THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

NORMAN HACKERMAN

Transcript of an Interview
Conducted by

James J. Bohning

at

The Welch Foundation, Houston, Texas

on

23 October 1990

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

RELEASE FORM

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Dr. Norman Hackerman

(Date) Oct 23, 1990

(Revised 20 February 1989)

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Oral History Program

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(Signature) Norman Hackerman

Dr. Norman Hackerman

(Date) Feb. 16, 1991

NORMAN HACKERMAN

1912	Born in Baltimore, Maryland on 2 March

Education

1932	A.B., chemistry, Johns Hopkins University
1935	Ph.D., chemistry, Johns Hopkins University

Professional Experience

1935-1939	Assistant Professor of Chemistry, Loyola College
1936-1940	Research Chemist, Colloid Corporation
1939-1941	Assistant Chemist, United States Coast Guard
1941-1943	Assistant Professor of Chemistry, Virginia Polytechnic Institute
1944-1945	Research Chemist, Kellex Corporation
	The University of Texas at Austin
1945-1946	Assistant Professor of Chemistry
1946-1950	Associate Professor of Chemistry
1948-1961	Director of the Corrosion Research Laboratory
1950-1970	Professor of Chemistry
1952-1961	Chairman of the Chemistry Department
1960-1961	Dean of Research and Sponsored Programs
1961-1963	Vice President and Provost
1963-1967	Vice Chancellor for Academic Affairs
1967-1970	President
1985-	Professor of Chemistry Emeritus
	Rice University
1970-1985	President
1970-1985	Professor of Chemistry
1985-	President Emeritus
1985-	Distinguished Professor of Chemistry Emeritus

Honors

1956	Whitney Award, National Association of Corrosion Engineers
1964	Joseph L. Mattiello Award
1965	Palladium Medal, Electrochemical Society
1965	Southwest Regional Award, American Chemical Society
1972	LL.D. (honorary), St. Edwards University
1975	D.Sc. (honorary), Austin College
1975	Honor Scroll, Texas Institute of Chemists
1978	D.Sc. (honorary), Texas Christian University
1978	LL.D. (honorary), Abilene Christian University
1978	Gold Medal, American Institute of Chemists
1981	Mirabeau B. Lamar Award, Association of Texas Colleges and Universities
1982	Distinguished Alumnus Award, Johns Hopkins University
1984	Edward Goodrich Acheson Award, Electrochemical Society
1984	Alumni Gold Medal for Distinguished Service, Rice University
1987	Charles Lathrop Parsons Award
1987	AAAS-Philip Hauge Abelson Prize

ABSTRACT

Norman Hackerman begins the interview by describing his childhood and the public education system in Baltimore, Maryland, noting the rigorous course work and individual attention students received at City College High School. He then recounts his seven years at Johns Hopkins University, where he received both his bachelor's and Ph.D. degrees and developed interests in philosophy and psychology as well as in physical chemistry. He recalls the different labs in which he worked before his commitment to work in Patrick's lab on silica gel studies. Hackerman remarks upon the difficulties the university encountered due to the Depression, and its effects upon laboratory equipment and research. He next describes his experiences teaching at Loyola College and consulting for the Colloid Corporation, his job with the Coast Guard at the Federal Lighthouse Service, his years at Virginia Polytechnic Institute, and his work on the Manhattan District Project. The final portion of the interview briefly summarizes his early teaching background at the University of Texas, his consulting work for the Lone Star Gas Company, and his creation of the Corrosion Research Laboratory (now the Balcones Research Center).

INTERVIEWER

James J. Bohning, Assistant Director for Oral History at the Beckman Center, holds the B.S., M.S., and Ph.D. degrees in chemistry. He was a member of the chemistry faculty at Wilkes University from 1959 until 1990, where he served as chair of the Chemistry Department for sixteen years, and chair of the Earth and Environmental Sciences Department for three years. He was Chair of the Division of the History of Chemistry of the American Chemical Society in 1987, and has been associated with the development and management of the Center's oral history program since 1985.

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INTERVIEWER: James J. Bohning

LOCATION: The Welch Foundation, Houston, Texas

DATE: 23 October 1990

BOHNING: As I indicated to you in my letter, I'd like to start with some of your early background and then we'll see what our time constraints are.

HACKERMAN: Okay.

BOHNING: Dr. Hackerman, I know you were born on March 2, 1912, in Baltimore. Could you tell me something about your parents and your family background?

HACKERMAN: Both my parents were immigrants. They came separately to this country as kids. I think my father was six years old and my mother was probably older. My mother had some schooling before she came, but my father never had any. I'm told he went to work as an eight-year-old kid, and just worked all of his life. He developed a capacity to read. He was never able to write very much, but he could sign his name. He was a tailor and ended up as a manager or foreman of a tailoring factory in east Baltimore, working for a large company.

I had one sister, and as with many immigrant people, they were insistent that we go to school. Both of us went to college. We were obviously not well-to-do, but they made do. My sister went to Goucher College and got a bachelor's degree there. (This was in Baltimore.) I went to Johns Hopkins. I went into what was known as the "new plan." The "new plan" was a six-year Ph.D. program with no intermediates. I'm jumping a little ahead of the story, but when it became apparent in the midst of the Depression that people without B.A.s were going to be hard to sell to employers, that is with a Ph.D. only, I slowed down and gave them the ten dollars for the diploma and got a B.A. while I was there. So I got both my degrees at Hopkins.

My parents really worked hard to make sure that both my sister and I could go to school. I worked during that time, and she worked during that time, but that was a pittance. We both went to private schools and they were both in Baltimore, so we could live at home. But still it was pretty expensive. As I remember, the tuition was $900 a year, which in 1929 was a major sum. Even riding back and forth on the streetcar for ten cents a ride was a problem, though we managed all right. Hopkins was a good place for me and I think Goucher was a good place for my sister.

BOHNING: I would like to go back to your growing up in Baltimore. Did you go to public schools in grade school and high school?

HACKERMAN: They were all public schools. The principal thing I can remember about my grade school education was what I'd call "domineering women" as teachers. I guess the difference in height makes a difference. But they were good teachers in the sense that they insisted on self-discipline in study. And that certainly didn't hurt me. I enjoyed that. I have a memory of only two of them. One was a third grade teacher whose name I can't remember, and one a fourth grade teacher whose name was Mrs. Meyer. I remember them because while they were both dominant, they were a bad guy/good guy kind of operation. The third grade teacher was a tough lady. The fourth grade teacher was a nice lady. But basically they were instrumental in providing for what I consider to be the important process in education, namely self-learning. Teachers are fine and they do help to guide and they kind of explain a few things; but back then, it seems to me, I got an instinctive feel for the fact that if you're going to learn you have to do it yourself. I think that is quite important. That's all I can remember of the grade school.

The junior high school was an interesting mixed bag because I went to a high school where maybe for the first time they got crowded enough so that they had to build some portables. I guess that's what they still call those temporary shacks. My class, the seventh grade, went into those portables. I remember more about portables than I do about what I learned there, except for one thing. This was in a working class neighborhood over in east Baltimore. In seventh, eighth, and ninth grades each, I had shops. I had tin shop, machine shop, work shop and print shop. It's quite obvious that the kids were being prepared for a job. I'm not all that great with my hands, so I was lucky and didn't fall into the trap of going in that direction. But it did leave several impressions with me. One of them is that it would be nice to be handy. Some of the kids that I was with were quite capable carpenters. Not many of us were good print setters. Besides, that work was on the verge of becoming an obsolete activity. The Mergenthaler machine was already operating pretty well. My best memory of the print shop was the irate Scotchman we had as a teacher. He made no bones about being irritated when we pied the print, which was frequent. But I have no impression of classes at P.S. 47, which was the junior high school. Back then the schools were numbered. P.S. 27 was the grade school and P.S. 47 was the junior high school.

But I have some vivid impressions of my high school, and they're varied. The high school I went to was (and is) called Baltimore City College, and not improperly so. I entered in 1925 and I finished in 1928, so I had three years. Up through that year there were people who went directly from that high school into state medical schools without any intermediate college training. The city had an educational system in which there was Baltimore City College, Baltimore Polytechnic Institute, which was engineering oriented, a girls' high school known as Eastern High School, and a girls' high school known as Western High School. The City College and Polytechnic were male only. Then they had one coed school called Forest Park High School, which was on the far west side of town and the direction in which the city was building. That was a modern school.

