CHEMICAL HERITAGE FOUNDATION

NORMAN HACKERMAN

Transcript of an Interview
Conducted by

James J. Bohning

at

University of Texas at Austin

on

8 February 1995

(With Subsequent Additions and Corrections)

ACKNOWLEDGEMENT

This oral history is one in a series initiated by The Chemical Heritage Foundation, on behalf of The Electrochemical Society. The series documents the personal perspectives of key actors in The Electrochemical Society and records the human dimensions of the growth of the Society during the twentieth century.

This project is made possible through the generosity of The Electrochemical Society.

HACKERMAN, N.

THE CHEMICAL HERITAGE FOUNDATION
Oral History Program

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(Date) Aug. 27, 1996

(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 third of three interviews with James J. Bohning of the Chemical Heritage Foundation, Norman Hackerman begins by reviewing the origins of his association with The Electrochemical Society [ECS], which was related to his interest in the oxygen electrode as a student. He recalls his first paper, presented at an ECS conference and published in the *Transactions of the American Electrochemical Society*, and the first colleagues he met at this ECS meeting. He next describes the character of The ECS at that time, comparing it with the American Chemical Society [ACS], as well as the origins of the society's journal and his involvement in publication and editorial activities. Hackerman touches briefly upon his committee work before examining the growth, structure, membership, and functions of The ECS during his appointments. Finally he describes achievements and obstacles during his tenure as Vice President and then President, and his view of the Society's influence on electrochemistry and related fields.

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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LOCATION: University of Texas at Austin

DATE: 8 February 1995

BOHNING: As I indicated, this particular interview is designed by The Electrochemical Society as part of a series on past presidents (1). We have an agenda that you have in front of you that I'd just like to go through (2), beginning with your origins of interest in electrochemistry. You and I have talked about that briefly before, but maybe we could just review that for this purpose.

HACKERMAN: Yes. Well, let me give you a little background. At the time I was at Hopkins [Johns Hopkins University], you may have read someplace that there was a deep depression, [laughter] so it seemed desirable to get out of the university, although I did get my degree in three years. But I was researching two problems. One problem had to do with electrokinetic potentials—the electrophoresis problem. Another one had to do with sulfur monochloride, not related to electrochemistry. Somewhere along the line I got interested in why the oxygen electrode wasn't reversible. You still can't make a reversible oxygen electrode. I tried to do things with graphite. Had I known about fullerenes, I might have used them, but I got interested in electrochemistry by virtue of this irreversibility of the oxygen electrode. That was in about 1935-1936. It was followed by an early interest in some corrosion problems, which came, I think, from a consultancy arrangement; I'm not sure about that. It was about that time that I became aware of The Electrochemical Society.

BOHNING: Was that through a professor, or were you aware when you were still a student?

HACKERMAN: I was still a student, and just transferring into the non-student category. I was finishing.

I really can't remember whether Dr. Walter A. Patrick, the fellow with whom I worked, was aware of The Electrochemical Society or not. The other people on the faculty I know were not. He might have been. I suspect it was because I began looking in abstracts to see what I could find out about the oxygen electrode, and I saw numerous references to the *Transactions of the American Electrochemical Society*, as it was then called. That's what I think is the origin.

BOHNING: Okay.

HACKERMAN: I can't remember an electrochemist in my student background, so it was probably self-induced in the second half of the 1930s. It was instigated by this oxygen electrode problem.

BOHNING: When did you first join the Society?

HACKERMAN: It was in the forties. Again, I'm finding it difficult to remember whether it was before I came to this place, the University of Texas, or not, but I think the first paper I published came out of here in 1946. Now, I remember that pretty well, because I was interested in the passivity of chromium. The paper was called something like, "The Electrolytic Corrosion of Chromium," (3) something like that. It was in the *Transactions of the American Electrochemical Society*, 1946. The student was Don Marshall, D. I. Marshall. That's the first tangible evidence I have of being in the Society, but I really can't tell. I know I joined before that because, as I say, I've been in 50 years.

BOHNING: That would be 1944 or earlier.

HACKERMAN: Yes. At least 1944 or earlier.

