CHEMICAL HERITAGE FOUNDATION

WILLIAM VON EGGERS DOERING

Transcript of an Interview
Conducted by

James J. Bohning

in

Philadelphia, Pennsylvania and
Cambridge, Massachusetts

on

9 November 1990 and 29 May 1991

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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> William von Eggers Doering, interview by James J. Bohning at Philadelphia, Pennsylvania and Cambridge, Massachusetts, 9 November 1990 and 29 May 1991 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0085).

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

WILLIAM VON EGGERS DOERING

1917	Born in Fort Worth, Texas on 22 June

Education

1938	B.S., chemistry, Harvard University
1943	Ph.D., organic chemistry, Harvard University

Professional Experience

	Columbia University
1943-1945	Instructor
1945-1948	Assistant Professor
1948-1952	Associate Professor
1947-1969	Director of Research, Hickrill Chemical Research Foundation
	Yale University
1952-1956	Professor
1956-1967	Whitehead Professor
1962-1965	Director of the Division of Sciences
	Council for a Livable World
1962-1973	Chairman, Board of Directors
1973-1978	President
	Harvard University
1967-1968	Professor
1968-1986	Mallinckrodt Professor of Organic Chemistry
1986-	Mallinckrodt Professor of Chemistry Emeritus

Honors

1945	John Scott Medal, City of Philadelphia
1953	Pure Chemistry Award, American Chemical Society
1962	A. W. Hofmann Medal, Gesellschaft Deutscher Chemiker
1966	Creative Work in Synthetic Organic Chemistry, American Chemical Society
1967	William C. DeVane Medal, Yale University
1970	Theodore Williams Richards Medal, Northeastern Section, American Chemical Society

1973	Humboldt Senior Fellowship, Federal Republic of Germany
1974	D.Sc., honorary, Texas Christian University
1980	Honorary Professorship, Fudau University, Shanghai
1987	D.Nat.Sci., honoris causia, University of Karlsruhe
1989	James Flack Norris Award in Physical Organic Chemistry, Northeastern Section, American Chemical Society
1990	Robert A. Welch Award in Chemistry

ABSTRACT

William von Eggers Doering begins these interviews with a discussion of his early life and family background. His parents were both musicians, and met while they were both studying music in Leipzig. When World War I broke out, they moved to the United States, and his father became a vital statistician. His father eventually got a job teaching at Harvard University's School of Public Health, and the family moved to Cambridge, Massachusetts. Doering was influenced by his teachers during his early education to pursue science. After graduating from high school, Doering attended Harvard University, where he was inspired to major in chemistry. At Harvard, he took courses with Arthur Lamb, Louis Fieser, Elmer Kohler, and Paul Bartlett. Kohler encouraged Doering to continue on to graduate school, so he earned his Ph.D. in organic chemistry at Harvard in 1943. During his years as a graduate student, he did some research with Louis Fieser on new explosives, including trinitrobenzylnitrate, as well as anti-mustard gas work with Eric Ball. After he completed his graduate work, he joined Robert B. Woodward's team at Harvard, who was attempting to synthesize quinine. Less than a year later, Doering took an instructorship at Columbia University, but continued with the quinine project in his free time. Doering outlines his relationship with Woodward, the difficulties of the quinine work, and the impact of that research on his career. Doering spent nine years at Columbia before moving on to Yale University in 1952. While at Columbia, he helped to establish the Hickrill Chemical Research Foundation, which focused on postdoctoral research. It was there that Doering did most of his work on carbene. In the 1960s, he was asked to join the Board of Leo Szilard's new organization, Council for a Livable World. For over fifteen years, Doering was active in lobbying for this organization. Throughout his career, Doering was also a consultant for various companies. At Yale, Doering became Director of the Division of Sciences, and began to realize that administrative duties were taking too much time from his research. He planned to go to the University of Karlsruhe, but Woodward offered him a position at Harvard. Doering concludes the interviews with a discussion of his graduate students, his colleagues, and his interactions with Fudau University in China.

INTERVIEWER

James J. Bohning is currently Visiting Research Scientist at Lehigh University. He has served as Professor of Chemistry Emeritus at Wilkes University, where he was a faculty member from 1959 to 1990. He served there as chemistry department chair from 1970 to 1986 and environmental science department chair from 1987 to 1990. He was chair of the American Chemical Society's Division of the History of Chemistry in 1986, received the Division's outstanding paper award in 1989, and presented more than twenty-five papers before the Division at national meetings of the Society. He has written for the American Chemical Society News Service, and He has been on the advisory committee of the Society's National Historic Chemical Landmarks committee since its inception in 1992. He developed the oral history program of the Chemical Heritage Foundation beginning in 1985, and was the Foundation's Director of Oral History from 1990 to 1995.

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INTERVIEWER: James J. Bohning

LOCATION: Philadelphia, Pennsylvania

DATE: 9 November 1990

BOHNING: I know that you were born on June 22, 1917 in Fort Worth. Could you tell me something about your parents and your family background?

DOERING: My mother [Antoinette von Eggers] was born in 1884 in St. Thomas, which at that time was owned by Denmark and where her grandfather was governor. She was Danish and came from an old family knighted in 1204. That's where the 'von Eggers' comes from. She was a concert pianist, grew up in Copenhagen and then concertized in Europe. By 1911 she was teaching piano at the conservatory of music in Leipzig. My father, Carl Rups Doering, was born in Philadelphia in 1889. His mother died when he was quite young. His Uncle George was very interested in music and pushed him quite hard to become a pianist and a composer. He was shipped over to Leipzig to study music and composition around 1911.

That's where they met. They were married in 1913 in Copenhagen and stayed in Leipzig after the war broke out. Sometime later there's a marvelous letter from his father to them, saying, "Look, the United States is going to get into this war and you'd better come home." So indeed they did. I think they arrived in New York in 1915, both looking for employment as musicians. It turned out that at Texas Christian University in Fort Worth there were two openings, my mother teaching piano, and my father teaching composition. That lasted for a while, but my father quite wisely decided that he was not going to be a [Johannes] Brahms or a [Robert] Schumann. [laughter] Because of this, and probably also because of the war, he decided to enter medical school at Baylor [University]. To me, one of the more amusing aspects was that he never went to college. He graduated from Central High School in Philadelphia, a high school with an enormous reputation for the quality of its graduates, and was admitted directly to medical school from this high school with many years of nothing but music intervening. [laughter] Apparently he commuted back and forth between Fort Worth and Dallas, where the Baylor Medical School was.

That's how I came to be born in Fort Worth. We stayed there for a little less than five years. When I was five, we moved to Baltimore. By that time my father had decided that practicing medicine was not what he was interested in but that he was interested in the application of mathematics to medical problems. So he went to the Johns Hopkins School of Public Health in Baltimore, where he got whatever the degree it is you get in public health, as a vital statistician. Statistics turned out to be his real love. We were there for three years before coming to Cambridge, where he had been appointed to the Harvard School of Public Health as a vital statistician.

BOHNING: Was your mother still teaching when you moved?

DOERING: She taught piano all her life, as a matter of fact. My father also loved music, so there was music in the house at all times. They found those years in Fort Worth, which after all was quite a long time ago, the most delightful in their lives in the United States. I have no understanding of it, but I still marvel at my little Danish mother who was used to being entertained by nobility, the Esterhazys, people like that, as she toured around Europe. She gave her first concert in the Tivoli Gardens in Copenhagen when she was eleven. They obviously just loved the openness and the naturalness and the sincere enthusiasm of the musical culture in the Fort Worth of those days, which was really quite active. And I suppose there was a significant German population in that part of the world.

It was very, very moving to me many years later to get an honorary degree from TCU. I met some of these old, inevitably, ladies, the men having all died off before! [laughter] But with their marbles still in place, they remembered all sorts of details. They were so glad to see what had happened to this little Billy that they had remembered as a baby in Fort Worth! [laughter]

BOHNING: Did you have any brothers or sisters?

DOERING: I had a brother who was nine years younger. In effect, coming to that point, I grew up as an older child during those years when your sense of self-importance is aggravated by being an only child or the first-born child! [laughter] One never can look into oneself and see to what extent that makes a difference, but in retrospect it’s perfectly clear that it does.

Harvey [von Eggers Doering] went into metallurgical engineering and died a couple of years ago of a stroke. So there were just the two of us.

BOHNING: You started your schooling in Baltimore, then?

DOERING: Not really, no.

BOHNING: You were six to eight years old in Baltimore.

DOERING: There was no schooling in Baltimore that I can remember. How that worked, I simply don’t know. The first schooling I can remember was in Cambridge. I remember going one year to the Agassi School and another year to the Peabody School and another year to the Russell School. I skipped the fourth grade and was put into the Shady Hill School, which at the time was a famous progressive school. It had been started by Mrs. Hocking as an educational experiment where we were not going to be put through all the rote learning and so forth and so on. (I don't remember her first name and I didn't know her.)

In those first three years of school, I was an incredibly sickly child. Those were the days when, if you got measles, you were likely to get pneumonia with it and be dead as a smelt within a few weeks since there was no penicillin or anything like that. So a bout with measles and pneumonia kept me out of school for months. From what I'm told, out of those three years I didn't have very many more than a few months of schooling.

I entered the Shady Hill School in the fifth grade. In that school three persons really worked quite an influence on me. There was a Mr. McCarthy, who taught Latin. He just loved Latin. He loved the order of it, and the fact that Latin was so well designed that you could say whatever you were thinking in a way that could not be misunderstood—by anyone who knew Latin! [laughter] To this day it works an influence on me. I write with no style whatsoever, but I pay a tremendous attention to writing in such a way that what I'm trying to say can't be misunderstood. I think Mr. McCarthy would be satisfied! Even though he would say, "You're no Horace or Virgil or Cicero." [laughter] In any event, you can get down on paper what you're thinking, <u>if</u> you know what you're thinking. That is one of the beauties of Latin to me, that it really required one to examine one's thoughts and try to discover what it was you were trying to say, rather than the usual way, "I don't know what I'm thinking until I've heard what I've said." [laughter]

Then there was Miss Ruth Edgett, who taught math. "A messy desk comes from a messy mind." [laughter] She was very rigorous. There were two of these older women. They were the type of lady that one could encounter some decades ago at places like Smith [College] or Mount Holyoke [College] or in various parts of New England, and I guess also in Fort Worth, Texas. They were women for whom "Women's Liberation" really just didn't mean anything. They were already liberated. They knew what they wanted to do and they did it with a great deal of self-confidence. It wasn't a question of competing with men—they knew who they were in the world. Old New England was essentially a matriarchal culture, with the women being the tough ones, in one family after the other! Some of the men might have been crooked and therefore made a lot of money, but the power really lay in these women. Well, Miss Edgett and Miss Putnam were two spinsters who lived in Beverly, Massachusetts, and commuted every day to Cambridge. Miss Edgett was a big, strong, solid woman who loved her math and loved teaching it, and Miss Putnam taught science.

The whole attitude of the school, the whole principle on which it was based, was to teach you how to think and how to ask questions. The emphasis was not on what you learned. To a great extent it was designed to make learning an experience. For example, when one was studying Greek culture—study isn't the word to use—you had to have Olympic Games and get dressed up in togas and run around the field and have a wreath put on your head at the end of it! [laughter] It wasn't unstructured in a real sense, but by comparison with conventional schooling, it certainly was unstructured. Those years at Shady Hill School were perhaps the only important part of my education. I stayed on through the ninth grade, by which time there was no question in my mind I was going to become a scientist.

Partly that's because my father was a vital statistician, and certainly partly because of Miss Putnam, the third one in the group that worked an influence on me. She was a very quiet, reserved, unsmiling person, whom from a distance I just sort of worshipped—like God. [laughter] So I knew I was going to be a scientist. I went from there to the Belmont Hill School, which was and still is a private school. Just to show how great my formal education was in the first nine grades of my life, at the end of my first semester at Belmont Hill I had two Ds and two Es. But I ended up getting an award. That was the first award in my life. It was for showing the greatest improvement! The only reason I got it was that I started so low! [laughter]

But part of the background, which I think accounts for a great deal of a subsequent attitude that I developed, was that of being an outsider, and of <u>wanting</u> to be an outsider. In the end it really had a big influence on my scientific work, one of avoiding any subject which was crowded or in which other persons were working. It had various consequences. One was that as soon as I had in effect opened, or very shortly after having opened a field, if other persons flocked into it, I would move on. I would try as hard as I could to make what seemed to me the important discoveries or follow the important consequences of the initiating concept. Divalent carbon would be an example. Then, as soon as everyone and his brother jumped into the field, as far as I was concerned there were other parts of the West to explore. You don't stay around any longer—you move on! [laughter] That's part of being an outsider. A lot of that really came from those years, perhaps from age ten on.

By the time I was ten, my father was in very serious psychological difficulties. He drank throughout his life. In those days it was fashionable to take barbiturates and something else, possibly belladonna. There was a trio. Every so often he would come staggering home. My mother would put him upstairs and bring up his meal. It was a very strenuous childhood in that sense. She was a remarkable woman in that, certainly misguidedly, she believed that it was the outside world that was making her beloved husband depressed. She was totally unsophisticated, in a modern sense of looking at somebody who's in trouble and trying to ask, "Well, what's the basis for all that?" She was enormously protective of me. She <u>never</u> had a bad word to say about him and only praise for his many very fine, wonderful qualities. He was an extraordinarily kind, gentle man, and obviously very sensitive. His own mother had died when he was ten or eleven, a bad time to lose a parent in the life of any young person. So he grew up in a household with no woman, in which he would come home from Central High School and have dinner ready for his older brother and his father. I cherished the little windows in his life when he was just fine and would talk to me about this, that, and the other thing.

I was very lucky in having a strong affection for both parents, and not being obliged, as so many youngsters that I see today are, to make a choice between one or the other. But even more critical, as a male child I did not have my father destroyed by my mother. Any modern wife would have really complained, because hers was not an easy life and he obviously was not progressing in his profession as well as he might. Part of the reason she taught piano all her life was to make a little more money.

But he worked so hard, and that was where his pleasures and his escape came from. I'm sure that just seeing that trait inculcated in me the conviction that your work is your most important thing. No matter how rotten the world around you might be personally, you can survive through your work. In the end he made a very interesting discovery. He was the person in 1928 who discovered statistically the relation between smoking and lung cancer. He was a desperately ill man, but nevertheless a wonderful, lovable person. So I had a chance to learn positive lessons and negative lessons, but never, never was he destroyed as an image.

[END OF TAPE, SIDE 1]

DOERING: One of the consequences of my father's condition, for me, was a very limited social life. There was always a great danger if one had a party that he would have too much to drink, or just wouldn't be able to get through it. It was a tremendous burden on my mother, and also on me, although I didn't look on it as a burden at the time. Maybe I should have been angry about it as they now say one should. But I just felt so sorry for him and so sad for not knowing how all of this was coming about. So there weren't very many people in the house. My mother had a few friends in Cambridge. As a little Danish baroness, she was deeply offended by the airs put on by old Cambridge ladies. As far as she was concerned, they were just all nouveau, who didn't know what upstarts they were! [laughter] My father had a few very dear friends, partly through his music. They would come to the house and he would accompany them in their singing or they would play four hands together. I guess as a youngster I would listen to that and watch it and think how good it was that they liked my father. But the big formal parties were pretty much out.

The only reason I went to Belmont Hill School was because of one of these persons. Alice Thorpe was a granddaughter of Henry Wadsworth Longfellow. She felt that the environment in which I was growing up was not good for me. My father was in and out of institutions, like Riggs in Southbridge, Massachusetts. He tried to take his life twice. It was not what one would have called an easy childhood, and she thought I ought to get out. She actually paid for my tuition at Belmont Hill School and so I boarded out there. It's only a few minutes from Cambridge, but I boarded there five days a week and then would come home on weekends. I have no way of telling whether it was a good thing or a bad thing, but that's what happened.

BOHNING: You said at this point you were already determined to do something in science. Had you started to refine that interest a little bit?

DOERING: It's an important question because the kind of interest that I developed in science was undisciplined in Shady Hill School. The purpose of the teaching there was that you should become interested, not that you learned anything in particular. You were to develop an intense interest in what you were doing. That's why you participated in Greek games. That became a part of you, but the rest of Greek history is still a pretty dim mystery in my mind. [laughter] But there must have been something great about it, because we put on Olympic Games. It was a very funny way of doing things, but effective.

At Belmont Hill School there was essentially no science. There was no biology, there was no chemistry. There was, of course, mathematics, and there may have been a course in physics, but that was it. So there was no possibility of doing any science. There was no specific focus at all to the conviction that I was going to be a scientist. My father spoke about something that meant nothing to me at all, namely physiological chemistry. We don't even know what that is now, let alone then. [laughter] And least of all, me! But when I went to Harvard I took physics and chemistry and math.

BOHNING: Was it just assumed that you would go to Harvard? Did you think of other places or was it because you were right there?

DOERING: I don't think we ever thought of any other place, not because Harvard might have been the best college or anything of that sort, but because really we didn't have any money. My father never went beyond assistant professor. He retired at a reasonable age, but he was never promoted in the School of Public Health. He was an enormously revered teacher. He would get up at 4:30 in the morning before every lecture. He never missed lectures because of all of this nonsense, all his troubles.

BOHNING: Amazing.

DOERING: Yes, it really was. It was a very strange life. But his students, who at that time in the School of Public Health came from all over the world because it was one of the two or three outstanding schools of public health, just worshipped him. So he obviously was deeply concerned about them, and about teaching them.

But in any event, the Depression was on. We would have meat maybe once or twice a week, and in between something that I still don't know how to make—simulated meatballs out of lentils. [laughter] If you think that's a delicious dish you've got another thought coming. So it was a non-materialistic bringing up. My mother was responsible for seeing that we moved from one place to another until finally we lived in a halfway reasonable part of Cambridge. But it's also very important in my attitude toward money. My mother, who worked hard at a time when we had very little money, nevertheless sent a large amount of that money back to her sister, who had married a Lutheran minister in Leipzig. Now, Lutheran ministers were not known for the high salary that they were paid. And 1921-1922 was a devastating time of high inflation in Germany, so they were always strapped. I just took it for granted that you didn't spend much money on yourself. You supported persons who were close to you and you did what you could for them. It was quite interesting in that connection, particularly in the United States, where as we both know the amount of money you make is to some extent a measure of your success in the world. But beyond that, more seriously it was a model which said you should care about other persons and you should be willing to make sacrifices for them. One of the ridiculous consequences is, to this day, if somebody throws out a round-bottom flask that is still usable, I just think it's a sin. [laughter] Wastefulness is just not in the proper order of the world.

Coming back to your question, it wasn't until I came to Harvard and took general chemistry as a freshman with Arthur Beckett Lamb. He put on marvelous fireworks from the thermite reaction, using aluminum and iron powder. There were sparks flying all over the people in the front row, burning holes in their coats. Imagine doing it today! [laughter] So I thought it was pretty exciting, and just wanted to go on and pursue it. The next year, as a sophomore, I took Louis Fieser's organic chemistry. There's no question that he inspired me to go into organic chemistry. I thought that was the most exciting subject I had ever encountered in my life, and he was so enthusiastic. He had a funny habit of giving bonus points—that shows how much mathematics he knew. You could get a grade of one hundred five percent by answering the bonus question. My goal was to end up with an average above a hundred percent. [laughter] I don't think I achieved it, but I came damn close. It was a subject that came very easily to me.

BOHNING: What was the attraction besides Fieser's enthusiasm?

DOERING: Certainly part of it was his personal influence. But partly it was doing exceedingly well in it. That is, all parts of it were easy. I don't remember that the three-dimensional stereochemical part was either easy or difficult or particularly fascinating. I just don't remember that. The fact is that some of the three dimensionality of organic chemistry has always been easy for me and appealing to me. But where the devil that came from, I don't think I could say. As it turned out, Fieser's course was woefully out-of-date in 1932 when I took it. It was probably a good course around the turn of the century or a little before. [laughter] But there is no question it was Fieser's that locked me in.

I think it was probably the junior year that I took Chemistry 105, Advanced Organic, from Elmer Peter Kohler. The intellectual level of that course in mechanistic terms was far above anything Fieser taught. Mechanism for Fieser was one molar equivalence of lassos. If you wanted to know how a reaction went, you had to know where to place the lassos; to eliminate HBr or whatever it might be. [laughter] That was mechanism. So I realized immediately I just had to forget the whole way of thinking that I had got from Fieser and start to relearn. But by that time I was totally locked into organic chemistry.

BOHNING: What kind of laboratory work had you done with Lamb and with Fieser in those first two years? What were the lab courses like?

DOERING: As far as the manual part of organic chemistry is concerned, I think I should go back further. As a boy of eight, nine, or ten, with plenty of time on my hands, I became enormously infatuated with making model airplanes. There was little money available and no kits available where you glued things together. If you wanted to make a propeller for this little plane, you started with a block of balsa wood and a knife, and you cut it out. And if you made one false cut, then that piece of balsa wood and that propeller were lost. [laughter] In my memory, that little block had cost a lot of money. It was a precious thing to have a little block of balsa wood. So I spent a lot of time carving it. In retrospect, it seems quite clear to me that activity developed a lot of what's ordinarily called manual dexterity. Beyond that, I developed a real liking for working with my hands, as opposed to working with my head. I don't think there's any question that's where it started, because all through the years at Belmont Hill School there was no laboratory.

I can't begin to remember what the lab was like in Lamb's course. (I'm a bad person to interview because I have an absolutely lousy memory for detail and anecdotes and things like that. [laughter]) I guess the Fieser experiments were straight out of his book, a green book as I remember it (1). He had a wonderful exercise that anybody who took his course at that time would remember and would recall for you. At the end of the course, he had a competition. Of the one or two hundred students in all the lab sections, the teaching assistants would pick out a few who seemed to have more ability in the lab than others. There might be twenty that would line up one afternoon and be given a sample. I don't remember what the starting material was, but the end product was Martius Yellow. [laughter] It's a famous thing that's in his book.

BOHNING: Yes, I know it well.