City College and Poly were very, very intense tenth through twelfth grade educational systems. They were much more difficult than the current colleges in terms of what they asked you to do and what they expected of you in return for what you heard. For example (now this was 1925-28), I had math through calculus. I had an introductory biology course, a full year of physics, and two years of chemistry including laboratories. I took organic chemistry in high school, and it's clear to me that the reason I stayed in chemistry was because of that place. Predominantly, if a kid--particularly from the east side of town--went on to college he was expected to be a medical doctor. In the yearbook of the year I graduated from City, it says I was going to study chemistry. That was my fourth semester of chemistry. Dr. Ingram and Mr. Robb were my instructors. But I can't remember being pushed in that direction by anyone. We had a laboratory instructor whose name I don't remember, who used to work with us. There were twenty of us or so in the lab. It was sort of one on one, very close attention.

BOHNING: What kind of work were you doing in the lab? Qualitative analysis the first year and synthesis work in organic?

HACKERMAN: Yes, simple syntheses. Separations, purification, I mean, the works. By and large I really didn't have to take it in college.

BOHNING: Do you remember the text you used?

HACKERMAN: No, the high school text I certainly don't.

BOHNING: I was just wondering if it was a college level text.

HACKERMAN: I don't remember. I don't remember the classes as well as I do the labs. But it's clear that must have had some influence on me. I was questioned a lot by my family and my friends, "What is it that you want to do? Do you want to be a pharmacist? Is that the idea?" They didn't understand this business of chemistry. The work at City was hard, but the self-discipline I had learned in grade school stood me in good stead. Were there many dropouts at that time? I haven't the slightest idea. I have an impression that not as many went on to high school. The kids started going to work when they finished the ninth grade, particularly in my side of town. So those who went in, I think, stayed. Most of us stayed. The school moved around a lot during my three years. I had three different sites. They had an old downtown location, and then I was in a temporary location and then in what was known as the "new" one. I went back for my sixtieth anniversary and to be added to their Hall of Fame at the same place which was still "new" to me, so it's still there. It is still a good school, still a highly disciplined school. Poly still does very tough studies in engineering. It is an interesting phenomenon in itself. There are now a couple of dozen high schools in the city, but these two have retained their character. But they are now coed.

BOHNING: When you went there, was it automatic because of where you were living, or did you select it and was there an entrance exam?

HACKERMAN: Both. If you wanted to go into engineering, the best thing to do was to go to Poly. If you wanted to go into medicine, the best thing to do was to go to City. If you wanted to do what I wanted to do, which I didn't know at the time, then City would have been the place. It was the liberal arts type versus the engineering type. I don't think there was a formal entrance exam, but they could somehow deter you. Remember, there weren't a whole lot of choices. There were just three schools I could have gone to. There was the coed school, which would have meant two times as long a streetcar ride, or these two, or go to work. I believe they filtered them out at Poly by very tough early math in the entering year. At City, the math was a little less difficult for the non-engineer types. Physicists nobody knew about, but engineers and pharmacists, that kind of thing. They were profession oriented.

BOHNING: When you went there what did you have in mind? You picked this over Poly, so obviously engineering wasn't something that was important. Did you have anything specific in mind?

HACKERMAN: Probably medicine.

BOHNING: Were your parents influential in that?

HACKERMAN: Yes, not influential in any overt sense, but you could tell that when they talked about doctors, that was the epitome. Doctors were looked on with almost god-like qualities in those days. I don't know if they deserved it, but that was the case! [laughter] So that was the peak of what a person in that condition could do. Business was not an important thing to them. Law wasn't very interesting and wasn't that important. But if you wanted to be professional over in my part of town, you became a dentist or a doctor. Or a pharmacist. Pharmacy was a very honorable position. At that time, pharmacists did a lot of prescribing. If you had a cold, you didn't run to the doctor. You went over to the corner pharmacy and he'd get you something, usually rhinitis pills.

BOHNING: You said that your father did a lot of reading. Had you been doing a lot of reading during this time as well?

HACKERMAN: No, I said my father learned to read.

BOHNING: Oh. Okay.

HACKERMAN: He learned to read, and he read somewhat laboriously. He read the newspapers pretty assiduously. I did a lot of reading, but I think that came from my exposure at P.S. 27. We did a lot of reading in class. There was one mistake that the educational system made on me--at least one. When I got through the third grade, I had very nice handwriting, with carefully written a's and b's. During handwriting class or whatever it was when they practiced it, this Miss Dominger let me sit in the back of the room and read, because my writing was pretty good. I did a lot of reading in third grade, and that sort of instilled the reading habit. But my reputation preceded me, so I didn't do much writing practicing in fourth and fifth and sixth grades. And when I got to the seventh, when we had to start taking notes, I did develop a problem. I had to start writing fast, whereas I had learned to write slowly and carefully. Now my handwriting is terrible. Most people can't read it, including me. I don't close the a's so they look like ce. I don't close the d's so they look like cl. The u's look like n's, and the w's like m's, and so on. So it is a scrawl, and you have to know the gist of what I'm writing about to get any idea. A few of the ladies that have worked for me can read it better than I can, but most people can't read it. That I have to ascribe to the third grade, where I really should have had some handwriting lessons.

BOHNING: How was the decision made to go to Hopkins? I'm assuming now you've got chemistry fixed as what you wanted to do.

HACKERMAN: I guess I decided to go into chemistry in December of my senior year. It was early enough so I could get that into the yearbook, so it would have to be at least by that time. The choice of where to go was fairly limited. It would have to be someplace in Baltimore or close by. I couldn't afford to live on campus. There was Loyola College, which is a Catholic college and fairly difficult to get into except through an affiliation with the church. The University of Maryland was a long way off, closer to Washington than to Baltimore. One would have to take a local B&O train to get to it.

I can't even remember if there was any question as to whether I would be admitted to Hopkins. I had to fill out the usual papers, but my record at City was good. I think I applied only to Hopkins. They took me, but I couldn't get a scholarship because they were awfully hard to get. By the time I entered there in the fall of 1928 it was beginning to show some strains that preceded the big stock market crash. The trustees had invested heavily in the B&O Railroad. The chairman of the board was president of the B&O. The B&O Railroad was already showing signs of pointing toward bankruptcy. By the end of my first year there, when the stock market had not yet broken, I don't remember anybody who was on scholarship in the freshman and sophomore class. It doesn't mean there weren't any, but I don't remember any. Then by October of that year, there was just no question that the university couldn't help because it was busted. In fact, the faculty wasn't getting paid.

BOHNING: And that's before the Depression?

HACKERMAN: No. It was after it started. But Hopkins had already begun to show signs of unwisely invested money before the Depression. By the way, we weren't hit by the Depression as badly as many because my father kept his job. But his little savings in S&L got wiped out. I haven't the slightest idea how they scraped the money together for the next year. I was working. I sold women's shoes, and that took care of the sort of operating expenses, but the $900 for tuition was a major problem. They may have borrowed it, that's one possibility. The following year it was the same, and in my last year I had a little relief. I don't remember whether it was a scholarship or some kind of teaching assistantship as a senior, but I got some help. I finished the Ph.D. in three years. So the whole shebang took seven years, instead of the six I told you the "new plan" had called for. I believe that my parents paid all or most of the tuition each of those seven years and I took care of most of the rest of it.

BOHNING: Was there any overlap with your sister being in school at the same time?

HACKERMAN: Yes, that was a problem. She was nine years younger than me. Maybe the last year of my time was the first year of her time. The pressure wasn't over in 1934 or 1935, but we probably got used to it. We probably learned how to cope with it. And things seemed to be a little easier at that time. For one thing, in 1929, 1930, and 1931, you really couldn't tell if you'd hold your job. By the time 1932 and 1933 came around, if you were still in there, then it was pretty stable. So you could borrow money and give it back. By 1935 it had hit rock bottom and those who were working were going to continue working if their health held up. So you didn't have the same degree of discomfort. But when I got out in 1935, there weren't any jobs. I applied for an instructorship at Ohio State. There were a thousand applications. O. [Oscar] K. Rice was one of them. At that time O. K. Rice was already well established. He didn't get the job, incidentally. I guess he wanted too much or something.

BOHNING: I'd like to go back and talk about your experiences at Hopkins and sort of work our way through those seven years. You had this extensive chemistry background before you got there. Were you able to skip some chemistry classes, or did you start right in with the normal sequence?