I developed a fairly good interest in corrosion while I was at VPI [Virginia Polytechnic Institute] in 1940, 1941, 1942, part of 1943. So I probably joined at that time. I do remember that chromium paper because—I'm getting a little recall—it was the first paper I delivered on my own. I was the chief in this case. I came from Austin because I used the Southern Pacific Railroad through San Antonio to New Orleans and then Birmingham. The meeting was in Birmingham, Alabama. The format at that time was the format that the Faraday Society was using. You presented a paper, there was discussion afterwards, and then the whole thing appeared as a *Transaction*. That was the only way you could publish it with The Electrochemical Society.

BOHNING: Didn't the paper have to be submitted and written in advance?

HACKERMAN: It had to be submitted in order to go onto the program.

BOHNING: Okay.

HACKERMAN: Some peer review process compared it or something, but you couldn't just submit it to the journal, because there was no journal at the time. I can remember a long train ride, and I can remember talking to the fellow at the University of Alabama who had reported the discovery of element 87, Alabamium. The same guy did Virginium and Alabamium, I think. Do you remember 85 and 87? He turned out to have been wrong.

BOHNING: Oh, yes.

HACKERMAN: At least the one who did the Alabamium report was the one I talked to at the Birmingham meeting. It was my first real contact with the Society close up. I'd been to American Chemical Society meetings, but even then, when you went to them, you didn't see the president or the editor. It was already not a very cozy kind of meeting. It wasn't as bad as it is now, though. The American Electrochemical Society was different. This was a small group of maybe two hundred people in one hotel, and they talked continuously—either formally or informally—for whatever number of days the meeting lasted, which I can't remember. So I was very much impressed by that.

BOHNING: Was there just a single session then, people giving papers?

HACKERMAN: I think there were no parallel sessions.

BOHNING: Okay.

HACKERMAN: I don't know how they did it, whether with this or somebody transcribing it in shorthand or whatever. They took down everything that people said in the discussion, it was all edited, and the whole thing was published. Have you seen the early Transactions?

BOHNING: No, I haven't seen the early Transactions.

HACKERMAN: Well, it petered out shortly after. But from the time the Society started in 1902 to 1948 or 1949, it was all Transactions.

BOHNING: How many other people did you meet and start making close contacts with at that time?

HACKERMAN: Well, there were two people I remember particularly, maybe three. One of them was Bob [Robert M.] Burns who, at the time, was director of the Chemical Division of Bell Telephone Laboratories, a very prestigious position.

BOHNING: Oh yes. I know the name.

HACKERMAN: Yes, Burns was a very able guy. He was a Princeton graduate, as I remember, a Ph.D. He was interested in materials. He had a book on coatings (4). Another guy was Herb [Herbert H.] Uhlig. Uhlig was a professor of metallurgy already, and he had very high standing in the field of corrosion. He edited the *Corrosion Handbook*, which was—still is to some extent—a bible, I think (5). The third fellow I got to know there was Frank L. LaQue, who was vice president for research and development at INCO, International Nickel, down in New York. Now these three people had established themselves pretty well. Remember, I was a newcomer. I was an assistant professor here, yet they were available. They sat and talked with me. I'm sure there were others. The secretary of the Society at the time—I can't remember his name right now. I might have it here. He was at Columbia in chemical engineering. He'd been one of Colin Fink's students, I think, and he'd sure beat me up for forgetting his name.

I think it was Henry B. Linford.

BOHNING: I thought I'd find it in here, but I don't right away. There's a listing of all kinds of people, but I can't find the secretaries.

HACKERMAN: It would be in the forties. Anyway, those are the people I remember from that first contact. I was pretty much impressed by them.

The members of the Society were a very homogeneous group of people. We all had basically similar interests. However, there were already divisions—Organic, Electroorganic, Corrosion, and one called Physical Electrochemistry. Let's see, what else was there? D&I, Dielectric and Insulation. That kind of thing. There were about a half-dozen divisions. But the Society was so small that even though occasionally there might have been parallel sessions, most of the time everybody got together. That was the nice thing about it. It was very noticeable

compared to the ACS. I understand that The Electrochemical Society was not a spin-off of the ACS, but that it was a breakaway. Do you know that story?

