring and Glasstone. Could you tell us a little bit about how this came about, and the pains and agonies of writing?

Laidler: Yes. In my first year at Princeton, Eyring asked me if I would be interested in collaborating with him on a book on the theory of rate processes. He felt there was a need for a book on this and he recognized that he himself was not a particularly good writer. He had difficulties putting things down in a logical order, and the results were not always intelligible. I also think he found it irksome to have to write things. He felt that I had some skill at writing. I don't think they were very considerable skills but they were surely better than his. So he broached the subject with me and I, of course, thought that this was a splendid idea. I had always enjoyed writing. We started to work on that. We drafted a certain amount of the book and then in the middle of my time at Princeton, Glasstone came along. Glasstone had previously written some very successful books in physical chemistry. There were two that I remember well, that I used as an undergraduate; one was called Recent Advances in Physical Chemistry**and the other was Recent Advances in General Chemistry.*** They were extremely valuable, because you have to remember that at that time there were not many good textbooks on physical chemistry at all. These were very helpful. They weren't, in fact, really textbooks; they covered special topics and they were extremely good. When Glasstone arrived, he was in the middle of writing his very large textbook of physical chemistry.**** That later appeared and was extremely successful; in fact, I helped him to some small extent with that textbook by reading the proofs for him. That was published shortly afterwards by Van Nostrand. Glasstone arrived with no particular objective except that he wanted to write, and so it seemed natural for us to ask him if he wished to collaborate with the two of us.

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*See note on p. 16.

**Samuel Glasstone, Recent Advances in Physical Chemistry (London: J. & A. Churchill, 1931).

***Samuel Glasstone, Recent Advances in General Chemistry (London: J. & A. Churchill, 1936).

****Samuel Glasstone, Textbook of Physical Chemistry (New York: D. Van Nostrand, 1940).

This is the way the book was written. I prepared a lot of drafts. Glasstone then took them over, developed them, and wrote them in his own style. Eyring, in fact, did very little of the writing. We went to him constantly for advice if we had difficulties. Of course, he read it all very carefully and he commented and on occasion we got into arguments about how things should be put and so forth. He was very helpful in that way. I don't believe however that he spent very much time--in fact my recollection is that he spent almost no time just sitting down writing a section. So it was rather a curious kind of collaboration. Eyring really was inspiring on subject matter. I drafted out sections and Glasstone polished them up. That was a good experience for me; I learned a lot about how to write from Glasstone.

King: The transition-state theory is of special interest to me, but it was also a highlight in Eyring's career. Can you say a little bit about the reception at the time this book was being written. Do you think that Eyring was aware that this was a very fundamental contribution to kinetics?

Laidler: Yes. I think he certainly was well aware of that. In fact, perhaps as was true of people at that time, he even exaggerated its importance, in the sense that he tended to say from time to time that the experimental chemists were now really out of business because he could calculate anything. He could calculate a potential energy surface because quantum mechanics would give him the exact answer as far as the energetics were concerned. And then, having got a potential energy surface, he could use transition-state theory, as we now call it, to calculate the rate. He felt that there was really no more left than that. Of course he realized that the theories were imperfect as they stood, but he thought that a little bit of polishing up of quantum mechanics would give exact potential energy surfaces so that one would know exact activation energies. The theory would then lead to exact rates of reaction, so that it would be easier to calculate these rates than to do any experiments.

Of course, this was common at the time, I'm not criticizing the fact that he said that. Dirac had a similar attitude toward quantum mechanics; he felt that quantum mechanics essentially put other people out of business. Fifty years or so later, we realize that experimentalists still have to do many important things and that it's not easy to see today that these theories are going to be the answer to everything. Indeed to answer your question, I don't suggest Eyring was being immodest about what he had done because he wasn't that kind of a person, but he did think it was of great importance. Of course he was right; it was of very great importance.

King: Another interesting historical event which happened around the same time was a simultaneous paper by Polanyi and

Evans along very similar lines.* Do you recall any special outcome from this episode?

Laidler: I am familiar with the background. Evans had worked in the Princeton chemistry department a couple of years or so before Eyring's paper appeared. Eyring told me that Evans had discussed reaction rates with him. Evans worked with H. S. Taylor, so that there was not a close association. Undoubtedly, however, Evans was made aware of the work that was being done by Eyring and Hirschfelder as collaborators at that time. I'm not sure that Hirschfelder had arrived when Evans was there; I'm not sure of that, but Evans was certainly aware of the approaches that were being made by Eyring with regard to transition-state theory. Now there was some question at the time as to whether perhaps Evans had learned quite a bit from Eyring and then had worked it up with Polanyi. At the same time we have to remember that Polanyi had been corresponding with Wigner and, in fact, in their paper they did acknowledge that Wigner had been very helpful to them. Of course, Wigner was very familiar with what Eyring was doing. So there should be no suggestion that anyone was stealing anyone else's ideas. It might have appeared, perhaps, to some that there was that question, but it was all really out in the open. I've heard alternative ideas, but my own view now is that everything was really quite straightforward. There was nothing improper done by anyone in that connection.

King: Well, certainly not known to each other.

Laidler: No.

King: Today is the fourteenth of October, 1983. When we stopped yesterday we were discussing the transition-state theory. I'd like to continue just for a short time on this topic. It seems that there was some hostility or opposition to the transition-state theory in the period we are talking about. Could you tell us why this hostility existed?

Laidler: Yes. the first incident that I'm aware of was the Lindemann incident in, I think, 1937. He was extremely critical of the theory. Another person who for some years was very much opposed to transition-state theory was Guggenheim who was then professor of physical chemistry at the University of Reading and was throughout his career a very distinguished chemist particularly in the field of thermodynamics, although he did also do some very good work in kinetics. Guggenheim had a number of serious criticisms, which about twenty-five years later at a meeting of the Chemical Society in Sheffield he more or less withdrew. But he was extremely critical on various grounds. He was concerned about the equilibrium

*M.G. Evans and Michael Polanyi, "Some Applications of the Transition-State Method to the Calculation of Reaction Velocities, Especially in Solution," Transactions of the Faraday Society, 31 (1935): 875-894.

hypothesis, the assumption of equilibrium, and also he was concerned about the fact that the reaction coordinate had to be defined properly in order to apply transition-state theory. He had some doubts as to whether this could properly be done. Rather curiously, however, in the book that Guggenheim wrote with R. H. Fowler he presents transition-state theory in a much more favorable light than in the various comments that he made at different meetings and in published papers.* Perhaps this was due to Fowler's influence on him.