HACKERMAN: No, I didn't. In fact, whether it was because I had that chemistry in high school, or whether it was because Hopkins had a policy of not letting you start if you were majoring in chemistry in the first year, I'm not really certain. But I didn't take any chemistry in my first year at all. I took some languages, I took philosophy, I took a course in psychology. I took no physical science or natural science at all. Philosophy, psychology, English, Italian, and something like political science. I took Italian because the instructor was interesting. I took philosophy because I had the chance to listen to George Boaz, who I'd already heard of. I took psychology because I was interested. In my second year, I took physics, chemistry, geology, and math--and none of the other stuff.

[END OF TAPE, SIDE 1]

HACKERMAN: I wasn't well advised about the physics course. I should have had the math the first year. I was taking differential equations and physics at the same time, whereas it would have been a lot better had I had differential equations behind me. We had nine hours of freshman chemistry laboratory, eight hours of physics laboratory, and one afternoon was geology laboratory. So it was a pretty husky schedule. G. H. Cartledge gave freshman chemistry. There were four lectures and nine hours of lab. I would guess that all the chemistry I knew by the time I got a Ph.D. I got that first year. The perimeter was certainly marked out.

BOHNING: Do you remember what text was used?

HACKERMAN: I actually have it. It was Alexander Smith's textbook (1). It was a lot smaller than the current text. They used to do it by the column of the periodic table. Reading the text was simple and straightforward, and there were no great problems. I got a great deal out of Cartledge's lectures. He prepared them carefully and they were sort of fully developed. When I knew him in that class, I always thought of him as a dour individual. I got to know him much better when he was at Oak Ridge later and I was at Texas. In fact, we worked together on some problems. But he was very straightforward and very clear and he generally clarified the questions I had out of the text without being asked. So he saw the same questions the class did.

It was a class of about a hundred. They were all pretty good students. So the distribution wasn't like that [curve], the distribution was more like this [peak]. And he knew that. He'd been there a long time. As a matter of fact he produced his words at a level that got us all interested at the same time. It was fast moving, thorough, and you could be reasonably sure that he was right in the light of that day in what he said. So it moved very rapidly. I have to tell you, four classes versus the current three and nine hours of lab versus the current three makes a big difference. You were prepared almost at the end of that year to be a chemist.

BOHNING: Were there many chemistry majors in your group?

HACKERMAN: No. I would make a guess that there were in that hundred a maximum of five or six. Most of them were premeds, with a smattering of other groups like geologists. Eighty percent of them were medical. The laboratories were adequate. The stuff that you had to work with was all right, but it wasn't outstanding. The first semester was a series of experiments, such as densities and reactions. The second semester was qual. That was tough. That was very tough.

BOHNING: Especially with nine hours a week.

HACKERMAN: Yes. We had lots of unknowns. For the first group I guess we got a couple of unknowns to sort of get used to the system. Then we got group IIA and IIB separately. They were tough. They were not the easiest thing in the world to work on. There was lots of H_2S in there. The Kipp generators were operating under the hoods, but the hoods were not working particularly well. There was no compromise. You turned in your report, and the guy looked it up, and put a check or an X or something. I forget what the grades were like. And if you didn't get it right, you went back and did it again. The second time you got a grade, because you couldn't stay there forever doing IIA. And you got the feeling that you knew what you were doing. Group II, that was a tough group. Then you got group III, which was tough, because the K_{sp}s were not as different as the ones in group II. Group IV was all right. Then you got to the alkali metals, and they were stinkers. Then we did anions, but anions were not difficult because we did spot testing. Then we would get the general unknown--"tell me what's in it."

BOHNING: In view of what you've been saying, and since qualitative analysis has rapidly been disappearing from the chemistry curriculum, how do you feel about that in light of your experience with it years ago?

HACKERMAN: I think that the trend away from laboratories is wrong, because it's still a laboratory science. If it involves the use of computers and the computer in the laboratory then it ought to be carried out. If you look on qual in itself as teaching you chemistry, that's not likely to happen except in a very limited fashion. If you look on qual as a means of instilling into you the need for careful work and thoughtful arrangement of what you're going to do, then I think it's very important. Certainly for the person who's going to be a chemist or a chemical engineer. And it probably is a value to the other people too, in the sense of organizing what you're doing, and just learning how to do things. One can say quantitative analysis is more of a disciplined activity, but I didn't see it that way. I thought the other was. Quantitative analysis by that time was sort of semipro, at least. I never felt that was a great shucks in molding chemical activity. But I think qual is pretty important. They really ought to go back to it, because you can do it without Kipp generators now. [laughter] In fact, what they ought to do is have students devise their own schemes based on some kind of program you can stick in a computer and say, "Here are the things we want to look for and here are the characteristics of those things. Devise a scheme." I think that could be done. That would be a nice creative kind of thing. But the tendency to low-rate the lab and get rid of it I think is wrong, even for the non-major. We'll come to that when I talk sometime later about science policy.

BOHNING: Yes, I just wanted to bring in that part about qualitative since we were talking about it. Were you discouraged at all by this extensive involvement, or were you encouraged to continue as a chemistry major?

HACKERMAN: Well, I was never discouraged, but there are two things, and this is the way it should be in college. I had my first taste of the geological sciences, my first real taste of philosophy, and my first insight into psychology. These were fields that I had no knowledge about before, and I really got very interested in all of them. I took three years of philosophy, all with Boaz and his assistants. Part of one of those years was a course in logic, which I've always thought was not only interesting but useful. I took a couple years of psychology, because back in the late 1920s and early 1930s, that was a mumbo jumbo area. [Sigmund] Freud was known as a name. I had a guy named [Knight] Dunlap, who was very highly thought of as a psychologist, I found out later. In fact, his name still appears sixty years later. He was not a very inspiring lecturer, but for some reason what he said attracted me. I still have a kind of peripheral interest. I serve currently on the advisory committee for the Center for Advanced Study in the Behavioral Sciences located near Stanford University. I've always had an interest in that. I've always had an interest in geology, and I've always had an interest in philosophy. But when the chips were down, roughly in my junior year, I stayed in chemistry just possibly because where would you get a job in these other three areas? [laughter] It wasn't too clear where you would get a job in chemistry either, but it was better than the others. So the answer is that I was never discouraged about chemistry but I suddenly saw other things I didn't see. I never really got an interest in engineering, per se. I never got an interest in medical school. That could have happened. With all my friends around me rushing off to medical school I might have just aspired in the same direction.

I took organic chemistry with E. Emmet Reid. Reid was a famous name at the time. He was a terrible lecturer. [laughter] He mumbled and talked to the board. It was just hard to understand what he was saying. We used his book (2). Organic never really impressed me because at that time it was mostly that you knew the name reactions. But then I had a course with F. O. Rice, and I would say that Rice was certainly a prototypical physical organic chemist. I took undergraduate organic from Rice. That was quite different. I was thinking about that last night when we gave the Welch prize to [William von Eggers] Doering and [John D.] Roberts. Rice preceded both of those guys and was not in as good a position to be recognized. He was a free radical man when free radicals were just [Moses] Gomberg's free radical triphenyl methane. He delved into photo processes and he looked at energy flows. That's very peculiar for a 1930s organic chemist. So I got a different insight but not enough to want to go into the field.

I never took a literal course in physical chemistry. I had a laboratory and a lot of separated courses like x-rays, but never an actual physical chemistry course. I believe that my intention had always been from the time I was taking these high school courses to be a physical chemist. Inorganic, by the way, was not much of a thing. There was no real coherent feel. You were either an organic chemist or an unorganic chemist. [laughter] And I decided on the physical.

BOHNING: What was it that attracted you to that area?