DOERING: You had to run through five or seven steps, and it was a race against time to see who could finish first. You had to get everything crystalline, take a little sample at each stage, put it in a vial, label it, and put a cork in it. You planned ahead, of course, but fundamentally you were given the starting stuff and then you had to go through these seven operations. He had done it in four hours, and was terribly proud of it.

Louis was a baby in so many ways it's just not describable. [laughter] During the War he got into napalm, and got pleasure in building little wooden houses out in the yard behind Converse. He'd alert everybody that he was going to be testing the next sample of napalm. He did this with no feeling of human compassion. What am I involved in? What am I doing? This is going to be used to put globs of burning jelled gasoline on Japanese. He would just be an absolute kid as this thing would go up in flames! [laughter]

So, in any event, I was <u>determined</u> that I was going to do the sequence faster than Louis had. Now, where that feeling of competitiveness comes from, I don't know. It doesn't seem to me to be reflected in other parts of my early life. So I just whipped through that damn thing and finished in three hours and forty minutes. Then the teaching assistant, who realized there was going to be somewhat of a confrontation over this, said, "Look, we judge both the time that it takes to do it, and also the quality of the samples that you submit. One of yours is a little off-color. Wouldn't you like to take the opportunity to recrystallize?" I said, "No way!" [laughter] "I'm staying with my three hours and forty minutes." Where did I learn that working with my hands was not so much work as an enjoyment in working with them? I think it came much earlier. You just came into that laboratory course knowing whatever the substrate was, you could handle it and you enjoyed handling it and you took pride in how you handled it. That's really true. That expresses my feeling toward experimental work.

BOHNING: Were you taking math and physics along with chemistry in those first few years?

DOERING: Yes. Math came very easily for me up to a point, and then it was as if I hit a wall. It was the kind of thing that marathon runners talk about. All of a sudden the next course was just incredibly difficult and not making any sense, for reasons that I didn't understand. That was the end of my math. Of course, I regretted it later, so much so that from my own experience I've advised any number of younger persons to take as much math as they possibly can, because you're not going to be able to learn it easily later in life. In fact, you're not really going to be able to learn it. It will never become a part of you. You may learn the operations or the techniques or the "rules" for doing it, but it's never going to be your language. The only persons I've ever talked to who were really theoretical physical chemists were persons like [John G.] Kirkwood and [Lars] Onsager. For them, looking at an equation was a language, and it spoke to them. But, I've got to try to figure out why is this in the denominator and why is that an integral and what does the integral mean. For Onsager that's just a language. He just reads it, in the way that certain musicians can sit and read a score and hear the whole piece. Those are the people who really handle mathematics. I wasn't in that category.

[END OF TAPE, SIDE 2]

BOHNING: You commented about Fieser being out-of-date and Kohler being mechanistically oriented, which was really relatively new in 1932 or so. How did the two compare in age?

DOERING: The year I took his course, Kohler was either seventy-two or seventy-three. Before the course had ended, he was dead of a brain tumor. So as that course went on, there were interruptions in his lecturing, and that was when [Paul D.] Bartlett substituted. After he died, as far as Bartlett's career is concerned, Bartlett continued with that course. It was called Chemistry 5 in those days. It's interesting from the point of view of the history of chemistry in the United

States, at least enormously interesting to me, that Paul took that course and brought it up to date in a way that a seventy-three-year-old man who's dying of brain cancer isn't really bringing it up to date. He was also trying very hard, and certainly instilling in you the notion, that it's in theory and mechanism that the heart of organic chemistry beats. It doesn't beat in synthesis. That's a form of applied organic chemistry. Organic chemistry is understanding how you transform one structure of matter into another structure of matter. So it is to some extent a philosophical attitude toward organic chemistry that was instilled by Kohler and then carried on and developed to the highest degree at that time by Bartlett.

Bartlett, having been at Harvard, had then under his tutelage some undergraduates. That by itself didn't have much influence on organic chemistry in the United States because so few of the Harvard undergraduates went on for Ph.D.s, both then and now. (This is an aside.) In all the twenty years I've been at Harvard, there have been five, plus or minus one, undergraduates who went on for a Ph.D. in chemistry. It's a funny undergraduate body. It's not an MIT bunch of undergraduates, that's for sure.

But at the graduate level, of course, all graduate students had to take Bartlett's course, Chemistry 5. Through that course the importance of physical, mechanistic, organic chemistry spread through the United States. Three-quarters of a graduating class first of all wanted a job in academic research and teaching. And those that got it, then, were very proud to introduce a course that wasn't available no matter where they went—Illinois or wherever. There wasn't a course in mechanistic organic chemistry. So in the span of within ten or fifteen years, that became a standard item in the senior graduate curriculum of chemistry departments in the United States. It finally led to the death of the course because in later years graduate students would already have had the equivalent, and no longer needed to take Paul's course. Paul's heart was breaking. "What am I doing wrong?" [laughter] It's that he had done it so well, and it had worked its influence right down into the level of upperclass undergraduates.

So Kohler played a key role in my going on with organic chemistry. Perhaps because of my childhood, I had very little self-confidence. I was sure all those years I was in Belmont Hill School that there was no way for me to go to college because I'd look at my fellow students who were doing so well. The only reason I got into college was that it was very easy at that time to get into Harvard. All you had to do was apply in those days. When you got in, they told you, "By the November hour examinations, a fair number of you will be asked to leave. And by the end of the year, one-third of you will be gone." And one-third was gone. Well, you can imagine the effect that had on the troops that remained. [laughter] In the first place, you felt damn good that you had survived! But more importantly, you worked very hard in order to survive, or you didn't and you were bounced out. So, it was a completely different atmosphere. If it hadn't been for that, today I never would have gotten into college. I mean, with all those damn exams! [laughter]

Well, this lack of self-confidence persisted, so that by the time I was a senior, I still had no idea that I was going to stay in chemistry. I had become very interested in sociology. There was a fascinating professor of biochemistry at Harvard, Lawrence Henderson, a physical biochemist. He had established the partial pressure relation of carbon dioxide and oxygen in the blood. He worked out the physical chemistry of the transport of carbon dioxide and how these components interacted with one another. After that, he lost interest in biochemistry and become interested in the application of the scientific method to the study of the interaction of human beings. Fundamentally, it was a question of whether you could apply the scientific method to sociology. He was not the originator of that approach, but he latched on to the work of an Italian economist, Vilfredo Pareto, who had tried to convert sociology into a science. He tried to approach problems in sociology with the methods of science. (I could go into this, but this isn't the place.)

My father had also become interested in Pareto. Somehow he knew Lawrence Henderson, and suggested that I take this graduate seminar that Henderson gave in sociology. It was really a course in reading the four volumes of Pareto's *Mind and Society* (2). Why I enjoyed that so much, I'll never know. I just became infatuated with it. I was majoring in chemistry, so I kept up those courses. But my grades gradually declined. That's another reason why George Kistiakowsky has always been pissed off, because I ended up getting a C in his physical chemistry course. [laughter] Meanwhile, I was spending all this time on this aspect of sociology, and for no credit. You couldn't take the course for credit. By the time I was nearing the end, it was a question whether I should go into sociology or into chemistry. Not having any confidence that I should do either, my father arranged to take me around to Henderson to talk to him about the problem of where I should go and what I should do. Henderson was very clear. He said "You have a knack for this kind of thing and you're obviously interested in it and you could certainly do it well." He also knew Kohler very well, and Kohler had just come back from one of his episodes with the brain tumor. Henderson said something to me which was absolutely decisive. "If you're the kind of person who can tolerate knowing where you are in the world—that is, what the value of your work, your contributions is—then you should go into chemistry. If you can't tolerate that, then you should go into sociology." Wow! What an impact that had on me! On what, as I look back, was still a very little boy. [laughter]

So I said, "Well, okay, I guess I'm man enough to know where I am in the world and not worry about it too much." Meanwhile, Kohler was encouraging me to go to graduate school. That came as a complete surprise, because here I was, surrounded by all these teaching assistants knowing so much more than I. I really had no notion of going on for graduate school and would not have without Kohler's encouragement. He said, "Well, if it's a matter of money, I will get you a teaching job at Hobart College." I can remember—clearly—Hobart College. "You go there for a couple of years and save up a little money and then you can come back to graduate school." [laughter] In fact, it wasn't a question of money. My grandfather, who had been a minor industrialist in Philadelphia, had left me ten thousand dollars. That was really a lot of money at a time when tuition at Harvard was four hundred dollars. I got a partial scholarship, so I ended up really never having to work my way through graduate school as a teaching assistant or anything like that. It didn't cost very much if you didn't spend very much, and if you were careful enough. In those days, you had to pay for your own breakage. So if you were careful about that, your bill at the end of the year wasn't so tremendous. [laughter]

When Kohler said that I should go to graduate school, I just took his word for it; I just believed he must be a better judge of me than I was. This old man really was a major influence on me. I just worshipped him. He was just something. The notion that he was still teaching and enthusiastic at seventy-three, even now makes a big difference in my life. I'm seventy-three years old, and I still think it's the greatest fun and just the most wonderful thing, to be able to continue doing research. Without Kohler as a model, who's to say?

BOHNING: That's true, yes. Did you do any research as an undergraduate?

DOERING: Yes. In the senior year I was doing a little research with Kohler. I then wanted to do my graduate work with him, of course. He had agreed that it would be possible and gave me a little problem to make <u>cis</u>-1,2-dibromocyclohexane. [laughter] The only isomer known at the time was the <u>trans</u>. He was fully aware of the importance of the cis isomer and how it would bear on elimination. Dear God, I mean, neither he nor I had a clue as to how to make the <u>cis</u> isomer! [laughter] (Many years later somebody did make <u>cis</u>-1,2-dibromocyclohexane!) But by that time I was so enthusiastic that I set up a little lab in the basement of our house and found another very enthusiastic person. In those days at Harvard, seats were assigned alphabetically, so that the persons I knew best in the class all had names beginning with D! So Aldrich Durant and I set up a lab and tried, in the time that was available to us at Harvard and in the time at home, to make this <u>cis</u>-dibromocyclohexane.

What's the impact of having done that? Just to be doing something by oneself on one's own. You identify with that problem. It's yours, and it's your chance to create something. God knows, it's very hard for me to look back at what my emotional attitude was. There's no question about the enthusiasm. I spent hours and hours at that. There's also no question that if Harvard College had been five years, I would have flunked out. In my freshman year, I got four As, and from then on it really went downhill. I managed to graduate cum laude on the basis of grades that came in the early part of my college career but certainly not the latter part! That was because I was spending more time on this little project and a lot of time on the sociology. Classes for me were for the birds by then.

BOHNING: What other chemistry courses did you take? Analytical?

DOERING: I took analytical chemistry with Gregory V. Baxter. I think he followed [Theodore W.] Richards. That experience was really great for the rest of my life; the notion of precision as opposed to accuracy and the attention to detail that goes into analytical work, the notion that you do have some control over the experimental errors involved. He had one very nice experiment, learning how to use the microbalance. Of course, in those days it wasn't a matter of pushing buttons and waiting for the number to come up on a screen. You watched this pointer swing. You would write down the numbers for the left-hand swing and the numbers for the right-hand swing and finally average them up. At one point, he said, "Well now, you've got to do that ten times to find out just how good you are *vis-à-vis* that balance," because at the end of eight or ten times you can get a standard deviation on the weight. Then he said, "By the end of the course, you'll do it again, to see how you compare, and whether you now are any better at handling this balance." [laughter] There were in that course really some little things of that sort that changed one's attitude towards one's work. In organic chemistry so much of it is qualitative, and to have had that quantitative attitude firmly implanted and to have a high respect for it, was very valuable.

Then I took a course with George Shannon Forbes. I remember so little about the course. I just struggled in that course because it was largely inorganic chemistry, and he was the dullest lecturer I think I have ever heard in my life. I was excited about other things and here all I could do was memorize this stuff. There was no unifying theme, or small unifying principles involved. Oh, God, I worked so hard in that course to finally get a B- in it. It didn't have much impact, that I can say. [laughter]

BOHNING: So you had pretty well decided that you were going to get your Ph.D. under Kohler's influence and then he died.

DOERING: And then he died, that's right. That was a serious experience for me. I have wondered since, whether as a person without much self-confidence, doesn't one latch onto certain persons as anchors, if you will, or as figures who were going to help you through what might be a very difficult part of your life.

But in any event, they brought on Carl Noller as a temporary replacement. Do you remember Carl Noller? He wrote a textbook (3).

BOHNING: Yes, I remember it was a big, green textbook.

DOERING: A big, green textbook, that's right. Carl Noller.

BOHNING: He was at Stanford.

DOERING: He was at Stanford, and he came as a visiting professor. Now, in retrospect, what the devil were they doing suggesting to a first-year graduate student that he do his research with someone who was only going to be there for one year? I'll be dipped. I just don't understand how they allowed it. What were they doing? Anyway, I decided I would work with Noller. By that time I was clearly somewhat disaffected with Fieser, and the choice had seemed to be Louis Fieser. I don't know why I didn't think of young Paul Bartlett. Maybe he was too close to my age. But anyway, I decided I would work with Noller. And what a disastrous year that was in a scientific sense!

BOHNING: You were doing that phase rule problem (4).

DOERING: Yes, but there was something before that. One of his students at Stanford had taken secondary butyl bromide, which is not a very complicated starting material. [laughter] What follows is unbelievable, but it gives one such an insight into the way most persons were thinking about chemistry in those days. Noller was not a nobody; he was one of the rising stars. That star dimmed fairly quickly, but in any event here he was. His student had taken secondary butyl bromide and triethylamine in an alcohol solvent. I state it that way because of the way the story developed. He had obtained a liquid product, which had a boiling point that wasn't inconsistent with 1,2,3,4-tetramethylcyclobutane. (This is long before IR [infrared] or anything like that.) Now, you can visualize how that would have come about. You take two secondary butyl bromides, two molar equivalents of lassos and you pull out two moles of HBr to form a four-membered ring. Wouldn't that be exciting? Oh, yes! Yes! That would be tremendously exciting. [laughter]

So I repeated the damn reaction and got a little bit of liquid, just as his student had. There wasn't much you could do with it. You could get it analyzed, and it clearly was not a hydrocarbon, because C and H didn't add up. [laughter] They weren't the right ratio, so it was something else. It turned out that the solvent that his student had used, and therefore I had used, was isobutyl alcohol. Now, don't ask me why in God's name anyone would have used triethylamine and isobutyl alcohol. The upshot of it was this liquid was an ether, secondary butyl isobutyl ether, which of course I synthesized independently to show that they were the same! At least it was an unknown compound! [laughter] But it's fascinating, isn't it, how the thinking was at the time? That you could even imagine making tetramethylcyclobutane by pulling out two moles of HBr from secondary butyl bromide. Absolutely fascinating!

So it was after that that he said, "Well, I'm very interested in the Grignard reagent, and the question of dialkyl magnesium compounds." Actually, we didn't go to work on making dialkyl magnesium, which at that time was done usually by precipitating out the magnesium bromide with dioxane, but rather investigated this phase diagram (4). So, I learned how to make anhydrous magnesium bromide.

[END OF TAPE, SIDE 3]

DOERING: At the end of that I realized the whole thing was just a total bust, so I spent the next semester learning how to blow glass. I would go down to the glass blower and learn how to turn tubing and so forth. That has been helpful and useful all my life, being able to do a fair amount of reasonable glass blowing.

BOHNING: What kind of a person was Noller at the time you were working for him?

DOERING: Aloof, a little apart, but a nice person as I remember him. I sort of kept a distance from him. [laughter]

BOHNING: Did anybody else work for him at the same time during that one year?

DOERING: Not that I can remember. Probably there was, but I don't remember who.

BOHNING: Certainly that wasn't expected to turn into a thesis problem.

DOERING: God knows what it was supposed to do. That ended up with him leaving, and I was told that I had better find somebody else. Well, Louis was the only guy. So I asked Louis if I could come around and see him. He said, "Well, I'm terribly busy, but maybe I can see you in three days at two o'clock in the morning." I said, "Fine." [laughter] But meanwhile I asked around, and found out that Louis was in the process of stealing vitamin K from [Edward A.] Doisy. There wasn't any question in my mind that it was just plain, simple stealing. Poor old Doisy would be sending his communications into the *Journal of the American Chemical Society*, the editor of which was Arthur Beckett Lamb, sitting there in an office at Harvard. He would call Louis in to referee the papers. Louis lived under the illusion that all quinones belonged to him. Anybody who was doing anything with a quinone had stolen it from him. [laughter] So that was his rationalization for taking this hot stuff, going into his laboratory, repeating it, and carrying it on a few steps further. Everyone was really mobilized to work the whole night through. If you go back to the literature of that time, you find poor Doisy, who was really the sole contributor to that, having the thing pulled out from under him by Louis. At that time, I was still idealistic and I can hope that maybe I still am. But I'm also a little more realistic, and I guess that kind of thing happens occasionally. But I was really so profoundly, deeply offended that I said, "If there's no one else to work with, I'm just leaving chemistry." I found it absolutely appalling behavior.

Then they said, "A new professor, [Sir Reginald P.] Linstead, is coming from England." And I decided without even having seen him that I would work for him, because I certainly couldn't work for Louis.

Then I just took a little bit of my remaining money and went to Europe. I bought a bicycle in Bremen when I landed and spent a couple of months bicycling around Germany alone. It was at that time that I found a little book by Erich Hückel on the theoretical foundation of organic chemistry, the *Theoretische Grundlagen der organischen Chemie* (5). I saw this in a window in Heidelberg, and I said, "God, I've never heard of anything like this." Then I paged through the book and had never seen anything like this. I thought, "Well, I'll buy it, and see what sense I can make of it." [laughter] So I kept on bicycling. Finally, the youth hostels were beginning to empty out and I couldn't understand why. I didn't read a newspaper or hear a radio. When I would get to a place like Munich, I would go to the opera. As a student, you could go for practically nothing so that you really weren't spending much money at all. And I kept bicycling. I went to Freiburg and bicycled up through the Black Forest and down the Rhine. It was clear something was happening, although I didn't have a clue what it might be. There were men in civilian clothes getting into groups in the square and marching. I thought that was silly, so I took a picture of one of the groups in one of those small Rhine villages. I was put in the police station right after I'd done it, and had the film confiscated! [laughter]

So the war broke out, and I bicycled on to Cologne, where I met a Texan who had a car. I convinced him that there was absolutely no point in going to places like Holland, because the Germans would invade immediately and we'd never get out that way, but we should drive to Denmark, where I had relatives. So I convinced him to drive to Denmark and that was a good thing. We finally got back on a freighter. And there was Linstead.

BOHNING: This would be the fall of 1939?

DOERING: Yes, in October. He suggested a problem on the general subject of the stereochemistry of catalytic hydrogenation. We would work on diphenic acid as the substrate, diphenyl with two carboxylic acid groups. There were six stereoisomers of the perhydrodiphenic acids, and my job was to make them and isolate them. I enjoyed that very much. I didn't see much of Linstead. Burton K. Wheeler was a senator at the time, and just before Linstead arrived, Wheeler, who was rather strongly opposed to our getting into the war said, "Beware of Linstead. He's being sent over here by the British government to get U.S. science involved in the war effort." [James B.] Conant was very upset by all that scandal. But it turned out to be true. Linstead spent most of his time at the British embassy in Washington, and was in Cambridge only once in a while. He pronounced my name, "Daring, daring." [laughter] But I liked him very much, and the problem was a great problem. It was very demanding in the laboratory.

"Speed" [Carl S.] Marvel had made one of these stereoisomers, and we wanted to know whether it was identical with one of ours. Linstead wrote to him for a sample, and Marvel sent on a sample of about four or five milligrams. I began fooling with it, and it was clear that it wasn't pure. By fractional crystallization I separated it into two isomers, figured out which was which, wrapped them back up, and sent him back two milligrams each of these pure acids. [laughter] Marvel never forgot it, and he never had any great affection for me. The rumor is that I would have had the Welch prize long before this if it hadn't been for Marvel sitting there blackballing it every time. [laughter]

BOHNING: He was on their committee?

DOERING: Yes. Every time it came up. [laughter]

BOHNING: That’s incredible.

DOERING: Oh, dear. Well, you can tell I had a great deal of fun out of it all.

BOHNING: When you started that work, in terms of the laboratory aspects you were really required to refine those classical techniques to work with small quantities.

DOERING: Yes.

BOHNING: Was it just that training from Fieser’s course, where you said you had to synthesize everything and learn all the basic techniques at that time, that you then applied to this problem?

DOERING: Yes. I don’t think there was any question about that. But I would add to it a personal quality of taking great pride in what emerged from anything I did with my hands. And, falling in love with crystals, as many organic chemists did. There was real joy in carrying out one of those fractional crystallizations where you moved across that stupid triangle. [laughter] It was necessary to develop new techniques for that. One of the techniques was handling what now you wouldn’t call small quantities, but quantities in the half-to-five milligram range. One of the techniques I developed for that is in [Kenneth] Wiberg’s laboratory textbook (6). It is a nifty technique, just using standard cheap, little ten-by-seventy-five millimeter lipless tubes.

There were some other people around who, at that time, got a lot of their jollies from refining techniques and developing new ones. One of the persons working with Fieser, not as a graduate student but I think as a postdoc or a senior laboratory assistant, was E. B. Hershberg, for whom the Hershberg stirrer is known. Well, God, I mean you make a new stirrer today, you’re going to have your head in your lap, right? But in those days, you’d take a rod, make a little circle at the bottom, a piece of nichrome wire and a recipe for how you turned it around, an appropriate cork borer, an appropriate diameter, and you’d finally get it so that it would make contact with the bottom of a round-bottom flask. These were considered significant advances. I was really heavily into that. [laughter] Oh, God!

BOHNING: There is a great move today to change undergraduate organic chemistry laboratory work to the micro scale.

DOERING: Yes.

BOHNING: How do you view that? I know what the safety and consumption requirement advantages are, but in terms of pedagogy, do students get the same feel for organic chemistry that you got when you started out in the lab?

