CHEMICAL HERITAGE FOUNDATION

NORMAN HACKERMAN

Transcript of an Interview
Conducted by

James J. Bohning

at

The Robert A. Welch Foundation

on

21 February 1992

(With Subsequent Additions and Corrections)

THE CHEMICAL HERITAGE FOUNDATION
Oral History Program

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(Signature) Norman Hackerman
Norman Hackerman

(Date) May 30, 1994

(Revised 17 March 1993)

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
	University of Texas at Austin
1945-1946	Assistant Professor of Chemistry
1946-1950	Associate Professor of Chemistry
1948-1961	Director, Corrosion Research Laboratory
1950-1970	Professor of Chemistry
1952-1961	Chairman, 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 Emeritus of Chemistry
	Rice University
1970-1985	President
1970-1985	Professor of Chemistry
1985-	President Emeritus
1985-	Distinguished Professor Emeritus of Chemistry
	The Robert A. Welch Foundation
1982-	Chairman, Scientific Advisory Board

Honors

1956	Whitney Award, National Association of Corrosion Engineers
1964	Joseph L. Mattiello Award
1965	Palladium Medal, The Electrochemical Society

1965	Southwest Regional Award, American Chemical Society
1972	LL.D., St. Edwards University
1975	D.Sc., Austin College
1975	Honor Scroll, Texas Institute of Chemists
1978	D.Sc., Texas Christian University
1978	LL.D., 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, The Electrochemical Society
1984	Alumni Gold Medal for Distinguished Service, Rice University
1987	Charles Lathrop Parsons Award
1987	AAAS-Philip Hauge Abelson Prize
1993	Vannevar Bush Award, National Science Board
1993	Doctor of Public Service, University of North Texas
1993	National Medal of Science

ABSTRACT

In this, his second of three interviews with James J. Bohning of The Chemical Heritage Foundation, Norman Hackerman begins by describing his work after coming to the University of Texas at Austin Department of Chemistry and starting the Corrosion Research Laboratory [currently the J. J. Pickle Research Center]. He discusses the physical chemistry textbook for premed students he wrote with Frederick Matsen and Jack Myers. He also recalls the events which led to his becoming chairman of the department after only seven years, his reorganization of the department, and characteristics of the department's faculty at that time. Hackerman also describes his consulting work for the Lone Star Gas Company, the progress of his research at the Corrosion Research Lab, and resulting publications. He focuses on the factors leading to his appointment as Dean of Research at the University, the work he undertook in that position, and his eventual promotion to Dean of Office of Government Sponsored Research. He also discusses his research for the API and mentions his students and subsequent publications. Hackerman concludes the interview with a summary of his rapid progression from Vice President to Vice Chancellor to President of the University.

INTERVIEWER

James J. Bohning is 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 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. He currently writes for the American Chemical Society News Service.

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INTERVIEWER: James J. Bohning

LOCATION: Robert A. Welch Foundation
Houston, Texas

DATE: 21 February 1992

BOHNING: Dr. Hackerman, the last time we talked, we had you at the point of arriving at UT [University of Texas] at Austin. You had started the Corrosion Research Laboratory within a few years after you arrived.

HACKERMAN: Actually, that same year.

BOHNING: We finished with your telling me about what it was like when you first started out there, at what is now the Balcones Research Center. We discussed why you chose UT and the tenure track position over a "safer" government position at the TVA. You started right in and published a large number of papers about corrosion very early at UT. But I was also curious about a textbook in physical chemistry which you published in 1949 (1).

HACKERMAN: That's a pre-medical physical chemistry book.

BOHNING: What was the origin of that book? Why did you do that?

HACKERMAN: There was a course at UT for premeds. They were kind of forward-looking. That was early in the game. It had become evident to a number of people on the campus that premeds who had some background in physical chemistry, that is, more than just general and organic, might have a leg up on their studies in physiology and biochemistry that they got in medical school. The course was already established, but it didn't appear each semester.

I got interested in it along with a fellow named [Frederick A.] Matsen, a theoretical chemist. I got interested in it because I took it on as an assignment. I gave the course. Matsen and I officed together and we'd talk about this on occasion. I'd tell him that the textbooks that were available weren't much good. He said one day, "Why don't we write one?"

We enlisted the help of Jack Myers, a plant physiologist with a good background in chemistry. He got his Ph.D. at Minnesota. He had courses from [Frank H.] MacDougall. I don't know if the name's familiar to you. He did some first-rate work back in the early part of the century. He was a good thermodynamicist and a fellow who really understood the field. So Myers, although a physiologist, had a lot of chemical background. He agreed to come in on it. He was to be our perimeter guy and tell us when we were outside the bounds of what medical students had to have.

We wrote this little book of about two to three hundred pages. Matsen and I wrote a sizeable portion of it at UT baseball games. Texas always had a good baseball team and we would go out there and sit on the third-base line. At baseball games you can write and look up and then write again.

Myers did his by himself and we incorporated it. MacMillan was interested in it and published it. I think there was a press run of five thousand and those were used in about twenty places. We never put out any other editions. But it was an interesting little book in the sense that it tried to tie together, at a very early stage, physiology, biological processes, chemistry and even theoretical chemistry. It obviously didn't shake the world, since it went through one printing, [laughter] but I think it was an interesting book. In fact, I've been looking for a copy recently, but I haven't been able to find it.

BOHNING: We have a copy at Penn [University of Pennsylvania]. I looked at it. It's in the library.

HACKERMAN: I might go up there and Xerox it. [laughter]

BOHNING: I'll see if we can find a copy for you, if you're looking for one.

I was curious about the response you got to this book, because as you said it was very early to be doing that kind of thing.

HACKERMAN: The students liked it. There isn't much question about that. I'd been told it was hard to get the students interested in the field. These were the good students who signed up for it, and they liked it. As I recall, I gave it three years running, something like that, until I became chairman of the department. There was little or no dropout, and that's a pretty good indication that the students liked it. I'd say thirty to thirty-five people took it each time. The only reason we didn't keep it up to date was that we all had our own things to do. Matsen went on to do a lot of good theoretical work. He's the guy who, among many other things, does spinfree quantum chemistry. Myers is actually a good photochemist also. His interest is algae. And my corrosion work had taken off, so we just didn't have time to write books.

BOHNING: There were some other things I wanted to ask about your research, but since you mentioned it, let's look at your becoming chair of the department in 1952. You'd only been there seven years; you were only forty years old, if I have the dates right. How did that come about?

HACKERMAN: When I came, Dr. [William A.] Felsing was chairman. He had been chairman for a while. He was a typical physical chemist and primarily worked in thermodynamic systems. He had chaired the department in a very avuncular fashion. Discipline was a little different in those days; it came up through the chairman. But the chairman was not a head; that was an important distinction. He had been chairman for quite a long time and was tired. He was succeeded by Robbin Anderson, another physical chemist, who wasn't all that interested in doing the chairmanship. At that time there were about fifteen people in the department. The older ones didn't want to take it on, Anderson didn't want to keep it, and Matsen was clearly not interested in being a chairman. While I wasn't particularly interested in being chairman, I didn't mind it. So, it sort of fell to me.