HACKERMAN: Well, I was reasonable in mathematics, but not great. That probably kept me from being a physicist. I never really thought about being a physicist, but if I wanted to be I'd have had to be a little more facile in mathematics for that. I was interested in materials more than forces, but I was interested in the characteristics of the materials rather than production of the materials. So that sort of points you in that direction. Actually, I had a very early interest in colloids and surface chemistry, which is peculiar because colloids was really on the way out. I didn't recognize it in the 1930s. As a senior at Hopkins, and because I had this "new plan" arrangement, I didn't take a formal physical chemistry course but I did take as a senior lots of graduate courses. I had Joe [Joseph E.] Mayer teaching quantum mechanics and statistical mechanics. This was 1932. Joe Mayer had just come back from Göttingen, where he had either been on a postdoc or instructing or something like that. He was a young guy, but recognizable as a real comer. His wife ultimately got a Nobel Prize years later. He was in the forefront of a different kind of chemistry. [Linus] Pauling had already written his new freshman book in the early 1930s and approached chemistry quite differently (3). I saw the Pauling book some years later. Of course, I taught. I was a teaching assistant, although they didn't call them that then. When I was a senior I had Mayer in quantum and statistical mechanics. No textbook. We had mimeographed notes, which I still have someplace. They're hard to read. I took x-ray crystallography with Emil Ott. The physical chemistry laboratory was a series of experiments under the guidance of A. B. F. Duncan that you did yourself. That is, they weren't the textbook experiments, but for example catalytic decomposition of ammonia on platinum surfaces, where you got experience with doing vacuum work, you got experience with catalysts, and you got experience with following rates and determining orders of reaction. So the last formal undergraduate course I had was the organic course that I took from Rice. And looking back, two undergraduate courses are all that I had at college--freshman and organic. I never took an analytical course. I never took a formal physical course.

Hopkins was a very good place to be at the time. You worked yourself through the educational process and then you demonstrated that you knew something. As much as they wanted you to know. But as I said, in order to be formally correct, I did plunk down ten bucks for an A.B. diploma in 1932. And I'd already done basically all of the course work for the Ph.D. I started working in the laboratory with W. A. Patrick. Patrick was the guy who had the silica gel patents, which I think he handed off to the Davidson Chemical Company. Davidson made silica gel for many years. They were the main purveyors of it. I started the work in Patrick's lab my first year, right in my transition from getting the bachelor's degree, and took only a few formal courses sort of now and then, but nothing too formal. A course using Noyes and Sherrill (4). A course in thermodynamics with Donald H. Andrews. I had an organic course with Reid which is where I learned that I couldn't understand what the hell he was talking about. That's all, I guess. From that point on, it was all laboratory work. At Hopkins at that time, what you did was go around and pick four laboratories and work in them for a couple months each for the first year. It was on the basis of that that you decided where you wanted to do the work for your Ph.D. dissertation.

BOHNING: I was going to ask you how you selected Patrick. What were the other laboratories that you spent some time in?

HACKERMAN: Well, I went into the x-ray crystallographic laboratory. I went into Reid's laboratory. I went into the laboratory of a guy named [Joseph C. W.] Frazer, who was a catalyst chemist. And I went into Patrick's. Reid was writing a book on sulfur chemistry, and in his laboratory I prepared a lot of thioethers.

BOHNING: Really.

HACKERMAN: Boy, I stunk! [laughter] I could get on a streetcar and get a seat with no trouble. I did that for several months. One lab accident I had was in Reid's lab. I was doing a Grignard, and I can't remember why I was doing it, but I was adding formaldehyde to a Grignard reagent. This was early enough in the game that we used soft glass. I can remember the paraformaldehyde was in a three-necked flask. Out of one of the necks was a U-tube. It wasn't actually a U-tube, it was two pieces of tubing with holes blown in and a cross piece fused into those holes. This was soft glass, so it was tenuous. The horizontal piece was wrapped with nichrome just out in the open with no asbestos, nothing on it, with battery clips holding it to wires that came out of the plug. I guess it had a rheostat so you could adjust it. At any rate, I had the paraformaldehyde being heated, and we weren't heating it over an electric system, we were heating it over gas. I had rubber stoppers on the tops of these vertical tubes, and the damn thing plugged. It had sublimed and got caught in the middle someplace, and nothing was happening in the Grignard where the stirrer was. I went to look over the top to see where the plug was, and the thing blew out. I caught this formaldehyde up my nose. I mean, it was a heavy dose. I had a devil of a time catching my breath. I hung out the window about an hour. But it selectively damaged my odor sensors. For example, I can't detect house gas. I can't smell skunks. But some things, like certain perfumes, are accentuated. I sometimes get in a cab with a cab driver that's got a little bit of something on, and I have to get out. [laughter] But I'm fortunate, that was the extent of my accident. It also decided me against being an organic chemist. [laughter] I think that was the straw.

I did the x-ray lab. That wasn't bad. Back then, in the x-ray labs, you packed the collodion tube with powder and put it down in the middle of this circular camera. We actually had an ion tube rather than an electron tube. You had to stopcock it, you had to crack it properly in order to maintain a proper vacuum. You also had to center this camera on the x-ray tube. You'd do that by reaching around the lead shield that sat there and you'd sort of fiddle with it to get it lined up. You had a zinc sulfide phosphor sitting back here and you'd look to get the bright spot straight. Well, just the ends of these two (little) fingers began to show burns. You had x-ray scatter all the time. I didn't worry about it for a while. But they began to show burns and I decided I wasn't going to do that one. So that got rid of two of the possibilities. [laughter]

[END OF TAPE, SIDE 2]

HACKERMAN: The catalyst lab was all right, but it was really very rote. Frazer had been instrumental in producing Hopcalite. Hopcalite was a mixed chromium, manganese, cobalt, maybe iron, four or five oxides used in the coal mines for the CO problem. It oxidized CO to CO_2. But it seemed like an awfully routine kind of thing. You prepared a lot of stuff, you precipitated all these things, and coprecipitated them, dried them, did a standard catalytic oxidation, and went back and did it again. So I didn't particularly like that.

But in Patrick's lab, first we prepared silica gel and then I got interested in a couple of things. One of them was to measure the zeta potential of the silica gel, for which I devised a scheme of trying to set the gel around a silver-silver chloride electrode, which was a nice trick in itself. I did a lot of work on that. The electrode was such that you could move water through the gel and could measure the zeta potential by that rate of motion. I decided to do it in an electrophoresis U-tube, in which you suspended the silica gel and watched the motion. It was an interesting thing. In fact, I had pretty much completed it when Patrick said he had an interesting problem he wondered if I'd do. It was to try to determine the molecular composition of sulfur monochloride dissolved in various solvents. Did you ever work with sulfur monochloride?

BOHNING: No, but I read your paper (5).

HACKERMAN: [laughter] Man, that's terrible. That's terrible stuff. If I knew before what I knew after, I never would have done it. I did it by the Cottrell method. I had two Cottrell flasks and just did the differential temperature measurement, one with the boiling solvent and one with the solution. From the boiling point rise and the nominal composition of the solution, you could determine how much polymerization there was in the solution. Patrick had been interested in it because he thought he had a way of refining gasoline using sulfur monochloride. This information was interesting but it turned out you just couldn't handle large quantities of S_2Cl_2. Even then, I knew enough to wear goggles because if that stuff got in your eyes some kind of protein coagulation took place and that was the end of your eye. But it was an interesting piece of work because I had to work with somewhat more elaborate apparatus than I had been up to that time. I had a suspension which permitted me to put a galvanometer inside of an air bath inside a water bath so I could maintain constant temperature. In other words, the basic experiment was a very precise measurement of a delta T. By the time December 1934 rolled around, I was farther along on this one than I was on the silica gel problem, so I used it for the dissertation. But that really was not my interest. I went back to surface chemistry later.

BOHNING: You never published anything on the silica gel.

HACKERMAN: No. That didn't get far enough for publication. There's something else about that. I remember that was about the time that the microelectrophoresis tubes were designed, the kind where you put a single particle on a microscope stage and follow the movement of that particle under a potential gradient. That seemed to me to be eminently more suitable than this gross method I used which was a U-tube and a suspension. I thought that the new method was farther along so I just didn't get back to the problem.

I stayed around Hopkins for a year after 1935. In fact, even guys like Joe Mayer had to look for ways to make enough money to stay viable during the Depression. He took a freshman teaching job at Hopkins. He didn't know any freshman chemistry. He was a very bright guy. Quantum mechanics he knew. But he didn't know about precipitating iron sulfide or how you determine chromium or any of that kind of stuff. So he asked me to be his teaching assistant (or whatever they called them) in the laboratory. I ran his laboratory and stayed around Hopkins for an additional year, and kept my research lab. I got interested in the oxygen electrode. If I knew how I got interested in it, I'd tell you, but I don't. But it annoyed me that it was irreversible. I sort of decided I was going to find out why. Now it's fifty-four years later and I still don't know why. At any rate, I spent the year trying to develop a reversible oxygen electrode and did some interesting stuff. That's why I got interested in electrochemistry. I did some interesting work, and indeed, if you neglect the difference in temperature between my room temperature system and the 1000° C. fuel cells of today, I was doing the same kind of work back in 1936 and didn't know it. That's the kind of anode they use now. I used a very pure graphite rod. I don't remember the solution anymore, but I was trying to get an oxygen electrode which would approach reversibility if I could change the temperature. I didn't know whether it would go high or low, so I scanned it. That's what I did during that year, and that was not publishable either. But I did get interested in the oxygen electrode then, and that gave me the interest in the fuel cell later on.