DOERING: I kind of doubt it. One reason I doubt it is that I created a new course shortly after I arrived at Harvard, which was designed to make up for the lack of a feeling for manipulating organic chemicals. It was a course to be taken by juniors and seniors. It was a small course, limited to perhaps twenty students, so you weren't trying to do it for all two hundred, including the premeds. Did it make any difference whether the latter had a feeling for organic chemicals? Well, it depends on your attitude toward croakers in general. [laughter] In this course you worked hard. You were given the opportunity to work on large-scale and medium-scale and so forth, and carry out fractional distillations. The students would gain experience with a variety of what you would call classical techniques that even by that time were going out of the elementary lab, in part because of expense and then later because of concern for safety. You would give these guys a little problem that involved repeating two or three steps in the literature but would end up with the possibility of doing something new, so that they were mini-research problems. That course still is one of the most successful at Harvard. And it does two things. For the people who didn't enjoy working with their hands and didn't ultimately embrace the pleasures to be derived from an organic chemical laboratory, it was very important in convincing them that experimental organic chemistry wasn't their cup of tea and this was not what they were going to do. On the other side were the people who just thought, "Well, this is marvelous and now I'm going to go on." It was good for first year graduate students who had a poor experimental background. They got into research much faster as a consequence of it.

That's a roundabout answer to your question, "Do they lose something by handling organic chemicals only at the micro level?" It's very hard to know what one really learns in the laboratory about the intellectual aspect of organic chemistry. In my mind, they're really separate. That is, it doesn't really make much difference to me that benzene is a liquid and benzoic acid is a crystal, in terms of my understanding of organic chemistry. [laughter] It makes a big difference to know that whenever you run a reaction in organic chemistry, you're bound to get a mess, and the problem is how to untangle it! [laughter]

These are really just two separate categories. It's important to know, to guess, what's in the mess, that's true. But really the pleasure that I got, and would still be getting and would have gotten all my life if I continued working in the laboratory, was enormous. There was just an incredible gratification of doing the job nicely, a sense of self-satisfaction. It's not to go around to anyone else and say, "Stuck in my thumb and came out with a plum," or whatever it is. It's just this profound satisfaction of doing it nicely and having it work out. Boy, today you get postdocs for whom crystallization is obviously a dead art! And you can't convince them that nature gave you this marvelous technique that by and large beats HPLC and various other separation methods. By and large, a crystal is the most demanding, most selective, most discriminating device for separation that we have. And to teach them how to do it when they're already twenty-eight years old. They really think you're left over from the cave age. [laughter] Well, I am, but...!

BOHNING: We've trained a generation of instrumentalists now.

DOERING: Yes.

BOHNING: We've put instruments as far down in the curriculum as possible, so everybody has a chance to use them.

DOERING: It's as closely related to a television set as they can get.

BOHNING: Yes. And I've noticed that when given simple questions they often start thinking of the most complicated answers.

DOERING: Yes.

BOHNING: Because they're tuned to thinking in terms of instruments. This instrument does this, this one does that. And they're all great techniques.

DOERING: Of course!

BOHNING: Sometimes you can get a quick answer without ever touching one of them. [laughter]

DOERING: Absolutely.

BOHNING: That's the part they miss, I think. And that's the fun of it, isn't it, to be able to logically work your way through the puzzle and come out with that answer at the end.

DOERING: Yes, and to solve it with your own hands, as opposed to watching the instrument do it. But even the operation of instruments is a funny thing. I now believe, in a mystical sense, that it's possible to learn the language in which that instrument talks and to become sensitive to it so that you know whether you're working the NMR machine at its limit, or you're less demanding, or how you've got to handle that capillary GC to get the correct results.

I just was raking a guy from China over the coals. All right, so what should he know about all of these things? But in any event, he had been using the high resolution capillary GC so damped down that he could go back to his boss with a curve that had an absolutely straight baseline and the two peaks that he thought we were interested in when we were measuring the kinetics (the big one and the little one) alone showing in a reaction that I know gives nine other products. He had turned those off by putting in a damping factor. And then he was surprised when I said, "Well, you weren't operating the instrument correctly. You deliberately operated the instrument as a cheap, crude, half-blinded detective."

BOHNING: Yes.

DOERING: He had no sense. His reaction was, "Well, what an unreasonable attitude. Look at these beautiful spectra." [laughter] It's very easy to let these instruments deceive you in certain ways. But some people are much better at it; they develop a rapport with it—there's a feedback loop. They look at that curve and they say, "Well, this is the way it's talking to me." It's ringing, or it's doing this. And other people are oblivious to it.

BOHNING: It's like Onsager reading an equation.

DOERING: Yes. It's a different language and some people learn it. Well, anyway, I learned to read the language of crystals. [laughter]

BOHNING: Certainly that showed through those three papers you had with Linstead (7).

DOERING: It's a nice piece of work, in retrospect.

BOHNING: Yes. There's some beautiful stereochemistry in there, and apparently it wasn't straightforward kinds of stereochemistry you were dealing with.

DOERING: No.

BOHNING: I was not clear on some dates. When did you get your Ph.D.? What year was that?

DOERING: I did not, in fact, get my Ph.D. until 1943. I had finished up all the work two years earlier, in 1941. I have had a terrible hangup all my life about writing anything, even a letter. If there was one thing I didn't learn at Shady Hill School or at Belmont Hill School, it was how to write. I mean, I'm essentially illiterate.

BOHNING: You'd never know that by your papers in the literature.

DOERING: But it's not something I do with pleasure. It's very hard work. It takes several drafts. It was not until the word processor came along that I really got pleasure out of it, because I can now revise a paragraph twenty-three times without having a secretary look at me as if I had just gone over the cliff forever. [laughter]

BOHNING: Somewhere in that time period you were working on anti-mustard gas compounds for a war project. Was that with Fieser?

DOERING: The job with Fieser was to develop new explosives. The compound we were after was trinitrobenzylnitrate. It's essentially trinitrobenzene with a CH_2 nitrate. The first thing one did in that reaction—this is typical Louis chemistry—is to get hold of some TNT [trinitrotoluene] and put it in one of those heavy Carius tubes! [laughter] Do you remember those tubes? They were for some sort of an analysis where you digested a compound with nitric acid at high temperature. These were eighth-inch-thick tubes, about an inch in diameter and a couple of feet long. So you put in about twenty or thirty grams of TNT, poured in a little excess of bromine, but no solvent. You sealed the tube, put it in an iron bomb with a wire wrapped around it to raise the temperature. [laughter] Well, dear God! The laboratories at Harvard were so arranged that there was an indentation in the wall where the windows were. In effect, if you put the heating tube in that little space and it blew up, the glass would hit this little part of the wall on the left and the other on the right. Well, of course, half the tubes blew up! The laboratory would be filthy with bromine and you wondered when the TNT was going to detonate! [laughter] There was powdered glass blowing all over the place. It was powdered so finely that you didn't get cut or anything. Then you treated this trinitrobenzylbromide with silver nitrate. [laughter] I was doing this about the time that a very close, lifelong friend of mine, Richard B. Turner, was going to leave. He later became a professor at Rice. He and I were in the same class of Harvard College, and unlike me, he didn't have enough money to support himself.

[END OF TAPE, SIDE 4]

DOERING: He had to support himself in a small way, so he made money by doing microanalyses for people who couldn't do them themselves. In those days, we were supposed to do our own semi- microanalysis on every new compound we made. A few of us decided, "Ah goodness, that's chicken stuff. You have to learn to do microanalysis on three or four milligrams." Well, Turner was one of those who did and Hans Heyman was also one. Turner was just about to leave. So I said, "Dick, we've got a sample. Could you do this just before you leave because it would make a big difference to Louis and me if you would run this analysis." It was trinitrobenzylnitrate, but I didn't tell him that! [laughter] On the theory that you should never prejudice an analyst, right?

BOHNING: That's right.

DOERING: This business of telling them in what range to expect the carbon and hydrogen is for the birds! And all you're doing is destroying their objectivity. So, Dick started to analyze it. Do you remember how that works? You weigh the sample into a little platinum boat and shove it into a quartz tube. You've got this platinized or palladiumized asbestos further down the tube and you follow it up with another packing. Gradually you move the burner down to vaporize the sample. Of course, as soon as the flames got near this platinum boat—POOF! His whole train was filled with carbon black! [laughter] Well, that ended his analytical career maybe a day or two before it was going to end naturally. [laughter] Those were marvelous times!

The Germans have a word to describe certain persons as *tierisch ernst*, which means having an animal-like seriousness about them. I must say there was very little of that in those days! [laughter] During my graduate work, I worked in a room with four Fieser students. (I was working with Linstead by then.) Five of us were in the basement of Mallinckrodt, which was far away, like the depths of Siberia. Louis would come in to talk to his people, and would invariably throw his cigarette, still burning, into the sink. So the game was to try to guess when he was coming down and then pour ether in the sink in the hope that it would catch fire. [laughter] Of course, a cigarette is something like the lantern used in mines. It's based on the principle that if you have a cold screen over the flame, the explosion won't go past the screen. I forget the name of the lantern. Well, a cigarette works that way because the ash prevents it. But, God, we were just so hoping this thing would blow up! [laughter]

BOHNING: How long did you work with the explosives group?

DOERING: I don't know, maybe a few months. I made a fair amount of trinitrobenzylnitrate, but it was so sensitive that if you hit it with a hammer it would detonate. It wasn't really any good—it just overdid it.

BOHNING: Did you get paid for that work?

DOERING: Oh, I can’t remember.

BOHNING: When you worked with Linstead you were really supporting yourself.

DOERING: Oh, yes.

BOHNING: You didn’t have a research fellowship or anything?

DOERING: No, nothing like that. Then I got back to working with Linstead for a bit and a fellow in the pharmacology department, Otto Krayer, who wanted to isolate and separate the various minor components in the digitalis group of compounds. So I prepared samples of digoxin and digitoxin and things of that sort for them for a while.

I then actually went through a very quite traumatic time in which Bob [Robert B.] Woodward comes out as an absolute hero. Part of the Eggers family included a minister in the German government, who was ultimately in the Nuremburg trials. He was one of the two persons who were acquitted in the Nuremburg trials. Nevertheless, he was not exactly the kind of person you wanted as a relative. His mother was Danish and was a sister of my mother’s mother so that they were cousins. I always knew him as “Uncle Hjalmar.” His name was Hjalmar Horace Greeley Schacht. [laughter] Can you believe it?

During the war, there was quite a lot of anti-German feeling, as one can expect. There was a very strong anti-Japanese feeling. This kind of thing is natural. There was a very close friend of the family, Dr. Day, who had been the attending physician at my brother Harvey’s birth. His son, Richard Day, was in Shady Hill School and in Belmont Hill School with me and later became headmaster of Exeter Academy and is now deceased. Dr. Day went to army intelligence and said, “You’ve got to get rid of this Bill Doering because there’s too close a connection with the Nazis.” Several of the people in the chemistry department said, “Yes, we’ve got to get rid of him.” And it was Woodward and Lamb, but particularly Lamb, who said, “No way. As long as he is doing graduate work he can stay, but he mustn’t work on secret work any longer.” So that took me off the mustard gas problem. Bob then took advantage of it to get money out of Polaroid to work on the quinine synthesis, the importance to the war effort of which could be justified sufficiently. But that was a bad time. I never told either of my parents about it. I was completely, totally quiet to them, because I knew my mother would be just desperately upset if she realized that that connection could really have seriously disrupted my career. Now, I don’t think I’m a total coward, but at the same time I wasn’t eager to get a gun on my shoulder and march off to war. [laughter]

BOHNING: Who directed that anti-mustard gas work?

DOERING: Eric Ball was in charge of it. He was in the medical school, and it was viewed as a medical problem. The purpose was to find a protective device against the toxic effects of mustard. It turned out to be a fascinating problem. [Saul] Winstein was just at the beginning of his work on the neighboring group concept, and mustard gas shows this funny property of solvolyzing, reacting with water, even in the presence of fairly strong bases, by first-order kinetics. That is, it's not a second-order displacement reaction even though the chlorine is a primary chloride. An S_N2 mechanism would have been a reasonable thing. So that means that the destruction of it is very tricky. You can't accelerate the destruction of mustard gas by introducing strong nucleophiles. The only way it's rapidly destroyed is by using alkali so concentrated that it changes the mechanism to an elimination reaction, and you ultimately get divinyl sulfide. Winstein had formulated the first step as the formation of a three-membered sulfonium intermediate, and that was later proved very nicely.

What we were looking for were nucleophilic reagents so powerful that they would compete with the nucleophiles in the body, not scavenging mustard gas, but scavenging the intermediate before it could latch onto various critical points in the DNA. (Not that DNA was known at the time, but in retrospect.) That was really very, very interesting because we developed all sorts of new nucleophiles, among them the exceedingly powerful thiophosphates. In the end, we found a compound that was not toxic in any way. You could put five percent of it in a rabbit's blood and spray it with mustard gas and nothing would happen. [laughter] But isn't it amazing how in those times I worked with large quantities of mustard gas? I distilled it and purified it and handled it in the hood. I once splattered the tiniest drop into my eye, but it wasn't enough to make any difference. Otherwise I never had any trouble. When you think back on it, that's what organic chemical techniques are all about. It's not that mustard gas isn't dangerous; mustard gas is very dangerous! But as an organic chemist, what difference does it make? You know how to handle things like that without getting them all over you! [laughter]

BOHNING: You said that Woodward came to your defense, along with Lamb. Was that your first interaction with Woodward, or had you been interacting with him earlier?

DOERING: Only in a very superficial way. I think partly he was serving his own interest. He certainly knew me well enough to know that I got around a laboratory pretty well. And by that time, he certainly already knew that if you were going to become a synthetic organic chemist, targets that were not recognized widely might be just as difficult to synthesize as targets that were widely recognized. So if you ask how he chose his problems (the synthetic targets), a necessary condition for his target was that it should be a widely known natural product. Because only if the target was widely known, would your reputation for having synthesized it be enhanced. I don't think there's the slightest question about that. This is not cutting him or anybody down. I think I knew Woodward well enough. If you go over the molecules which he did synthesize, they almost without exception fall into that category. That's all deliberate.

The real defense for that, in my estimation, is this—there's another purpose in synthesizing natural products. Suppose I discover a new reaction and I want it advertised. Then I should use it in the synthesis of a natural product. Most synthetic organic chemists clearly go down huge lists of natural products until they find one, the synthesis of which will be appropriate to this new reaction. You see a hell of a lot of terpene and sesquiterpene synthesis, where the natural product is chosen to fit the reaction. The big synthetic chemists, the ones who distinguish themselves, are in the Woodward class. That is, the target is chosen, but you didn't choose it because you knew easily how to synthesize it. Now you're going to devise a way of doing it. The challenge is much better. It falls into the same category as the old saying, "Well, if you're going to marry for love, she might as well be rich." [laughter] If you're going to choose a target, it might as well be an easily recognizable one. But you're not choosing it because it will be easy to synthesize.

So I don't think there's any incompatibility between Bob's behavior and his practice of choosing well-known, well-recognized molecules. Certainly, selecting quinine during the war, with malaria a concern—can you imagine a better choice?

BOHNING: No. I do want to get into some of the popular press descriptions of the quinine synthesis in the *New Yorker* (8) and *Life* (9) and the *New York Times* (10). But first, let's come back to the Woodward-Polaroid connection. How did he and [Edwin H.] Land first join together?

DOERING: Bob was a consultant for Polaroid. I don't know what they were trying to do at that time. I think I'm right in saying that the first crystal to be used in making Polaroid film was the quinine salt of triiodide. I think that's right. That is the crystal that had birefringent properties and also the physical property of crystallizing in flat, thin plates such that when you put them into a polymeric support and stretched it, they would line up. Land was very much concerned about quinine not being available for that use. I don't think he dreamt that synthetic quinine would have anything to do with solving Polaroid's problem. It was more a matter of sentimentality. Quinine had put Polaroid into business; why not support its synthesis? Bob's job in consulting with them had been to find substitutes, which I think he did very quickly. I don't think there was much of a problem.

Land was an extraordinary man. He was very, very intelligent, sharp, energetic, foresighted, really a remarkable person. I never knew him very well, but I certainly came to know him well enough. He recognized that Woodward was just in a different category, so that he would have viewed Woodward by himself as a unique asset. [laughter]

BOHNING: There is a letter dated June 1, 1942 which states that they would pay him one thousand dollars, "payable each month." I assume the thousand dollars was over a year's period, or would it have been each month?

DOERING: I would've guessed over a year, because Bob certainly wasn't floating in money and was really pressed financially.

BOHNING: So he viewed the quinine synthesis as an intellectual exercise as much as any remote practical application.

DOERING: Oh, sure. He wasn't the least bit concerned about any application for the synthesis. He had selected it as a target for the reasons that at least I've implied. You couldn't have latched onto a natural product the synthesis of which would have attracted more attention at that time, with the boys going off to the Asian jungles and the Cinchona trees in the hands of the Japanese. [laughter] The plantations hadn't been moved to South America yet. If I remember correctly, the first plantations came during the war. And then it was also a challenge in the sense that, long before the war, other persons had tried to synthesize it. [Vladimir] Prelog was in the process of trying to synthesize it. [Paul] Rabe had tried before him. And it had been the goal of [William Henry] Perkin to synthesize it at a time when syntheses were developed by looking at the empirical formula, in this instance of quinine, and breaking it down into two components. I forget what it was that he put together, but it might have been a toluidine, which when treated with sulfuric acid should have added up to quinine. Wasn't that the first synthesis of his dye?

BOHNING: That was the beginning of the whole synthetic dye industry.

DOERING: Yes. But what an intellectual story that is, isn't it? From putting empirical formulae together, sticking them in a flask, and throwing in a reagent! [laughter] Dear God! So it wasn't a question of synthesizing a molecule that no one had tried to synthesize before, rather there was a history of failure. That, from Bob's point of view, would let the chemical world know that they were dealing with someone quite special.

BOHNING: Where was Prelog in his synthesis?

DOERING: His story is that he read about it in the paper and just couldn't believe that two youngsters had put this together. [laughter] He still tells the story with great amusement.

BOHNING: How long had his group been trying?

DOERING: That I don't know, but it was a matter of years. He had this as one of his major goals.

BOHNING: What was Woodward's approach when he laid out the scheme? Did you participate in the construction of the scheme?

DOERING: Absolutely not. No. There was no intellectual input on my part in that original scheme. As the thing was going on and it ran into a little trouble here and there, I put in a change of reaction here and a change of reaction there, but that's really minor, minor stuff.

[END OF TAPE, SIDE 5]

BOHNING: For the most part, Woodward was responsible for the design of the quinine synthesis. Did he participate in any laboratory work?

DOERING: No. Laboratory work really wasn't Bob's strength, and he had no liking for it. I suspect that goes back to his childhood. His story, and obviously I can't verify it, is that he became interested in organic chemistry when he was ten years old and at some point after that taught himself German so he could read *Beilstein* (11). He would read *Beilstein* in order to find either the mistakes that people had made or problems that hadn't been solved. Much of his early work comes from and clearly is stimulated by having poured through *Beilstein*. The entire santonic acid problem, from a mechanistic point of view, really had a tremendous impact. Now one gives it as a problem in elementary organic chemistry, but at the time it was one of the really big structural problems, one of the real mysteries. He clearly came across that from his reading of *Beilstein*.

Why do I say all of that? Because he clearly never did anything with his hands as a child, in comparison to my model airplanes. I think that just means that the connection between your brain and your hands and so forth goes undeveloped during years that we haven't delineated precisely. The time to learn a foreign language is between five and eight. The ear sound-producing mechanism link is still fluid enough so that you can learn to speak with no accent at all. And you have to keep speaking it during those years. By the time you're eight, then that process begins to rigidify. (All of this is now well established.) And you're locked in for life. You will never speak that language with a bad accent again. You may not know the language, but you won't be able to speak it badly. I suspect the same thing will turn out to be true about the brain-hand connection. You hear of people like [Gregor] Piatigorsky taking up the cello when he's twenty, but I would wonder whether that's true or apocryphal.

Bob could boil water, but I think it was pretty tough to boil an egg. He just was totally impossible.

BOHNING: Did he come in the laboratory as you were working through the sequence?

DOERING: Oh, he'd be in there watching most of the time. He was a very sharp observer with a very, very sharp eye. He was able to direct later graduate students through what he had seen, an experimental experience which he had acquired vicariously rather than through his own hands.

BOHNING: Apparently it looked like it was going to be fifteen steps that were pretty straightforward when they were laid out on paper. What were the principal stumbling blocks?

DOERING: Gee, the one that we didn't resolve for three months, I've sort of forgotten what it was! [laughter] I could go back and figure out which one it was that really took a lot of juggling around before it was cracked. But really the biggest stumbling block came right at the end, which is in the paper, I think (12). It's a long time since I've read it. That was in the resolution. The detailed history of working on the problem is odd.

I think I began it in February of 1943, as near as I can remember, and I plugged away as hard and fast as I could. Sometime in July there was an opening at Columbia for a one-year instructorship to teach navy students at night. I decided that I should take that job. Well, Bob was absolutely enraged. He was outraged at my stupidity in taking such a low job. [laughter] But he was further totally destroyed by the thought that his quinine synthesis wouldn't be carried through. I don't think there's any question he recognized he had somebody working on it who was going to see it through if he had the time. And I said, "Now Bob, don't worry about it, just don't worry. I'm going to work hard until the last day when I have to go to Columbia in September, and then I'm coming back for two weeks in Christmas vacation and I'll work on it then. Maybe I'll have a little time to do something while I'm down there, and I promise to see it through." Still, there were a fair number of steps to be done. So after six weeks he began speaking to me again. It was just a reality of the world that he couldn't change. [laughter]

And indeed I did leave. So he said, "Well, fine. I have money and will take on a person who will bring up the supply lines." (His name was Dick [Richard S.] Corley.) Because we were down, as you always are in a synthesis at its leading edge; you never have enough stuff to work with. Then you've got to go back to the beginning and fight your way through it all over again. He made arrangements for kilograms of meta-nitrobenzaldehyde to be prepared by some company. He was certainly doing his part to further it. He wasn't just sitting there waiting for it to turn out.