Hinshelwood's attitude to transition-state theory was quite different. On the whole, Hinshelwood approved of the theory and appreciated it, although in practice he didn't make very much use of it, which is rather surprising. He tended to continue to use the older kinetic theory of collision, although he certainly presented transition-state theory very clearly and in a very favorable light in his textbook, Kinetics of Chemical Change.**

A number of people over the years made criticisms; some of them were not put into writing. One did hear comments that the theory is oversimplified, which indeed it is. Some people didn't pay too much attention to the theory and tried to find alternative approaches to the problem of reaction rates. The difficulty is that all of the alternatives involved so very much more work that it is hard to know whether they were ultimately any better. And I think that during more recent years there has been a reaction in the direction again of transition-state theory. Some people who originally rejected the theory now accept it as a good starting point and are trying to improve it in various directions. This has happened over the last fifteen years in particular.

King: I wonder if I ought to ask you to sum up your memories of your days at Princeton. It seems to me that the 1930's was a particularly exciting time. Do you have any special memories? Were you aware that this was a special time with a lot of exciting new work?

Laidler: Yes. I think that those of us who were graduate students at the time were very much aware of the fact that this was a very important period in the history of physical chemistry, particularly of chemical kinetics. I think this was due to the fact that there had been this revolution in thinking about reaction rates. I think it was felt by us, the graduate students at Princeton, that this theory of absolute reaction rates, as Eyring called it, or transition-state theory as we now call it, was a tremendous advance because it threw light on chemical reactions in a very original way and was really going to transform our thinking about chemical

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*Ralph H. Fowler and Edward A. Guggenheim, Statistical Thermodynamics (New York: Macmillan Co., 1937).

**Cyril N. Hinshelwood, The Kinetics of Chemical Change (Oxford: The Clarendon Press, 1940).

kinetics. So, indeed, it was a very exciting period. I think we all realized the tremendous power of the theory in the sense that it could be applied to so many different kinds of processes, not only chemical reactions, but also physical processes such as diffusion and dislocations in solids and things of that kind.

King: We are coming now to the time when you left Princeton. Your time at Princeton was interrupted by the outbreak of war?

Laidler: Yes. I had done about a year at Princeton when the war broke out and I had to think about what I should do. I soon realized that there wasn't at that time any possibility of crossing the Atlantic as a civilian, and the Commonwealth Fund people recommended that we continue our fellowships. I think that this was really the best procedure, because I think if I had gone back to England at that time, I would have wasted time, not really done anything useful, because the scientific world was disorganized at that time. People were put into war jobs and so on. I think it was more useful to continue education and then to go back. In 1940, however, when my fellowship ended, I had to consider whether to go back to England, which would again have been difficult unless I had done it in some official way or I had enlisted in the army. In the meantime Dr. E. W. R. Steacie, who was then director of chemistry at the National Research Council of Canada, had visited Princeton and given a seminar. I spoke to him after his seminar and told him about my dilemma. He wrote to me later and offered me a job in Ottawa with the National Research Council to work directly with him on a war problem. I accepted that position and went to Ottawa.

King: Could you tell us a little bit more about the work that you did during this war period?

Laidler: Yes. Steacie's laboratory was concerned with free radical reactions. I was put on to a problem involving free radical reactions in the gas phase that also involved the synthesis of a substance that might have been used as a poison gas during the war but never was. So we were trying to find out something about the mechanism or the formation of this substance.

King: How long did you do this?

Laidler: Well, I only remained with Steacie for about eighteen months because in the meantime I had been offered a position by the Inspection Board of the United Kingdom and Canada to do some work connected with gun and rocket ballistics, and also with the design of gun and rocket propellants. I thought that was perhaps more interesting work that was also more useful to the war effort. So I accepted that position. I worked for some time in Valcartier, which is near Quebec City, in the Province of Quebec. I spent some time in England during the war working and visiting laboratories in various places, including the laboratories of Woolwich Arsenal and certain other laboratories in England doing war work.

King: Do you remember the general feelings when the war was declared over?
Laidler: Oh, yes; very much so, of course. That was a matter of great elation. The war was declared over twice; the war in Germany ended first and then about a year later, I think, the war with Japan ended. Of course, the relief came when the war ended with Germany, because it was clear the war wasn't going to go on very much longer. The pressure was off and, of course, there were great celebrations. One recollection that I have of the day on which the war ended was that I wasn't able to get any dinner that evening because I happened to be in Toronto visiting Professor George Wright with whom I was doing some work on propellants. We had been doing some work during the day and then the announcement came that the war was over and we decided to go to a restaurant and have dinner. We found all of the restaurants in Toronto that day closed because everyone was out in the streets celebrating. He invited me to go back to his home and we had something to eat there.

King: So in fact you spent the war years here in Canada. At what stage did you decide that you would stay in North America?

Laidler: Well, I think that wasn't so much due to a decision on my part. At the end of the war I did write to universities in England. I wrote to Hinshelwood and said I was interested in returning to England and taking up an academic position. He did in fact offer me a rather junior position in his laboratories in Oxford. I was also offered another position at the University of Bristol, which tempted me very much. But then I got a very nice letter from F. O. Rice, from Washington. He was chairman of the chemistry department in the Catholic University of America. I was quite intrigued by this position because it did seem to me perhaps more what I wanted. It was a position which gave me independence as a researcher, as an assistant professor in that department. At the same time I knew that my colleague from Princeton days, Walter Moore, was also taking up a position in the department, and a third person was also appointed, Hugh Hulburt, who was a theoretical chemist. So this was a very interesting offer that I had. In the meantime I had married. My wife comes from New York City, so she was interested in remaining in North America rather than going to England. So, the combination of circumstances was such that the offer from Washington seemed to be the most interesting one.

King: Tell us a little bit about your experiences with F. O. Rice in Washington.

Laidler: Yes. Well, there was rather a curious aspect to the appointment that I had in the department of chemistry at the Catholic University. Rice had the idea, which is an interesting one I think, that he should have people in different fields who were in fact basically physical chemists. So in my case he had the idea that I, although a physical chemist, should teach biochemistry. Now, of course, in my case that really meant physical biochemistry. I had studied some biochemistry, but I don't pretend to be a biochemist by any means. I had been interested in the kinetics of biological reactions, of enzyme-catalyzed reactions. So, the arrangement was that I would go to the department and teach the course in biochemistry; I think it was the second-year course in biochemistry. I would also teach a course in chemical kinetics and certain graduate courses, in addition, in physical chemistry. Some of my research, at least, would be in enzyme kinetics and indeed it was.