It was kind of an important thing for me, because it crystallized some stuff that I didn't know I could do or wanted to do. Management capability was one of those things. I had no management background of any sort, but organization in my own business was always the first thing, so that's what happened to that department. For a department which had been run in, as I said, an avuncular fashion, I began to run in a more organized fashion—careful not to step on the toes of the faculty.

For example, I walked down into the tunnels in the subbasement of the chemistry building, which at that time was about twenty-five to thirty years old. I walked back in the tunnels and found the remnants of a system, which is kind of interesting. There were just boxes and boxes of Jena glass, beakers, flasks, side-arm flasks, all sorts of glassware. This came about because at that time any money you had left over at the end of the state fiscal year you spent; otherwise you had to give it back. The way to spend it was to buy chemical equipment; that is, you didn't throw it away. I guess Jena glass was popular in the late 1930s, early 1940s, in the interim between soft glass and Pyrex.

These boxes of things had been bought in Germany and stored down there, just literally scores of them. But while I was down there I found lots of bottles filled with mercury, and pieces and bits of silver and gold and platinum, which had been used in the physical chem lab. I made a very thorough search of the place myself and collected the mercury and metals and sold them. I think it was legitimate. I sold them, and I had about ten thousand dollars out of the sale. For the first time I had money to support research of the faculty members of the department and gave them each whatever it was, maybe a thousand dollars.

That was kind of unheard of at the time, because you bought things out of M. O. and E., Maintenance, something, and Equipment, for your classes and you sponged off of that. That's the way you did your research. If you needed glass, you'd go to the freshman lab and get tubing.

Research activity was pretty high. That's the reason I went to Texas. When I went down there, I saw that most of the faculty members had some research activity going on. Felsing was a physical chemist who did a lot of work with vacuum systems. He was constantly in the laboratory with his own torch. And the torch was bought either with his own money or was bought for the physical chemistry lab and he was using it. Or both.

At any rate, this money was the first time, as far as I know, that people there had their own money to buy what they needed or what they wanted in the way of chemicals or glassware for their own research laboratories. So I became a very popular chairman pretty quickly. The quick way to become a popular official is to support something with money. Mr. [George] Bush is finding that out now. [laughter]

So that worked out very well. I continued that investigation. I then went up into the attic and found that it was a shambles, a very dangerous place. It was just full of abandoned materials. Dr. [Harry L.] Lochte, for example, used to get crude oil and extract it for the naphthenic acids it contained. The residue would go up in the attic. [laughter] You've been down here in the summer. It's not cold up in that attic, I'll tell you. I'm reasonably sure that we were lucky, because we went past the flash point of some of those things. What may have happened is that the volatiles evaporated during the colder weather, and they worked out that way.

I went up there and then decided that there were a lot of things that were useful, but that I couldn't get to them. I mean physically I couldn't get to them. So I scared the administration by telling them that they had a very unsafe building. They sent a crew and cleaned out the place. We got more mercury and other metals. I had a little cache of cash, which I was able to use for the second and third year.

I had a grant that I'd gotten in 1946 that kept on going, and by 1953 other members of the faculty began to get research grants, so I could step out gracefully and not support them any further. But for two years I got them used to financial support, and then they went off and did their thing with grant proposals.

BOHNING: What kind of external sources of money were available in those days?

HACKERMAN: ONR [Office of Naval Research] had started. By 1954 NSF [National Science Foundation] had started. In fact, this place, the Welch Foundation, started in 1954. NIH

[National Institutes of Health] was already on the way, so basically they were able to do it. In addition to that, Roger Williams, who headed up the Biochemical Institute, had made an arrangement with the Clayton Foundation for the support of his biochemists. I think by that time the faculty was at twenty, and four or five of them were biochemists. The Clayton Foundation provided the support for all of those biochemists with the proviso that they didn't go outside and try to get any other funding. In other words, it was an operating foundation. They literally paid half their salaries and all their research support.

So there were a fair number of sources. If you were interested, able, and really aggressive, you could do really well. So there was ONR, NIH, NSF, and the Welch Foundation. I believe the Office of Army Research, as it was called at the time, started about the middle 1950s. Toward the end of the 1950s the AFOSR [Air Force Office of Scientific Research] started. Oh yes, there was also the AEC [Atomic Energy Commission]. Dr. [George W.] Watt and Dr. [Leon O.] Morgan were both inorganic chemists. Watt had been with [Glen] Seaborg at Chicago and Morgan, as a matter of fact, had gone up there also. He got he got his Ph.D. with Seaborg at Berkeley in radiochemistry. They had AEC money.

BOHNING: What was the age distribution in the department at that time? Were there more younger faculty or were the older faculty still dominant?

HACKERMAN: Of the fifteen when I became chairman, six were what you'd say were the older faculty. If I were put in the middle group, there were about five of us and four in the younger group. We brought a lot of people in during the decade of the 1950s. We didn't keep a whole lot of them, on purpose. That is, we didn't do so well at bringing them in; I guess that's the problem.

We did indeed contribute to the problem of "publish or perish," there's no question about that. At that time, about the middle 1950s, I'd already had a grant for ten years. Matsen and I were consultants. He was with Humble Oil Company, Watt was with DuPont, and I was with Mobil. Others began to do the same thing. So the exfoliation of the department to the world had begun. This may sound like an old fogyism, but it's gone too far, because he and I and the others there did not let the outside interest influence the inside stuff.

We did indeed spend a lot of time with students, and in fact until I became chairman, I taught either three or four classes every semester, as did all the rest of them. When I became chairman I went down to two. I taught freshman chemistry every year from the time I got there until the time I left in 1970. The eight o'clock in the morning freshman class was mine, and it was a big one with two hundred and fifty to five hundred. And then I taught a graduate class, either in surface chemistry or electrochemistry; they would alternate. Matsen taught freshman chemistry, quantum mechanics, statistical mechanics, things of that sort. We have sometimes been charged with leading the charge off the campus. That's probably true, but that was not the intent. The intent was to broaden our view of the system.

As you well know, the faculty members are pretty well involuted. Outside, the industrial people looked on them as being fuzzy-haired, or egg-headed, or whatever term it was. I guess it was egg-head, because Adlai Stevenson was around at the time. I thought that my contact with Mobil, and other companies after a while like Dow and Carbide and lots of others, made me a better chemist. Maybe not from the point of view of chemical theory, but from the point of view of chemical use. I still think so. I am of the opinion that you can still do that and your research and be interested in students. Those are synergistic; one went with the other. Now, I'm afraid the tendency is, "I'm now so busy doing outside things I need to minimize my inside things." I think it's about to turn around again.

BOHNING: I remember Farrington Daniels taught freshman chemistry for many, many years as well, with that same intent in mind—that it required the best people to be in that freshman chemistry course.