BOHNING: You said at the time jobs were very hard to find. Before we go to that, how was Hopkins faring through this part of the Depression?

HACKERMAN: Well, they were bankrupt. Not in the current sense, but they couldn't pay their bills in 1932 and 1933. I guess people didn't know what you did with a university that couldn't pay its bills, so they probably carried them. The faculty wasn't getting paid either, and some of the very good ones had to leave. Hopkins had a first-rate chemistry department. When I came there as an undergraduate, [Harold C.] Urey was there. This fellow [Emil] Ott went on to become the director of research at Hercules in Wilmington. [Paul] Emmett was there. Emmett's the guy with [Stephen] Brunauer and [Edward] Teller [in the BET isotherm]. He was there, and he had to leave. He came back, but he had to leave at that time. Maurice Huggins was there. I think he went up to Eastman Kodak in Rochester. Dave Harkins was an outstanding x-ray crystallographer who went to G.E. A whole lot of first-rate people had to go. They just couldn't afford to hold onto them. Guys like Reid stayed; he was actually retired sometime in that general period. Patrick stayed. He apparently was independently well off from his silica gel patents. Frazer stayed because he was going to retire in a few years. There was a guy named [William M.] Thornton there. Neil Gordon of the Gordon Conferences was there, but he went off about 1934 or 1935 to Central College. Now, I don't know about the other departments, but I'm sure they had the same kind of problems.

The university had a very tough time. It had to exist on its cash flow and its endowment, which was not too functional. But it managed to get along. It's hard to see how it did it, seeing how it works now. If you wanted to do an experiment, you had to root around the place to find the stuff to do it with. You didn't go to a catalog. I think somebody was saying yesterday [at the Welch Conference] they had two hundred dollars. Two hundred dollars was hard to come by. We didn't have two hundred dollars. I can remember making my own mechanical relays. You had to get a wooden spool that came from cotton thread for sewing and find an old rheostat and take that wire and wind it on this spool. Then you had to get a piece of soft iron, etc. I had to make the whole business.

BOHNING: What about chemicals?

HACKERMAN: It's a little hard for me to remember, but I'm sure all the chemicals we used came out of the central storeroom. We probably used to put spatulas into the bottle and get out what we needed. In my case the chemicals were fairly simple except when I was doing the sulfur chemistry for Reid. He supplied it, but I don't know how he got it. In my own lab, basically distilled water, and salts or some acids or something like that. It wasn't a great problem. At least I don't remember it as a problem. But the equipment I do remember as a problem. I remember spending weeks building thermostats, finding a box, making sure the insulation was good in it, then rooting around the cellar and getting a stirrer, and finding a bracket so I could put the stirrer on. I made my own thermoregulator. You would get your glass and a bulb. Then you'd go looking around for capillary tubing and then put it under a microscope and hope that you'd have a uniform tube. You would seal that on top, find a thimble or something that you could use as a cap, and put a tungsten wire into it. The regulator was obviously the expansion of mercury. Then you calibrated that against a reasonably good thermometer. They had good thermometers, I'll say that. Precise thermometers. So you'd spend literally ten days building this thing, and that's the reason it took a year to do something at the time. Now, of course, you buy it, and it takes six weeks to get it from the supplier. [laughter]

BOHNING: Earlier you said you had applied to Ohio State and found there were a thousand others also applying. What about industrial positions? They were probably scarce too.

HACKERMAN: They were very scarce. There was a fairly good connection with Du Pont. They would send people down from Wilmington, but I never got an offer from them. I think there were probably one or two other industrial outfits. In 1934 there were about twenty to twenty-five Ph.D.s at Hopkins. Nobody in that group got a job. I can well remember seeing them in one of the larger laboratories in the building on a WPA project. The WPA project was to make Hopcalite as many different ways as they could. You know how these desks are. There would be three guys at this desk, three at this one, and so on. There were about twenty of them. They had these large ungraduated cylinders about that big [three feet] and maybe three inches in diameter, and they'd have a long stirring rod. Every one of these guys would be stirring this stuff, and it would be purple and green and blue, precipitating out in different forms. None of them had jobs.

The year I graduated there were about twenty of us. Hopkins had a good plan. This "new plan" not only involved people locally, but anybody who could be admitted could finish in six years. They came from all over the country because somebody, I think it might have been Gordon, had developed a scheme which involved getting industry to put up money for fellowships which were geographical. That means there would be one from the Northeast, one from Utah, one from down here, for example. The group that got together to get Ph.D.s in 1935, the ones I went with (most of them took four years, I took three) were from all over the country, literally. When we finished, most of those guys disappeared. They wanted to go back to the regions they came from. Of the half a dozen of us or so who were local, none of them got jobs except me.

I got a job at Loyola teaching the physical chemistry laboratories and I had this job with Joe Mayer I was telling you about. Between the two of them, I guess I was making $1300 or $1400 a year, something like that. I had already started doing a little consulting. That's kind of interesting, and that just occurred to me. Somebody came to Patrick and said they'd like him to do a particle size distribution and a characterization on a clay which they thought was bentonite. I presume that meant they had found a deposit and they wanted to know whether it was worth trying to mine. Patrick, as I said, was at least well off if not more. He turned to me and asked me if I wanted to do it. Since I wasn't doing anything I said, "Yes," which meant I had to learn how to do particle size distribution.

You've got a settling tube, which was a vertical tube with an almost V-shaped piece that came off at the bottom, was bent and then had a nearly horizontal segment to it. The by-pass tube came off a small distance from the bottom. You put the suspension in the large tube and it supported a column which you amplified by this nearly horizontal piece. As that stuff settled out, its density decreased and the meniscus moved back and you could read the rate at which it moved and from that you could do a settling curve. So I did that. I guess it took an hour to make the tube and set it up and another hour to do the experiment. I did it a couple of times. I came back to see Patrick later that day and said, "Here, I've got the settling distribution curve." He said, "That's fine. Send it to so-and-so and send them a bill along with it." I said, "Good, I'll make $25." He said, "No. If you do that, they're going to think it's not worth anything because you're not charging them very much. Charge them $200." So I did and they wrote back, thanked me for the information, and sent me the check. I learned a very valuable lesson from that--don't underrate the value! [laughter]

I think because of that he sent someone to me again. This was a man who currently would be called a venture capitalist. That is, he himself was not technically oriented. He had some money and he went around looking for things that he could invest in. He was a guy who had invested in what was then called "no belt," this elastic piece that they have in slacks. This guy had already done that, so he had made some money. It was about 1936. What he had was a device in which suspensions or emulsions could be produced that were fairly stable. He was interested in using this on a commercial scale, but he didn't quite understand what he was talking about. His name was Mr. Harry Hardy. After Patrick had referred him he came to me to find out what this stuff might be good for. Well, it occurred to me that there were a number of things. For example, one of the things at that time in the grocery was that after a salad dressing, like a French dressing, was placed on the shelves it separated in a very short period of time. That was undesirable. Even if you tried to disguise it with a color, you'd still see it. Also at that time, before your time probably, when you got a bottle of milk you could see the cream layer on the top.

So I said to him, "Look, it seems to me that if one could homogenize that milk, you'd form a material that would be easier to sell." Well, that was fine, that's what he wanted to know. So I came back later and he had undertaken the deal with whoever this inventor was and asked if I would help develop the use of this machine and that he would pay me $35 a week and give me some stock. I can't remember how much stock was involved. One of my friends there, who got his Ph.D. the same time I did, was already married and already had a child. All the rest of us were unmarried. I thought this guy deserved some help, so we split the job, he and I. We each got $17.50 and we each got half of whatever the stock was.