This later led to a slight argument with F. O. Rice which I think was resolved. He became concerned that although I was publishing papers in enzyme kinetics, I was also publishing papers in gas kinetics and on the theoretical aspects of non-enzyme kinetics. He, for some reason that I never quite understood, felt that I should devote my attention entirely to enzyme kinetics. At any rate, this matter was resolved and I did continue to work in the other aspects of kinetics as well as in enzyme kinetics.

King: So you were in Washington a total of nine years, during which time F. O. Rice was head of the department. Could you tell us a little bit more about this period and also about the scientific aspects of the work?

Laidler: Yes. There were some problems during that nine year period that we were in Washington. When I say "we," I'm thinking particularly of Walter Moore, Hugh Hulburt, and myself. We knew one another very well and we thought very much alike. One of the problems arose from the fact that Rice saw fit to run the department in what we thought was a very autocratic way. He was, I think, officially <u>head</u> of the department rather than chairman of the department. In other words, he thought that he was very much the director, and that decisions could be made entirely by himself without his having to consider the opinions of the other members of the department. This did lead to some difficulties at various times. On one particular occasion he was anxious to appoint a certain individual to be an assistant professor in the department. We thought that person was not a suitable member of the department. He consulted us and the department was fairly unanimous, in fact I think we were thoroughly unanimous, in rejecting the suggestion; in other words, we were unanimously opposed to this appointment. He became very angry at this stage, and said that he was going to make the appointment in any case, which he did. As it turned out, it was not a very satisfactory arrangement. That kind of thing happened, I'm afraid, rather a number of times. As a result, the three of us decided that we couldn't remain in the department very much longer. I was the last of the three to leave. I remember that Hulburt left first, then Walter Moore left, and then I left in 1955. It was unfortunate because Rice had done some very distinguished work himself in kinetics, and if he had had a different attitude in the running of the department it could have been a very strong department. He created a situation in the department that caused people not to want to remain in the department. He lost a number of people that way.

King: You mentioned that Rice had done some distinguished work in kinetics, on free radicals in particular. Could you elaborate?

Laidler: Of course. Rice was a pioneer in the development of the mechanisms of organic reactions, in particular organic decompositions. After Paneth had discovered, I think it was in 1929, that one could detect free radicals by using the mirror technique, Rice began to apply this technique to organic decompositions. He discovered that free radicals were indeed present when organic molecules decomposed. For example, ethane decomposes into ethylene and hydrogen, and previous to that work it had been assumed that this was a purely molecular mechanism, that the ethane molecule simply splits off a hydrogen molecule. Rice showed, however, that free radicals were present in the reaction system, so that free radical reactions are obviously involved.

In 1934 Rice and K. F. Herzfeld, whom I also knew very well in Washington--he was chairman of the physics department--collaborated with one another. Herzfeld had been, of course, interested in chemical kinetics; he had done some important theoretical work in the early days of chemical kinetics, in the early 1920s. Well, Rice and Herzfeld collaborated on a very important paper on organic decompositions, in which Rice devised certain mechanisms and Herzfeld applied the steady-state treatment and arrived at expressions for the rates of reactions in terms of concentrations of the reacting substances.* This was of very great importance because it resolved the dilemma that people had had; namely, that one can get quite simple kinetics out of these reactions in spite of the fact that it appears that they are very complex. The Rice-Herzfeld mechanisms showed that very complicated mechanisms, involving a number of different stages, can under certain circumstances lead to quite simple kinetic behavior. This opened everyone's eyes, I think, to the fact that things were not as simple as they appeared in organic reactions; that one has to take into account these free radical reactions.

Rice himself took the view, that eventually turned out to

*Francis O. Rice and Karl F. Herzfeld, "Thermal Decomposition of Organic Compounds from the Standpoint of Free Radicals. VI. The Mechanism of Some Chain Reactions," Journal of the American Chemical Society, 56 (1934): 284-89.

be correct, that most of these reactions occurred entirely by free radical mechanisms, and that the molecular mechanism did not occur to any significant extent. In the case of the ethane decomposition, for example, Rice denied the possibility that there was any molecular splitting of an ethane molecule into ethylene and hydrogen. He insisted that the reaction went entirely by the free radical mechanism.

In the middle 1930s in Hinshelwood's laboratory--and this was done particularly by Staveley--some important work had been done on the effect of nitric oxide on the kinetics of these organic decompositions. What Hinshelwood, Staveley, and others found was that when nitric oxide was added to, let's take as an example, decomposing ethane, the rate was reduced, but not to zero. It was reduced to about ten percent of the original rate. Hinshelwood interpreted that very reasonably by saying that it must be that this residual ten percent is the molecular mechanism and that the remaining ninety percent is the free-radical mechanism. This, at the time, seemed to me very reasonable, and I continued to believe that myself until about 1960. Rice, on the other hand, took the view that this was not so, that there was something very special about the behavior of nitric oxide. It does act as a free radical since it has an odd electron. Rice took the view that this evidence was not valid and that in fact these reactions go entirely by free radical mechanisms. So there was a sharp controversy between the Hinshelwood school and the Rice school.

I backed the Hinshelwood side at that stage, because it did seem to me that that was the simplest explanation for the results with nitric oxide. I had a number of arguments--very pleasant arguments, these were very amicable arguments--with Rice about this matter in which I represented the Hinshelwood point of view and he disputed it. I found the arguments a little frustrating because Rice really had no good arguments to raise against the Hinshelwood point of view. He was arguing on intuitive grounds, as if he knew how molecules would behave and he couldn't believe that molecules would split up in the way that was suggested by the molecular mechanism. It was a slightly frustrating experience to argue with him because he couldn't really produce any valid arguments against the Hinshelwood view. It is, of course, ironic in the circumstances that he did turn out to be right.

King: When did these conversations take place?