HACKERMAN: My philosophy was that you could put a graduate student in to teach a graduate course, because graduate students in the graduate courses had to learn it by themselves anyway. It didn't matter. I don't mean you can neglect graduate students, by any means. My procedure for dealing with graduate students is primarily in bull sessions, without formality and without the threat of examination and that kind of thing. I thought that full faculty members ought to teach undergraduates. And that we did. I'd say that those that didn't show a bent for it, we got them out of there. But the rest of them all taught. Dr. Felsing taught freshman. Dr. [Henry R.] Henze, who was a senior guy when I got there, taught. I don't think Dr. Williams wanted to teach freshman, but he taught sophomore organic, which is the equivalent. So it worked out well.

BOHNING: Do you think that the department was more physically oriented? Did it have a strength in one specific discipline of chemistry?

HACKERMAN: No, it was, as most of the departments at the time, organically oriented. It had a very strong bias toward biochemistry because of Roger Williams, who himself was an organic chemist turned biochemist. He was always interested in biochemistry staying in the department. He was dead-set against the separation that was taking place fairly regularly at that time. He said chemistry was what he wanted, not bio. So, he was an important influence.

The other organic chemists, Henze and Lochte, were typical synthesis people or organic analytical people. The physical chemists were able, but there wasn't a great reputation. That reputation began to be made in the 1950s and it involved the strong input of the inorganic chemists who were there at the time, particularly Morgan, who was labeled an inorganic chemist but whose interest was largely physical. Matsen had begun to make a good reputation in theoretical chemistry. My electrochemical work was going well. So we began to attract young physical chemists. Actually, it wasn't until the 1960s, when we brought W. A. [Albert] Noyes [Jr.] down here. He had retired at Rochester. He came down, and we sort of put in his hands the identification and sequestering of good, young physical chemists. He did a great job from the time he got there until the time he died.

The department grew rather rapidly from 1952 until the time I left in 1970. When I left there were probably thirty-five to forty people in the department. The fields had spread and now had begun to really lap-over into chemical engineering in part, to biology in part. I guess one place we really weren't successful was that we never could convince the geologists to go strongly into geochemistry. They were good field geologists, but they weren't all that interested in basic science. Now they are quite different.

BOHNING: There must have been a strong petroleum connection there with the geology department.

HACKERMAN: Yes. Mining in general. There had been some mining out in the western part of the state in the early years, but petroleum was the main thing.

The chemistry department began to make a reputation that moved it from the bottom first hundred to the top part of the first hundred. I'd say we were between twenty and thirty. From 1970 on it grew faster and better, but it got a real spurt in the middle 1950s.

BOHNING: In your list of publications there are a couple you identified as important ones. Just looking again at this period up to 1961, while you were chairman of the department, one was the "Action of Polar Organic Inhibitors in Acid Dissolution of Metals" (2).

HACKERMAN: The reason I thought that was an important one was less because of its importance in corrosion than because of its importance in interface science. You've got to remember, back at that time the corrosion inhibition was looked upon as anything you could do to put a three-dimensional structure between a metal and the solution, whether it was a paint, foam, scale, enamel—anything that would just separate the two.

That's perfectly legitimate, but it didn't explain the influence of the organic inhibitors, which had been used from time immemorial. As a matter of fact, in the nineteenth century, people used to dump in egg white or molasses, all kinds of things, which clearly depended on certain organic components of those natural materials.

[END OF TAPE, SIDE 1]

HACKERMAN: Back in the 1940s, Darcy Shock and I worked on a little problem which came to me from the NGAA, the Natural Gasoline Association of America. A fellow by the name of Tom Bacon of Lone Star Gas Company had a problem. He was in Dallas and he had heard that somebody in Austin was working in the field of corrosion and corrosion inhibition.

He came down to see me one day. His problem was that the Lone Star Gas Company had a couple of wells in the Opelina Field which was about one hundred miles east of Dallas. I forget what it was near. The problem was that they had two wells sitting fairly close to each other; these were natural gas condensate wells. One of them, Tullos Number Two, gave them all kinds of trouble, but Tullos Number One was fine. He just didn't understand it. He wondered if I would be interested in trying to help him understand it.

That was kind of new. I was already consulting for Mobil, but that was different. I'd go to Mobil and listen to their problems and let them talk until they found the solutions, and then go home. [laughter] This was to help this fellow, Bacon, find out what was wrong. I thought a long time about it but then I decided I wouldn't put a lot of time into it, but I'd go out to see what was up.

Shock had been a graduate student of mine who couldn't pass organic. The organic prelim just killed him, so he stopped at a master's degree. We opened this Corrosion Research Lab out at what was the old magnesium plant, currently the Balcones Research Center. This was an ideal problem for him. It required an understanding of the system, but it wasn't a degree program. So in essence the first of the research of the Research Assistants appeared that way. Again, it was moving the university into what it's now doing, perhaps more than it should be, but it's doing it.

We decided that the way to find out what was going on was to expose some coupons in the flow lines. Sure enough, the coupon in Tullos Number One corroded very rapidly. Tullos Number Two didn't. We looked at these with what now appears to be very unsophisticated instrumentation, but it was sophisticated. With optical systems and interference things we detected a film on the one that didn't corrode, but a thin film, one that didn't alter the appearance of the metal. It still had the steel color to it. The other one didn't have anything like that. It was very rough and corrugated.

My first statement to Bacon was, "You might have the same name on these, but they're not coming from the same horizon." Now I was naïve enough to believe that those guys knew everything there was to know about the field, but the fact was they didn't know a damn thing about it. When they decided to test my proposition, it turns out they weren't from the same horizon. They were at the same depth, but the horizon dipped. So that was the first thing. The second thing I suggested to them they wouldn't do. Namely, take the flow from Tullos Number Two and take it down below and bring it up. I propositioned them that there was a natural inhibitor in the Tullos Number Two horizon.

That's when I got interested in it because I went to Dr. Lochte. I told you he was an organic chemist and did a lot of extraction of things from crudes. He suggested that it may be naphthenic acids. So we began to do some studies on naphthenic acids, and sure enough we could reproduce those films easily. That was the principal contribution of the Corrosion Research Laboratory.

From there on I got quite interested in this business of organic materials in low concentration and the influence they had on the metal solution interface. I then ran across an article, which was not in the scientific literature, but the technical literature, which described the introduction of toluidenes into a system that was corroding. The toluidenes were found to be different from one another, ortho-toluidene being much more effective than either para-, meta-, or toluene. It was known as the ortho effect. And that's all; it was dropped right there. I think somebody out of Tulsa, maybe Cities Service or Exxon were working on it.

I wondered why the ortho position was so important. That got us started on the interest in the structure of the organic material and the way in which it disposed itself on the interface. The reason that paper was important is that by the time that paper came along I was convinced that while these may not be great commercial inhibitors, there were distinct differences which were structurally related. That had to depend on either of several possibilities or on some combination. Namely, on the what I'll call the Lewis acid-base characteristic. In most cases, these were electron donors. Or on its capacity to cover, which meant you had to know something about the architecture.