So he hired Corley, and Corley got to work. Supposedly I would come back in December with a fair amount of material at hand. And indeed there was some, so we pushed ahead another step or two during that period. Now then, when were we going to get it finished?

I said, "Well, I'll come back on Easter." So I did that, and once again we were working from seven o'clock in the morning until three or four o'clock in the morning for two weeks. By the spring vacation, Corley had come to a crucial stage in the scale-up. You make an amino ester and the meroquinene ester that one is synthesizing has got a three-carbon ester chain on it. Then you're trying to put in an amino group which you're eventually going to convert by Hofmann elimination of it's congeneric quaternary ammonium salt to the crucial vinyl group.

At that stage you really have to be careful because if you're not, the amine will cyclize with the ester to an amide and then you're finished. You really can't handle it after that point. So you had to be terribly careful. I had spelled all of that out. In those days, you used to evaporate solvent off with one of those rotary evaporators. You put it in a round bottom flask, did a vacuum distillation and pulled the stuff off. But that meant that you were heating it with a steambath or something, and of course that was fine, as long as there was some solvent left to be evaporated and to keep it cool. Well, this hapless turkey went out for lunch while his whole batch was concentrating, and he came back and of course it was all useless. It had all gone over to the amide. There was not a thing you could do with it after that. Nothing. Absolutely nothing. I expected to come back at Easter with a great pile of material and just work along gloriously. Instead I came back from Columbia with two hundred and eighty milligrams of what we hoped was the homomeroquinene; that is, the methyl ester that had to be coupled in a Claisen condensation, an ester condensation with whatever it is, 7-hydroxy—I can't remember any of this stuff! You've got no business talking to me about it! [laughter]

That was disheartening, both to Bob and to me, that we had only two hundred and eighty milligrams. So I did the condensation and the decarboxylation and now we had what we hoped was a racemic sample of quinotoxine. That was as far as the synthesis had to go, because it's very easy to convert quinine to quinotoxine. One has to open the bicyclic system of quinine to monocyclic and then to regenerate. All of that had been done by Rabe. The practical goal was, in effect, the synthesis of meroquinene, but our goal was one step further, namely, to show that we could get optically active quinotoxine that was identical with that derived from the natural product.

So we got a little of this stuff and then there was the problem of resolving it. Well, Bob had done all the preparation for that resolution on the following premise. It's really quite interesting and I guess it's in the paper (12). The first actual resolution that [Louis] Pasteur ever carried out in the classical sense was the resolution of dl-tartaric acid with natural optically active quinotoxine. All the textbooks have it as being with quinine, but it's not. If you go back to the original note, it's with quinotoxine. So the notion was, we'll try the resolution of tartaric acid with quinotoxine. If that works, then why shouldn't the resolution of dl-quinotoxine with (-) tartaric acid work? It looks as if it's an identical situation.

So fine, off one goes with this couple of hundred milligrams of dl-quinotoxine and tartaric acid, and that's not an easy resolution. It's a long fractional crystallization again. Finally, nothing more would happen. In other words, the resolution was complete. Well, it was complete but for the fact that the resulting product had one-third the activity of natural quinotoxine. At that point, Bob collapsed. When Arnold [Thackray] was asking whether

Woodward ever went into a depression, well this was the only time I've seen him in a depression. He went home and he went to bed for three days! [laughter] He was incommunicado!

So there I was with this result. He was sure that the reaction had failed because, at an earlier stage in this Hofmann elimination, there had been two directions in which it could go. One was outside to make the vinyl group and the other was inside to make the ethylidene group. All the literature said the Hofmann elimination was one that went according to Hofmann rules and not Saytzeff rules. But he was sure now that's what had gone wrong, and the whole synthesis had collapsed because there was no way of going back. So he was crushed to the point where he didn't show up for three days and was just depressed. I had heard of cases where a resolution stops at one-third. In other words, one part is one enantiomer, and one part is racemic, so it's effectively diluted by a third. Why not take a chance on that as having been responsible rather than a complete failure of the synthesis? I put together some dibenzoyl tartaric acid and carried on the resolution. Finally, I called him up and said, "Look, I've got a sample and it has the right rotation." Whereupon he returned to life! [laughter] But it was really sort of an amazing operation because in the end it turned out I had done more than eighty crystallizations on this roughly two hundred milligrams of product.

BOHNING: That's incredible.

DOERING: I had kept a sample of this quinotoxine. They had a little chemistry museum at Columbia in the Chandler Laboratory. Walter Gensler was really a fine old fellow. He had retired and wanted this sample of quinotoxine to put into the museum. So I just gave it to him and then forgot about it. As far I was concerned, it was lost. A few weeks before he died, Walter Gensler, who had been a graduate student of [Robert C.] Elderfield at Columbia, sends me this little package with the tube in it. He said, "Maybe this is something you should have." [laughter]

BOHNING: So you have it back now.

DOERING: So I have it back. It doesn't have much in it, I'll tell you that! [laughter] Whether someone tried to empty it or whether I emptied it, I'll never know.

But that was the most traumatic time in the synthesis, right at the end when it looked as if it had completely failed, but in fact hadn't. It was just a very rare example. There are not many examples of it.

BOHNING: Was Polaroid paying for all of this?

DOERING: Yes.

BOHNING: Was it done in a Polaroid building or it was done at Harvard?

DOERING: It was done at Harvard. There's a misconception in some of the stuff. There's no question. Nothing ever touched the Polaroid labs.

BOHNING: In some of the press reports afterwards...

DOERING: ...you get the impression that it was Polaroid.

BOHNING: The work was completed on April 10, which was Woodward's birthday.

DOERING: It came out as a birthday present, which is quite wondrous! [laughter] Well, it was a pretty big birthday present! Not many people get a birthday present like that!

BOHNING: I remember a professor of mine telling me years ago, after there was more than quinine in his stable of synthetic products, that the molecules wouldn't dare defy him. [laughter] You get part of this mystical quality that he is associated with.

DOERING: Well, that comes out later. I mean, there was none of that in my time! [laughter] None of that at all!

BOHNING: What was different about his approach and that of Prelog's?

DOERING: I don't know how Prelog was approaching the problem in a schematic sense. I don't know if Prelog has ever told anybody. Maybe he's not so proud of what the results were. [laughter] Next time I see him, I should ask him that question. I love old Vlado, he's a marvelous guy.

But really one of the big differences, not to compare it just with Prelog, but with other people, is the incredible tenacity that Woodward had. If there was one lesson I learned from Woodward that changed my entire career (let's say at least the lesson fell on fertile ground), it was to stay with a problem. Once you abandon the problem, the outcome is certain. It will be failure. Of course, in directing the research of anybody, that is the critical question. Because again and again and again, the problem will come to what appears to be a halt. And then you've got to go through the agony of deciding whether you were responsible for it because your chemical intuition or planning or whatever was inadequate. Or whether it's the operator, the person doing the work in the laboratory, who is responsible for not pushing it through. That's a terribly painful, but enormously important, decision. That's perhaps the most important decision that one makes in the course of bringing a project to a successful conclusion. When is it being blocked by inadequate technique? When can small changes and so forth get around it? Or when do you call it a halt because you just got your ideas, your intellectual contribution, on the wrong track, which it often is. That's just a bitch of a decision to make. Many persons make that decision too early.

Where you have complete control over it is at the planning stage. So in developing a synthesis like quinine or any of the others, it's the person who will really stay with it until the idea comes that at least on paper looks as if you now have a workable sequence of reactions. Bob was a superb example of that cliche, "Genius is ten percent inspiration and ninety percent perspiration."

I was close enough to Bob at a time when he wasn't insulated by his awesome reputation and he wasn't working so hard to create an image. He was far more accessible. In that santonic acid problem, I watched him work—on paper, not in the laboratory at all but on paper—at least for three months, day in and day out, to try to figure out what the hell the structure of santonic acid was. The reaction was a simple one-step reaction, and finally he did crack it.

[END OF TAPE, SIDE 6]

DOERING: It was amusing in an organic chemical sense why it was so difficult. In the end you would say it was the difference between looking at molecules in two dimensions and looking at them in three dimensions. Once you really began to look at it in three dimensions, then the solution came through.

I was enormously impressed by Bob's tenacity—that when you throw in the towel, the game is over. That has made a tremendous difference to me, with problems in my own work, where I couldn't see what to do, couldn't see how to make any progress with it, how to come up with an answer. That, once you stopped thinking about it, it was over. That's a big lesson to learn.

Once in a while we were kicking through those problems of which Bob had an enormous collection from his reading of *Beilstein* (11). Twice a problem came up where he was working on it and I was working on it trying to figure out what the right solution was, and I got there before him. I can't tell you what kind of a feeling that was! [laughter]

BOHNING: I can imagine!

DOERING: He wasn't very happy about those two instances at all. [laughter]

BOHNING: His successes are legendary; what about failures? Did he ever pick a target molecule that he was unable to achieve, or gave up on? Let's follow up on what you've just been saying.

DOERING: I simply don't know. I left in the fall of 1943, was clearly an active part of things for the next year, and then really lost touch. So I would only see what was coming out in the literature. It wasn't as if he and I were in monthly contact about how things were going. Someone else would have to answer that.

I know he was very, very much disappointed with his vitamin B_{12} synthesis, which in the end was a success. But all the beautiful things that he had put into that synthesis to come out with the right isomer in the end were for nothing. He came up with a mixture of six substances that, had not Waters HPLC been coming into business at the same time, would have been a dead end. He was rescued by the introduction of a new technique for separation, and he knew that. We talked about it as much as one could talk about things of that sort.

BOHNING: You've mentioned Corley before. There's also an acknowledgement to a Donald Sparrow in the quinine paper (12).

DOERING: I don't know what Sparrow did. I don't know that I ever met him. He was after my time and before Easter. [laughter]

BOHNING: Was Fieser aware of what was going on?

DOERING: You know, I don't know. A strong antagonism between Bob and Fieser built up very early on, because Bob thought Louis Fieser was just a very dim bulb. Well, he also thought a lot of other people were dim bulbs. [laughter] But Louis didn't take kindly to him, and apparently after I left, it really became very bad, and Louis was kind of pushed aside. Mary [Fieser] still carries a very strong antipathy for Woodward, and it's even a little hard for her. She essentially has to say that Doering never worked with Woodward, that it never really happened. [laughter] But that apparently was very intense. There were other people who were there who would know about it; I didn't know much about it.

BOHNING: I mentioned this before, and I've got here a whole stack of articles on quinine that appeared in the public literature.

DOERING: There's the *Life* magazine article (9).

BOHNING: Yes, that's the *Life* magazine one. There's one from the *New Yorker* (8), *Science News Letter* (13), *Business Week* (14), *Newsweek* (15), *Time* (16), and of course the very incredible *New York Times* (10) piece, which puts it right up there with synthetic rubber. Since the Japanese had cut off both quinine and rubber sources, you were right there behind the synthetic rubber project.

DOERING: Yes, it really got a play. In part, of course, because Polaroid Corporation was dealing directly with consumers and therefore had a very active public relations group with very strong contacts and connections. They knew how to do this kind of thing. They just took it and ran. Boy, look at all of that. Can you believe it?

BOHNING: It's intriguing, that the *Life* article actually tried to summarize the chemistry all the way through.

DOERING: Yes, people don't do that anymore. The photographer for this layout was Fritz Goro. God, he was good. What an attempt!

BOHNING: Yes, I was amazed by that. There are three...

DOERING: ...pages of chemistry!

BOHNING: And then there's a little basic...

DOERING: ...course in organic chemistry! Isn't that something!

BOHNING: I can't imagine any popular magazine even beginning to think of doing something like this.

DOERING: Not even dreaming of it. [laughter]

BOHNING: Did you help participate in putting all this together?

DOERING: Oh, sure. I worked very, very hard with Fritz over all of this.

BOHNING: That's really fascinating.

DOERING: It really was quite an effort, wasn't it?

BOHNING: It also appeared in the *Reader's Digest* (17), but this one in *Life* was most striking, by far.

DOERING: I have two original copies of the magazine, which I dug out in connection with this Welch award in October. What a bunch of bores, just between us. They announced the award back in March or April, and then it's not until October that they present it. Instead of getting some reputable chemist to say a few remarks about why the guy is worthy of the award, they make a video. So they send up a crew of three guys with a ton and a half of equipment and take up three days of your life putting together a video that they then show on a screen. [laughter] Frank Westheimer was the first guy to get this Welch award after they introduced the idea of having two big screens at the end of a huge banquet hall, a banquet with five hundred people or something like that. Absolutely luxurious. Incredible expense. The video starts out with—did you ever see that old movie "Fantasia," which starts out with the beginning of "*Also sprach Zarathustra*"? [laughter] And bubbles coming out of the primordial soup! [laughter] Then the first little lizard or something crawls out, and finally there is something on two feet. It ends with a big picture of Frank Westheimer! [laughter] Poor old Frank! I think it was the beginning of the end for Frank! Incredible. But this *Life* piece was a decent try.

BOHNING: What was Woodward's reaction to something like this [the magazine articles on quinine synthesis]?

DOERING: Oh, he was all in favor of it. Every bit of halfway reasonable advertising was just what he wanted. He reveled in it. Yes. I just stood back in sort of an amused way and couldn't believe what was going on, but there it was. That's what was going on.

BOHNING: All of this certainly makes Polaroid look good, since they are frequently mentioned.

DOERING: Oh, sure. Well, it's all Polaroid PR, you see.

BOHNING: Many people must have thought we were going to be able to synthesize all the quinine we need—that's the impression you get from a lot of the popular press. And then here's Polaroid saying, "But we're not going to make any money off of it. We're going to donate this process." [laughter]

DOERING: Right!

BOHNING: But they knew there was never anything commercially going to come out of that at all.

DOERING: Nothing was going to come out of it!

BOHNING: And the starting materials are coal...

DOERING: And air and water. That's right. Total synthesis—coal, air and water. [laughter] And Izzy Stone got in it. What magazine was Izzy Stone publishing in those days? It was a liberal publication. He began to make the connection between Polaroid and the Dutch quinine cartel. [laughter] So that brewed all up and went into a big thing! I've got to look to see if I have some of that Dutch quinine cartel stuff.

BOHNING: Incredible.

DOERING: It was a funny time. But boy, people did respond. There was a junior chamber of commerce award, and Bob and I were among the ten recipients that year. Then the City of Philadelphia Chamber of Commerce gave us the John Scott Award. It was a great big copper medal.

BOHNING: Well, this was before penicillin and we're just into the war.

DOERING: That’s right, that’s right. Terrific. “Some eight hundred million people are infected with malaria and millions die every year.” [Reading from *Science News Letter* (13)]

BOHNING: According to *The New Yorker* (8) you and Woodward had lunch with Land after you told him that you had finally succeeded. Then you left the formula under a chair in the restaurant and had to go back for it.

DOERING: That may be. [laughter] Does it actually say that?

BOHNING: Yes.

DOERING: Well, I can’t remember it.

BOHNING: It sounds like a great story.

DOERING: Let’s just assume they took the right story. All I can say is that at the time I certainly wasn’t trying to exploit or embellish the situation in any way. I didn’t even realize I was quoted. I’ll be damned if I ever saw that article.

BOHNING: Yes, that’s an interesting one.

DOERING: Yes. It’s pretty good. [reading *The New Yorker*] I won’t vouch for that at all. [laughter] Look, the guy had to write an article. Veracity is somewhere in that game, but not all that high.

BOHNING: You spent your first year at Columbia teaching naval cadets?

DOERING: Down at the College of Pharmacy.

BOHNING: What were you teaching them?

DOERING: Elementary organic.

BOHNING: How many students did you have?

DOERING: About twenty or thirty I guess. And God, I was so scared I practically flashed my hash before every lecture for the first two or three weeks.

BOHNING: Really?

DOERING: Oh, I've never been so scared in my life. Just terrified.

BOHNING: Did you use a text?

DOERING: I imagine. [laughter] It was so bad that I said to myself, "Listen, you're in the wrong profession. You might either have to get out of teaching or get over this kind of nervousness and anxiety." So from then on I just sort of worked on it and gradually the nervousness went down. But boy, was that rough.

BOHNING: If you taught at night, what were you doing during the day?

DOERING: Oh, they had given me a laboratory.

BOHNING: So you started up on research. Were you doing any of the quinine work there?

DOERING: A little, yes. I was poking around with the resolution, repeating a little of Pasteur's work. Somebody snitched my laboratory notebook of those years. It had some nice stuff. It's something that I would really have liked to have had, because I did a lot of laboratory work for those first few years with my own hands.

After that year was over and quinine was out, they decided they could keep me on for another year, but that I must never teach undergraduates. There was an old codger by the name of [Clifford D.] Carpenter in the department. When he heard that I had never been a teaching assistant in a laboratory, he said, "Well then he can never, he may never have anything to do with undergraduates." [laughter] So I began by teaching graduate students, which was okay.

We have to get into the story of my first student, because it ultimately made such an unbelievable difference in my life. My first student was a middle-aged lady, Ruth Alice Norman Weil. She came around and asked if she could do work with me. I think that was a little hard for her, because she couldn't imagine anybody so young being a person that you worked with. I gather that the other professors had turned her down because she was older, she was female, and so on. So I gave her a problem which in retrospect was a perfectly decent, modest problem. But again, I was so scared it wasn't going to work that I went into the laboratory and worked very, very hard for a couple of weeks. I did the whole thing. Three years later, she got the Ph.D.! [laughter] But I had finished the whole thing! You can see how much self-confidence I had. I never breathed it to anyone. She's now dead, so that's publishable material. Can you imagine how she would have felt? [laughter]

Personally for me, one of the really tremendous things about the quinine synthesis was a fundamental change in attitude to my own work and what I might do. I told you that I had nothing to do with the planning of the quinine synthesis; that is, the intellectual part, except for putting in a little modification here and there. I felt that I had simply been a pair of hands. I mean, I did recognize that I'd been a pair of fairly good hands, because he never did a synthesis again with one person. But I was just starting out at Columbia and then I was absolutely convinced (and I suppose still would be if anybody asked me) that I would never do anything remotely resembling the quinine synthesis in importance.

It's not that I figured out by then a sense of, or a list of, criteria for giving meaning to the word "importance". It's a subjective word. It's a question of how you react, how other people react, how I react. It's not an objective concept. But nevertheless, in a field like organic chemistry, which is effectively infinite, there is an unlimited number of things you can do. And it's going to be just as hard to work out a minor, trivial problem of lesser significance as it is to work out a problem of greater significance. By and large, significance is not related to difficulty. Sometimes it is, but by and large it isn't. Having convinced myself, which I think was right, that I would <u>never do anything</u>, here I was, twenty-six years old, just at the beginning. I hadn't even started my own work.

In looking at the ideas I had, well God, they really are pretty minor ideas. They really didn't amount to very much. I then said, "Well, you'd better get out. Or, you'd better have some other way of leading your life." That other way is a perfectly obvious way, and that's to say to yourself, "Look, if I do the best I can, or ninety percent of the best I can, in selecting among the problems that I have to work on the one that I think is most interesting, and if I do the best I can, then I don't have to worry." Then the cookie will crumble the way the cookie crumbles. And there wouldn't have been anything I could have done to go back and do it better. On the other hand, there are other ways of responding. One that seemed the only obvious one to me was to get the hell out of chemistry right then and there. [laughter] Your best days are over! You're twenty-six years old!

That was a terribly important lesson, because it freed me from worries that I see have plagued colleague after colleague. So I never really worry how people are going to receive my work. I never did after that. I'll try to look for problems that in my mind are important, significant problems and then I'll try to do the best I can to bring them off. If you've done that, you have satisfied your own curiosity. Presumably, that is what the game is about. And if your curiosity was limited and you were fascinated with dull things, well, you still did the best you could and therefore you don't worry about it. Don't be hung up about it. That has been a tremendously liberating thing and a wonderful thing for a person who didn't start with a lot of chutzpa or self-confidence, however you want to put it. Extraneous forces or influences have never really biased my choice of a problem. I would never get myself into the position of Woodward, of saying, "I'm going to work on the synthesis of quinine because so many people will be in awe of it if it succeeds." I'll just do what is of greatest interest to me, and what more can I do? Well, there are other things you can do. You can go to work in this field, where there are a lot of people stirring around. Anything that you do, even a small thing, will have a better chance of being appreciated. That's been enormously helpful. That was really the big, important lesson that I got out of the quinine experience. [laughter] Plus, it was nice to have the recognition. Columbia paid me twenty-four hundred dollars a year for that first year. If I hadn't had a little of that ten thousand dollars left over I couldn't have lived.

BOHNING: So you still had some of that money left. [laughter] When did they make the decision to keep you on a second year? This was still wartime, wasn't it?

DOERING: Yes, we're talking about 10 April 1944. It was just within weeks after the quinine synthesis came out that it seemed pretty clear to them that they would look fairly funny if they let me go. [laughter] That obviously was the factor.

BOHNING: You said you couldn't teach undergraduates with Carpenter sitting there.

DOERING: Yes. [laughter] That's right.

BOHNING: What did you teach in your first graduate course?

DOERING: I guess it was a course like Chemistry 5, Advanced Organic at Harvard, which I taught for quite a long time. Then they finally relented and let me teach sophomore Organic Chemistry. Those were marvelous years, the Columbia years. The feedback between student and teacher was so strong. Many of the kids were older because they'd been through the war.

[END OF TAPE, SIDE 7]

DOERING: They came to what was a very small college in a huge university with twenty or twenty-five thousand students. But the college was no more than two thousand or twenty-two hundred. It was just a small college in this huge environment. Many of them came from the Bronx High School of Science and there was a Brooklyn school and there was Peter Stuyvesant. There was Andy Streitwieser. He calls himself Streitwieser and I call it Streitwieser—it's spelled Streitwieser. Andy and I still have a conflict of how we should pronounce his name. [laughter] He came from Stuyvesant.

BOHNING: Well, you ended up with quite a crew in those Columbia days. There was [Jerome A.] Berson and [Kenneth B.] Wiberg.