Laidler: The discussions I had with F. O. Rice occurred between 1946 and 1955 when I was in Rice's department. There had been discussions before that between Rice and others, naturally. Hinshelwood, of course, was critical of Rice's point of view. Hinshelwood felt that Rice wasn't facing the facts which clearly indicated that the molecular mechanism was significant. I was just referring to my own part in this since I happened to be with Rice and happened to have worked with Hinshelwood; I was very familiar with the nitric oxide work. I didn't actually do any work with nitric oxide, but my colleague, who was a student with me, Hobbs, whom I mentioned before, did in fact do work on the ethane decomposition with nitric oxide. I discussed that work with him quite a bit and we were quite convinced from the results Hobbs had obtained, for example, and the various other results, that there was this molecular mechanism. When I left Washington in 1955, I still was of the opinion that Rice wasn't really facing reality. He didn't really have any reason to doubt Hinshelwood's ideas. At the same time, most of us recognized, and I certainly recognized it myself, that there was a possibility that Rice was correct and that there was something special about the behavior of nitric oxide.

At about the time that I left Washington, I forget whether it was just before or just after, about that time, people started to make studies using isotopes in which they investigated isotopic mixing. They studied isotopic mixing with various amounts of nitric oxide, including having an excess of nitric oxide in which one supposedly had removed all of the free radical reaction. Some of this work was done in Russia by Voedodskii, and some of it was done by Rice and Varnerin in Washington. These isotope studies certainly supported Rice's point of view that the free radical reactions were occurring even when one had complete inhibition by nitric oxide. The evidence was very strong with regard to the reactions we studied, no question about it. One couldn't think of any alternative explanation for these experiments, assuming them to be correct. They did turn out to be quite correct because they were repeated. So this, of course, made one realize that Rice was in fact correct even though his arguments weren't very convincing.

Some of us in our own laboratories--I was in Ottawa when we started this work--decided that we would make further investigations on this nitric oxide inhibition problem. So we did study a number of reactions inhibited by nitric oxide using better techniques than had been used previously. We used gas phase chromotography to study the individual products of reaction. By about 1955, new and powerful techniques were being introduced into gas kinetics. Up to that time, most of the early work in Hinshelwood's laboratory had been based entirely on pressure changes, that is, on the measurement of the total pressure and the change of pressure during reaction. Well, we now realized that this is rather unsatisfactory because the course of the reaction is not properly represented by the pressure changes. This clearly is so if one has a number of different products formed. There are great complications arising in that case, so that pressure changes weren't really very satisfactory. The original nitric oxide work was all based on pressure changes, and we realized that that was not satisfactory. One had to investigate the rates of formation of individual products and the rates of consumption of the reactants. We were able to use gas phase chromotography to do that. So we were able to find out the effect of nitric oxide on the rates of formation of individual products of reaction. When that was done, we realized that there was an explanation and this was put forward first, I think, in a paper with my graduate student Wojciechowski, who is now professor of chemical engineering at Queens University in Canada.*

He and I first put forward the suggestion that the explanation for the Hinshelwood results is that nitric oxide, being a free radical itself, is actually generating free radicals as well as removing them, and that matters are much more complicated than Hinshelwood had supposed. Hinshelwood supposed, originally at any rate, that the effect of nitric oxide is simply to act as a scavenger of free radicals, that is, to remove the free radicals from the system. So his idea was that when you have plenty of nitric oxide there are no free radicals left, so that any reaction that occurs has to be molecular. He put forward what seemed to be a strong argument that there couldn't be an equilibrium; you couldn't say there is an equilibrium removal of free radicals, because if that is so then increasing the concentration of nitric oxide would remove more free radicals and would reduce the rate further. Our explanation was that nitric oxide is introducing new free radical reactions and that it is also removing free radicals. When you have added an excess of nitric oxide you are in a steady state in which you have not reduced the rate to zero, because the nitric oxide is still producing free radicals. So that explanation got us around the dilemma that seemed to be caused by the Hinshelwood results. This is the explanation, I think, of the behavior of nitric oxide. We published a number of papers on this between 1960 and 1970 and I haven't done any work on it since 1970.** As far as I know our explanation of the nitric oxide inhibition is now generally accepted.

The net result, of course, was to show that the nitric oxide work, although very interesting in itself, doesn't really contribute to the problem of whether or not there are molecular reactions. In fact, as it turned out, it really caused confusion. That, I think, was an interesting example of an experiment that seemed to be a very valuable one, the experiment in Hinshelwood's laboratories in the 1930s, of

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*B.W. Wojciechowski and Keith J. Laidler, "Free Radical Mechanisms for Inhibited Organic Decompositions," Canadian Journal of Chemistry, 38 (1960): 1027-34.

**See previous note. Also, Keith J. Laidler and B.W. Wojciechowski, "Kinetics and Mechanisms of the Thermal Decomposition of Ethane. I. The Uninhibited Reaction," Proceedings of the Royal Society, A260 (1961): 91-102.

Keith J. Laidler and B.W. Wojciechowski, "Kinetics and Mechanisms of the Thermal Decomposition of Ethane. II. The Reaction Inhibited by Nitric Oxide," Proceedings of the Royal Society, A260 (1961): 103-14.

Keith J. Laidler, N.H. Sagert, and B.W. Wojciechowski, "Kinetics and Mechanisms of the Thermal Decomposition of Propane. I. The Uninhibited Reaction," Proceedings of the Royal Society, A270 (1962): 242-53.

Staveley and Hinshelwood; but which didn't solve the problem that it appeared to solve. In fact, it confused the issue. In the end, of course, it was a very useful thing for this work to have been done.

King: Chronologically we're a little bit out of order, but this brings us very neatly into your time in Ottawa. After you left Washington you came to Ottawa in 1955. Could you tell us about the work you've been doing since?

Laidler: Yes. I have already mentioned some of it. We did the work on gas kinetics about the first ten years I was in Ottawa, from 1955 to 1965 or later. We worked to a considerable extent on gas phase kinetics and we were

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Keith J. Laidler, N.H. Sagert, and B.W. Wojciechowski, "Kinetics and Mechanisms of the Thermal Decomposition of Propane. II. The Reaction Inhibited by Nitric Oxide," Proceedings of the Royal Society, A270 (1962): 254-66.

Keith J. Laidler and B.W. Wojciechowski, "Inhibition of Organic Decompositions," Transactions of the Faraday Society, 59 (1963): 369-76.

Keith J. Laidler and M. Eusuf, "Kinetics and Mechanisms of the Thermal Decomposition of Acetaldehyde. Part I. The Uninhibited Reaction," Canadian Journal of Chemistry, 42 (1964): 1851-60.