I began to be interested now in the chemisorption rather than the physical adsorption. It was before others had talked about it, in the early days; I began to think in terms of two-dimensional compounds on the interface. This was an obvious involvement with catalysis and lubrication and a whole bunch of interfacial phenomena. That's why I thought that paper was important. In fact, I gave a talk to the New York Academy of Sciences somewhere back then, in which I talked about two-dimensional compounds, and I got hooted at. Guys didn't think it was the kind of thing to talk about. It was too fanciful. That was in the early 1950s, and now of course you see them through interferometry or scanning tunnel microscopy or something like that, and they're there. At that time we had to infer. So much of my research then went over to the business of what can you do with molecules that will permit you to make a prediction about what might happen. That led to a very good series of papers with [Kunitsugu] Aramaki on the structures and effects of polymethyleneimines in corrosion inhibition (8).

BOHNING: At the same time that paper came out, there was another one which fits in with what you are saying, about "Charge-Transfer-No-Bond Adsorption" (4).

HACKERMAN: That was before it's time, I've got to tell you. I think that was published in the *Journal of Chemical Physics*. The response to that was zilch. [laughter] It was a good paper. Matsen and I were on that paper, and my student [A. C.] Makrides. It was before its time, but nonetheless it did presage what was going to happen in interface science. Al Makrides got his degree that year or maybe the year before.

I've got all my reprints lined up in the file someplace.

BOHNING: Just a little bit after that you had a patent which struck me as being out of the line of what you'd been doing. This was a process for de-inking printed waste paper (5).

HACKERMAN: Actually it was a little before that; wasn't it about 1949?

BOHNING: This one was 1956. There was another one that came much later, too (6).

HACKERMAN: That's right. It was not in line, but it was in line with the proposition that I would take on students who I thought were capable of doing something in the field of interfaces, if they came to me and said that this is what they wanted to work on. This guy had been in the paper industry and had an idea he wanted to check. I looked at it and in fact felt capable of helping him do it. This was a proposition that the carbon particles adhered to the paper fiber in large part electrostaticly, and that if you could develop a solution which helped disperse the paper, and alter the charge on the carbon particle so that a) it was of the same sign, and b) had a different value, you could cause them to spring apart by putting them in an electric field.

And it worked. We just took a big iron pot and an iron electrode. I guess it was a cathode and the pot was the anode. I don't remember what the solution was, but it was a concoction. We took newsprint, beat it up, put it in there, and put the charge across this thing. The solution wasn't a good conductor, so there was a big drop right at the electrode; I don't remember if it was the anode or the cathode. What happened was, the carbon just jumped out, left the white fiber right around the electrode. All you had to do was draw it off. So we got this patent, which I gave to the student, a fellow named [William J.] Krodel, because it was his idea. He tried to sell it to the paper industry. This is the reason I thought it was the 1940s. Maybe by the time it was issued it was that late.

BOHNING: It could have been applied for much earlier.

HACKERMAN: We tried to sell it to the paper industry at the time because after the war new pulp was two hundred dollars a ton or something like that. We thought we could do this for about ninety or one hundred dollars a ton. But the paper industry, like other old industries, circled the wagons; they didn't want to hear from us. This was different capital equipment, and recycling was not very popular at the time. You burn the old paper and cut some new trees down.

Krodel had the patent; I assigned it to him. He spent a lot of time trying to convince people in east Texas to use it, but they never did. It's a good patent.

BOHNING: Another paper in this time period that you identified as being important is "Anodic Phenomena at an Iron Electrode" (7).

HACKERMAN: One of the two interests I had in the field of corrosion was this business of the organic molecules and the way they oriented and the way they bound. The other was an old, old interest in passivity. For some reason, while I was in graduate school I got interested in it.

It is a very curious phenomenon; I don't know how familiar you are with it. You take a piece of iron and dip it into concentrated nitric acid, you get a lot of hydrogen coming off and then it stops. Or, if you take a piece of iron and put it in less concentrated nitric acid, or even sulfuric, and pass a current through it, the current stays high. When you get to a certain potential it drops way down. It stays that way until you get to what's called a transpassive region, when iron oxidizes to FeO_4^{2-} in a +6 oxidation state.

This passivity takes place on the iron with no apparent scaling between the metal and the solution, so what ever is formed is very thin. It has a very peculiar property. That is, it transmits electrons readily. A scale doesn't, though. Scales are either insulators or ion conductors generally. Iron oxide is either a semiconductor or an insulator. You can destroy it by a variety of things. Chloride ion, for example. There's an old experiment known as the Lillie experiment. You take an iron wire and put in concentrated nitric acid. Hydrogen comes off momentarily and some brown gas, and it stops. If you touch the top of it with a piece of copper, you see a pulse run down. It was known as a Lillie nerve model. I've got an old 8mm movie of the experiments done at Bell Telephone Lab, I guess in the early part of the century. I don't know how early it is. The Lillie nerve model was demonstrated in this movie. That was because the passivation has been destroyed by an electrical pulse.

I've always been interested in that, as I was in the oxygen electrode back in the 1930s. I did a lot of work myself on the oxygen electrode. I never solved the problem, but I did a lot of work on it. I then got interested in passivity. I was convinced that there was no scaling involved.

At Cambridge there was a guy named [Ulick R.] Evans who said there was a three-dimensional iron oxide that forms. It had only the peculiarity that it was dense. It had very little porosity, and the reason for the passivation characteristic was just pure separation of the metal and solution. That didn't sound reasonable to me, because the amount of current that could flow through that passive film was so high. You actually could use it, if you were careful, as an inert electrode. So one could do an electrometric titration of a redox couple if so desired. You had to be very careful; you couldn't have chloride there and some other things. That meant that current flowed through that thing like it was a metal; it had to be very thin. It had the same kind of peculiarity that ceramic high-temperature superconductors have. It passed current when it shouldn't have, by our then-known standards.

I gave a paper at the first Passivity Conference, as it was called, at Heiligenberg, Germany. I suggested the reason it was so conductive was that the iron-oxygen combination formed very rapidly and the material was amorphous rather than crystalline. And it spread rather than grew. That is, rather than moving perpendicularly from the surface it spread laterally across the surface. It had to be well-packed, nonporous, and a sufficiently thin film so that electrons could move through it fairly readily, i.e., of Angstrom-type thicknesses.

That paper actually got some interest. I don't know if the name Carl Wagner means anything to you. Wagner was one of the German scientists who was captured and brought over here at the end of the war. He was a very nice fellow whose only real interest was science. He had no family. He was involved with this Heiligenberg Conference and he got pretty interested in the paper.

My Cambridge buddies jumped up and down on me at that meeting and said that was silly. Anybody knew that it had to be iron oxide and it had to be three-dimensional, and they could prove it because they could strip it off and show it to you. I maintained that they were confusing cause and effect. What they stripped off was the iron oxide made by the oxidation and stripping solution. We had serious arguments about that.

Cause and effect is an interesting thing, by the way. It's what screws up most researches, not keeping those straight, including mine.

At any rate that paper was the last that I really did seriously in passivity (8). I figured if those jokers couldn't accept what was clear, the hell with it. So I haven't done much since then.