BOHNING: Well, there's a little bit of it in here. The ACS was reluctant to form a division of electrochemistry.

HACKERMAN: Yes. Fink and Charles L. Parsons, whom I think was secretary of the ACS, apparently didn't get along.

BOHNING: Okay.

HACKERMAN: That's secondhand. I didn't know either of them directly, but secondhand sources say that there was a dispute. They wouldn't form a division. Fink just seceded—I guess that's the right word.

There was another thing. The ACS at that time was pretty pure chemistry. I don't think they had any chemical engineering in it. I know they didn't have biochemistry, because there wasn't any. In fact, there probably wasn't chemical engineering when it formed. The people who were in that in the first third of the century were chemists who enjoyed chemistry. Some of them I know got to the position of believing that application was dirty; it was prostitution. That still hangs on with some people. Electrochemistry at that time was predominantly applications—plating, coating, corrosion, that kind of thing—all of them less than acceptable in pure chemical circles. So it was easier to break away. You'll notice that the ACS doesn't have a journal of electrochemistry. It gets close in the Journal, which has papers in electrochemistry in a so-designated section. The *Journal of Physical Chemistry* does the same thing. But they don't attract the good papers. I shouldn't say that. They don't attract the bulk of the good papers. So the Society was a very nice place for a young guy to get involved because you could feel involved right at the beginning.

BOHNING: Would you say that academics in the Society were in the minority then?

HACKERMAN: Yes. The Society had a noticeable industrial leaning. Industrial electrochemistry was one of the divisions. Buffalo was a major site for meetings by virtue of the fact it was a center of the electrochemical industry. Of course, the meetings were all in the Northeast someplace, but they got to Buffalo about every fourth or fifth time. The main place in the Society that had "pure" chemistry was the division labeled Physical Electrochemistry. This was the division to which I adhered first; then corrosion came later. I developed a pretty close

friendship with Burns, with Uhlig, and with LaQue, for that matter, and with some others—Bob [Robert] Mears, who was research director at Aluminum Company in Pittsburgh. Some others I can't recall right now—Mars G. Fontana, who was at Ohio State University at the time. All these people were active. Some were certainly less pure chemists than I was, so I helped hold up the left wing of the Society. [laughter]

But I had lots of interest in application. I have always thought that pure chemistry, or pure science for its own sake, was all right, but not for me. I preferred to have it tied to something pertinent to society; but when I got into that something, I'd try to get as much purity as I could. When I got into the corrosion field, for example, adsorption hadn't been much used in explaining observations. When I attempted to explain corrosion and inhibition by adsorption phenomena, there was a certain amount of tutorial needed. [laughter] But it did catch on, and it spread very quickly. I'm not trying to say that these guys didn't know what they were doing. They just didn't have that same interest.

The ECS was a very comfortable society right from the beginning. From the time I made this first trip in 1946, I pretty much went there, not exclusively, but I stopped going to the others—to the ACS meetings, for example—except occasionally. I retained an interest in non-charge transfer interface science, which meant that I had to go to meetings of the Colloid Division of the ACS to get some of the things I wanted. The Electrochemical Society is more complex now, but it certainly remained the way I described it above for at least twenty years. Then it began to grow a little more complex. I enjoyed it. I went to every meeting, you know, twice-a-year meetings. When they asked me to do something, I did it. That's one of your questions here (2); what reason did I have for becoming active as a Society member? Activity. People asked me to do something, and I did it. That's all I know. [laughter]

BOHNING: Okay.

HACKERMAN: Of course I enjoyed the people, and I enjoyed the field. I think I was president in 1955 or 1957, something like that.

BOHNING: In 1957.

HACKERMAN: In 1957. So that was about ten or eleven years after I got in with both feet. I'd done an awful lot of work in the meantime for them. It was easy to do, and they needed somebody to run for a third vice president. They asked me, I said, "Okay," and they got me in line. They still do it—get elected to third, move to second, to first, and then by election to president.

BOHNING: I see.

HACKERMAN: That's the way it works. The president's position is in the last year of the sequence. I think the president stays on another year on the board. Then you rotate off. I rotated off in 1958.