DOERING: Berson, Wiberg, and Bill [William E.] McEwen, who then spent the rest of his life at the University of Massachusetts.

BOHNING: Wasn't Herb Meislich in that group?

DOERING: Oh, yes, Herb was in that group. Al [Alfred P.] Wolf didn't go into academic life. He went on to Brookhaven [National Laboratory] and was the person who developed positron emission tomography—PET. It required a fascinating development in organic chemistry. You had to take this unstable, very short-lived C_{11} that came out of a cyclotron, and within a half an hour convert it to a close relative of glucose. He really spent a lot of time developing a dimension in organic chemical techniques and reactions to which the rest of us were oblivious. Namely, doing the thing in a very short time. He finally got that whole conversion down to about half an hour. It's very interesting to hear him talk about it. Al Wolf was a great guy.

Sarah Jane Rhoads went to the University of Wyoming and was really one of the first of the prominent female organic chemists. It is interesting that in those days in that part of the West there was no discrimination against women. It was still pioneer country. You knew damn well you couldn't do it alone, you needed a woman, and a woman who could fire a rifle as well if she needed to. [laughter] She was fully accepted as chairman of the department, a wonderful person. There really were a lot of people. Those were fabulous days.

I've been very, very fortunate, until I came to Harvard, to be in a place that had good physical chemists. Good in the sense that they were willing to take an ignorant organic chemist and spend time to explain phenomena in a language that I could understand. I remember going to George Kimball with this little Hückel book I was telling you about that I bought in Heidelberg (5), which in the 1940s was just totally unknown among organic chemists. Well, it was also considered trivial by theoretical people like [Henry] Eyring and Kimball. But George sat down, so patiently, and explained the whole thing to me. Talk about reading the language of mathematics! He said, "Well, this is the equation and of course all that means is that you take a polygon, a regular hexagon, and put one apex at the bottom of a circle and then you draw lines from the center through these things and that automatically gives you the energy levels of the Hückel molecular orbitals." [laughter] That's the way I never read a cosine in my life! [Howard] Zimmerman later published a paper as if he'd really discovered something, and George just passed it off. And I say, "Well, fine, that makes it easy for me." [laughter] Joe Mayer sat in on every lecture of my Advanced Organic Chemistry to find out what we were doing, what our problems were, how we thought and so forth. After every lecture he would come up and we would talk for fifteen minutes, half an hour. I can't tell you the educational value of that, after having got a C in George Kistiakowsky's course! [laughter]

BOHNING: What about [Louis Plack] Hammett?

DOERING: Louis and I never got along. In the first place, when I arrived and for the next two or three years, he wasn't there. He was still doing something connected with the war effort. Then he came back and was anything but a sprightly character. It would really be unfair to say that he was a dim bulb, but it was impossible to carry on a conversation with him. He was born in Portland, Maine, and to say that he was a constipated down-easter is an understatement. His wife was an outgoing person and very sociable. I don't know how it came up, but she once described him as always being two martinis behind the rest of the world. And it was true. Louis never smiled, he never laughed, he was never enthusiastic, he was never pessimistic. He just didn't exist, as far as I was concerned. [laughter] And I was unimpressed with his baloney of free-energy relations. I still am. I never took to it. And to whatever extent I might have, when I went to Yale, Jack Kirkwood said, "You know, this is the biggest disgrace in theoretical chemistry." [laughter] And explained why!

But the upshot of it was that Louis was partly responsible for my leaving Columbia. Elderfield was also responsible for it. Elderfield was a very heavy drinker and when he'd had too much to drink, he was one of the nastiest guys. Some persons become a little more openly like the way he or she is, when they've had a fair amount to drink. But this would bring out unbelievably nasty qualities. He would come into the weekly seminar really half drunk, and then would belabor these students in a merciless way. Even though I was only an instructor at the time, I started to defend the students. And I really didn't care much what he thought. He just took an unbelievable dislike to me. That was a great difficulty for me. Had it not been for the quinine, I never would have been kept on. Many years later it was a matter of, "How can we fire this guy?" I mean, "Wouldn't we look silly?" People would try and bring us back together again. God, I remember going off for a one-week fishing trip with Bob Elderfield alone up in northern Canada. This was sort of a simple fishing trip, bait casting out of a canoe and camping at night. We were trying to have a rapprochement. I guess that's the word for it. [laughter] I would sit there throwing out my plug and was obviously having much better luck than he was, and he was getting more and more angry. [laughter] Oh, Lord, he was solidly in the Illinois school, too, so that was bad news.

When finally the offer came from Yale, around 1951, I went to Louis and I said, "Look, to carry on our work in a competitive way we need an infrared machine." It was Baird or

Thomas that was putting out one of the early machines. Up to that time Ralph Halford, a lovely, wonderful guy, but a minor infrared spectroscopist who had come from Berkeley working with G. N. Lewis, would do these things. Louis said, "Absolutely not. There is no way that I will spend any money on an apparatus like that for organic chemists. It's physical chemistry, and they alone can have it and use it. You don't know what you're talking about. And in any event, it isn't of much use in organic chemistry anyway because there is no such thing as a carbonyl frequency." Well, there isn't, you know, from a theoretician's point of view. There are vibrational frequencies of the entire molecule. It just so happens—and you know the rest. [laughter] So I said, "Well, I guess I'll go to Yale." Now, as I said, I was getting away from Elderfield. But then the department exploded. Elderfield left within a year, and David Curtin left for Illinois within a year. Hammett was a very difficult man. You can have Louis Plack Hammett. The first edition of that textbook was fabulously important (18). It worked a tremendous influence on one person after the other. It really was great. There is no question about it at all. But as a person to work with, gee.

BOHNING: Did others have the same experience you had? Did they have trouble getting new, younger people in? Were there more people joining the staff as you did then at that time period?

DOERING: Yes. There was a tremendously bright, brilliant student of Woodward, Harold Conroy. He didn't stay, and later went into theoretical chemistry at Carnegie-Mellon. Walter Gensler was there as an instructor then. Other than that, the department wasn't really expanding. The year after I left, of course, they went after Gilbert Stork. Gilbert said, "You know, I simply won't come without an infrared machine," so then Hammett gave him one, that bastard. [laughter]

I loved Columbia, and it was by far the most stimulating, exciting time of my life. It was all downhill after that. It was partly the students, partly it was the wonderful faculty club. There was a chemistry table where you would gather, and science was discussed. Any question that came up, you could begin discussing. It really was a teaching table for faculty. It was just a very exciting time.

BOHNING: It's approaching six o'clock, and I realize that you need to leave. Thank you very much for spending the afternoon.

DOERING: Well, thank you.

[END OF TAPE, SIDE 8]

[END OF INTERVIEW]

INTERVIEWEE: William von Eggers Doering

LOCATION: Harvard University, Cambridge, Massachusetts

INTERVIEWER: James J. Bohning

DATE: 29 May 1991

BOHNING: As I indicated, I wanted to pick up where we left off last time. You were still at Columbia. We had talked a lot about the Columbia department and the people who were there. But there are a number of things that I still wanted to ask you about the Columbia period. One is that when [Vladimir] Prelog was a guest lecturer there, he comments in his autobiography (19) that you were his host during the two months that he spent at Columbia. Could you tell me something about Prelog in that Columbia period?

DOERING: That was the first time, of course, that I had met Prelog, and in his eyes Woodward and I were both somewhat notorious characters. He had been working on the synthesis of quinine for several years. If you ever have a chance to interview him, it would really be marvelous, because among all the persons I know, he has a clearer memory for anecdotes and things that have happened in his life than anyone. It really would almost be worth a trip abroad. [laughter] He recounted this story of his total shock when he first read about the synthesis of quinine by two persons of whom he had never heard.

When he said that I was his host, that was in a formal sense. I looked after him. But for the most part, he was spending time with older persons in the department. I remember many enjoyable occasions with him, particularly the times that he came out to Katonah, New York, and the Hickrill Chemical Research Foundation. He was a wonderfully enthusiastic man, with an incredible knack for relaxing the persons with whom he was dealing.

At the scientific level, at that time we really didn’t have very much to talk about. He was still interested in synthesis, and I was doing my best to move away from it. [laughter] So I don’t have very much to contribute to that visit of his.

BOHNING: I want to talk about the Hickrill Foundation. But before we do, in your list of publications, in this early Columbia period, you had one paper with René Dubos at Rockefeller (20).

DOERING: Yes. My connection with Dubos came about through a Harvard College friend, Gardner Middlebrook, who had been very interested in chemistry but went into medicine. After his medical degree, he went to work with Dubos as a research associate. I must say, I found Dubos extraordinarily stimulating, and so did a thousand other people in the world. [laughter]

He had found a new antibiotic from *Aspergillus Ustus*, if I remember correctly. We helped with the isolation, and worked for a bit on the determination of the structure, but did not complete the structure. Oddly enough, it was just a year or two ago that it crossed my mind; I wonder whether the structure is now known. So far, I haven't been able to track it down.

Then we collaborated for quite some time on some of the dyestuffs, on pyronine, an anthracene-like structure with nitrogen and oxygen. It was a very powerful, not quite antibiotic; it was just anti-life. It killed almost everything with which it came into contact. [laughter] It had that lack of discrimination that gave it no useful application at all.

We became very good friends. His uncomplicated and unspoiled dedication to science and to knowledge was, in those early years, really a very useful, valuable reinforcement to me. Perhaps naturally I gravitated toward the persons who were more motivated by curiosity than they were about where they were going in the world. What impressed me about Dubos was the breadth of his interests. In a certain sense he was, in my experience, a forerunner of my encounter with Leo Szilard, who certainly shared that quality of being widely interested outside of science, in the human condition in the broadest sense. Not so very many of my chemical colleagues have been of that sort; that is, both intensely interested in their science and at the same time having an interest in, and a feeling for, the rest of the world.

Both Leo and René were foreigners. I'm trying to think of others. Well, obviously Linus Pauling has had that kind of breadth. He's a person whom I met on several occasions, but he didn't work any particular influence on me. Rene was the first. I was very fortunate.

BOHNING: Do you think that physicists have been more inclined to look outside of their discipline than chemists?

DOERING: It seems to me that they have been, yes. But that's from what I read in the newspapers more than anything else.

BOHNING: I'll come back to the Council for a Livable World a little later, because that is one area that I wanted to talk about. But the other paper that struck me, especially in view of your discussion of Fieser last time, was that you had a paper with Fieser (21).

DOERING: Oh, yes, on the Ponzio reaction.

BOHNING: Yes.

DOERING: Whatever that is! [laughter] That paper came out of his work on explosives. I got along perfectly well with Louis. I had no real trouble with him at all. By that time my regard for his science was certainly clouded by my conviction that he was, in very important areas, out of date, and making no effort to keep up. He was a remarkable showman, and he just loved working on explosives, and then later, on napalm. Thank God, I never got into that. [laughter] It was going on all around us, with Louis setting up little wooden houses in the courtyard between Converse and Mallinckrodt and bombing them. He took great pleasure in their destruction!

BOHNING: He has a book with an obscure title, something like *The Scientific Method*, in which he describes a lot of his wartime activities (22). In that book there are pictures of some of these things occurring out here.

DOERING: Those were the high points in his life. There's no question about it. When he came back after the war, he was dressed in his fatigues and armed with a pistol, [laughter] to recount his stories and experiences to us.

BOHNING: At this point, let's move on to the Hickrill Foundation. I've read a little bit about its origin from your graduate student that you talked about last time, Ruth Alice Weil. Could you elaborate on how that all came about?

DOERING: Ruth Alice Weil was my first graduate student at Columbia. She was an older lady, perhaps forty-three or forty-five. She had majored in English at Smith and then decided that she wanted to go into biochemistry. She had prepared herself for a Ph.D. degree in chemistry by taking night courses and extension courses at Columbia. By the time I arrived she was ready to start with graduate work. I offered her a problem, and she decided to work on it. That was the very first problem that I started anyone on. I was so scared that it wouldn't work that I went into the laboratory for a couple of weeks to do all of the things which I had suggested that she do. [laughter] I must say, I still feel guilty for never having told her, it having taken her the usual two or three years to work it out. But I felt a great deal better knowing that it was going to work.

In any event, toward the end of her stay, at one of the regular Columbia afternoon teas, we were talking about the limitations on the kind of problem one could address with graduate students, because of the responsibility one has, or should at least assume, for their getting a degree and having a reasonable thesis. If one had an idea that was really far removed from current chemical knowledge, it really wasn't fair to try those out on graduate students, but postdoctoral co-workers were the appropriate way of working on that kind of problem. Now one has to remember that at the time there wasn't any real money, or source of money, for postdocs. For me, as an instructor or assistant professor at Columbia, there was none at all.

I just sort of dreamed along with her and said, “God, how wonderful it would be to have a place with four or half a dozen postdocs, where one could really try some of the more risky ideas.” She said, “That sounds like a wonderful idea to me. I’ll talk it over with my husband.” That remark meant absolutely nothing to me, because my attitude toward the students that I had at Columbia was, when they dropped out in the afternoon and took their subway and were dispersed around New York and New Jersey and God knows where, it was none of my business.

I hadn’t realized that she lived in Katonah, New York, which was about an hour’s drive from Columbia. I had <u>no</u> idea that she came from a wealthy family. Her maiden name was Norman. I forget her father’s first name. I never met him; he was long since dead, but it was he who had started to make something of Sears Roebuck. Later, he brought in his cousin, Julius Rosenwald. Between the two of them, Sears Roebuck became a fairly large concern from this small, almost local store that it was when they took over. But Norman and Rosenwald didn’t get along very well, so Julius bought him out at what was a pitifully small amount of money compared to that which Julius Rosenwald himself ultimately made. She had inherited money from her father, and had no intention of using her chemistry as a means of earning a living or anything like that.

So she came back a few days later and said, “I’ve talked it over with my husband. He thinks it would be a wonderful idea to build a laboratory on part of the estate,” which it turns out that they had in Katonah, New York. During all those years, I’d never been out there, never had a clue. That really was the beginning of it, and for something like a quarter of a million dollars they put up an absolutely beautiful place for six postdocs. There were several smaller houses on the estate, and she built a couple, so that they could live right there. [laughter] It was just absolutely out of this world!

For me, it came at a time in my scientific career that couldn’t have been better. It really was an incredible opportunity to work on problems that otherwise I wouldn’t have undertaken. I was also very fortunate in having as the first person in charge of that laboratory, a former student of Paul Bartlett, Lawrence Howland Knox. I was so devoted to him that I’m taking this time to talk about him. He was black, third generation out of New Bedford, Massachusetts. His mother must have been really quite a lady, because he was beautifully mannered and well-spoken. He had a tremendous pride in the family from which he came. There were three brothers. He had his Ph.D. from Paul Bartlett. One of his brothers was a physical chemist who worked at Eastman Kodak all his life. The third was very successful in the State Department. His father had been a cabinet maker in New Bedford. Really amazing!

It meant we could tackle problems like the tropylium ion and tropolone. Larry himself was one of the three or four most gifted experimentalists that I’ve ever had work with me. It was there at Hickrill that the insertion reaction of CH_2, or methylene, was discovered by using the photochemical decomposition of diazomethane. That by itself would have stopped you from giving it as a problem to a graduate student. [laughter] But Larry had absolutely no hesitation in making moles and moles of diazomethane.

They really were sensational years. The spirit out there was so great, because the people all lived under what for them, too, were idyllic circumstances. It really was great. That lab must have opened in 1949, or something like that.

BOHNING: I have you down as director of research from 1947 on.

DOERING: Then it opened in 1947. I'm bad on dates. It would have been about that time that Prelog was there, I believe.

BOHNING: Yes, because he mentions going out to the Foundation.

DOERING: Well, he was somewhat impressed with it. [laughter] I don't think anything like it has happened to anybody. It was just one of the damnedest coincidences. I was married in 1949, and then we moved out there.

BOHNING: So you left the city and moved out there.

DOERING: Yes. I had the dubious pleasure of driving back and forth. [laughter] But it was worth it.

BOHNING: Where did the name Hickrill come from?

DOERING: Their estate was named Hickory Hill, so it was just a meaningless contraction of that.

BOHNING: Did the staff grow after you started it, or was it pretty constant?

DOERING: No, it was just constant. There was room, in effect, for six postdocs, and almost all of the time there were five or six persons there. Most of them, or certainly half or a little more than half, came from abroad, largely from England and Germany. Those were the days in which there were large numbers of German postdocs coming over because the laboratory facilities in Germany were still not by any means reconstructed, nor did they have the most modern equipment. On top of that, it was widely recognized that they had fallen way behind, not only because of the losses of so many fine Jewish chemists, but because of their isolation.

The other reason was just the nature of the German system; the older professors were under literally no pressure to keep up. So you had an unbelievable phenomenon in Germany. A real giant was Hans Meerwein, who was revolutionizing the notion of carbonium ion mechanisms in the early 1920s, if I remember rightly. He published the big papers on the rearrangement of camphene, camphene hydrochloride and so forth, in 1921 and 1922. One decade later, two decades later, three decades later, practically nobody in Germany knew what he had been doing. It's only after the war, when Americans came over and said, "My God, there's a real power here in Marburg," where Meerwein was, that they began to feel good about recognizing him. The other one, of course, was Erich Hückel, about whom I think we did talk a bit. It was not until he was recognized outside of Germany that he was given the time of day inside of Germany, and even then infrequently.

BOHNING: When they set up the foundation, you said they built buildings. Did they set a fund aside to endow the postdocs and provide for their support?

DOERING: It turned out they had a foundation, the Norman Fund, and money was allocated each year for paying the postdocs and providing the chemicals and buying the apparatus and so forth and so on. There never was the slightest trouble with that. In other words, in comparison to the size of the operation, the fund had a lot of money. It was substantial. I suppose a fund which might have had twenty million dollars in it in 1950 would be a hundred million dollars or two hundred million dollars today. And prices were different. [laughter]

[END OF TAPE, SIDE 1]

BOHNING: You said that it was difficult to get money for postdocs at that time. How did your other colleagues at Columbia react to your having your own research institute?

DOERING: I have no idea how they reacted. I know that there was no unpleasant reaction to my face, but I'm not sure that there wasn't a certain amount of resentment. It was such a fabulous laboratory. But it didn't give me any trouble.

BOHNING: Your association with that continued well into the Yale years as well.

DOERING: Yes. The lab must have closed about 1959. It was just a flowering of about twelve years. It was really quite amazing.

BOHNING: What happened to the facilities after it was closed?

DOERING: Ruth Alice very unhappily had a stroke a few years after it began. In fact, she had dinner with Michael Dewar in London, the evening before she had the stroke. The after effects of that stroke were so severe that she couldn't think either of working in the laboratory or keeping an active interest in science. Shortly after that, her husband died.

The pleasure, if you will, that the whole operation had been giving her, and it was a great satisfaction to her, was in effect taken away by the terrible stroke. Her children had very little interest in it, and did not want to keep up the estate once it was clear that Ruth Alice would be better off living in New York City, so they decided to close it down.

Meanwhile, I had moved to New Haven in 1952. Larry Knox left very unhappily, having divorced his wife; he went to Mexico City to work for Syntex. In retrospect, you can see small disintegrations in various parts of it. In other words, the great spirit that had kept it going and got it started and held it together for nine or ten years began to fall away.

They either gave or sold the whole estate to a private school in the area, the Harvey School. They took over the laboratory as a place to teach science to their high school students. Just a couple of years ago, I visited it, and it was recognizable. [laughter] But it's a run-down shadow of the intensely active place that it had been.

BOHNING: In addition to Knox, wasn't Maitland Jones [Jr.] there also at one time?

DOERING: Yes. His parents, Maitland and Irma Jones, lived in the next town, Bedford. As a young boy, he had shown an interest in science. The Joneses knew the Weils, so when the lab was set up, they came over to ask, "Is there any possibility that you can take this little boy for the summer?" He was either twelve or fourteen. Mait says he was fourteen. I say he was twelve. [laughter] I knew I wouldn't be available all day long, but Larry said, "Oh, sure, that would be fine." So Mait started out. He was near the end of grammar school!

He remembers, and it's amusing to talk to him. We were trying to figure out what kinds of things we could give this young boy to do. We couldn't have him cleaning glassware all day long. I taught him how to make melting point capillaries, or at least enough so that he could help. We'd get these huge tubes, I would hold them in the flame and rotate them until they were a molten mass. Then I'd give him one end and say, "Now run to the end of the laboratory." [laughter] Which he would do at top speed, and we would draw out yards of melting point capillaries in that way.

I'm sure that little things like that, that have precious little to do with chemistry, with molecules, with chemical transformations or anything like that, for all of which he was much too young, nevertheless gave him a tremendous boost in feeling the excitement in the whole enterprise. Here was this small boy wandering around among postdocs. Then he went on to Andover [Phillips Academy], and from Andover to Yale, by which time I was there, and he did his Ph.D. work with me. I became very close friends with the family. So that was an odd offshoot of the Katonah laboratory.

BOHNING: How did you find it, separating your efforts between graduate students at Columbia and postdocs out at Katonah? As you said, you kept your problems sort of separate. The more daring ones, you were doing out at Katonah. The ones you were pretty sure would get the student his degree you were doing at Columbia. How did you keep that all sorted out?

DOERING: There were no problems keeping it sorted out in my own mind, but the people at Katonah came to Columbia every week for the research seminar. During the year, each of them would give a couple of talks. These were modeled as well as I could after Woodward's problem-solving seminars. Every so often, particularly in the summer, the students at Columbia would come out to Katonah, for a big cookout. Actually, the connection was very close between the two groups.

The quality of the seminars, needless to say, was very much higher for having the postdocs taking part. That comes back to what I feel is a very important part of the educational process, and so do a lot of other persons, that the competition that a graduate student has from fellow graduate students or from postdocs around him, is crucial in setting the sights and the standards. It's true in all sorts of activity, from tennis through to chemistry, that it makes a big difference with whom you're playing the game. So that seminar was enormously successful.

BOHNING: Did you have a library at Katonah?