There was a very good experimental paper I had with [William H.] Wade (7). This was one in which we did electrochemical potential dynamics before it became a term. We would drive the potential across this passive region and then reverse it and come back. You could make this whole thing reversible if you didn't go too far. This was a current on the abscissa and potential on the ordinate. You'd change the potential and measure the current. You could drive across the plateau if you went only a fraction of the distance, which we thought at one time was critical, but it turns out not to be. You could go back and retrace the curve. But if you went just a little further the whole thing dissipated. You'd see the gas begin to form and you'd go into the active region.

That Wade paper was a very good one. Wade, by the way, is a member of the faculty at Austin now.

BOHNING: I did notice the Center for Electrochemical Research listed on the board at the department. When did that start? Was that much later?

HACKERMAN: Yes, that's Alan Bard's group. I'm sure I was chairman then. We brought Bard in. He had been in [James J.] Lingane's lab. I brought him in because we had an opening in analytical, and I was pretty well convinced from talking to him that he was broader than just pure instrumental and analytical. I think he's still in the analytical division. He is obviously much broader than that.

BOHNING: You made a comment earlier about the selection of new faculty, and the fact that you either were or were not successful at properly identifying that when they came in. In retrospect, are there any reasons for that? In a more general sense, how do you identify a person for a slot that you have available?

HACKERMAN: The standard way is that you talk to them, you listen to them and his supervising professor. You get recommendations and you learn pretty quickly that, unless somebody dislikes a person, they are always going to be higher on a person than you can expect out of them. So the question is what kind of factor you can put on them. That depends on who it is you're talking to, so you have to now evaluate the recommender rather than the recommendee.

Furthermore, in the 1950s Austin was still a pretty isolated place. To get to it you could fly in. For example, if you flew down from Boston, you'd fly down to New York, from New York to Memphis, Dallas-Ft. Worth, Waco, Austin. It was not an easy trip, I'll tell you. [laughter] Or, you could come by train and take it overnight; actually, two days and a night. People on the east and west coast just weren't that easy to move. You'd get the outdoorsman, but then you had a problem as to whether he came because he could go hunting and fishing more readily or because he came down to work. What you sort of deliberately had to do was say, "Look, we know we're not going to get the best guys right at the beginning, but we want people we could stepladder on. And we were perfectly willing to be lucky, too.

It turns out we were lucky in at least two cases that I can think of off-hand. One's Bard. Why Bard came down, you'll have to ask him, because I can't tell. He's a New York boy, went to that New York high school, the Bronx High School of Science. He'd been picked up from there and went to Harvard. It seemed to me he had plenty of opportunities. But he did choose to come down there, and it was a great break for us, because he's good.

The other great break for us was Alan Cowley. Cowley is an inorganic chemist. He's currently a fellow of the Royal Society. I think he had taken his degree at Imperial College, and was working at ICI [Imperial Chemical Industries] when we found out about him. I happened to be in England; in fact, it may have been for that Passivity Conference I was telling you about back in Germany. I talked to him and he came. It wasn't as evident at the beginning that that was a great move. He was quite satisfactory, nothing wrong with him, good instructor, but he really has developed a first-class reputation as an inorganic chemist. They came about the same time, and that literally gave us the step on which we could begin to build with the people that Noyes had in mind, specifically in physical chemistry. During that same period of time we must have taken in about fifteen people. There were those two plus Nate [Nathan L.] Bauld, who also came from Harvard, plus Steve [Stephen E.] Webber. The others we let go.

[END OF TAPE, SIDE 2]

BOHNING: When did you bring Noyes down?

HACKERMAN: In the early 1960s. I was actually in the central administration by that time. I don't think I was chairman, but I was involved with it. The major mover in that was George Watt, who was a counselor in the ACS, and otherwise fairly deeply involved in chemical business, so to speak. He's the one who would see Noyes.

Noyes was retiring at Rochester. I suspect the difference between 107 inches of snow and no snow impressed him. He and his wife had a very pleasant ten years in Austin. As a matter of fact, they're buried there.

BOHNING: That brings us to the point where, as you just said, you had moved into the central administration. You indicated earlier that when you became chairman without any management background, you liked doing it and that you found you had skills in that area. What led up to your moving up as the dean of research in 1961?

HACKERMAN: The chemistry department became notable on the campus for being managed well. The administration had fewer problems with us. We were sort of on the forefront of the movement to become part of the world rather then be an ivory tower. We were ahead of Harvard and Yale, for example. Yale still hasn't gotten to it [laughter]. As I said earlier, I'm not sure it hasn't gone too far, but it was important that universities realized that they were not there for totally esoteric reasons. They had to start thinking of real people rather than fuzzy-cheeked boys and pretty girls, social events, beauty queens, and whatnot; universities were real and important things in the scheme of things.

Chemistry led the way. Some of the others are still kicking and screaming, as a matter of fact. But the administration at that time was perceptive enough to recognize that this may be the wave of the future, so they tapped me for some other things. In fact, the first thing I was tapped for was to fill in for the dean of the graduate school for eight or ten months while he went on leave. I'm not even sure what date that was, to tell you the truth, but it was in that same period. I took that on along with the chairmanship; I didn't give up the chairmanship. I went over there and found out that you could do that work in fifteen minutes a day. It was no great problem. [laughter] The graduate dean never did forgive me for that. He was polite, but he was cool.

That's where I began to look at structures outside of chemistry. Up to that point I looked up at my dean and the president or vice-president. I went to the graduate dean's office for those eight or ten months, and recognized that the graduate dean was a horizontal blanket on a vertical system, and therefore couldn't be very powerful. All these vertical arrangements went right to the president, and so the graduate dean was flanked. All he could do was sign diplomas and theses and dissertations. He had no money. That's still true. Graduate deans are not very powerful. If they have any effect, it's by virtue of their personality and not their office.

So I began to get a little understanding of greater management systems. By that time, the first fellow who had been asked to look at sponsored research, a man named [Charles P.] Boner, a physicist, had more of it than he wanted and he went back to being director of the Defense Research Laboratory, with which I was involved also, by the way. That was a laboratory that stemmed out of the war and harbored an underwater acoustic laboratory. They did submarine signals, how to detect them and how to prevent giving the signals out. He was up there as associate director, and Eric Walker, who was later president of Pennsylvania State University, was the director of it. After the war was over, the two of them determined that this work should continue.

So an underwater sound laboratory appeared at Penn State [Pennsylvania State University], and one appeared at Texas, and a third one appeared at Washington. I can't remember what that connection was. That became the Defense Research Laboratory of the University of Texas, funded by the Navy, doing classified work, by the way, on the campus. Penn State had the same thing. Their building was right smack in the middle of their campus. I think the Washington one was at Friday Harbor, so it was off campus.

I became associated with DRL as a faculty appointment in 1947 or 1948. It was important to me because it offered me a place to put students that I couldn't support anyway, to do work that I was interested in. I had three people who worked over there and got their degrees with me. In fact, one of them was way off of what I'd been doing here. Ray Hurd did some work there, although I forget what he did his dissertation on. I'd forgotten about DRL. That was a fairly important part of my early time at Texas.