BOHNING: So you actually started that third vice presidency within eight or ten years after you went to that first meeting.

HACKERMAN: Right; 1957, 1956, 1954. I probably was elected in 1954; about eight or nine years.

But in about 1949, Burns and Uhlig got their heads together and, with others, decided that publishing Transactions was not the way to go and that they had to go to a journal—a journal in the true sense of the word—where papers were submitted and accepted or rejected, and published without discussion. The Society did go over to that either in 1949 or 1950. Burns was the chairman of the Publications Committee, I think, and Uhlig was the editor. They had a lot of trouble; they didn't get many manuscripts.

BOHNING: In what direction were they going?

HACKERMAN: Well, I guess they'd begun to spread out some. The National Association of Corrosion Engineers had started, so of course some papers were going there. The industrial electrochemists were saying, "This is too esoteric for me." They started getting out of the *Journal of the Electrochemical Society* a sense of the less pragmatic publications. The *Journal of Physical Chemistry*, at that time, was called the *Journal of Physical and Colloid Chemistry*. The sequence is not certain, but in about 1951, I remember Uhlig calling me and asking me if I would take on the job of technical editor. He would be editor and do the writing of editorials and worrying about makeup and all that stuff. I would be chairing the publication committee which would do the reviews. I did that until 1969, twenty years, at which time I became the editor. I'd been the editor for a long time, in fact. After Uhlig left, Cecil King of NYU became the editor for a while, and then I became editor. I held that position until 1989 or 1990. So I was involved in the Society's editorial activity from 1951 to 1990.

BOHNING: That's a long time.

HACKERMAN: It's a long time, [laughter] and I did it through two university presidencies. But I had one or another assistant working with me all the time.

The journal went through a drop in manuscript submissions—one issue had had three papers in it, and it probably was three out of four manuscripts received We began to talk it up at the Society, because Society members were publishing elsewhere. In about two years, it had grown back to being a full-size journal. The original size was about nine inches by seven inches. We went to the full-size format of eight and a half inches by eleven inches, probably by the time I was in the vice presidential sequence. I did both of those at the same time. Also, I was president and technical editor at the same time.

By the sixties we were beginning to have the problem of, "Can we pay for this stuff?" We instituted page charges which we hadn't had before. We used to give away the reprints, which we had to start charging for. I remember we talked about the advertising, and although it was anathema to most of the people to go after advertising, because that obviously is prostitution of a sort, we did finally.

The journal now is a very robust journal. The Society began to acquire interests which came to the edge of, say, physical electrochemistry or dielectric and insulation. From dielectric and insulation, it wasn't hard to get to semiconductors. The first thing you know, we had lots of papers in that area. We then divided into an electrochemistry section and a solid-state section. That's the way it's still set up.

BOHNING: At the time of the change from Transactions to the journal, did the meetings also change? That is, you no longer had to submit a paper in advance?

HACKERMAN: Yes. You now had to submit a title and abstract only. Also, the divisions began to grow as interest in the fields stirred up.

You know, there's a philosophic difference between the way chemistry was prior to the big push in the fifties and following the Soviet space—

BOHNING: Sputnik.

HACKERMAN: Sputnik, yes. The more people came into the field, the larger and larger was the percentage of people in the field who were interested in application. Since The Electrochemical Society had that kind of a cachet to it, it began to grow pretty rapidly. A reason could have been that, at that time, the purity of the chemistry in the ACS was not something that

the new people wanted to talk about. They wanted to talk about use. In fact, a lot of societies started up. The NACE [National Association of Chemical Engineers] was about ten years old at the time, and other corrosion societies started up at that time also. So The Electrochemical Society probably gained by virtue of the fact that there was a sizable influx into the field of chemistry, and into the field of science, for that matter. Those who had a better appreciation for application tended to show up in The ECS area. The journal gained with it, so that it went from the three manuscripts—three papers published—to where I was fielding six or seven hundred manuscripts a year. Particularly by the time I'd gone to Rice [University], it was at least that big.

BOHNING: What was your rejection rate? How did that fare over that time?