Keith J. Laidler and M. Eusuf, "Kinetics and Mechanisms of the Thermal Decomposition of Acetaldehyde. Part II. The Reaction Inhibited by Nitric Oxide," Canadian Journal of Chemistry, 42 (1964): 1861-71.

Keith J. Laidler and M. Eusuf, "Kinetics and Mechanisms of the Thermal Decomposition of Propionaldehyde. Part I. The Uninhibited Reaction," Canadian Journal of Chemistry, 43 (1965): 268-77.

Keith J. Laidler and M. Eusuf, "Kinetics and Mechanisms of the Thermal Decomposition of Propionaldehyde. Part II. The Reaction in the Presence of Nitric Oxide," Canadian Journal of Chemistry, 43 (1965): 278-89.

Keith J. Laidler and M.H. Back, "Theories of Inhibition by Nitric Oxide," Canadian Journal of Chemistry, 44 (1966): 215-25.

Keith J. Laidler and J. Esser, "The Pyrolysis of Ethane in the Presence of Nitric Oxide," International Journal of Chemical Kinetics, 2 (1970): 37-61.

Keith J. Laidler and H.P. Schuchmann, "The Pyrolysis of Acetaldehyde in the Presence of Nitric Oxide," International Journal of Chemical Kinetics, 2 (1970): 349-80.

particularly concerned with certain organic decompositions. As I mentioned before, we repeated some of the experiments but used better techniques in order to investigate the rates of formation of the various products. We did work with pure systems in the absence of nitric oxide and we investigated the effect of adding nitric oxide. We also did some work in which we added propylene, which is an inhibitor for gas reactions, and this led to our being able to formulate a mechanism to explain what the effect of nitric oxide was. So I think that covers the gas phase work.

In parallel, my students and I did a number of investigations on enzyme-catalyzed reactions. This is work which I had started in Washington in 1946. We continued that work. Some of it was involved with investigating the action of hydrolytic enzymes. We were concerned with measuring activation energies and so forth, the effects of pH to a considerable extent, on the rates of enzyme-catalyzed reactions. During this period I was able to develop a treatment to explain pH effects. Of course, there had been many treatments before of pH effects. What we did was not particularly different from previous treatments except that, perhaps, we generalized some of the previous treatments.

One thing that we did pay considerable attention to, around 1960, was the question of the formation of two intermediates in enzyme-catalyzed reactions. In the original Michaelis-Menten scheme it is assumed that the enzyme and substrate come together and form one intermediate which is an addition complex, and this breaks down into products. There has been much evidence that certain enzymes involve a clearly defined second intermediate, which is formed after the enzyme substrate complex. The formation of the second intermediate may also occur at the same time as the elimination of one of the products of the reaction. We devised schemes to explain that. We also worked quite a bit on the pH effects that are to be expected when one has that particular complication of having the second intermediate; and we worked on the temperature dependence of reactions under those conditions.

At the same time, we initiated some work on high speed kinetics using the stopped-flow techniques and the temperature-jump technique. By doing so we were able to obtain more detailed information about the mechanisms of enzyme-catalyzed reactions. During 1970, or possibly before, I became interested in the effects of enzymes when they are immobilized. We particularly studied two kinds of immobilization. One is immobilization in which the enzyme is trapped in a gel. We would polymerize a monomer in the presence of the enzyme so as to form a gel and cut slices of gel so that the enzyme would be trapped in the material. We were particularly interested in that kind of system because we know that in biological systems enzymes are not completely free. They are attached to solid material. The other kind of immobilization that we studied involved attaching the enzyme to the inside of a tube. We usually used nylon tubing because that was convenient. We would have the enzyme attached to the inside of the tube, so that when we passed the substrate solution through, the reaction would take place. This has the very great advantage of preserving the enzyme. Enzymes, of course, are quite expensive and difficult to obtain in many cases. So it's a great technical advantage if one can bring about an enzyme reaction and preserve the enzyme so that it can be used over and over again. We weren't so much concerned with the practical side of this as with elucidating the kinetics of the reaction under those circumstances. There are some special problems that arise when enzymes are immobilized in that way, the main point being that diffusion control is very often important. So we devised methods of arriving at a conclusion as to the amount of diffusion control in a given system.

I was fortunate in the middle of the 1970's to have the collaboration of a professor from Japan, Professor Koboyashi. He was a chemical engineer and was very skillful at devising theoretical equations for chemical engineering problems. He played a very important role in developing equations to explain the behavior of enzymes when they are immobilized by being trapped in a gel and also when they are present on the inside of a tube. Since that time we have applied the equations to various enzyme systems.

King: In addition to the work you've just mentioned, Dr. Laidler, you've also written a large number of textbooks on kinetics. Thinking back, on all of your work, can you tell me what aspect of your work has given you the most pleasure and why?

Laidler: I suppose the book that has had the largest influence has been the one simply called Chemical Kinetics, which has been used as a textbook fairly widely in various parts of the world.* I know it's used in Russia quite a bit and it's used in India and Japan. This has had some influence. It is a general book, a basic textbook on chemical kinetics. I am now preparing a third edition. The second edition came out about fifteen years ago, so the third edition is clearly called for. I suppose that general book is the one that I feel is the most useful. The one on enzyme kinetics, I enjoyed writing very much.** A second edition of that came out in collaboration with Peter Bunting and I may some time bring out a third edition of that. That book approaches enzyme kinetics very much from the point of view of the physical chemist rather than from the point of view of the biochemist. I'm not sure that it has had a great influence on biochemists who, I think, tend to prefer a more biochemical approach to chemical kinetics and to enzyme kinetics. But I know that some physical chemists have found this approach useful. I got satisfaction from writing that, but I think perhaps it didn't

*Keith Laidler, Chemical Kinetics (New York: McGraw-Hill, 1950).

**Keith Laidler, Chemical Kinetics of Enzyme Action (Oxford: Clarendon Press, 1958).

exert as great an influence as the other book.

King: For years you've continued to communicate with some of the major personalities in kinetics, some of whom we've already mentioned. We've already discussed the scientific aspects of their lives. Could you recall now some of the more human aspects of their personalities?

Laidler: I suppose we should start with Hinshelwood, whom I knew first, since I knew him when I was an undergraduate. Although I didn't see a great deal of him after 1938, I did see him from time to time. So I know a fair amount about him. I visited him in Oxford a number of times and he was in Ottawa on one occasion.