DOERING: Oh, yes. We had a library with the common journals, *JACS* [*Journal of the American Chemical Society*], the English journals, the *Journal of Organic Chemistry*, *Beilstein*, and a few others. Then, because the postdocs came in to New York regularly, they used the library at Columbia as well. The library was a lovely place to study.

BOHNING: It sounds like a Shangri-La of some type.

DOERING: Oh, it was. It was really remarkable. A lovely place. I have some pictures of it somewhere. I've got to put things like that in order. [laughter]

BOHNING: I don't think I've ever seen any pictures of it.

That period of time was certainly an intense one in terms of the kinds of things that you did scientifically, and I'd like to go through some of them. I'm not sure whether it's easier to do it by topic or in chronological order. But I'm curious, because you started at Columbia in 1943, teaching naval students, and at first, if I may say so, living on the quinine synthesis.

DOERING: Yes!

BOHNING: But then certainly establishing yourself very quickly with new concepts. How did you arrive at these ideas, speaking in general terms first? I know Woodward spent a lot of time reading *Beilstein*, and got many ideas in the early days from reading *Beilstein*. How did you make that move into your independent areas?

DOERING: To begin with, as I said before, I was determined not to stay in the synthetic part of organic chemistry. I had become fascinated with mechanistic understanding, through Kohler and Bartlett and Woodward. As I think I mentioned, one of the remarkable qualities of Woodward as a synthetic organic chemist was his conviction that mechanisms could be applied with tremendous effectiveness and allow one to have control of the various reactions that were involved in synthesis. He was himself intensely interested in mechanism.

With that self-imposed pressure to stay away from synthesis and to move into mechanism, I suppose at the beginning I was simply trying to do two things; that is, to create through the application of whatever mechanistic thinking I could do at the time, new reactions, and also to see whether there were mechanistic questions that hadn't been answered in connection with well-known reactions.

I must say, as I go back over those problems, they seem naive to me now, and did just shortly afterward. The problem that Ruth Alice started on was, in retrospect, a very small problem, which I'd analyzed as best I could in such a way that it appeared that pyridine could take the place of the conventional carbonyl group or ester and so forth, in the Michael reaction. And that's all there is to it. It was a very, small step. On the other hand, it turned out to be useful. It was a way of approaching the development of new reactions which is so commonplace now that it isn't worth talking about. But at the time, not so many persons, I guess, were trying to create new reactions on the basis of applying those few general mechanistic principles that we thought we knew and understood.

There was a great deal of stimulus at Columbia itself. Tuffy [T. Ivan] Taylor was more an inorganic chemist, but he was set up to use radioactive tracers. That stimulated me to think about questions that you couldn't answer without labeling. So work just gradually moved in that direction.

BOHNING: Deuterium analysis was pretty unsophisticated at that time, as I recall, using the falling drop method.

DOERING: It was falling drop. There was no question. It meant you had to have enough material so when you burned it, you got at least one drop of water. [laughter] I got into that, and then moved on to radioactive carbon, where I never did any of the measurement work but collaborated with Taylor. Columbia was, in effect, an isotope center. [Harold] Urey had been there, after all.

It's typical of mechanistic organic chemists that the advent of a new analytical technique very often stimulates new questions that would have been fruitless to ask because there would have been no way of arranging for an answer, or for testing ideas emerging from having asked the question. Isotopes obviously opened that possibility for a lot of people. Already in those years I became interested in the so-called "no mechanism" reaction, and that was just an accident. Arthur Cope was at Columbia until 1945, so we overlapped. One of my students, Lillian Levy, was married to Harold Levy, who was working with Cope, and so there was always a lot of talk about what in God's name was going on in the Cope rearrangement. [laughter]

That started me thinking about that whole area, and an interest that runs right through my entire mechanistic career. It doesn't begin to bear fruit for ten or eleven or twelve years after I first started thinking about the Cope, having come up with nothing but frustration. During that period I had not been able to think of anything sensible to do. Everything one knew about free radical chain mechanism reactions, about reactions with carbonium ions, carbanions as intermediates, intervention of light; nothing was relevant to the Cope rearrangement. During those years, I couldn't think of anything constructive to do about the Cope, except tell people that it was an example of a "no mechanism" reaction.

BOHNING: I remember seeing that in one of your papers, that you mentioned it that way (23).

DOERING: It's very strange to me, that one can go on thinking about a problem or about a set of observations for years before something finally, at long last, happens. It's just the perseverance, I think, and the unwillingness to abandon the problem that finally leads to something.

BOHNING: You overlapped briefly then with Cope at Columbia. Did you have any discussions with him about it?

DOERING: No, I found him impossible to talk to. I sort of liked him, but he was, in my estimation, a stuffy, straight-laced guy. There was nothing that I would call a sense of humor.

[laughter] A heavy touch, let's put it that way. I suppose he must have sensed it. I don't think he ever liked me, particularly. My relation with Cope wasn't as bad as my relation with Hammett or Elderfield. Those were very stormy years.

BOHNING: And yet they were very productive for you.

DOERING: Highly productive. Yes.

BOHNING: It's amazing, in a way, considering the environment in which you were operating. Your research group out in Katonah must have been a great relief for you, to have that working for you.

DOERING: Yes, but goodness, the other thing which was fabulous was the quality of the graduate student at Columbia in those days, beginning in 1945-1946, when they began coming back from the war, older and more determined to make up for lost time. Goodness, think of the students who came through there. [Kenneth B.] Wiberg, [Jerome A.] Berson.

BOHNING: [Andrew] Strietweiser?

DOERING: Andy Strietweiser, yes. [laughter]

BOHNING: You don't agree on the pronunciation of the name, as I understand it.

DOERING: I agree with you. He calls it Streit-wise-er. And Al [Alfred P.] Wolf, who spent his whole life at Brookhaven, where he did very fine mechanistic work. What he's best known for is putting together the whole process of positron emission tomography. He was a remarkable fellow. Gosh, it was just one after the other. It was sensational.

BOHNING: Maybe we should just talk a little bit about some of the things that occurred in that period of time. You had the first resolution of a tertiary aliphatic alcohol, which was very important for later mechanistic studies (24).

DOERING: Yes.

BOHNING: Was that a sidelight on something else?

DOERING: No, that was deliberately done to gain more insight into the S_N1 reaction. Certainly there had been stereochemistry done before, but it had involved benzylic stabilized cations, and it seemed to me that one should get down to the basic one, the aliphatic one. That it hadn't been resolved just seemed like a challenge. There were reports in the literature that, in principle, one never would be able to resolve the tertiary alcohol, that three alkyl groups wouldn't make enough difference. The secondary one would obviously have a hydrogen as a small group, and so forth.

But there's an example. Harold Zeiss came in, and he got his Ph.D. finished in nine months. He was just unbelievably gifted in the laboratory. Really fabulous. He went on then to spend some time at Yale, where, in a really remarkable collaboration with Lars Onsager, discovered the bis-benzene chromium zero complex. When we get to the Yale part, I can't help but talk about Onsager. I'll bring it in there.

It was really an exciting time. Andy did marvelous work. Stemming from that, Ken Wiberg did also.

BOHNING: But the tropolone work was all done out at Hickrill.

DOERING: That's all Hickrill. That's Larry Knox. The first preparation of tropolone was in two percent yield, as I recall, a permanganate oxidation of tropilidene. That's an illustration of how you get carried on by work that you're doing. Tropilidene was not so easy to make, until we started thinking about methylene as a possible intermediate. Then there was the earlier work by [Theodor] Curtius, of ethyldiazoacetate in benzene, to give what turned out to be tropilidene.

[END OF TAPE, SIDE 2]

DOERING: So that's how we got into the addition of CH_2 to benzene, to make tropilidene (cyclokeptatriene). And finally into the insertion reaction into the carbon-hydrogen bond. In effect, working in one area and wanting a much simpler synthesis—if you will, a method of preparation—provided a lot of the stimulus.

BOHNING: Did you have much interaction with Michael Dewar at this point?

DOERING: I met Michael for the first time in 1956, I guess it was, in London. But long before that, I had acquired a tremendous admiration for his originality and creativity. After all, the contribution which he made to the mechanism of the benzidine rearrangement was a completely new idea. No one had any idea that two pieces could hang together and slip over each other and break a bond but not fall into two pieces, and then come back together again to give the semidine rearrangement and the benzidine rearrangement.

All of those things I knew from the literature. Of course, it was Michael who suggested the tropolone structure for stipitatic acid and colchicine. As a brash young man in England, he was being treated really quite badly by the old establishment. [laughter] When we met for the first time, we hit it off, and we've been lifelong friends. I think he's enthusiastic about my contributions, and I know I'm enthusiastic about his.

BOHNING: I talked to him in February, in Florida (25).

DOERING: Did you?

BOHNING: Yes. I have four hours on tape with him.

DOERING: Oh, for goodness sake.

BOHNING: It was an enjoyable time.

DOERING: I should think so. Providing he didn't spend all his time talking about his last two years at Texas. [laughter]

BOHNING: Oh, some of it, but not all of it.

By the way, I talked to my research mentor from NYU [New York University] this morning, who's now at Northeastern, and he said to send his regards. His name is Karl Weiss.

DOERING: Oh, yes. Yes.

BOHNING: He stumbled across another way to tropylium bromide (26).

DOERING: That's right. That's right.

BOHNING: He said this morning that they had this terrible mess that they couldn't do anything with, and then he finally realized what it might be. I think they used the diazoester and benzene.

DOERING: Yes, I think they used the ester.

BOHNING: How was this whole area received? In many respects, it was really quite a breakthrough. How was it received by the chemical community?

DOERING: I would say almost immediately and with great enthusiasm. I think much of it came from Woodward. Whenever one of these new things came out, he received it with tremendous pleasure and enthusiasm, and would present it in his seminar at Harvard. Gosh, when anything was discussed in Woodward's seminar at Cambridge, it was all around the rest of the world in no time at all. [laughter]

BOHNING: Did you discuss any of these ideas with Woodward?

DOERING: No.

BOHNING: May I ask if there was any reason why? Or is it just a matter of circumstance?

DOERING: Well, we were geographically separated. Our paths crossed very infrequently. When they did, we talked about each other's work, but it was just very infrequent. That is, he was moving in another part of the world, and I wasn't moving so much anywhere. [laughter]

BOHNING: I guess that the carbene work was done at Hickrill, but you were already at Yale. Is that true? It isn't clear what the time sequence is.

DOERING: I'm not so clear on the time sequence, either. Tropolone came out in 1951 (27). So we were already working on the reaction of diazomethane and benzene before that, as I remember, in 1949 and 1950, just as soon as we set tropolone as a goal. From then on, it really began, and we stayed with it. The insertion reaction we got into quite independently. Once the war was over we really did our homework and got back to the literature, which wasn't being abstracted well at all. It turned out that [Hans] Meerwein had been looking at the photochemical decomposition of diazomethane in diethyl ether, and had isolated normal propyl and isopropyl ether.

There was a confusion exacerbated by [Rolf] Husigen, that the reaction might have been going through an ylid, where CH_2 was attacking the oxygen to produce an $CH_2^{-}O^{+}$, which was followed by a rearrangement. Such rearrangements have subsequently been found. But the clean demonstration of the insertion reaction was on the hydrocarbon. We did that deliberately, because when you irradiate diazomethane in benzene, you get two products, tropilidene and toluene. Later it turned out that there's a higher temperature rearrangement of tropilidene to toluene, but that has nothing to do with the insertion reaction.

Horst Prinzbach, who's at Freiburg, tried to show that the insertion reaction, at least into a carbon-chlorine bond, didn't involve the separation into two species. So the carbene work begins in 1949 or 1950, and then is just carried on as one can see one's way to carry it on. For example, that insertion reaction, when we first did it using pentane, isopentane, and cyclohexane as the substrates, was very difficult to analyze quantitatively. Gas chromatography was just coming along, and for fifty dollars we bought a detector from Gowmac. We put the whole thing together for a little under a hundred dollars. [laughter]

BOHNING: You coined the name carbene, is that correct?

DOERING: With the help of Bob Woodward and Saul Winstein. The three of us were together one evening, and I was explaining how interesting these reactions were, and that it looked now as if divalent carbon was really involved. The history of divalent carbon, by the way, is very spotty, because back around the turn of the century, [Julius] Stieglitz and [John U.] Nef were very enthusiastic about divalent carbon as an intermediate in a wide variety of reactions, where it played no role whatsoever. I think they did imagine that it was playing a role in the Reimer-Tiemann reaction, the chloroform-phenol reaction, and that turned out to be more or less correct. But as far as aldol condensations involving carbenes is concerned, it just wasn't in the cards.

But they were explaining everything that way, and so it was discredited. Yet it's the oldest mechanistic hypothesis, I think, that one can find in the literature, going back to 1862. [A.] Geuther had been working on the alkaline hydrolysis of chloroform, from which one gets carbon monoxide as one of the products. The question is how? So he threw in a mole of "lassos" and pulled out HCl and had CCl_2 left, right? Which was then hydrolyzed to carbon monoxide. [laughter]

So divalent carbon is an old, old idea, but I think it was stimulated by the few things that we did, and it has become a respectable part of organic chemistry. Once respectable, it went on to develop to a fairly high degree. But it has that funny history of going through a long period of being completely discredited.

But anyway, the three of us were talking about it, and I said, "Now you've got to give it a name." So we talked about various possibilities, and finally decided this one was pretty good.

BOHNING: What were some of the other possibilities? Do you remember?

DOERING: I don't remember.

BOHNING: Which of the three of you suggested it originally?

DOERING: I don't think anybody knows that either.

BOHNING: I'm just curious. I didn't mean to push that too far.

DOERING: Well, I think we all had so much to drink that our memories wouldn't have been trustworthy in any event. [laughter]

BOHNING: During all of this period of intense activity, you still were not terribly happy at Columbia. When you moved to Yale, had you been out searching for other positions, or did they come to you?

DOERING: Oh, no. No, they came to me. I'd had an offer from Wisconsin a year or two before that, which I turned down. At that time, my personal family life was really in bad shape. I was divorced in 1954, with three very small children, and it really was a heartbreaking decision to make. It was just such a distressing, painful, exhausting relation in which you make the decisions you can't evaluate, because you have no control experiment of any sort. It did seem to me that I couldn't go on that way, not sleeping nights, and just incessant difficulties. It had a bad effect. There was a lot of work at that time that never got published, just because there was a limit to the amount of energy and time.

BOHNING: I can identify with that.

DOERING: However you want to describe it. So the Wisconsin offer was out of the question as far as I was concerned, because it would have taken me too far away from the children. When I was divorced, thereafter I had the children every third weekend, and then for six weeks each summer. But Yale was possible. Still it was a long drive, picking them up, from New Haven to Leonia, New Jersey. The George Washington Bridge, for me, is an unhappy symbol.

BOHNING: I have a similar feeling about the Tappan Zee Bridge; I won't tell you why, but it's very similar.

DOERING: All right.

BOHNING: So I understand. What was the Yale department like when you got there?

DOERING: It was pretty well broken down. It was in 1950 that Jack Kirkwood was brought in by President [Whitney] Griswold with full power, not to fire tenured persons, but with full power to try to make it one of the first class departments, among the top twenty in the country.

Let's see, who was there when I arrived? [Arthur] Hill was there. [Werner] Bergmann was there. He was a very, very fine, wonderful gentleman who worked in steroids. I had interviewed him back in 1942 or 1943, with the possibility of going to Yale to work with him. But I didn't find it as stimulating at that time, and it was a pretty dull place. Who else was there? Jim [James] English [Jr.], who was also a very fine person, but not a terribly active person. Howard Zimmerman got his degree with Jim English. And then [Harold G.] Cassidy was there. The organic life was pretty low.

BOHNING: Didn't English and Cassidy have a textbook (28)?

DOERING: Yes. That's right.

So Kirkwood was responsible for my coming. Also, Onsager was there. Therefore, the physical chemistry was very strong. Julian Sturtevant wasn't as yet in biochemistry. The other physical chemists were just satellites revolving around Onsager. [laughter] [Raymond M.] Fuoss and old [Herbert S.] Harned. They were solution chemists and physical chemists who were getting their theoretical inspiration from Onsager. So really the two stimulating people were Onsager and Kirkwood. Among physical chemists, Kirkwood certainly was a giant in terms of the breadth of his competence. There was no part of physical chemistry in which he didn't feel comfortable. He was the only physical chemist I've ever met, parenthetically, who was enthusiastic about the cumulative examination system. He thought physical chemists ought to be able to know something about all of physical chemistry. And Onsager. They were very happy with it. But all of the others were so specialized that they really didn't like it. In my experience, it never worked for physical chemists, except at Yale.

It was Kirkwood and Onsager who told me just not to worry very much more about mechanism of reactions in solution, that it was too complicated, and that there weren't ways of studying it. We were having enough trouble just finding out what sodium chloride did in water, without worrying about the S_N1 reaction. [laughter]

When they heard that I was doing work in the gas phase, "no mechanism" reaction, they said, "That's where you should keep going." Which was very odd.

BOHNING: Did you get to teach undergraduate organic, since you weren't able to do that at Columbia?

DOERING: Well, at Columbia they finally did let me teach it.

BOHNING: Oh, they did?

DOERING: Oh, sure. That all worked out. Carpenter's influence declined pretty rapidly. [laughter] Teaching undergraduates, the elementary organic, has always given me tremendous pleasure and satisfaction. I made a habit of not keeping lecture notes, and trying to rethink parts of the course, at least, every year. What should one be doing about bringing in mechanism and resonance and things like that? It had tremendous impact on my research. I have never worked on a problem without wondering whether, if it worked out successfully, it would be of sufficient importance that it would find its way into the beginning chemistry. It seemed to me that carbenes would, if it worked out. [laughter] I always got great, great pleasure from those aspects that got into the elementary books.

BOHNING: I noticed that some of your papers were done by students who had postdoctoral support from Hickrill, but it wasn't clear whether they were working at Hickrill or whether they were at Yale, working up there.

DOERING: When the laboratory at Katonah began to close down, the Weil children were very good about giving some support to me for postdocs at Yale. Even when I moved to Cambridge, Frank Weil and his two sisters made a fine donation to Harvard for the support of my organic chemistry as long as I should remain active, and then for the support of organic chemistry. So the relation to Hickrill has continued at a much reduced level, in a less important way.

BOHNING: What were the facilities like at Yale? Sterling Labs was opened in 1926 or 1927, somewhere in there. So you were there thirty years later. Were they still pretty well supported?

DOERING: They were well supported once Kirkwood arrived, so there really was no problem. For example, Kirkwood was able to see, long before any of the organic chemists, that NMR would revolutionize structure determination. I remember him coming back from a meeting of the Physical Society and sitting down with me, and telling me it was going to revolutionize things, and why and how. So there really was no problem with that.

Also at Yale was Dick [Richard L.] Wolfgang, who was a radiochemist, an extraordinarily brilliant man, and a person who valued originality I would say more highly than ninety percent of the physical chemists I've known. He had wonderful set of radiotechniques going. He was the first person to couple GC with radioactive counting. You could work with quantities so small that you couldn't see the blip on the GC, but it would come out on the counter end of things. It was he who got me interested in chemical dynamics and RRKM theory and the whole problem of how energy is used within a molecule finally to generate the reaction, the transition state and so forth. I should have learned all of that stuff much earlier from other persons, but Dick Wolfgang was an enormous stimulus to me.

It's really been true all my life. At Columbia, I found George Kimball, Ralph Halford, and Charlie Beckman, really very stimulating. And Joseph Mayer. They were stimulating in the sense that they changed my way of phrasing questions, so I just don't keep rephrasing them in the same old way. And at Yale, Kirkwood and Wolfgang certainly played that role for me.

BOHNING: You mentioned Onsager earlier.

[END OF TAPE, SIDE 3]

DOERING: Lars Onsager was a person whom I admired and enjoyed enormously. Whenever I had any questions, he would take the time to explain the answers to me very carefully. But the one thing that I really remember has more to do with his attitude toward chemistry than it does with any specific influence that he worked on me. I have never encountered a person whose knowledge of all of chemistry could compare with that of Onsager. He was profoundly interested in inorganic chemistry, he knew the structures, and he followed the new ones that came up by x-ray. He had all of that in his head.

When I arrived, Harold Zeiss was an assistant professor, following up the work by [F.] Hein, a German chemist who had examined the reaction of chromium trichloride with Grignard reagents like phenylmagnesium bromide. He got out many weird compounds whose structures weren't known. Somehow Onsager became interested in these forty-two papers that Hein had written, to a point where he knew every melting point, every property that had been recorded on these unknown substances. He would come in in the morning to ask Harold. "What's new?" on this and that. It was Onsager who finally realized that these compounds were chromium zero compounds.

What had made it very difficult was that, in the reaction of phenylmagnesium bromide with chromium—I think it was trichloride—you can tell what kind of an inorganic chemist I am—the product looked as if it consisted of chromium and three benzene rings. It was Onsager who finally realized that one unit was benzene and the other was diphenyl, that it was just some radical reaction that had intervened to link two of the benzene rings together to give diphenyl. Then these two things had reacted with chromium zero. So the prominent compound wasn't a simple one in that sense. But it really was Onsager's doing. That's the first pi complex with a metal in its zero valence state, dibenzene chromium. Then that all got cleared up pretty nicely.

But to see that guy's interest, intense interest, and then this wide knowledge—because after all, here was a person who was trying to calculate how fast protons should diffuse in ice and is worrying about the difference between helium three and helium four. [laughter] Oh, he was unbelievable. He really is the quintessential example of a guy who sleeps through the seminar and then appears at the end to ask a devastating question. [laughter] It's as if he knew the whole story before it started.

BOHNING: It was during this time, if I remember correctly, that you interacted with Leo Szilard. Well, let's see, maybe it's a little later than the point we're at.

DOERING: I first met him in 1952 in New York. He had become interested in the world population problem and had the idea that the basic knowledge of the reproductive process might have advanced further than applications that could have stemmed from the basic knowledge. First, on the ground that few scientists are concerned about practical applications of their basic work, by nature. Second, in the area of birth control, there were strong prejudices floating around. He convinced the Conservation Foundation in New York, which was concerned with the conservation of animals, that among them was the human. He wasn't concerned about the survival of the other animals, because he could have said, "We've got to control the human race in order to spare the rest of the world." But he didn't have that notion yet.