BOHNING: Those were the days when there wasn't any objection to classified work going on on-campus?

HACKERMAN: No comment. I was a member of their staff probably until I became vice-president. Then it would of been in conflict.

BOHNING: Here's the list I was looking for. Ray Hurd.

HACKERMAN: Does it say when did he got his degree? It would be in the middle 1950s.

BOHNING: The papers with his name on them were published in 1955, 1956, and 1957 (9). "Electrokinetic Potentials of Bulk Metals."

HACKERMAN: That's it. That was a little beyond what I'd been doing. What we did over there I guess is no longer classified. It was on something called solions.

Solions were interesting little devices which came too late because transistors and other things came. They were little electrochemical cells with flexible sides, and cast in polyethylene with wires fused into them. There was a platinum membrane with a hole. On one side was a KI-iodine solution of some concentration. On the other side was a KI solution with no iodine in it. This little hole permitted the movement of the concentrated solution through here. When that concentrated solution came through, it put an electrical bias on the current hook-up where you could read it.

Now, what made it go through? Well, the flexible plastic membrane, sitting in the water, would pick up the signature of a submarine. The wave would come through, push this thing through, and would do it in a certain fashion so you actually got a signature by reading the current. That's what they were doing. That was the classified work, among other things they were doing.

The question was, "What do we know about that system?" One of the things we didn't know was the effect of the motion on that little hole. So Ray and I devised a dissertation topic which was very legitimate. That is, to determine the electrokinetic potential by electroosmotic flow of a platinum tube of about the same dimensions as this hole. It was great because those guys could pay for things, like buying a piece of platinum and the instruments that we needed to make measurements.

Ray Hurd was a fellow who had a truly green thumb in research. He could jump to a particular point without intermediate understanding in ways that were hard for most people to do. He did a fine piece of work. I forget where he published that, but it got a lot of attention at the time, because it was one of the few papers in electrokinetics that had appeared for a long time. Way back they had done some electroosmosis, and this was just at the time when people were beginning to get interested in electroosmotic water recovery from sea water. That was a good piece of work. At any rate, that's what DRL meant to me at the time. It gave me a chance to support some students.

What I had started to tell you was that Boner, who had been the associate director of the Harvard Underwater Acoustic Laboratory, came back to Austin. He had been a professor of physics at Austin. He became dean and vice-president. He became the first director of an office of government sponsored research. That may not have been it's exact name. But he got tired of that and went back to DRL.

This fledgling little office, which had two people in it, was therefore unheaded by a faculty member. That was not thinkable at the time, so I was asked by the then-president, who was Logan Wilson, to head it. Then he had a perplexing problem. What do you with a faculty and a non-academic title? So he called me Dean of the Office of Government Sponsored Research. I had a man there by the name of Jens Jacobson who was the functional director, and I was the tie to the faculty.

I did that for a couple years. That was actually my first administrative job outside the department. The reason they did that was because I had been the first to have a grant and by the time I did that it was ten or twelve years old. Besides that, I never complained about other jobs. They didn't pay me any more. Nothing happened except they gave me the title, and I ran the office. I kept my chairmanship as well. The next step, though, was different.

BOHNING: What was the principal function of this office? To find research or grant opportunities for the faculty?

HACKERMAN: No, actually, it was the other way around. It was how to fit this outside perturbation signal into the system without discomforting the system. That's still a problem. In fact, it's a bigger problem now. Now, the office itself—this is an opinion, not a fact—is part of the discomforture. It has grown to the point where it is a canker in the system. It is not an expeditor. It is not something the faculty uses to make themselves feel comfortable. It dictates what the faculty has to do. There's something wrong with it.

When we started, my instruction to that little staff was, "See what the faculty wants. See if we can do what they want with the money from outside. Do your very best not to be bound to the rules we already have, but if you can't do it, then just don't take the money." That's unthinkable now. You don't not take money. [laughter]

That office ran pretty well, mainly because Jacobson was a very able guy. He's dead now. He was very understanding of the faculty and their needs and desires. He did precisely what I told you. He tried to make it fit and if he couldn't, he'd tell all concerned to forget it.

BOHNING: Did this apply outside of the science and engineering area?

HACKERMAN: At that time there was nothing outside science and engineering, although there may have been something in psychology. That probably got started in the early 1960s. They began to get some support, but it didn't apply to anything else. Geography, anthropology, and the social sciences were pretty much excluded. So the answer is it was predominately science and engineering. The engineers had very quietly been consulting for years without ever telling anybody. In fact, they were using equipment and facilities they weren't supposed to. They may not have known they weren't supposed to use it; they'd just used it. They'd do water analyses and things of that sort and charge some bucks for it.

The engineers didn't really get involved with grant operations until maybe the middle 1960s. I may be wrong about that, but I think that's about right. The engineers were operative engineers, not quasi-scientists, as many of them currently are. They got a little upset by the way the scientists were harvesting money and decided to do the same thing.

You may remember that the National Science Foundation really didn't have much of an engineering division until the late 1970s. The engineers really jumped up and down and raised the devil, threatened to have a national engineering foundation. The engineers really got their act together in the 1970s.

So, I don't think they had a lot. It was primarily chemistry, physics, and astronomy; not math. Up until the 1980s, math didn't do a whole lot of this kind of stuff.

BOHNING: Computer science was still a fledgling group.

a) teaching my classes and b) doing my research. They probably thought I was kidding, because they said right away, "Sure." So I took it on and started in September of 1961.

Joe Smiley was a very nice fellow who has a good appreciation of what academics were. He loved a good time; he loved good stories. He just didn't like writing budgets, that's all. I learned one of my first great lessons about administration—that the budget writer runs the place. I wrote the budget. I came in September 1961 and started on the 1962-1963 budget a month later.

I had no experience. I didn't know what you do. I had the advantage of a very fine business manager. He was not yet vice-president for business. It was a fellow named Jim [James H.] Colvin. He and I got along very well. He had come there about a year before that in the first change, when Smiley became vice-president. Jim and I got along very well, and still do. I'm sure he must have helped me over the budget-writing process. I don't remember any details, but we got a budget out that first year. In fact, I was running the university. Joe had a fine time. There were no problems between us. We really got along well. But I learned that big lesson. If you write the budget and know what's in it, you sure as the devil know how to operate the place. It turns out that Ransom didn't like writing budgets either. He was interested in the library, collections, and archives. If you didn't bother him in those three places you sort of could do what you wanted. So, in a sense, I had this whole shabang.

[END OF TAPE, SIDE 3]

HACKERMAN: 1961-1962 was a pretty big time for me. I had a lot of papers out that year, a group of maybe sixteen to eighteen (10). I had four big labs on the first floor of the chemistry building. By that time I had an API project which involved heats of wetting. That was again an interface problem. I started with a fellow named Hung Li Wang.

Because I'd been involved with corrosion in industry, as a consultant at Mobil, and with all the corrosion work I had done, API had asked me about what other information they could get that might help them understand the wetting of oil field formation rock. I said one of them would be the energy of interaction between water and that stuff and oil. So they asked me if I'd write a proposal and I did. The idea was to build a microcalorimeter. Back then microcalorimetry was not that simple; it isn't now, but thermal measurements are more sensitive and insulation is easier. But we built a calorimeter.