Hinshelwood was a very remarkable personality in many ways. He was a very unusual person. He was an extremely scholarly person who didn't have many of the ordinary human interests; his main interests were in things of the mind. As we mentioned earlier he was very interested in languages and he was very interested in literary matters. I would not say that he had, outside of these purely scholarly matters, very few interests. He had some that might seem surprising. For example, he was very much devoted to the Marx Brothers and always would go to see a film showing the Marx Brothers. I think perhaps that is understandable, that he should have liked the Marx Brothers, because there is an interesting logic about some of the things that they said, particularly Groucho Marx. I think the play on words that sometimes comes into Groucho Marx's comments would have given Hinshelwood very great pleasure. Of course, I enjoyed listening to Groucho Marx's comments myself. I think perhaps one can understand that. Again, in a somewhat scholarly way, Hinshelwood was interested, perhaps, in the linguistic aspect rather than anything else. He certainly found them extremely amusing.

He was very sociable in many ways. Every year he had a magnificent dinner for all of his students, undergraduates and research students. And it was done in a very tremendous style, probably the sort of thing that would hardly be done today. We all wore full evening dress, that is, white tie and tails. It was a most lavish dinner given in the old buttery of the college, and it had a considerable number of courses with a different wine with each course. After the dinner we would go out for port and cigars and so on to a special room where Hinshelwood delighted in having us play at charades. Again, I think his interest in charades was somewhat of an academic nature. He liked the play on words. He was involved in that particular pastime. Another thing he did was to take us to the theatre as his guests. He particularly liked to take us to see magicians. In the intermission and afterwards he would discuss with us the way in which the various tricks were done. He had his own theories as to how the different tricks were done. Again it was rather an academic interest in the art of the magician. So that shows some of his very human qualities.

As a student I always found him extremely courteous, if somewhat cold and distant. Always courteous, and as I say, on occasion extremely hospitable, but one always felt a considerable gulf between him and the rest of the world. I've heard it said by people who have known him for many, many years, known him very well for many years, that they didn't, in fact, come very close to him.

He was a nervous kind of person and I think he found the war years quite a strain. He was in his forties during the war, not particularly old. I've been told by people who were closely associated with him at that time that he did become somewhat more difficult to deal with after the war. He was not as friendly and not as easy in his manner to other people. This was, perhaps, because of the great strain he felt at that time. He was always courteous with people with whom there were no particular problems. If one visited him he was always extremely courteous. I gather, however, that if one's behavior was not what he expected it to be, he could become very angry. I myself saw one instance of that when I visited him in Oxford during the last war. A dog was observed in the corridor and it emerged that a young army officer had brought his dog into the building. Now one may, of course, say that that wasn't the appropriate thing to do. What did rather surprise me, however, was that Hinshelwood should get into such a rage about it. He was uncontrollable. His rage was quite uncontrollable about this matter of the dog, and it showed that his nerves were considerably frayed. He threatened the officer with court martial and all sorts of things, which was really rather absurd and, of course, the man was rather upset. He could obviously have been dealt with in a much gentler way just as effectively.

King: Did he dislike dogs in general or was it just the fact that it was in the wrong place?

Laidler: I don't think he disliked dogs in general. I'm not sure about dogs; that wasn't what was concerning him. It was the fact that someone would bring it into his department. He himself was a great cat lover. He always had a cat in his room in Oxford and went to great lengths to feed it. Sometimes he had several, although I think there was only one when I was an undergraduate. The cat was always prowling around. He was very, very friendly with the cat. He enormously enjoyed his cats.

He painted. His rooms were lined with his own paintings, some of them of cats. He also made paintings of his laboratories, one of which is reproduced in Hartley's book, Studies in the History of Chemistry.* His paintings were very good; they were very accurate and the colors were very pleasant. So that was another interest of his.

King: Who else comes to mind?

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*Sir Harold Hartley, Studies in the History of Chemistry (Oxford: Clarendon Press, 1971).

Laidler: Well, of course, Eyring. Have we said enough about Eyring?

King: Well, I would like to hear some of the more human aspects of Eyring.

Laidler: Yes. He was an intensely human person, at the opposite extreme from Hinshelwood. He radiated friendliness. He had a very boyish attitude when I knew him and, I believe, for many years afterward. When I knew him he was about forty and he used to challenge his graduate students to do certain feats of jumping. He had very powerful legs and he was able to jump over a chair from a standing position. None of the rest of us could possibly do that. He would race some of the students, running various distances. I played handball with him a number of times; he was very keen on handball. One summer when he was away, I think it was the last summer I was there, I remained in Princeton, but I had sold my car, and Henry Eyring lent me <u>his</u> car. It was the sort of thing he would do without thinking anything of it. It was just natural for him to lend me his car because he was going away and wouldn't need the car. So he was really quite extraordinary in personal ways like that. He always had a big smile and was always pleasant to everyone.

King: Eyring was, of course, a very devoted Mormon. does this aspect of his life show up?

Laidler: Oh, yes. Very much so. Before I went to Princeton I met in New York City with the Commonwealth Fund people who told me that Eyring was a Mormon. I must confess that at the time I didn't have any idea what a Mormon was, so they told me a little about it. They thought I should know because I was going to work with him and the matter might come up. Actually they needn't have told me, because Eyring told me almost on our first meeting. He informed me that he was a Mormon, that he was convinced that his was the right religion, and that any other religion was wrong. So from time to time he made serious attempts to convert me to the Mormon faith, without any success. It was all done in a very friendly and amusing way--I mean there was no embarrassment about it, no pressure. He just was anxious that I know all about the Mormon church and he hoped that perhaps I might become a Mormon. I discussed it with him of course. He realized finally that there was no possibility of my becoming a Mormon, and so when I left Princeton at the end of my stay there, he said something to me that was really very touching. He said, "Well Keith, I've worked on you; I've tried to make you a Mormon, but I haven't succeeded." He continued, "Never mind, because I know that in the next life I am going to go to heaven as a good Mormon, and our church teaches that I will be a god when I go to heaven. I therefore will have considerable influence, and I will arrange for you to be in <u>my</u> heaven and we will continue to work together on scientific problems." I had a little difficulty in keeping my face straight, but I did, I think. I thanked him very much, because I was indeed very touched because it was very sincere and very natural that he would do that. That was what he believed.