The system was a hell of a system. There was an electromagnet which operated a heavy piece of steel about 3600 times a minute in vertical oscillation. It was a hardened piece of steel which was kind of an electromagnetic hammer. Into the hole of that hammer you'd put another rod, and that rod would have on its bottom some plates. The plates were made of chrome nickel. You could vary the thickness of the plates, but you couldn't vary the diameter except to make it smaller. You couldn't make them bigger because of the container diameter. They were fitted on the rod and tightened. The point was that this was a high speed churn, for one thing, but also you got the natural frequency vibration in the nickel plates. Finally, also because you could make the clearance pretty close, you've got this narrow gap right at the edge which provided a shearing effect. All three of those things did the same thing. That is, they helped you disperse material. So this other fellow and I took the basic design and built the glass system and began to make emulsions. There was a horrendous noise in a room this size with that hammer going. We learned early to wear ear plugs. We made good homogenized milk right away. We finished this about 1937, and we told Hardy we had the stuff he wanted. The problem was the Submarine Signal Company about that time was also looking for homogenized milk. They developed a system which acoustically disturbed a plate of nickel and ran the milk down over it and it was a lot easier than our system. So we lost to them. And homogenized milk began to appear in 1937 or 1938, not before that.

I told you about the salad dressing. Well, it occurred to me that the way to suspend the oil and vinegar was to put in carbon black as an emulsifying agent. We made a very stable oil and vinegar emulsion with this material. Hardy was all excited and I was all excited and he took it home to his wife and she said she wouldn't use a black salad dressing! [laughter] It was black, but stable. I tried to point out to her that the carbon black would probably do the digestive processes some good. But we lost that one too. Anyway, we lost them all, finally. For example, we did bismuth subsalicylate, which was used for treatments of syphilis. These are needle-like crystals which were injected into the patient. They'd have terrible pains where the large particles collected. Well, we broke that stuff up pretty good, and had it developed. The people at the Hopkins hospital were very interested in it because it certainly diminished the pain problem. But they developed a better way of treating syphilis. So we were behind the curve all the way and finally folded about 1939. I held the stock for ten years and took it off in income tax.

[END OF TAPE, SIDE 3]

BOHNING: Is that what was called the Colloid Corporation?

HACKERMAN: That was the Colloid Corporation.

BOHNING: So you were involved with this, and you were also teaching at Loyola.

HACKERMAN: And looking for another job.

BOHNING: At Loyola was it just freshman chemistry?

HACKERMAN: No, I just did physical chemistry laboratory. There were eight or ten or twelve people. It was a very nice size. I enjoyed it. They were pretty good students. I was often amazed at the groaning board that they set up for the faculty with drinks and everything to go with it. It was purely stopgap. You had to be a Jesuit to be on the faculty. In fact, the guy who taught the course was a Jesuit priest, Father Schmidt. Obviously that was a dead end for me.

BOHNING: Through all this time were you still actively looking for another position?

HACKERMAN: Oh, yes. I was not married at the time. I didn't get married until 1940, but the lady who was to be my wife would type letters for me constantly. I sent four or five hundred letters worldwide to universities, even to the University of Witwatersrand in South Africa, where I got turned down too. [laughter] Nothing but turn-downs. The ACS started its employment clearinghouse somewhere in that general period, about the second part of the 1930s, because there were a lot of people looking for jobs. That employment clearinghouse was a pretty good deal. They also had lots of ads that I would follow.

I spent time in Washington, which is only forty miles away. "Only" is not the proper adjective, but it was forty miles away. I was particularly interested in trying to get into one of the regional laboratories of the Department of Agriculture, which was set up by an act in about 1936 or 1937. There were four labs around the country--Albany, Oregon, someplace in Louisiana, and someplace in Illinois. They were to do basic research as well as more applied agricultural research. So I went over there and camped out in the Department of Agriculture a lot trying to get a job in one of these places. Some of my friends who had graduated with me had finally gotten what were called P1s by the government. Professional Grade 1. They were shrimp inspectors in Biloxi, Mississippi. Three of them went down there. I think the pay was $2600 a year. A couple others had gotten jobs with paint companies in Baltimore. There were half a dozen paint companies and they became chemists, doing analyses on incoming materials and demonstrating how well they were putting stuff together. I went over there and tried to get one of those DA jobs. I struck out. I didn't have agricultural experience or they didn't want physical chemists. Whatever it was, I didn't get it.

But while I was there I learned other procedures. They gave tests for these professional jobs on occasion, and I would take them. You took them to get on the list. Then if somebody wanted a P1 or P2 or whatever, they'd look at that list and then write you if they wanted you. So I got an invitation from the U.S. Coast Guard on a P1 job at Staten Island. The Coast Guard had just taken over the Federal Lighthouse Service that handled lightships and did buoys and things in the various waterways. The Coast Guard was just getting jurisdiction for that in about 1939. The Lighthouse Service had a laboratory at St. George on Staten Island. St. George is where the Staten Island ferry comes in. Right alongside that ferry slip is where this lighthouse laboratory was. There was a dock and there were buoys on it and ports for lightships and lighthouses and a two-story building. The top story was a chemistry laboratory, cunningly hidden, so to speak. I went there and took that job. It was $2000 a year, and a P1 rating. My job was to do a bunch of analyses as needed. There was another fellow already there by the name of Ryel Bailey, who was a B.S. chemist out of Iowa and who had been in the federal service. How he got to this place, I don't know, but he had been in federal service most of his life. I thought he was a pretty old guy. He was maybe forty-five, but I was twenty-five. We did analysis on brass, on hemp, on paint--white paint, gray deck paint, all kinds of paint--and other things of that general nature.

The interesting thing was that the Coast Guard had never had a laboratory before. I can remember a paint sample coming in from some company, and I took a sample out of it and did the various analyses. You swing off the pigment to see how much pigment is in there and you analyze the pigment to see how much barium sulfate there is and how much calcium carbonate and so forth. Then you do the vehicle. At that time they were just getting into synthetic vehicles, contrasted with linseed oil. I finished the analysis and wrote on the report that this stuff didn't come up to specification. I gave it to Bailey, and he sent it to the Coast Guard captain over in Brooklyn, where the offices were. He gave it to the company, and the company looked surprised, apparently, took the shipment back, and about three days later brought another one in. They sent it to me, and it was the same damn stuff. All they did was move it off the site. And I rejected it. This time the captain and some irate guy from the paint company came over to see us, and the irate guy became un-irate when he saw we had a laboratory. He didn't know there was a lab testing this stuff, because it had just moved over from the Lighthouse Service. The next time he sent in a good sample, because apparently three times and out, and you have to pay triple damages or something. So they sent in a good sample.

At any rate, this was an analytical laboratory. It was interesting for a few months, because he learned some analyses and I learned some physical tests on wire and rope and that kind of thing. But it certainly wasn't what I wanted to do.

BOHNING: How did you get the position at VPI [Virginia Polytechnic Institute]?

HACKERMAN: Let me see, how did I get the VPI job? I went from the Coast Guard to VPI. And I got the VPI job in the fall of 1941.

BOHNING: Before the war started?

HACKERMAN: Yes, before the war started. The VPI thing was like this. I went to an ACS clearinghouse and got the job at VPI. I got married in August of 1940. I'd been working at the Coast Guard from the year before. I stayed at the Coast Guard until the end of the summer of 1941. So I was at the Coast Guard about a year and three quarters. Half of that time I was unmarried, living in a boarding house up in St. George. When I got married we lived in a place called Richmond, on Staten Island. My wife worked at Columbia. I got this VPI job through the clearinghouse and we went down there in September 1941. The job was an assistant professorship in chemistry. My principle duties would be to do the physical chemistry laboratory under a fellow named [Philip] Scherer who was lecturing, and as much freshman chemistry as they needed done, whatever semester it was. I took it without too much consideration because I wanted to get out of this routine analytical job. I had done each of the analyses once and that was enough.

Blacksburg was a very nice place. It's a very small town. VPI was one of the essentially military colleges, with required ROTC. One of the first things that bothered me was when I talked to the department chairman, Dr. [John W.] Watson. I said, "What are the chances of getting a little bit of money to do some research?" He said, "In this state the research is done in Charlottesville." I hadn't been smart enough to ask that question before. As it turned out, there was research going on there. Scherer was doing some research in both natural and artificial filaments. When I asked about that I was told that was different. He had a deal. I found a kindred spirit on the faculty, a fellow named Jack Addlestone, who was an associate professor. (I came as an assistant professor.) He had tenure. He said that he did research there, but nobody knew about it. He did it up on the top floor where nobody watched him, and he said that I was welcome to come up and use the lab. It turned out we were both interested in sort of the same thing. I got interested in corrosion at the Coast Guard and he had some deterioration interests, whether metals or nonmetals. So we did a little bit of work together.