Wang got his Ph.D. with me. Some years before he had been working at Mobil in Dallas. He got interested in this program so we designed and built the calorimeter. One day he came to me and said he had a call from his father in Shanghai, who was a teacher there. He was told he had better get back to Shanghai or the father would have problems.

The son had been a Chinese Air Force man stationed at Bergstrom Field in Austin. When the war was over and he was mustered out, he came over to the department to get a master's degree and he chose to work with me. He's a very good guy. He got the master's degree in a year, with no problem. Then he decided he wanted to do the Ph.D., so he stayed and got a Ph.D. with me. Then he went to work with Mobil. He liked it here and he didn't want to go back.

We published a few things (11). I think he went back to China in the middle 1950s. I've seen him twice since then. He went back and wrote me a letter a short time after he got there saying he missed his Plymouth [laughter], that he had a job with the Institute for Petroleum Research in Harbin, and that it was awfully cold up there. Then I didn't hear from him for about twenty years.

Then I suddenly got a letter from him one day in the 1970s, saying that during the Cultural Revolution he had been underground. He had just gone to work in the fields as a peasant because the guys with degrees were getting their ears chopped off. He said that things were better now and he was back in a research center in Beijing, or at that time Peking. He was enjoying his work. It was an institute for chemical physics, I think it was called. Then he came over here while I was still at Rice, I think 1981 or 1982, and he had become director of that institute. I saw him last about three years ago when he gave a lecture at Austin. He was still director, but getting ready to retire. So he turned out to be all right.

The API project then went on with this fellow Bill Wade, whom I told you about. He had got his degree with me on that passivity work. He went out to Berkeley and worked with Seaborg for about three years, and when Wang went back to China I asked him if he wanted to do a post-doc on the API project, which he did. He published some pretty good papers, as a matter of fact.

While he was there, he was one of the guys picked up and made an assistant professor. There was a little ruckus about that because the faculty really didn't like the idea of former students coming back while there was still an active faculty member who was the advisor of that former student. But they didn't complain too much, and I already had enough managerial weight so they were careful. He worked his way through and got tenure. He's currently a professor. He's a real expert in microemulsions, making and breaking them, so he's maintained his interface science. He consults all over the world.

BOHNING: When you started writing these budgets, was that an eye-opener, to see a much larger budget picture than you had just with the chemistry department?

HACKERMAN: No, the budgets weren't that much bigger. [laughter] They were bigger than in chemistry, of course. I've had very few surprises in the administrative area. The budget was what I would consider normal. It wasn't particularly distorted. I've never been paranoid about things of that sort. Some people are. Physics gets much more now. You see, I quickly came up with a solution to that. You don't use dollars, but dollar equivalents. One dollar for a psychologist is ten dollars for a chemist is a thousand dollars for a physicist. There's nothing wrong with the fact that the physicist has a hundred times more. That's equivalent. It worked; it kept people from biting my ears off. It's true, as a matter of fact; there are equivalences. The mathematicians would get their bowels in an uproar. "Look how much more money you guys get than we get." I'd say, "You don't need any more to do the same level of work." But then it got to the astronomers and I was in trouble. [laughter]

Those are simple little things which helped me constantly. You probably can't do that if you take a management course; they've got to be more complex than that. It helped me constantly. I've very carefully stayed away from all these charm schools where you go for six weeks to learn how to carry on management. I've never been to one. I don't object to other people going to them, but I kind of have a feeling that there is too much "by the book" kind of thing. I prefer to write my own book.

BOHNING: You commented last time that way back in grade school one of the things you learned was the importance of self-learning; so that's been a philosophy of yours for a long time.

HACKERMAN: And a procedure too. The things you should get from teachers are enthusiasm and interest, but for the detail you have to do it yourself. That's the way I learned management. I'm not a hail-fellow-well-met. I don't believe I've ever slapped anybody on the back yet. I'm not a quick one to call people by their first names as they do now. I find it a little uncomfortable. But basically, I am sensitive to people and their needs, whether I think their perceived needs are overblown or not.

One of the problems with most management is that the manager pays lip service to the person's needs by describing all the good things he does for him, or she does for him, but without really having any real feeling about it. My best example of that is not me, but this fellow Colvin I mentioned to you. He's retired now. Colvin was a business manager in an academic institution, which is kind of an oxymoronish statement because academic institutions are supposed to be non-organized. That's the way thinking gets done. So a guy who's business manager, which implies a high degree of organization, has to deal with an unorganized system. And Colvin was adept at doing that. That is, he knew how to feel what people wanted and even when he couldn't do anything for them, he made them feel as if he was impressed by their needs.

Business managers in general don't do that. They operate by a kind of a book. Currently up there, for example, if you think you need something, you go through a long rigamarole and may or may not get it; if you do, it's such a long time that you've forgotten about it. My business manager used to wander the campus himself and see needs and attend to them in a hurry. That's something you don't teach; you either have it or you don't have it. You can point out to people, "That's the way it ought to be done," but many times when they do it because you say that's the way to do it, it's done with discomfort on all sides.

I guess being natural is the important thing. Most people aren't natural if they operate out of a book. Those are simple lessons, but they work. I also learned that fairness is in the eye of the beholder. It has nothing to do with you, [laughter] the one who trying to be fair.

BOHNING: I was going to ask about your relationship with the chemistry department, now that they had one of their own in a much different position.

HACKERMAN: I've been very fortunate with things like that. Ordinarily they'd say, "That son-of-a-gun is gone. Let's use him to get what we can, but he's not one of us anymore." One of the things we had at ten o'clock every day was that a half dozen of us used to go over to get coffee. I continued doing that out of my other office now. I'd meet them over at the Drag, as Guadalupe Street was called, and they got pretty used to me. There were no trappings. They'd beat on me then as they used to before and vice versa.

I taught that freshman class every semester. I've never been on leave. I taught every semester at eight o'clock and it was a big class. At first it was two hundred and fifty when it was in a room that held two hundred and fifty. Then we moved to another room and we had five hundred and fifty. Students were used to me. I was not isolated. I had to go to that class. I taught the five hundred and fifty students and whatever number came up afterwards. I had to walk back across the campus. They got used to seeing me. Until I left Austin I had somewhere between ten and twenty people in my lab. I had to go back to the department for oral exams and final orals and things of that sort. So I was in the department a lot.

I'm a restless guy, so I wandered the campus a lot. I'd say to Jim, "Look, that light's been out over there for three days. Do something about it." Between us we had a very much hands-on kind of function and I think everybody on campus considered us as part of the bunch rather than "they" and "we." That made it easier; literally, a lot easier.