As a Mormon he led a different kind of life from practically everyone else in Princeton, apart from his own family. As far as I know he and his family were the only Mormons in Princeton. He would not drink anything alcoholic and, indeed, would not drink tea or coffee, regarding them as stimulants and therefore undesirable. He would not go to the theatre or the movies. This did create a difficulty, of course, for his family, and he was very conscious of this. He discussed it freely, the fact that it was hard for his boys to go to a school where everyone else in the school could go to the movies and they could not. He was quite unhappy about this, and also he was unhappy about the fact that things that the Mormon church taught to be very evil were practiced openly by people whom the boys were supposed to respect.

There was one incident, a very interesting one, that occurred when I happened to be in Eyring's office. One of his sons came into the office, no, I think two of his sons were in the office. It was the end of the day, I think. H. S. Taylor, the chairman of the department, came in smoking a cigar. I'll never forget the expressions on the faces of the two boys who were about ten and twelve. They were obviously very baffled at the fact that this great man, this great scientist who himself was a very religious man--the Eyring boys were brought up to show respect for Taylor--was committing this great sin, smoking a cigar. Eyring was quite worried about incidents of this kind. He discussed them very freely. I understand that that was really the reason why he left Princeton a few years later and went back to Utah. He felt, and perhaps his wife felt even more strongly, that they were placing the boys in a difficult position. They were being brought up in an environment where they were the only ones to hold to certain beliefs, and everyone else they saw had a different attitude about life.

There was one other incident involving Eyring that caused some amusement at the time. He was very much opposed to the theatre on moral grounds; he felt that the theatre was evil. Someone suggested to him, and I think it was perhaps an unfortunate thing for this person to have done, that he ought to go see a play in New York City and judge for himself whether it was as evil as all that. Unfortunately this person suggested the play "Tobacco Road," which was then running. "Tobacco Road" is, of course, a great play, but it involves some rather sordid scenes of incest and so forth. Eyring went to see this and, of course, his view was confirmed by seeing this play. As far as I know, it is the only play he ever saw in his life. I think it was most unfortunate that it should have been so. If he had gone to see some of the plays of Shakespeare he might have felt differently about it. But I think "Tobacco Road" was probably about the worst play that he could have seen at that particular time. Let me see now, are there other things about Eyring? That covers his religion I think fairly well. It has been written up in other places and

I needn't elaborate on that, I think.

His personality, of course, as far as science was concerned was rather different, rather special. I had mentioned already that his way of working was more or less just to discuss things freely and eventually come up with some good ideas and work them through and then submit a paper for publication.

Eyring was not good at writing. He was rather careless in the way he wrote papers. He was much too busy thinking about the next ideas and didn't really pay much attention to the form of the papers. When he was at Princeton, I think this was covered fairly well by the fact that his graduate students usually wrote the papers. I certainly felt that I had to write the papers. He was very agreeable to this. He was very happy if someone undertook to write the papers for him. In later years I got the impression that perhaps his graduate students didn't assert themselves in this way, and so some of his later papers weren't well written and didn't have the impact on the scientific public that they might have had if they had been better organized.

King: We've already said something about his relationship with Taylor who was head of the department. Is there anything else that you can recall from this period. Did they ever collaborate on a paper?

Laidler: Oh, yes. They did just before I was there. They collaborated on work on radiation-induced reactions, the influence of radiation on chemical reactions. I think there were perhaps only two papers on which they collaborated, but they were always extremely friendly and they had great respect for one another. Taylor was quoted as saying at one time that the best thing he ever did at Princeton was to bring Eyring to Princeton. He had tremendous admiration for Eyring. There was never any suggestion of jealousy on Taylor's part. Of course, he didn't have to be jealous because he was a great man himself. But one always got the impression of very great friendliness between them, and things always went very smoothly between them. I think Eyring had a very happy time at Princeton because Taylor did everything to make his position at Princeton a satisfactory one.

King: So in fact Eyring seemed to spend a lot of time working with people from Britain, Taylor and, of course, yourself and Wynne-Jones and who else?

Laidler: Well, there's Bryan Topley, who was there a few years before I was, and M. G. Evans, of course. Those are the only ones I can think of.

King: Did he ever express any opinions as to the difference between working with English and American colleagues?

Laidler: Oh, well, he had rather strange political views particularly before the war. Of course, when I was with him the war had either started or was imminent and he very frequently told me that he really didn't want any part in helping the British. He said this in a very friendly way; one couldn't be offended. He made it very clear that he was an American and wasn't going to fight any British wars. At the same time he always told me that he liked English people. He was very nice in that way, but he wasn't going to fight their wars.

There was a rather amusing sequel to this conversation. I remember that during the war, I had occasion to visit Eyring at Princeton, probably in 1944. I, through my war work, was able to read confidential reports, some of which had been written by Americans. I had, in fact, read a confidential report written by Eyring; I think it had something to do with detonation. I made the mistake of mentioning to Eyring that I had read this report. His reaction was rather surprising; he became really quite angry. Not with me, but with the fact that his government had seen fit to release his report to someone outside the United States. I explained to him that we were allies at that time and that I did in fact have access, up to a certain level, to secret papers in my field and that there wasn't very much he could do about it. He remained quite angry and said he wasn't sure that he was going to do any more work for the U. S. Government if it was going to show these papers to people outside of the United States.

He had this rather extreme nationalism which was perhaps a little surprising in a man who was so amiable in general. One would have thought that he would be more of an internationalist and not worry about nationalities, and rather, have thought of people as being people no matter where they happened to be born. As I say, he got on well with people from other countries. But he had this sort of political nationalism in the sense that as far as war was concerned at any rate, his war was just for the United States. In fact on one occasion he did say--now I think this was perhaps partly joking, but to some extent not--I think this was probably said before the war, that he would fight if Princeton, New Jersey, were invaded, but he wasn't going to do anything until that happened. So he held an even more extreme view than just fighting for the United States. He wanted to fight just for Princeton, New Jersey. I don't know how serious he was. He said everything with a laugh, so it's hard to know just what he really believed.

King: Today is the eighteenth of October, 1983 and this is the last of three conversations with Professor Keith Laidler at the University of Ottawa. When we stopped last week, I asked Dr. Laidler to recall for us some memories of the more human aspects of some of the kineticists that he's known. Dr. Laidler, you had just finished talking about Eyring. Are there any other personalities?