The war broke out three months later, and the nature of the place changed. For example, while I was still doing the physical chemistry lab, I was also doing at least three freshman lecture courses. Then, toward the end of the 1941-1942 academic year, the army had developed a program known as the ASTP (Army Specialized Training Program). The Army Specialized Training Program was to identify soldiers who could gain by being more highly educated and in turn the army could gain by using their increased qualifications. They sent us eight soldiers from different places, and one of the things I was to do was to work with these eight guys. They had varying degrees of experience in chemistry and they were all college types. That is, they either were going or had been going or could have gone to college. I spent a lot of time that first year finding courses for them and developing activities for them. Incidentally, of those eight I think five ultimately got Ph.D.s after everything was over.

That was a very interesting period, but the following year (that's 1942-1943), the company commanders had learned that here was a place they could get rid of their malcontents. So instead of sending the good guys, they sent the bad guys. And the bad guys came to us in large numbers. I now had to give a two-hour freshman chemistry course seven times during the week. Fourteen hours of those classes. And these were not guys who were interested. They were pretty restless fellows. So that was not very nice. I didn't really appreciate that. Obviously, there was no time to do anything else. Research was clearly out.

So I enlisted in the navy and was offered a commission as an ensign. I went up to Baltimore and took the physical exam, passed that all right, and received orders to report to Cornell to gunnery school, which I thought was pretty peculiar. I'd never fired a gun in my life--rifle or pistol or anything else. I forget what the date was, but then I got a call from a guy named Zola Deutsch in New York who told me that I was to report to the Woolworth Building for non-navy duty instead of going to Cornell. I haven't the slightest idea how they knew about me or anything else. I went up there, and it was to work on the Manhattan District Project. I guess this was late 1943. I was to work with Deutsch and a guy named [Al] Lunnum, and a woman, Mrs. Kelly. She was a secretary. We were the materials group of the Manhattan District Project, specifically for the K-25 portion. It was my job to go to various laboratories, primarily in and around New York but also further out, establish some contacts, watch what was going on, and not tell anybody from one group to the other. I was just to see whether there was excessive duplication, whether the duplication was useful, and follow the course of the research they were doing, which was predominantly in this case the detrimental effects on nickel of a variety of materials. These were predominantly fluorine containing materials. In fact, that was my first trip to Cleveland. I had to go to Harshaw [Chemical Company] about chemical problems, and to the Chrysler plant that was making tanks. I don't mean military tanks, but tanks to hold the bundles of diffusers that were going into the K-25 plant. The latter were made of Monel metal. So I did a lot of traveling in that area. Oak Ridge was another place I had to go to. I also went to Princeton, Bell Telephone Labs, the SAM laboratories (which was an old Nash dealer building north of Columbia), and Columbia itself. That's what I did for about a year. I had to move away from my wife, and we had a kid at that time. They stayed in Virginia and I went to New York. I lived at the Y on 63rd Street just west of Central Park.

It was at that time in the fall of 1944 that the ACS had its meeting in New York. I went out to the employment clearinghouse. It was at the Pennsylvania Hotel, right across the street from Penn Station on 33rd Street and 7th Avenue. One place I had already written to was the University of Texas. Another one was the TVA (Tennessee Valley Authority). They both had openings. I met with Drs. [Henry R.] Henze and [Roger J.] Williams of UT. I got offers from both UT and the TVA. So the question was whether to go to Tennessee or to go to Texas. Tennessee was pretty far. Texas was terribly far. But my wife said, "Go to Texas," so we did. It was a damn good choice.

BOHNING: Were there any particular overriding reasons for your choice? One was academic, the other was a government type of installation.

HACKERMAN: That's right. The government installation, or quasi-government installation, was a lot safer. That is, the job--as long as I didn't screw it up good--was permanent. The other was a tenure track. I had to do something in four or five years. As I said, I asked my wife where she would rather be and she said Texas, so we went to Texas with some opposition from family and friends. They thought we were going to get scalped down there.

BOHNING: Did you come out here to visit before you made the decision?

HACKERMAN: Yes, I came down by train on a trip in October of 1944. I couldn't get on an airplane then. Airplanes took a long time anyway, because they were DC-3s. The train was an overnight train through St. Louis, two days and a night. I got to the Austin campus on the day that they fired the president. So they weren't really very interested in talking to me! But I did look first this time at whether they did permit research, not did they support it. And yes, there were about nineteen or twenty people on the faculty and two-thirds of them were very active. Not in the current sense of active, but they had laboratories, they had graduate students, and they gave Ph.D.s. VPI didn't at that time. So I looked at that, and that was first. What was the load? They had cut it to three classes a semester, so that's nine hours. Well, I'd been used to twelve and fourteen, so that was no problem. It was a nice place. It was a nice October day like this. Aside from the fact that there was some turmoil, which I wasn't bothered by. I didn't deal with presidents; they weren't my problem. My wife said, "Go here," so we came here. It turned out very well.

[END OF TAPE, SIDE 4]

BOHNING: Did you start teaching introductory chemistry right away, or did that come later?

HACKERMAN: No. I taught one or more sections of freshman chemistry and a course in colloid chemistry predominantly for chemical engineers. It was required for chemical engineers. I would have probably two of the freshman classes and the colloid class one semester, and two of the freshman classes and probably a section of physical chemistry in the other semester. That's the way I started. The freshman classes would be two lecture hours and a recitation section and at that time oversight of the lab because it was associated with that lecture section. I had one of the recitation sections and then the others would be taught by other faculty members and some senior teaching fellows. In the labs, I would have teaching assistants. I would say there were two hundred per class, and that would mean there would be about eight lab sections. The labs were once a week for four hours.

There were forty people in the colloid course. I had a teaching assistant, and it had a lab. There were three lecture hours a week and then a four-hour laboratory just like the physical chemistry course. It was a very fine course in this sense. People sometimes come to me in airports and other such places and say that they had me for a certain class. The ones that are most vigorous in their approval are the ones who took colloids. Now, these are older people who are telling me this, because the colloid requirement was dropped from the chemical engineering curriculum probably in 1955. So I gave it for ten years.

BOHNING: Was that standard in the chemical engineering program to have a colloid course, or was that unique to UT?

HACKERMAN: No, it was certainly not usual. Unique, I'm not sure. But it was based on two guys in chemical engineering--a fellow named [Eugene P.] Schoch, and a fellow named Ken Nobe. These two people insisted that it be a requirement, because they recognized that one of the things that their chemical engineers came up against in plant operations and in the development work that goes on around industrial plants are colloid-type materials. It's just loaded with them. That's the reason these guys come up and say they're glad they took it, because they were prepared for the kind of thing that they were going to see in their reactors, such as emulsion formation, emulsion breaking, suspension, movement, things of that sort. They stopped doing it in 1955 because the chemical engineering department introduced these more quantitative modeling systems. They had to move something to do it, so they moved something that was not in their own course. Both Schoch and Nobe had died, and that was the end of it. That was the thing that went. But it was a good course.

When it went I replaced it with a graduate course in surface and interfacial chemistry, which included some of the colloid material. It alternated with a graduate course in electrochemistry. By the time that came into being, we were down to two classes a semester instead of three. So I would give a freshman class and an advanced class. In fact, I gave a freshman class the whole time I was at Texas. Every year, every semester. I gave freshman classes at Rice for a while until I had to be away too much and I couldn't quite make the schedule.

BOHNING: Yes, I want to come back to that in our next session. You had a very early paper on the equilibrium concept and the way it was being taught (6). That was in 1946, right after you got there.

HACKERMAN: Right after I got there.

BOHNING: So you obviously were involved in pedagogy in teaching the introductory courses.

HACKERMAN: Yes. We'll get to it later, but I believe that's as important a thing as I've done. As you said, we'll talk about it when I get to science policy.

BOHNING: Your interests in corrosion carried over right from the beginning.

HACKERMAN: Yes. My interest in corrosion was sort of augmented by the work I did on the Manhattan District Project.

BOHNING: But it started in the Coast Guard?

HACKERMAN: It started in the Coast Guard. I was brought into the Manhattan Project, as it turns out, because of deterioration of materials problems and the concern of what would happen in the K-25 plant if fluorine got started on something. The real worry was that a workman would leave his overalls or his gloves inside when we flushed it with fluorine. [laughter] There was another worry, and one that was more real, I think. Not that a guy might leave his glove inside, but it was hard to predetermine what would happen if moisture got into the system. To that end, it was my job in this wandering around the laboratories doing the work to infect the minds of some people to carry out experiments. Not to tell them, because the instruction was not to say anything. I had to get them to do this, to infect them to the point that they would do experiments that would involve the simultaneous injection of water vapor in fluorine under conditions that were considered to be problems, and to determine what the effect would be.