HACKERMAN: It wasn't too inconstant. I would say it stayed at about twenty percent of true rejections; there were maybe ten percent where authors decided to withdraw, so I guess we published six to seven out of every ten we got. It was lower at first and higher later, because when people began to understand they were being scrutinized, they didn't submit papers that couldn't stand the scrutiny. That's a fairly standard thing. I'm sure we got rejects from other journals at the beginning, but all manuscripts got the standard peer review by people in the field.

This is before there was a certain amount of feeling of insecurity, with some peer looking at your fresh work. Currently, people who write proposals are constantly concerned about the work being lifted. With papers it's not as bad, because they're a year behind everything else. At first the peer review was indeed just exactly that. People did look at it; they looked at it carefully, thoughtfully, and not with any other intention except to have nothing but the best of them in the literature. That's a different matter. There are variations on that theme now which are disturbing.

BOHNING: In what way?

HACKERMAN: Well, it leads to turning down papers that perhaps ought to be massaged further and used. It leads to having ideas ahead of time rather than behind time. It leads to the kind of thing that Neil [Elbridge] Gordon was trying to develop when he set up the Gibson Island Research Conferences that became the Gordon Conferences. They made possible conversations between peers from universities and from industry and government, but without recorders and without photography. Just talk; no notes. The general understanding was that you would not go out and talk about this stuff until it appeared as a publication somewhere.

[END OF TAPE, SIDE 1]

BOHNING: Were you the one who set up the peer review, or was that in place before you took over as technical editor?

HACKERMAN: It was in place. We had divisional editors—Physical Electrochemistry, Corrosion Division, D&I, and Industrial Electrochemistry. My job as technical editor was to look at the paper—I saw all the manuscripts—and decide which divisional editor ought to deal with it, then send it to him—and occasionally her; there weren't a lot of hers—with some comments on it, such as, "Better give this close scrutiny because I can't tell what it is, but I can smell something"—that kind of stuff, intuitive stuff. At first I used to write on every one of them, but at six or seven hundred, you don't have time. When we were down to fifty, I could do it; that's just one or two a day. So I suspect that the peer review system was in place when I got there. Uhlig probably set it up.

BOHNING: We were talking about the peer review origins. Essentially, you set up a pool of people to whom you sent out manuscripts. You had reason to believe they would be good reviewers.

HACKERMAN: Yes.

BOHNING: Did you find that you had to massage that pool a little bit? That there were people you couldn't rely on after a while?

HACKERMAN: Yes. You found out which people gave you cursory reviews; that was the more important. Then you found out those who put it under a pile and you didn't hear from them for six weeks. Or six months, for that matter. You also tried to avoid the people who did gratuitous insults—"What's this guy saying?" instead of, "This doesn't seem to be right." That's not quite the same thing, is it now?

BOHNING: No.

HACKERMAN: We developed the reviewers through the divisional editors, with whom I met twice a year. Of course they were also available on the phone and by mail, so I got to know them pretty well. The problem was that in order to diminish the load on each DE, we had to have multiple DEs for each division. The first thing I knew, I had about eighty divisional editors. That became noticeable at these meetings; you had to talk loud.

When I left, they went to a different system, a better system. We didn't pay anybody anything, by the way. We didn't give them secretaries. I had a part-time secretary, but none of the DEs had secretaries. Now they do just like the JACS does; they have eight or ten of these guys, give them secretarial help, and they pay them five or ten thousand bucks a year.

So through those editors, I developed quite a package of people I could turn to for different kinds of things. Sometimes I bypassed the DE. If there was something I was particularly interested in, I'd do the reviewing myself. I don't know where the lists of reviewers are, but we had long lists in the business. We never had to publish them at that time.

Some years ago, a review was a review. You might not like it; you might think the reviewer was a nut or a stinker or both, but you didn't have much recourse. Now, you had recourse to me. I would look at the whole package; if it looked as if the author was really getting diddled, we'd send it to a different set of reviewers. Then if their review was diametrically opposite, we'd go to work with another set. We spent a lot of time on those things.

BOHNING: Oh, I can imagine!

HACKERMAN: Yes. It wasn't done out of hand.

BOHNING: Well, and you had so many other activities through all this period anyway. To me, it's astounding that you were able to do this journal business.

HACKERMAN: Well, this was the journal pile; this was the other pile.