Laidler: Well, of course there's H. S. Taylor whom I knew very well at Princeton. I have already said a few things about him, but perhaps I can make a few other comments. He was a very fine person in every way. A very good department chairman, of course; he ran a splendid department at Princeton and was always a friendly, genial person. Everyone liked him. He always had a personal interest in his students. Because of the fact that he was so very efficient, he was perhaps not the most popular of people at Princeton. I did hear some people in other departments complaining somewhat about Taylor, but it was always to the effect that Taylor was doing so well for chemistry. People felt that he was getting things for chemistry that they were unable to get for their departments. Of course, that almost inevitably happens when a person is very efficient and also slightly aggressive. I think it has to be said that Taylor was slightly aggressive, and that he did fight for chemistry. Chairmen of other departments who were not as aggressive did not do so well for their departments and tended to resent Taylor's activities. Apart from that, I never heard any criticisms of any kind. He was really a very distinguished person who achieved a great deal in his lifetime. He was a tremendously hard worker. He did a number of things, not all in chemistry itself. After he retired he did a lot of things. He was very much involved in the Woodrow Wilson Foundation, for example. I think those are the main additional things I have to say about Taylor.

King: What about some earlier Oxford figures?

Laidler: I knew, of course, a number of Oxford dons when I was an undergraduate, but none extremely well at that time. I got to know some of them better later on. I knew E. J. Bowen at Oxford. He was a very quiet, rather shy person and unfortunately I never really got to know him very well. I wish I had myself been more aggressive and had talked to him more, because I know from having seen him afterwards that he was a most friendly, helpful person and it would have been very nice to have had the opportunity to get to know him. I think, however, that he was the sort of person who would not have taken the first approach. He always smiled very pleasantly when he saw me and I saw him many times as his laboratory was next to Hinshelwood's. I think he knew who I was when I was an undergraduate. He always smiled, but we never really got into any kind of conversation. I have heard from other people who knew him well that he was a splendid person. He was so retiring that I think that his rather distinguished research efforts were not noticed as much as they should have been.

D. L. Chapman is another one whom I might mention. Chapman had been a student of Vernon Harcourt. Now, when I was an undergraduate, Chapman was in his sixties and was perhaps even more retiring than Bowen. I went to some of his lectures. He had a reputation of being a very good lecturer, but he was rather dry, not at all inspiring. He didn't attend meetings very much. I don't ever recall him going to meetings of the Alembic Club, which was the chemical society and which was quite active in my time. We had regular meetings, with invited speakers. One didn't really get to know him at all, but I knew some students who worked with him and had a very high opinion of him. He had in his time, of course, done extremely distinguished work. I knew his wife by sight, Mrs. M. C. C. Chapman, also in her own right a very distinguished kineticist, who had published a number of papers on the hydrogen-chlorine reaction and in other fields.

I knew Wolfenden who shortly afterwards went to the States and was for many years at Dartmouth College. He was an electrochemist and he taught electrochemistry and was a demonstrator in the physical chemistry laboratory.

There was also Linnett, J. W. Linnett, Jack Linnett, who unfortunately died rather young some years ago. He eventually became professor of chemistry at Cambridge where he succeeded Norrish. I knew him very well. I got to know him later extremely well and he was an extremely friendly person who did very distinguished work in kinetics and some very distinguished work in other fields, such as molecular theory. Those were the main people I can remember while I was an undergraduate. Of course, I met many of them a number of times later on.

King: And after you came to Ottawa you worked with the distinguished kineticist E. W. R. Steacie. What about the visitors that came from the United States to visit Steacie's lab.

Laidler: Yes. I don't recall any particular visitors during the war years, but after I came to Ottawa in 1955 I do recall with very great pleasure a number of visits from W. A. Noyes, Albert Noyes, who was professor of chemistry at the University of Rochester and was a very close personal friend of Dr. Steacie's. He was always welcome to come to Ottawa and did in fact come, for a number of years, about once a year. He spent a few days each time and always gave a talk on his own research. He did this before Steacie died which was in 1962, I think, and he continued to come to Ottawa a number of times afterward. He was always extremely popular as he was always so kind and helpful to everyone. We always enjoyed his visits and we missed him when he finally was unable to come any longer.

King: Finally, Dr. Laidler you have an unusual hobby for a chemist, acting. And you've acted in a number of professional plays in Ottawa. Could you tell us how this interest developed?

Laidler: First of all, when you say a number of professional plays, that's very kind; but, at the same time, your statement requires a slight correction. It is really an amateur theatre I'm connected with, The Ottawa Little Theatre, although we do say that we have professional standards. We have professional standards and although some of the people who do act are extremely good, all of the actors and the directors are amateurs in the sense that they are not paid. I think it is very difficult to answer the question of how I got involved or why I got involved in acting.

I suppose there is a close affinity between lecturing and acting; I find it so. I find that experience in the theatre is helpful with teaching, and vice-versa. I think it is helpful for an actor to have done some lecturing, although it isn't quite the same kind of activity. But I have always enjoyed giving demonstrations while lecturing. I have always enjoyed putting on a little bit of a performance in special kinds of lectures such as the Christmas lectures that we've had at the University of Ottawa, for a good many years now, since about 1958. I think we have had lectures every Christmas and I've always enjoyed participating in those lectures. The Christmas lectures are designed for young people. To some extent that is a theatrical performance.

To come back to the question; when I was at school I enjoyed being in plays and from the age of about twenty-four or so on, I did participate in plays. I have played a number of parts in a variety of different plays. I've never, of course, found time to do it to a very great extent. I usually do, perhaps, one play a year, because when one is performing plays in the Ottawa Little Theatre, it is really quite demanding. Rehearsals last for a number of weeks, and occur perhaps every other night and every Sunday afternoon. When the play is on, or for two and a half weeks, one is, of course, tied up every evening. So it is quite demanding and quite tiring. I couldn't do it more than about once a year because it would take up too much of my time.

It is a relaxation that gets me away completely from the scientific world. One is really in a different world in some respects. As I say, it does have this affinity with lecturing, but in many respects, one is in a different world altogether and one meets completely different people. Most of the people one meets know nothing whatever about science. I think it's probably healthy for scientists to get to know people who know nothing whatever abut science, and this is a very easy way. One very naturally gets in close touch with people, and when they discover one is a scientist, they are always rather surprised. I think it doesn't hurt science for people to meet scientists doing that kind of thing, and to realize that scientists are in many ways just the same as other people.

King: Well, I'd have to say that I'm extremely sorry not to have had the chance to see you perform in a play during my year here. Dr. Laidler, thank you very much.

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