Even so, his idea was to bring in biologists and medical people whose research had been in the area of human reproduction, and question them about what they had done and ask if there were any possible applications. It was just a device to direct their thinking toward it. But it was clear to him that he needed an organic chemist to listen in. One of the demographers, Kingsley Davis at Columbia, who later went to California, gave him my name. We did that project together, and that was quite fascinating in itself. Nothing much came out of it. It turned out, of course, that the Pill was being developed in a Catholic institution by [John] Rock, where for some reason they didn't have the wit to put a stop to it.

Leo and I then never lost contact. We became very good friends. Some years later, we wanted to see whether anything could be done politically to turn back the development of nuclear weapons and the prospect of their use.

BOHNING: I understand that grew out of a series of lectures that he gave.

DOERING: Yes. "Are We on the Road to War?" or something like that. Which I never heard. [laughter] But it was a nice idea, and it was a typically Szilardian idea. The American system has a Senate in which each state has two members regardless of its size. The House of Representatives was nothing that you could ever control, but the Senate looked to him as if you could get control of it, quite literally. He wanted an organization that would in effect attain this control by raising money to help elect senators who were already, in a general way, opposed to the use of nuclear weapons and who could see their long-term danger to the survival of a reasonable world. The organization would then have an educational activity going on in Washington once the new senators arrived, bringing people in to give seminars. That started in 1962.

So he called me up and asked me to come down and join the board and I said, "Yes." As you know, the missile crisis was in October of 1962. It was within months. Leo, who had kept alive during the Nazi period by having a suitcase packed, even at that time in Washington had a couple of suitcases packed so that, if he felt things were getting too hot, he could leave. He called me and said, "Look, you can't make any progress here in Washington. All the doors are closed. The place is just the calm before the storm. You should leave." He called several of his close friends, and said, "You should pack your bag and get out."

I said, "No, I can't do that, because my children are here." But I remember that really ominous weekend when it hadn't been decided. The kids were with me, and we just drove around and I figured, "If it really goes, I'll drive to Maine." But he left and went to Geneva, saying that he thought he could be more effective there. You can imagine what the reaction of a lot of people was to his just pulling out. He never really recovered his position in Washington as an authoritative, reliable figure.

When he left, it was a question of who would take over the operation. I said I would begin to do a little of the dirty work, the daily work. For the next sixteen years, I spent, on the average, one day a week in Washington, arranging seminars. I'm no lobbyist, at all. But I got to be very good at helping with the elections. I think that's because I'm an organic chemist, and as you know, you pay attention to detail. To get people elected, you have to pay attention to the smallest details in their campaigns. In retrospect, I don't think a single organization of that sort has very much impact. I finally simply ran out of steam. I couldn't write any more fund-raising letters.

BOHNING: There was a controversy in the 1970s about campaign funds and the way it was done, and some ruling from some government bureaucrat. There was a flurry of interchange in *Science* magazine about that whole affair. I know you're quoted, because you were president at the time (29).

DOERING: Yes. That whole problem of controlling our elected officials through the use of money is a pretty nasty one. In the end, of course, what was done turned out to be counterproductive. The political action committees were given the right to contribute up to five thousand dollars, and in the end that was just hopelessly counterproductive, because hundreds of these PACs were set up. I guess we realized it at the time, and so we were, internally at least, in quite an ambivalent position.

Most of us realized we should be out of the business. You shouldn't have a system that was corruptible in this way. We thought, of course, that our cause was a good one, and therefore we were on the right side, but nevertheless in principle it was a bad thing to do. I think most of us knew that. But we also knew that this change was going to be devastatingly bad, and that has turned out to be true.

BOHNING: But the organization still exists.

DOERING: The organization still exists. It raises a great deal of money. And then how effective it is, you have to ask. By 1974 it was pretty clear that there was a move to what we call the right, and particularly in the small states, where we'd had pretty good fortune. The very first person that we supported had been George McGovern, back in 1962. We raised something like twenty-five thousand dollars and gave it all to George McGovern. He won by five hundred and some odd votes. [laughter] So there was a god smiling on the operation. There wasn't any question about that. It's also true that South Dakota was a small state and twenty-five thousand dollars, just as Leo had expected, went a long way.

BOHNING: Were you surprised, in those early days, about the extent of financial support coming from people towards the candidates? As I understand it, they would contribute, they would write the check in the candidate's name, but it would go through your organization.

DOERING: Yes, they would mail the check to us, and we'd put a rubber band around it. That's right. So as an organization in that sense, we were never contributing. We did occasionally give a thousand dollars here, or two thousand dollars there. But yes, I was surprised. I felt we had a tremendous responsibility to those persons. It seemed to me to be precious money that they were giving. It wasn't tax deductible. It was the hardest sort of money for them to be giving. I think that's one reason why I worked really quite hard at it, and particularly hard at identifying good candidates and trying to assess realistically whether the money that we gave would make a difference.

At least I considered it, and Steve Thomas, who was working with me, also considered it a failure if we won by more than five percent. It was just as bad as losing. You'd misspent their money if you won too easily.

Washington has a funny attraction for people, in the sense of making them feel important. Then you realize very quickly that the senators and congressmen with whom one deals, for the most part, have as their number one priority in any event, getting reelected. That's number one. Everything else is secondary to that.

But on top of it, many of them I would just simply describe as egomaniacs. That is, no issue was of much importance except insofar as it was going to increase their chances of getting elected. I found a lot of them very unsavory types. For example, I was awfully pleased to see [Alan] Cranston finally get it. [laughter] He had seemed to me to be a really doubtful character from the very beginning, although he created his image as the great liberal. But dealing with him and talking to him was something.

[J. William] Fulbright was one of the noble people. There were two or three Southerners for whom accomplishing something useful, for helping to make a change, was important. Many of the people we elected, I guess, were better. But think of the weakness in a Frank Church. We played a big role in getting him in the first time. Then these weaknesses develop, and you've got no control over it at all.

BOHNING: Who were some of the other people who were active on your board? Charlie [Charles C.] Price was involved.

DOERING: I got Charlie on the board, because as you know, he played a big role in the World Federalists. He was really quite an effective lobbyist, going around to senators' offices and talking to them about a problem. Matthew Messelson was on the board. He's very well-known because of the controversy over bee-droppings in Vietnam. Was this yellow stuff a poisonous gas? He's explained it to me five times, and I refuse to understand it. He thought that his slender connection to [Henry] Kissinger was the way to get things done. He would actually try to block the council from moving in certain directions, particularly during the Vietnam War, because it would offend Henry. Well, you can imagine how kindly I took to that position.

Roger Fisher, of *Getting to Yes*, the title of his bestselling book, the international negotiator. Oh, we're still good friends, but Roger could only see things in the light of what they might do for Roger Fisher. He was tough to deal with. Who else was on the board?

BOHNING: Wasn't John Edsall involved early on?

DOERING: He was involved early on, but he was never on the board. Leo had some <u>very</u> complicated device for setting the thing up. I could go back and dig it out. The Council for a Livable World was intended to be the action arm of the Scientists for a Livable World. Edsall was one of the Scientists for a Livable World. We never heard from them again, needless to say. [laughter] Oh, glory, it was a funny time. But I don't think it was time misspent. I think an organization like that changes the atmosphere a little. My goodness, you can't look to one thing and say, "Well, if it hadn't been for the Council for a Livable World, the antiballistic missile fight would have gone the other way." It went the way it did because people didn't want those damn things in their backyards.

BOHNING: Were most of your financial contributors scientists?

DOERING: A lot of them were, yes. Support was very heavily drawn from the scientific community.

BOHNING: How did you find that aspect of it, fund-raising?

DOERING: As I said, I finally got worn down, because you were forever writing letters that were going a little further than the reality as it existed or you could reasonably expect it to exist a year from now, or two years from now. It wasn't my cup of tea, but I tried to learn it as well as I could and not say things that were just downright lies. In the end, I thought we needed more effective lobbyists, and I thought George Kistiakowsky would be a great guy. I worked very hard to get him on the board, and as soon as I did, as soon as that was accomplished, he turned against me in a personally nasty way that made it very, very easy for me to resign, to sever all connections with it in 1978.

And that hostility goes back. I told you that a couple of weeks after I arrived at Harvard, he took me aside as we were walking back from the Faculty Club and said, "Bill, I want you to know that I voted against your coming here." [laughter] That was just another example of the spillover from Woodward. I was in the Woodward camp. Woodward and Kistiakowsky appeared to have been at odds the whole time that they were here. Woodward was determined that the organic part of this department should excel, that they should be better than the physical chemistry part, of which Kisty was the most influential spokesman.

I told you about the Yale incident with Roger Adams saying that if you want to ruin the department, just take on Bill Doering. That association with Woodward tells a lot about Woodward as a second-order spillover.

BOHNING: That's amazing.

[END OF TAPE, SIDE 4]

BOHNING: Are there other examples of that? You mentioned some pretty big names. And last time you mentioned Carl Marvel.

DOERING: It's hard to say. You don't know who reviews your grant proposals. I've always had terrible trouble raising money to support my work. There's no question about that. But I can't think of any other examples off the top of my head. There probably are.

BOHNING: Well, those are some good ones, that's for sure. We'll come back to the Harvard situation in a little bit. But I also wanted to ask you about some of the consulting that you did. I noticed, for example, a paper around 1956, on the structure of the Buchner acids (30). What struck me, though, is that it was a contribution from Hickrill, and from Humble Oil in Baytown. I know you did some consulting for Humble, and I was curious about that connection.

DOERING: In the first place, I don't remember how I got started consulting at Humble Oil in Baytown. If I remember right, it was 1952. That was the time when Joe Franklin, the physical chemist, was in a sense running or strongly influencing the research at Humble, and so a lot of it was basic research. The work that he did with Frank Field in what is called high-pressure mass spectroscopy, where you could observe secondary ion reactions, is almost at the beginning of ion-molecule chemistry. It was an extraordinarily stimulating place to consult.

It was Joe Franklin who felt that an industrial laboratory, to keep itself alive, needed outside people to come in, not just as consultants, but to give courses. He had organized a Humble lectureship. I never gave one of them, because I was a consultant, but [Paul] Bartlett did and [Frank] Westheimer did. It was quite a roster of persons who went there to lecture morning and afternoon, to give examinations, for an intense two weeks. The people who took part in the course had to take the exams, had to be graded, but were relieved completely of any other responsibility. It was a remarkable experiment.

Needless to say, it came to an end, but as far as I know, it was unique. I don't think DuPont does anything like it. Hercules certainly doesn't. Monsanto never did. Procter & Gamble, for which I consulted for a while, didn't. None of the drug companies do. It was really remarkable.

And so in Baytown, Texas, there was a really vibrant microenvironment. It was really early on, from that connection, that I got a whole different perspective of how to look at chemical reactions. I forget what the Baytown contribution was on the paper you mentioned, but there's another paper that I think is much more striking. Nobody pays much attention to it, but it's also from Humble Oil. They were running large-scale Friedel-Crafts reactions.

BOHNING: Oh, yes, I've got it here. Paper number 65 (31).

DOERING: [laughter] Good.

BOHNING: It talked about why the catalyst activity declined.

DOERING: They'd run alkylations and rearrangements of methylated benzene, xylenes, and produced huge fractions of highly alkylated aromatic compounds. Eventually the catalyst would die. This was really plaguing them in the methylation reaction. When the reaction was dead, beautiful crystals remained on the bottom. Tons of them. [laughter] It had never occurred to them to look in there to see what had happened to the poor aluminum chloride. You can isolate these crystals and recrystallize them. It turned out to be the tetrachloroaluminate of the heptamethylbenzenonium ion, a completely stable carbocation. This discovery antedates all of [George] Olah's work. Baytown was a great place. I still can't get over the sight of that mass of crystals of carbonium ion.

BOHNING: What did they do, just shovel it out and dispose of it?

DOERING: They just threw it away. Of course, once you knew what it was, there wasn't anything you could do to prevent its being formed. At least they got a paper out of it. [laughter]

BOHNING: We were talking about your consulting connections. You had a paper in 1962 on the pyrolysis of thujone (32) in which you had somebody from Esso doing molecular weight determinations, and you had someone from Procter & Gamble doing deuterium exchange rates. I'm struck by how you had your consulting people doing parts of this paper for you. I always thought it was the other way around. [laughter]

DOERING: Well, I've always enjoyed it. But, finally the Humble enterprise ended. The law changed in Texas, so that an out-of-state company that owned a Texas company could actually control and run it. Esso had owned Humble for some time, but there wasn't any way that they could really get their hands into it. Once they could, the laboratory at Baytown was shut down. Baytown land had sunk eight feet anyway, as they took water out of the table. Half of it's under the Gulf now. [laughter]

I continued consulting at Esso at a time when Al [Alan] Schriesheim was there. He was generally concerned about possible applications of carbanion reactions in petroleum. Then about that time, [Charles] Pedersen came out with the crown ethers. That's one important event. The second was that another industrial chemist, by the name of [Harold E.] Zaugg, a person who seems to have almost no credit whatsoever, discovered that dimethyl sulfoxide, hexamethyl phosphoramide, worked very special effects on carbanionic reactions. Schriesheim saw the potential in these discoveries and really exploited them. It was a very exciting time!

So I really have had very good luck with the places at which I've consulted. I probably got more stimulation than I gave.

BOHNING: In those early days at Baytown, do you think part of that was because they had plenty of money in which to indulge themselves in fundamental research?

DOERING: I think so. That's one thing, and the other is that the oil companies make their money somewhere else. They don't really expect to make it out of research. If somebody discovers polypropylene, that's fine. If it's really important, they will buy it. It's as simple as that. That was the attitude at Procter & Gamble. It was the attitude at Esso and Humble. But we would look much more refined if we had a research organization going. [laughter]

I'm certainly not exaggerating my assessment of it, although I may be exaggerating the real situation. But in the end, it was terribly frustrating for so many of the persons working in those laboratories. They'd be given a problem. It would be approved. You can proceed to develop a synthesis for a perfume, for example, at Procter & Gamble, where a lot of perfume work was carried on. It's one of the subliminal ways of keeping your product in use. In the wash basin, for example. If you get used to the odor, you don't want to change and get up one morning and find a funny smell in the bathroom when you're used to an old familiar one. So they had perfume work going on.

After years of doing beautiful synthetic work, they'd say, "Well, it's really not of any interest to us. We've got another source of the natural product," or "We're abandoning this odor anyway," or something. These people would just collapse from disappointment, because they had relied on management for the decision that, if the project were successful, it would also make money for the company. And management, as I've encountered it in all my experience in consulting, is a total failure at that. I never consulted for the Polaroid Corporation, but obviously Din [Edwin H.] Land was exceptional as a manager of industrial research, in that he said, "If we do this successfully, it will make money. We'll see to that. And you, as someone who contributed either a developmental scientific activity, or basic research, will get a lot of satisfaction from it."

But in one industry after the other, the management is so far removed from the job they're supposed to be doing, that the feedback to the poor people doing the research could be very depressing. Night after night you'd go out to dinner with these guys, and what you would encounter was a great deal of frustration, disappointment, feeling of a life being squandered. Many of them, ambitious and needing money and so forth, would make the move and leave science and go across the divide into the management area, where, it would turn out, there were managers above them whom they couldn't influence in the usual way. So from that point of view, my consulting experience in industry has left me very jaundiced.

BOHNING: Is there a lot of turnover as a result of that, in the areas you were in, like Procter & Gamble or Humble?

DOERING: Not so much turnover as withering. To a point where I guessed that the half-life of a Ph.D. entering industry, a half-life as an active scientist at the bench or in close connection with research, might be as much as eight years, but probably was less. Everywhere I've been it's been like that. Now, I've been at the worst places, in a certain sense. That is, Procter & Gamble and Esso. There was a book written by someone (was it White?) about the big industrial research companies and how they worked. Procter & Gamble and Esso were two of them, at any rate, he described.

I consulted for years on polyurethanes at the Carwin Company, outside of New Haven. It was a very small company that had bought an aniline formaldehyde process from Goodyear [Tire & Rubber Company]. Carl van Winkle intended to develop the polyurethane field. He was really quite successful, and finally sold it to Upjohn. One of my former students became the director of research and, ultimately, a vice president of Upjohn. He was really great. Born outside of Baghdad, he had a remarkable feeling for both the business end of things and the science end of things.

BOHNING: You also did some work with Stauffer, but there's no date. Is that very early?

DOERING: That's very early, and not for very long, and was relatively unsatisfactory.

BOHNING: And most recently with Dow.

DOERING: That didn't amount to anything. Dow certainly doesn't know how to use a consultant, in my experience.

BOHNING: I was just curious. You're back in Texas, the Freeport area this time.

DOERING: Yes. I was just one of the chattels that came along with their purchase from Upjohn of this polyurethane division. They didn't quite know what to do with me, so for a couple of years they just tried to be nice.

BOHNING: You had some kind of administrative position at Yale, as the director of the division of sciences?

DOERING: Yes, for three years, during the first two of which Kingman Brewster was provost. Kingman had been two years behind me at Belmont Hill School, but we had known each other quite well. When he came to Yale, one of the first things he saw was the relatively low state of science. He really wanted to improve matters. Improving matters in anything like that involves getting in people from the outside, and not promoting some others; it involves gradually upgrading. The first two years were really very good. Because was a very close friend of Mac [George] Bundy, he had learned all about the system that Conant had introduced here at Harvard. When a department, no matter which, seemed to be losing its reputation, there was a mechanism by which the president could assess the situation and try to formulate steps that could be taken to improve it.

That mechanism was to identify, let's say, half a dozen leaders in English history, and invite them to Cambridge. You can do that very easily as president of a substantial university. The president just calls the first couple of persons, says that he's trying to find out who are the active leaders in the field, and were they willing to give a few names. Before you knew it, you've got a pretty good consensus. So it was easy to do, even if a president had never fully realized that he had a declining department on his hands. [laughter]

Kingman was fully aware of that mechanism and set it up. We had some really fine meetings on where chemical engineering at Yale, for example, should be going. It was an inbred, minor department compared to that at Princeton or Berkeley or several other places. Then he would begin to take steps. But, around that time, President Griswold died, and Kingman became president. That was now a different story, because he had to keep the faculty on his side, and could no longer go around tearing the place up. I think both he and I realized that things were going to have to move very much more slowly. That was number one.

The second thing is that I made the foolish mistake that has been made by many, many other persons in science before me, that somehow you could keep your research going and take over major administrative work. But if the administrative work has any meaning for you at all, it's going to consume a lot of your psychic energy. You're going to have to worry and fret about these problems. So it became clear to me that I was failing to maintain my research, and that my science was going down. I was determined to quit.

Not only that, I was determined to leave Yale so that the telephone wouldn't ring and I just wouldn't be on call any longer. I was thinking of leaving the country, possibly going to Germany, to Karlsruhe, where [Rudolf] Criegee wanted very much to have me come. That was 1966, a poor year for the German economy, so that the kind of negotiation that I could work out wouldn't have given me enough money to get my kids through college.

When Bob Woodward heard about it, he said, "Look, there's no way that you're leaving the United States. If you're really going to leave Yale, then I'll open a position for you here." That's how I came here. But it was directly from a feeling (and after all, in 1967 I was fifty years old), that if I was going to stay in science, I really needed a new kick in the ass. The way to get that is to move.

BOHNING: You never took a sabbatical, did you, up to that point? At least I don't see any record of it.

DOERING: No. In Cambridge, of course, you take sabbaticals but always stay here or give a few lectures, but the idea of going someplace is not considered.

BOHNING: Well, let's talk a little bit about Harvard. So your presence here was the direct result of Woodward's preventing you from leaving the country, as it were.

DOERING: Yes. No question, but that's right. He must have fought very hard. I don't think he had to fight hard with his organic colleagues, but he obviously did, as I told you before, with Kistiakowsky and the physical chemists.

BOHNING: Who was chair then?

DOERING: E. J. [Corey]. Very shortly after I arrived, it was clear that it wasn't a move to my liking. The major reason why I accepted was, of course, to be near Bob, and have that stimulation. But already at that time, Bob was spending half his time in Basel and Europe, and was already beginning to let graduate students do whatever they wanted to do, without any oversight. They were lucky if they would see him twice or three times a year.

Some of the worst dissertations I've ever read in my life were under Woodward's sponsorship in that period. He asked me to serve on two or three committees, but then never again, because some of them were just awful. But Bob's ego was big enough so he was going to push them through no matter what, and that was distasteful to me.

It was about that time, 1965, the Woodward-Hoffmann rules came out. I would have already started on so-called diradical rearrangements that were then classified as Woodward-Hoffmann-forbidden. When the first papers came out, he was quite incensed. He had some kind of notion that all thermal reactions would have to obey Woodward-Hoffmann rules. I never even had a chance to discuss it with him. My first big paper was with [Krishna] Sachdev (33). It's an impossible paper to read. It was on the stereochemistry of the thermal rearrangements of substituted cyclopropanes, and Bob was sure it was wrong, that it had to be wrong.

So he sent a copy to [Roald] Hoffmann. He asked me, "Is it all right if I send it?" I gave him two copies of the manuscript as soon as it was complete. One went to Hoffmann and the other to Gerhard Closs. They both said, “Well, nature can tolerate this.” [laughter] After that, Bob and I never talked chemistry again.

BOHNING: Really?

DOERING: Yes. We continued to have the best of personal relations. He was going through very bad times. I know more about his personal life and difficulties, I’m sure, than anyone else in the world. But somehow he had developed the feeling that I was taking something away from the Woodward-Hoffmann rules. That was deeply disappointing to me, and furthermore his heavy drinking was getting to him.

BOHNING: Did he have that same attitude towards others who might cross him, as it were?

DOERING: Well, one certainly hears stories to that effect. I don’t know anything from first hand experience.