BOHNING: As you said, you must have had some efficient help.

HACKERMAN: Oh, yes. In fact one of them, the lady in Houston, is still down there, Jackie Bourne. She was great.

Do you want to know what other pre-presidential activities? I think I told you I didn't fiddle with sections or divisions. Publications is where I did all of my work.

BOHNING: Well, I did read someplace that you were on a committee way back in 1950 that designed the Palladium Medal award.

HACKERMAN: Oh, yes, that's right. I'd forgotten about that. The idea of the Palladium Medal stemmed from a number of places, but Uhlig and Burns were the real drivers in that Society for ten years. The Society had an industrial electrochemistry medal, which I ought to remember because I got one of them; it has a name on it. That's about all they had, that one. Herb wanted to have something in physical electrochemistry that represented theory, the less practical side of the house. I'm sure it was his idea to use the word "palladium." Pallas Athena is the Greek goddess of something. That's where that came from.

So the Palladium's a nice white medal; it's more or less stainless. In fact it is stainless, because its corrosion products won't discolor the system. The sketch of the medal that was drawn has the goddess on the front; I don't know what was on the back. I think maybe it has an Electrochemical Society seal on the back. Carl Wagner got one of them, Uhlig got one of them, I got one—the early ones.

BOHNING: You got it in 1965.

HACKERMAN: Yes, in 1965. It came every other year—still does, I believe.

Yes, I served on a number of committees, but I don't remember those things; the big things I remember. The editorship I can remember, but I didn't have a local section to belong to, because there weren't any down here. I did work in the Corrosion Division and the Physical Electrochemistry; I'm sure I was chairman of both those divisions at some time. If you say I was on the Palladium Award Committee, I probably was. Other than that, I don't know. I was always busy at those meetings, I know that.

BOHNING: You were chairman of the Corrosion Division in 1950-1951, so that first ten years you were a member of the Society, you were extremely busy within the Society.

HACKERMAN: I was busy.

BOHNING: You really were.

HACKERMAN: Maybe they were looking for a sucker. But I wasn't a sucker in any sense since I enjoyed it. I didn't particularly want to be president, but that's the way it worked.

The important thing about the Society at the time was, it was always broke. I mean they were always sucking air for money. It was about in that period, in the middle 1950s, that they decided to go after advertising. We also, at that time, decided that each of the divisions really ought to take a look at itself and see if its charter was correct, and there were some variations. There was an abortive effort to establish another journal. It lasted only a short time, four or five years. What was it called?

BOHNING: I remember reading about it. Was that *Electrochemical Technology*?

HACKERMAN: Yes, *Electrochemical Technology*. It was an attempt to reestablish the position of the Society as being industrially oriented. I think what Uhlig, Burns, and I did was to switch it into, not a theoretical society by any manner or means, but a greater appreciation of background chemistry on which everything else is based. Some of the people in industrial electrochemistry flew by the seat of their pants in other sections. The D&I division, Dielectric and Insulation, was the same way. We knew what was a good insulator, and we made a small variation on that. They actually didn't like it. In fact, that probably led to the establishment of some small societies that were specifically oriented toward the electrolytic industries at the time, for example, alkali production. *Electrochemical Technology* was designed to show that the Society was interested in that area. It failed because there just wasn't enough interest to keep it loaded with papers, and it became a burden to have two journals when in fact one was sufficient. The burden became evident fiscally to the industrial people. They quit backing it, and it disappeared.

BOHNING: It would seem that industrial people would be reluctant to publish papers anyway.

HACKERMAN: Well, they liked reviews. You see, they want lots of reviews.

BOHNING: Okay. I see.

HACKERMAN: In fact, every division had a review every year, and we didn't have room in the journal with all the manuscripts that were piling in. The authors who were being peer reviewed didn't really like the idea of being mixed in with a bunch of reviews that were not peer reviewed. You couldn't peer review those, really, because you went to the expert and said, "Tell us what happened last year in the chloroalkali industry or in aluminum production." You went to the one you thought was the best person, so for peer reviewing, there were no peers. At any rate, the editors would have to read that stuff and decide whether the fellow who was talking about paint and its formulation and application knew what he was talking about, and the editor didn't know!