There was not only the problem of perhaps an accidental burn. These tubes must have been fifteen feet long. They were just nickel powder pressed together and they had to be porous. They didn't have a whole lot of structural strength. But also, you couldn't tolerate the growth of nickel fluoride into those pores or you wouldn't get diffusion. This process depended on $U^{235}F_6$ and $U^{238}F_6$ and $U^{239}F_6$. We wanted the $U^{235}F_6$ to come through, and be concentrated. Now these were cascaded. There were great bundles of these tubes in a tank and then the tanks were in cascades and you'd run them downstream. The stuff inside would go one way and the stuff outside another way. But if it didn't get outside, it wouldn't do you much good. I don't know if you've ever seen that K-25 plant at Oak Ridge.

BOHNING: I've seen pictures.

HACKERMAN: It's a huge place. I mean, big is big, but this is huge. And it's just a whole bunch of these things one after another. The concentration's not great in each bundle of tubes. If they didn't concentrate here, then there's no point in sending them to the next one. So there was a worry about what would happen if the nickel corroded and say the hydrated fluoride had a bigger volume than the nickel it had displaced. What would happen is a drop in porosity. So we had to get these experiments done and do them independently. Then I compared them so that the reproducibility was really pretty clean. Joe Blow here didn't know that Minnie Moe there was doing the same thing. So when we got results that were comparable, then we were pretty comfortable with it. That was the corrosion problem.

Corrosion is not just red iron oxides. It's a variety of things. When I got to Austin, I had done that little bit of work at VPI and also the Manhattan District Project, and I was interested in it. It was the easiest thing to set up. By that time, corrosion was a little different. It had electrochemical aspects where electrolytes were involved. The whole thing was buttressed by the fact that shortly after I got to Austin, not too far into that first year, I was visited by a fellow by the name of Tom Bacon, who was employed by the Lone Star Gas Company in Dallas. How he got to know about me, I have no idea, but he said there was a field east of Dallas--the Opelina field--in which they were having peculiar difficulties with a couple of wells. He wondered if I would be willing to take a look at it.

That was my second consulting job. (The first one was the suspension business.) I said, "Yes," without really knowing what was going on.

They took me out in the field and what was happening was that they had two wells side by side that had the same name, Tulles Number One and Tulles Number Two. They assumed that they were producing the same fluids. These were condensate wells. One of them was corroding badly, and the other one wasn't. He wondered if I could help him find out what was going on. Well, I approached it in a fairly straightforward way. I got him to insert some steel coupons into the flow system and then took a look at them. It was fairly clear to me that there were considerable differences in the two fluids. When they went back and looked, they found they were producing from different horizons. So the fact that they were sitting cheek by jowl had nothing to do with it. It was where the hole was, i.e., the depth. But they were impressed by the quick response. So then the question was, what about this one that's corroding. What could they do about it? I established that the Tulles Number Two had a fluid in which there was material that acted as a natural inhibitor. The composition of liquids were fairly much the same. In other words, they both should have corroded but one had something else in it. There was a guy named [Harry L.] Lochte on the faculty there. He was an organic chemist who knew a fair amount about petroleum composition. We came to the conclusion that there were naphthenic acids in the one which was not corroded. I suggested to them that they take the flow of one of them and run it back up through the other! They wouldn't do that, but that would have done it for them.

But I got interested in inhibitors. I was impressed by the fact that some minimolar amounts of materials could make that much difference. My interfacial chemistry background explained why. You don't need a lot to cover a surface. Relatively, you don't need much. That got me interested in inhibitors as a research area and that's when I got started on them. So I was interested in inhibitors. I had been interested in the oxygen electrode, as I told you earlier, and that got me interested in passivity. So the two main thrusts I had were how do inhibitors function and what is this thing called passivity.

BOHNING: You also had the Corrosion Research Laboratory. Did you start that?

HACKERMAN: Yes.

BOHNING: How did that come about? Was that for funding purposes too? I have the date as 1948. So you were only there a short time.

HACKERMAN: The first fellow I had working with me came to get a graduate degree. He had been employed at the International Chemical Company's magnesium plant just north of Austin. It was a war-induced plant and why it was placed there I don't know. They transported dolomite from west Texas and the idea was to produce magnesium from that dolomite. International Chemical designed it and operated it and this guy worked for them. He was a B.S. chemist. I came to Austin early in 1945, the war was over late in 1945, and this plant must have closed in the summer of 1945. It never produced any magnesium, or only a couple thousand pounds of it. He came to my lab looking for somebody to work with. That was [D. A.] Shock. He was a pragmatic type. He was not really an academic researcher. He did do a master's program with me. I told him he wasn't going to do the kind of research that a final oral committee would agree to, unless he wanted to go over to chemical engineering. But he didn't want to do that. Instead, I had a number of practical problems being offered to me, and I got him to work on those.

It became evident that there were going to be more things to do than would properly be done in an academic building. Meanwhile the magnesium plant had been given to the university and they were looking for things to do out there. I said, "Why don't we set up a corrosion research lab?" We took over one of the change houses, rebuilt it as a lab, and Shock went out there to work. That's where that came from. We had a few grants and contracts, things of that sort. We did a lot of practical work. That was known as the Off Campus Research Center. Or it was sometimes called the Off Center Research Campus. [laughter] It limped along.

There was a lot of space, three hundred acres or so, and a lot of buildings. There were old change buildings and chemical buildings, things like that. We put an accelerator out there while I was vice president. It now is a very important part of the University of Texas complex. It is the Balcones Research Center and it has about 700,000 square feet of space. There are a variety of laboratories and other kinds of things. So it has become very important. That's where MCC (Microelectronics Computer Center) is, as a matter of fact. But it started from this little almost wasted change room. By the way, that laboratory is still out there. It's not a corrosion research laboratory. But that was the genesis of it, and that went on until Shock was picked up by Conoco. He went up to their research lab in Ponca City and ultimately became director of chemical research. When he left, the corrosion research lab went.

BOHNING: I noticed that you also had some early support from the Research Corporation.

HACKERMAN: Yes.

BOHNING: And ONR (Office of Naval Research)?

HACKERMAN: ONR started quite early, about 1946. ONR was my main support system. In fact, the first support came from ORI (Office of Research and Inventions) and for about six months I was under that, just by letter agreement. Then the navy converted it to ONR. That went on from 1946 to 1968, without proposal. As I remember, every December I'd write them a letter of what we'd done, tie in some reprints to it, and say what we would do, and they sent more money. They kept doing it for twenty-two years, and I thought that was pretty good! We got a lot of work done for them, though. One of the things they wanted was for me to get directly involved in their problems, and I didn't think that was a good idea. Not that they didn't have problems or not that I might not be able to help them. That's where I got the germ of this. I thought the important thing was that in the process of carrying on this research, which could be useful to them as well as others, I had a pretty good stream of people coming through. I've had about sixty or seventy graduate students, sixty Ph.D.s and maybe eight or ten masters. That was what was important. And we should come back to that on science policy.

I think we'd better go to the luncheon. Is this a good place to stop?

BOHNING: Yes, I think this is an excellent place to stop.

[END OF TAPE, SIDE 5]

NOTES

1. James Kendall, _Smith's College Chemistry_ (New York: The Century Company, 1929).

2. E. E. Reid, _College Organic Chemistry_ (New York: D. Van Nostrand Co., Inc., 1929).

3. Linus Pauling and E. Bright Wilson, Jr., _Introduction to Quantum Mechanics_ (New York: McGraw-Hill Book Company, 1935) or Linus Pauling, _The Nature of the Chemical Bond_ (Ithaca, New York: Cornell University Press, 1939).

4. Arthur A. Noyes and Miles S. Sherrill, _An Advanced Course of Instruction in Chemical Principles_ (New York: Macmillan Company, 1929).

5. W. A. Patrick and N. Hackerman, "Studies on Molecular Weight Changes of Sulfur Monochloride," _Journal of Physical Chemistry_, 40 (1936): 679-688.

6. Norman Hackerman, "The Equilibrium Concept in Beginning College Chemistry Courses," _Journal of Chemical Education_, 23 (1946): 45-46.

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