[END OF TAPE, SIDE 5]

BOHNING: I have heard stories about some of his postdocs. You were talking about students being free to do whatever, and I guess the same thing was true with some of the postdocs. It would have been in the 1960s, I guess, when there was a revolt of the postdocs on the chlorophyll project?

DOERING: I’d only heard about it second-hand, but I think it was in the chlorophyll work. [Raymond] Bonnett in England, by the way, has finally published that paper (34). I don’t know whether you saw it. It came out as a special contribution.

BOHNING: I didn’t see it, but somebody told me that it was out.

DOERING: Yes. That was a labor of love.

BOHNING: You came here in 1967. Compare Harvard with Yale, if it’s possible.

DOERING: One of the really strong points at Yale was the concern for undergraduate teaching. It was simply in the atmosphere that, no matter who you were, it was an important part of your life to teach undergraduates. As a result, the undergraduates had a much better time of it. They worked harder, with more enthusiasm, accomplished more, and therefore were more fun to teach.

Here, there was no tradition of that sort by the time I arrived. God knows what it might have been before, when everybody taught undergraduates. Presumably it was in the air then. But by that time, Woodward had been here for how many years, and had never taught an undergraduate in his life. Corey had been here for how many years, and never taught an undergraduate course. So that there was now a division in the department between those who were carrying the undergraduate teaching burden—and it had become a burden; you automatically used that word, without thinking about it—and those who were above it: those whose research was so important—their contribution to science, the teaching of postdocs—that all the rest was unnecessary.

At Yale, I think the only professor who didn't teach undergraduates was Onsager, and that's because nobody would have been able to understand his English. Even the graduate students referred to his courses as Norwegian I and II. [laughter]

It was a real shock to me, coming to a place where something that I enjoyed doing, from which I got tremendous satisfaction, was no longer valued. Furthermore, Bob disapproved of my teaching undergraduates.

So ever after I'd say, "Gee, if you have a choice of sending your kid to Yale or Harvard, as far as chemistry is concerned, I certainly would send them to Yale, without any doubt." I think that's still true.

BOHNING: The junior professors are the ones that do the teaching at the lower level?

DOERING: Yes, in general. I shouldn't be saying these things, but David Evans is very good about it. He comes from a tradition where teaching undergraduates is important. I don't think Stuart Schreiber is ever going to teach an undergraduate. George Whitesides has taught an undergraduate course, but only with essentially the full-time assistance of another junior professor, who may have given half or three quarters of the lectures. And then that was not one of the big courses, let's say science for non-scientists, with emphasis on applications and the relation of chemistry to living processes and things like that.

[Yoshito] Kishi tried, and I tried very hard to get him into it, to arrange that he should teach a higher level undergraduate course, but it really was too strenuous for him, and for them. That wasn't really a matter of unwillingness. He values it. I guess Jeremy [Knowles] taught undergraduates once or twice, and of course did a superb job of it. It's now at a point where at least every year there's a visiting professor here to help teach the undergraduates. That really wouldn't happen at Yale, even today.

BOHNING: I can remember years ago, Farrington Daniels always taught freshman chemistry and felt that it was very important to have the best people teaching at that level. Is what you are describing more or less unique to Harvard, or is that common? What about Berkeley?

DOERING: I don't know. I think it may be unique to Harvard. Or let's say this must be the prime example of it.

BOHNING: What else about the Harvard period? How many years has it been?

DOERING: I was active for twenty years before retiring. The other thing that strikes me more generally, and I don't know whether it has to do with Harvard, although I think it does, is that the quality of graduate student, at least those that I have been able to attract, was certainly highest at Columbia. Now then, we said those were very special years. Many of those kids came out of the Bronx High School of Science, or Peter Stuyvesant, and were really very specially gifted, and certainly highly enthusiastic. Columbia was then a small college, with some two thousand students in a university with a total enrollment of twenty thousand or so in those days.

There again, as later at Yale, the professors cherished their little college. There was no question about your enjoying teaching undergraduates and being convinced that it was very important to do. Furthermore, the feedback loop was so strong, because these students were so enthusiastic. Yale was sort of in the middle. By the time I got here, the times seemed to have changed. For some reason, I had my best graduate students at Columbia, some very good ones at Yale; but here it was rather a disappointing lot. That can be explained, one, by my getting older. Two, by the extraordinary magnetism of the synthetic activity at Harvard, that led to a self-selection process going on. They were not looking to Harvard for mechanistic organic chemistry or whatever you want to call it.

So from that point of view, it was a bad move for me. But I repeat, that certainly could be my fault. It was also the period in which the National Science Foundation wasn't supporting physical organic chemistry very well. Just that period. So there's probably no single reason. But I would have been much better off in Karlsruhe. [laughter]

BOHNING: There is still some early work that we hadn't talked about. One of those is the bullvalene period. I was always intrigued by the story you told me of how you found Hückel's book bicycling through Europe. And twenty years later or so, here you are being one of the first to use it, in a very extraordinary way.

DOERING: But not without the help of George Kimball and other people at Columbia who explained it all to me. But the Bullvalene thing is just a direct outcome of the early years, worrying about the Cope rearrangement and getting into this whole business of no-mechanism reactions. The big contribution, of course, in that whole area is the Woodward-Hoffmann rules. I don't think there's any question about that. But I've made some contribution to the reactions that don't obey the rules. [laughter]

You see, one of the most interesting things about the no-mechanism concept is that it forced one to ask, "What questions are left to answer?" The only question left to answer is, "Why is the activation energy what it is?" If there are stereochemical choices, why is the activation energy for one of them higher and for the other one lower, which in concerted processes has its complete answer in the Woodward-Hoffmann rules. But it forces you to think about enthalpy and entropy as the bedrock factors in chemical reactions. The ultimate purpose of chemistry is to translate structure into those parameters. Why doesn't butane go to hydrogen and two molecules of ethylene? It's not an arrow-pushing problem. It's not a Woodward-Hoffmann problem. It is a bedrock problem to which no one at the moment can give an answer.

I got on the energy kick once I realized what the Cope rearrangement was all about, and with an artificially created Holy Grail of trying to get the Cope rearrangement to go so fast that its activation enthalpy would be zero. Then what kind of a molecule would it be? With the Cope rearrangement having an enthalpy of activation of thirty to thirty-five kilocalories, you could then put the rearrangement into a three-membered ring and bring it down into the twenties. Then you could do further things that gradually get you down to homotropilidene. Then having realized that you could realize this homotropilidene rearrangement, it just came in a flash that, if you were to introduce a threefold symmetry, it would be a very remarkable molecule indeed.

There have only been two or three flashes like that in my life, but they really are great.

The effort to reach zero activation energy keeps people going. Michael Dewar at the theoretical level is one. Several other persons are at the experimental level, attempting to bring that activation energy down by various devices. It still hasn't gotten to zero. I think nature doesn't like zero. [laughter] But Bullvalene was really quite fabulous.

BOHNING: There's some interesting nomenclature in all of those compounds.

DOERING: Well, you must know that to be worth living, life has to be somewhat amusing for me. One innocuous way of getting a little pleasure is to find names for things. But the Bullvalene name actually came from graduate students, Mait Jones and Bob Willcott. It's funny how a name like that sticks, too.

BOHNING: The alternative is hopeless, if you're going to use standardized nomenclature.

DOERING: It is hopeless. So it might as well be silly.

BOHNING: Peter Leermakers was one of your postdocs. Can you tell me something about him?

DOERING: He was on the wild side already as a postdoc. He would come in in the morning and have his few ccs of alcohol. But he was a very stimulating, imaginative guy, and a person I came to like very much, and for whom I had really very great hopes. But it was hard to get him to finish things in the laboratory. I remember on one problem, I only got him to complete it by betting him a hundred dollars that he couldn't do it. I thought that was a cheap price to pay. It really, really was too bad that he died, because I think he was settling down. He had a great deal of promise. He was the first to work on the energy flow problem.

BOHNING: Are there any other postdocs of note that we should talk about that we haven't discussed?

DOERING: There is a list of these now very influential German professors, [Horst] Prinzbach at Freiburg, and Gerhard Schröder at Karlsruhe, Wolfgang Roth, [W.] Kirmse, who stayed largely in the carbene field, but then made very important contributions in the carbonium ion field. Gerhard Klumpp in Holland. Their influence on German organic chemistry has been quite significant. There wasn't a hell of a lot of mechanistic organic chemistry going on in Germany at the time. So there's the Huisgen influence, and then there's the influence of these four people mentioned above. They are at the top of the group in Germany now. All are very enthusiastic characters. Particularly Prinzbach. You don't know Prinzbach, do you?

BOHNING: No.

DOERING: What a wild guy he is. Amazingly successful.

BOHNING: How would you characterize the change in physical organic chemistry? You've watched it now for a long period of time. You said that it's hard to get funding for it now.

DOERING: Yes.

BOHNING: Do you think it's seen its heyday? Is it past its peak, or does it just need to regroup?

DOERING: I don't think it's begun to peak, because the criterion one would use is this. Write down a new reaction that you haven't seen before. Can you tell how fast it's going to go under what conditions? Or, if you conclude that it's not going to go very fast, can you then tell me, use such and such a catalyst, and it will work in such and such a way, and it will give you so many kilocalories and so forth and so on. We're obviously very, very far away from that point. It's illustrated further by how little one knows quantitatively about the role of solvent, particularly in ionic reactions.

And how slow the progress is! After all, the effect of dimethyl sulfoxide, crown either, and a few other solvents on organic reactions were really just discovered, not invented. They did not emerge from a state of the science, where you know so much that you can say, "Well, it's never been done, but I can predict it will be done, and work in this way." I think that many mechanistic problems are now very difficult, particularly because so many of them do involve the solvent.

In my mind, another closely related area is the understanding of enzyme catalysis. In my view, enzymes are idiosyncratic solvents, if you will, in which the reactions occur, and the protein is doing a variety of things. Mechanism in that area is progressing very slowly. It's not simply basic organic chemistry that's progressing slowly at that level of understanding. I think the problems are tough. When you combine their difficulty with very little money, and an already dwindled audience, then it's going to be some time before the basic study of mechanism recovers.

It's now very difficult to find any junior person who can teach a course in advanced mechanistic organic chemistry, because they have not themselves been taught one. Or worse, they haven't worked with a person who did. Another way of putting it is to ask the students, "With whom would one work if one wanted to be a mechanistic organic chemist?" Most of the ones that you would now mention are old. Or gone. Notice that I now use "mechanistic organic chemistry," rather than "physical organic," because not a new crop but a different crop has appeared. Now there is a large number of chemists who apply new and developing physical methods to organic chemical problems, be they mechanistic or structural. They are more often concerned with the application of the physical method than with problems in organic chemistry.

In other words, let's say a physical organic chemist who is in NMR spectroscopy and developing two-dimensional NMR, is not doing it with a particular chemical problem in mind, but rather, "How can I advance the resolution power of this method?"

You had persons who became wedded to one particular physical method, and made big contributions to it. No question. But that is not a person who said, "Here is a problem in the transformation of one chemical into another, one structure of matter into another, into which I want to gain further insight, or to which I want to give further understanding," and then use whatever methods might be needed. I think that's the big difference, that the mechanistic organic chemist is driven by a question or a problem, and is trying to figure ways of probing at it.

If my problem requires high pressure, I'm not setting up high pressure equipment, but going to Frank Klärner in Essen and asking him to collaborate. Whatever needs to be done. If it's radioactive isotopes, there'll be someone around who's willing to do that. So it's the mechanistic organic chemist who has disappeared.

So you are quite right when, in asking the question, "Do you think it's slowed down?" "Yes." "Do you think it's come to an end?" "No." I think that the people are no longer there. You see persons like Peter Dervan who start out as mechanistic chemists, and very quickly move somewhere else. Maybe it's because the problems are too tough. Maybe it's because there's no money in it. Maybe it's because the audience is very small.

[END OF TAPE, SIDE 6]

DOERING: You can be sure it will take some time before it comes back, because the young people just aren't there any longer.

BOHNING: Where are their interests? Is it all tending towards the biochemistry areas?

DOERING: In large number, they certainly are. To be a mechanistic organic chemist, you have to be intensely interested. You have to stay with a problem and think about it for a long, long time. Molecular biology, it seems to me, has made it possible to do new things without hard, frustrating intellectual work that goes on for years before you see your way through. At this essentially taxonomic stage, it's relatively easy, but it, too, will eventually reach a stage where that is no longer true.

Go back over parts of organic chemistry. For example, the analysis of natural products. There was a time, at the beginning, when it was exceedingly difficult, not only to isolate substances, but then to establish their structure. Then it went through a period where it wasn't quite so difficult and more and more persons could become involved. By now, isolating a natural product and determining its structure by the old methods would require some masochistic streak running in you. [laughter] There are ways of doing it very easily.

E. J. Corey, for example, believes synthesis has gone through that stage. In the early years, it was really tough. It was a tremendous challenge, and the persons who stayed with it had enormous stamina and fortitude and determination to reach the target. When you get around to interviewing him, he'll say, "If the molecular weight is a thousand or below and it's a natural product, which means that it's been made, then you can make it." [laughter] I think he's convinced that synthesis is going the way of structure determination.

BOHNING: Coming back to what you said earlier, does the advent of the instrumental techniques that we have available today that didn't exist when you were at Columbia allow you to ask more challenging questions now?

DOERING: Yes.

BOHNING: Because you now have different techniques available to probe even further.

DOERING: Yes. I think, without any question, that's true. When a new technique comes along, a person active in the subject asks, "What does that allow me to do? How does that allow me to think in directions that I would have cut off because they weren't leading to any experimentally related outcome?" To that extent, I think you find a few persons always pushing those techniques to their limit, within a matter of two, three, four, five years; you use them up pretty quickly.

BOHNING: You had deuterium very early on at Columbia and people who could help you with that. If they hadn't been there, you probably wouldn't have used that.

DOERING: Absolutely right. And if deuterium hadn't existed, I would never have thought of trying to discover it. [laughter]

BOHNING: Someone gave me a photocopy of this picture, and I realized that the same picture is right over here.

DOERING: It is the same one.

BOHNING: What was the occasion of that picture?

DOERING: It's taken abroad, Paris, as I remember it. Beyond that, I don't remember the occasion. I think it was a Ciba-Geigy [Corporation] conference.

BOHNING: I'm struck by two things in looking at the photograph. One is the beard and the other is there's no bow tie, but a regular tie.

DOERING: And a regular tie. Terrible! I grew the beard quite explicitly, very shortly after I arrived at Harvard, in 1967, at the time of the student unrest. I was teaching Chem 20, the undergraduate course. At that time, and even today, the majority of the students were premeds, with the intention of going to medical school. Events were obviously getting in the way of their preparation. Organic chemistry is required for admission to medical school. Meanwhile, this situation finally reached a point where the university said that nobody had to take final examinations that year. They could all take makeup exams.

You would agree with me, I think, that to come back after a summer, in which you will have been spending time on God knows what, and then taking a makeup examination in the fall, is going to reduce your chances of doing as well as you would have if you had taken it in the spring at the regular time. I was very worried about that prospect, as in one course after the other they threw in the towel and joined the rebellious movements. So I did two things. One, I wanted to make it clear to them that I wasn't a part of the establishment. It just crossed my mind that growing a beard in those days would do that, would help.

The other thing I did came from the Council for a Livable World. There was a powerful film based on Hiroshima that had been put together by the U.S. Army in Japan, but the army wouldn't release it. However, the Council did get it released. This was a very, very deeply moving film of footage taken by Japanese photographers. It was about a twenty-minute film. I said, "Look, I know that the questions that you're all concerned with are of intense interest to you and importance to you, but it's going to be a long life, and taking the examination in this course, keeping up studying with it, is very important for you. I think it will put things in perspective if you see this film. Nobody's required to see it. I'll just put it on at the end of one of the last lectures."

I guess most of them stayed. I have never seen such a shocked bunch of kids in your life. To talk about things being wrong in society was one thing, but to see this film was another. The upshot of it was that almost ninety percent of them took their final examination. It wasn't for years after that that I finally shaved the beard off. And that only happened because I'd had too much to drink, and someone shaved off one half of it. So then I had a serious question to face. [laughter] What to do? I decided it was more sensible to shave the second half off.

BOHNING: Are there any areas that I haven't touched on, that you would like to comment on?

DOERING: I think I'll say a few word about my Chinese venture.

BOHNING: Oh, yes. That was one thing on my list that I didn't ask you about. I'm glad you brought that up.

DOERING: I think that's certainly the kind of venture that I got into as a direct result of having known Rene Dubos and Leo Szilard and other persons who really had the conviction that alone you could do something in the world, that there weren't problems that were so big you couldn't tackle them. I'd always had as one of my close friends, an older Chinese who did his graduate work with Fieser at the same time that I did, and in the same laboratory. Frederick Chew-ming Chang. He's eighty-six now.

I had known something about China, and it occurred to me that, after the Cultural Revolution was over in 1976, by 1980 the first undergraduates would be coming out, and that chemistry must be in a really parlous state in China. At that time, delegations were coming through Harvard, and I signed up to meet with one group and explain that I would be willing to help in any way I could. One thing that I thought might be helpful was for the Chinese to bring together a bunch of their teachers from the university, and have me just go over how we taught undergraduate organic chemistry (or how I taught organic chemistry) at Harvard, so that they could see what kind of changes had occurred in the last twenty or thirty years.

I said they didn't really have to worry about travel expenses, that I would pay my own way. I didn't hear anything until finally I got a letter from the president of Fudau University in Shanghai, an old mathematician, saying that it would be good for me to come, perhaps in the fall of 1980. I went over for three weeks, came back to be with my graduate students, and returned for another three weeks, and went over really a year's work, lecturing in the morning and giving them problems.

The Chinese had done their part well. They had brought in about fifty persons, ranging in age from forty-five to more than seventy. There were no younger persons they could bring in. They just didn't exist. The Cultural Revolution had cut it off <u>totally</u>. I've got a picture of that class somewhere. Yes, here's the picture of these Chinese characters, studying their organic chemistry.

The upshot of it was that, without exception, each one of them said, "Look, what you're talking to us about must be a graduate course. There is no way we could begin to make these changes." For the most part, the differences had to do with mechanism, as opposed to remembering the names of two thousand organic compounds!

I returned from that experience with the conviction that they couldn't be helped by working with the teachers already in place. The only way you were going to be able to help was by bringing young persons over here. They asked me if I would come back the next year and help them get research started. I said, "Yes," but meanwhile had made a good contact at Fudau University, in Xu-zhi Gong. He had received his Ph.D. from Hyp Dauben at the University of Washington, and saw clearly in what a terrible state chemistry was in China. He and I then drew up a plan by which they would devise a procedure for selecting forty or fifty students nationally, and make arrangements for them to go to a language school in southern China that was being run by UCLA at the time.

So we drew up this plan, and I said I would take the responsibility for placing the students in what I considered universities in the top thirty or so in the United States. We would see that their tuition was paid by the host university. So we drew it up, and sent it to the Ministry of Education at the time. I then went to Beijing and talked to the Minister about it. "How can you, as an individual, do that?" I said, "That's my problem, and I guarantee that I can do it. I've got enough friends, even without any of them knowing. So I can do that."

I must say, the Chinese did a superb job, and it was a good plan. They knew what their kids had been taught. We couldn't. I knew there was an incredible gap between our examinations and theirs. Our teachers of organic chemistry or physical chemistry would say, "We can't admit anybody who has taken such an elementary examination." But my conviction turned out to be right. If you get the best kids out of a system, even though that system may not be the best, they'll do all right. I wasn't able to convince all the places that I tried to get them into, but I convinced enough to take the fifty. The first class came over in 1982.

I ran this side of the program until things began to change. Then Li Peng, who is the premier now, became head of the state education commission. The last time I went to Beijing and talked to him, it was clear that he wasn't interested in anything like this program. So then the word came out, "Next year we're going to cut it in half." I said, "You're not cutting my program in half. You're cutting it out. All the people who took part in this in the early stages were my friends, and I'm not going to go back to half of them and say, 'Sorry, I'm dropping you.'"

But anyway, about two hundred and fifty of those kids have almost all got their Ph.D. now. Nobody was bounced out. Not a single one failed a course. Can you believe that?

BOHNING: That's quite a record.

DOERING: Yes. Then when you get the assessments back, it's what you might expect, except for the fact that a third of them were really rated as "the best student I've had in twenty years" or something like that. There's a fine middle third, and then there was a third that might just as well not have come, but nevertheless nobody's bouncing them out, because they're turning out perfectly good work. This program is an example of trying to accomplish something, doing the best you could, and in the end having fallen far short. But I thought it was worth a try.

BOHNING: Has anything else like that been done, that you know of?

DOERING: It's the only one in chemistry. There were programs in biochemistry and physics. But these were arranged either officially or semiofficially through a national organization, and therefore their students had to be available to, if it had been chemistry, all one hundred and eighty-nine places that give a Ph.D. in chemistry. That seemed to me to be a pretty inefficient way of trying to raise the standards in China.

Then you know in part what happened. Even before Tiananmen Square, two students, both physical chemists graduating from Rice, went back to China. They were the first. But when they returned, they were asked, "What kind of skills do you have? Did you learn English?" They said, "Yes." So they put them to teaching English. Those two kids are still teaching English, with Ph.D.s in chemistry. There's a third person that I know about, but there wouldn't be very many more, because the Chinese network is as fully developed as any network I've ever seen. If anything happens to one Chinese, all other two thousand of them in the United States know about it.

One went back from Canada, and they found out that he'd learned how to drive a car, so he became chauffeur to the president of the university. So it wasn't just Tiananmen Square. It's the old professors who were just damned if they were going to let these youngsters back into the university and show them and everybody else up. From the point of view of China, it was a total failure, and it's an opportunity that's lost. We'll be damned lucky if twenty-five years from now good ones will be going back.

But I enjoyed it. I went over five times. The program ran very smoothly, and did just what it was supposed to do.

BOHNING: I thank you again very much, for another enjoyable afternoon, and I appreciate your taking the time.

DOERING: Thank you.

[END OF TAPE, SIDE 7]

[END OF INTERVIEW]

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