It was different at Rice, and maybe we'll come to that later. At Rice I didn't grow up with those people. I was already on a we/they basis when I came. I changed that after a while, but the fact is they were not used to me. At Austin they were used to me; there are no ands, ifs or buts. The fact that I had to be away for a regents meeting or something like that, was totally understandable to them. Now they don't know when the guy's away to a regents meeting because they never see him anyway. It doesn't make any difference. [laughter]

I went to the gym at five o'clock every day and played somebody at squash. I went to class at eight o'clock Monday, Wednesday, and Friday. Occasionally I had seminars, or actually gave a graduate class or two every once in a while. When I was on the campus I was on the campus. I wasn't in my office. I think the students and faculty just got used to me. That's the easiest thing. When I appeared at faculty council meetings or at student assembly meetings, I wasn't from outer space, because they might have seen me that morning in the cafeteria.

BOHNING: How did you feel working in this much broader picture, which got even broader in a few more years as you moved up that administrative ladder?

HACKERMAN: I can't say that I noticed anything. Talking to you like this is one of the few times I've looked backwards. I wasn't following Satchel Paige's admonition, but I've always looked ahead. Whatever I'm doing is what's natural, and the expansion wasn't noticeable.

I guess there were a few things that were noticeable. We had the accident of having a local boy as president of the U.S. During that period of time my wife and I were at the White House a few times, which we would not otherwise have been. That was noticeable. We stayed over there a couple nights and went to a couple of their state dinners.

By late that decade I was on the National Science Board and I began to have contacts in Washington which I hadn't had before, so that was a change. That was a transition. But the other transitions were not particularly noticeable to me, a) because I didn't look back and b) because I was just doing what I was doing.

I guess also because I didn't change a whole lot of things. I added a few things, but I still had my class, which was a very comfortable thing, still had my graduate students, put out about two ar three Ph.D.'s a year, still raised hell getting the papers published. I didn't publish them like they do now, but six a year, that wasn't bad. Roughly that's what it was.

BOHNING: I can remember getting graduate school brochures from the chemistry department at Rice, which was the annual mailing that would go out to our department at Wilkes College. Your position at Rice was prominently displayed in that brochure, and I can remember saying to myself, "How do you keep all the balls in the air with such a diversity of activities?" As you said, you were teaching and had Ph.D. students and were writing papers and still doing very high level administrative work at the same time.

HACKERMAN: I don't know. I mean, it just got done. It was not that hard. I put a lot of hours in. Actually, I began to learn how to do things more compactly, as I went on. I never liked a desk with paper on it. I learned to come in and go through the stuff in a hurry and get rid of the things that could be done quickly and get it down to the few things that took a little more thought.

The proposition is that there are a lot of decisions that you have to make that you can make very rapidly. You're going to be wrong a certain number of times and right a certain number of times and it doesn't matter how long you take on them. The trick is to try to make sure that you got those things out which have importance down the line. Most of them don't. Most of them are simple decisions. Occasionally you run into one that looks simple and isn't, but at least you've narrowed it down.

I used to have a stack of papers this high when I'd come back from Washington D.C. after a couple of days, and in about an hour I could get it down to that. Then those I could take home with me and look at them a little more carefully. So I did develop some procedures that permitted me to speed things up and compact things. I couldn't compact my lectures. I really had to go over them each time because I had different ideas about what I wanted to say to them. I could do something about my graduate students. I could point out to them that, "Look, I'll wander in here but when we talk you'd better say something significant or I'll go on to somebody else."

I don't know. I wonder sometimes myself, because along with all the things that you said, I had a lot of activity in Washington. When I first came to Rice I was on the National Science Board, and within four years I was chairman. I spent a lot of time in Washington, going back and forth.

BOHNING: Maybe if we could spend just a few more minutes, I'd like to at least finish up here looking at the progression of positions you had. You became vice president in 1961 and then vice chancellor for academic affairs in 1963 and then president in 1967, so that's a rather rapid progression up the ladder. Could you talk about how those changes occurred and why they occurred?

HACKERMAN: First, I'll tell you that the changes in 1963 and 1967 were the same change. When Joe Smiley became the president at Colorado—he had been the president of Texas for one and a half years—Ransom, who was the chancellor, wanted to change the system. He had never wanted to be chancellor. He wanted to be the president of the University of Texas, but he had to defend himself so he became chancellor. He came to me with the proposition, "Look, Smiley's leaving. You're vice president and I'm chancellor. Why don't you become vice chancellor for academic affairs for the system?" The system by now had grown. It had Arlington in it. It had the University of Texas at Houston Medical Center. It had the Dallas Southwestern Medical Center. It had some others, about ten in all. He said, "Why don't you become vice chancellor for academic affairs and I'll be Chancellor. We don't need a president. We'll just run it ourselves."

So I had to be concerned about the academic problems for all these branches, which weren't difficult to deal with. I had travel time, but I also had all the activity of the Austin campus. I wrote the budget. Ransom was not interested in that. What he was interested in was protecting archives, the humanities research center, and the library. We came to an agreement earlier that I wasn't going to impact on those. I was interested in the library myself. Archives and the humanities research center less so. The library is obviously the core of the university.

So we went along that way for four years, with me taking care of all the academic concerns of all the campuses plus what would have been the presidency of Austin. By virtue of being the vice chancellor for academic affairs, I sat along with the chancellor on all the regents' meetings, open or closed session; it didn't matter, I was at all of them.

When Frank Erwin became chairman of the board, he became concerned. He said, "We really ought to have a guy named president." That's fine. They got a search committee and the search committee said, "Let Hackerman do it." He said, "Do you want to do it?" I said, "Okay." So I became president. But the minute I became president I could no longer sit inside the regents' meetings, because I was no longer of system status. I now had to sit in the anteroom till they called me. I hadn't realized that. [laughter] But I also didn't have the other problems, so I became president. I had done all of it already. Actually, it was a diminution of the things I had to do because I didn't have to worry about El Paso anymore, or Dallas or anything else. Now there was another vice chancellor of academic affairs, and in principle I had to deal with him, but I had begun to acquire a power of my own. I'd acquired enough power so I didn't have to worry about the vice chancellor of academic affairs.

[END OF TAPE, SIDE 4]

HACKERMAN: It's kind of interesting, because all of these things piled up, all of the activities, and I don't ever remember being very stressed by demands. One thing is that I did it my way and it worked out pretty well. But when I finished with it all, then I had a problem, because then I didn't have that many demands on my time. So I had to pile a lot of things together, but that's a later story.

BOHNING: I guess maybe this would be a point for us to stop then. Thank you again for spending a couple hours with me.

HACKERMAN: I'll tell you, it's actually a pleasure because you're making me recall things I'd forgotten.

BOHNING: Well, I'm glad.

[END OF TAPE, SIDE 5]

NOTES

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4. F. A. Matsen, A. C. Makrides, and Norman Hackerman, "Charge-Transfer-No-Bond Adsorption," *Journal of Chemical Physics*, 22 (1954): 1800-1803.

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10. For a complete list of the publications of Norman Hackerman, see the Chemical Heritage Foundation oral history research file #0083.

11. Hung Li Wang and Norman Hackerman, "Sorption of Gases on Metal Powders and Subsequent Change in Metal Reactivity at Room Temperature," *Journal of Physical Chemistry*, 56 (1952): 771-774.

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