If you had sent it out to somebody in the field, the author would hear about it. [laughter] So it started out dead in the water.

The one they're doing now, the *Interface* one, is quite different. I don't know if you know *Interface*.

BOHNING: No, I don't.

HACKERMAN: It's a second journal; it has review articles in it. How they review it I'm not sure, because I've never been involved with it. It has all the news, all the meeting paraphernalia which we used to publish in the journal, and so on—quite a few pages a year. All that's now in the quarterly journal called *Interface*, which for example has a lot about the winners of the Young Authors Award, that kind of stuff—very nicely done. It takes a load off the journal, but it doesn't put an unbearable load on trying to find these review papers. Now, I notice that the editor of *Interface* has been moved to the editor of the journal, so he can't do *Interface* anymore.

BOHNING: When *Electrochemical Technology* started, had more academics joined the Society by this time?

HACKERMAN: Yes.

BOHNING: Through your influence?

HACKERMAN: They had increased. I don't know whether it was my influence or not, but more of them came, and the industrial people who came were younger and more apt to listen to the university people. Now, the university people never did learn to listen to the industrial chemists. That's an overstatement, but you get the flavor of it.

The Society went from several hundred to a few thousand members. The ratio of academic to industrial probably never varied a whole lot, but the voices were predominantly academic. From the time I was very active in there, that certainly was true.

We couldn't emulate the ACS. For example, we couldn't have an employment clearinghouse of any great consequence. But we could have a little thing, much more informal—to help people get jobs, for example—and we did set one of those in place. Never called it a clearinghouse; that would have been overblown. Our meetings would have one thousand to twelve hundred people there; ACS meetings have ten, twelve, or fifteen thousand. Incidentally,

the proportion of members who came to meetings was much higher in The Electrochemical Society than it was in the ACS. The ACS gets ten or twelve thousand, but they've got a hundred and fifty thousand members. We'd get one thousand, and we had four thousand members.

BOHNING: Quite a difference.

HACKERMAN: Yes. It was characteristic of the differences between the two because the society members knew each other, more or less.

BOHNING: How does the dues structure compare? I don't have any feeling for that at all.

HACKERMAN: It did not escalate anywhere near like the ACS's did, because the superstructure is still very small compared to the ACS. I think there are probably six or seven thousand members now, and the number of people in the Pennington headquarters may not exceed twelve. The ACS has a hundred and fifty thousand members, twenty times as much, but they probably have six to seven hundred people working for them in those two big buildings in Washington. This does not include the staff involved with Chemical Abstracts, a very valuable entity.

They've become a lobbyist group. They wouldn't like to hear that. But they go on the Hill a lot, how's that? That kind of thing. The percentage of technical aspects is less than it is for the smaller societies, The Electrochemical Society being a case in point. I've always been concerned about the ACS no longer being a scientific society.

BOHNING: Well, let me ask about another general aspect of The ECS. That is, I don't remember seeing anything about a focus on education.

HACKERMAN: No. That really wasn't—still isn't—our agenda.

BOHNING: Was that done purposely?

HACKERMAN: Oh, I think just by difference. Or indifference. They never looked on that as being a major target for them, at any point in my time.

When I was editor, I rarely wrote editorials. I thought they might have massaged the ego of the editor, but by and large there wasn't much point to it. I did write one once; I'm not sure I ever got it published. Occasionally I'd edit the manuscript, just for the hell of it, and this was probably a fifteen-pager. I've always been concerned about the number of words that people use to say what can be said in five or six or seven. I edited one manuscript and took "the" out. I took it out enough times to fill a page. I removed about two hundred and fifty of them. I know I considered writing an editorial with nothing but "the" in it. I can't remember if I ever did that or not, but I've never really felt the necessity to produce them regularly.

BOHNING: While we're just mentioning the education aspect, did electrochemistry ever come into its own in an academic environment? Were there any places that gave courses? It wasn't a standard kind of thing.

HACKERMAN: Chemical engineers, some of them.

BOHNING: Okay.

HACKERMAN: Chemistry, no, except in a few places. I had it here, for example. Fontana gave it at Ohio State. Uhlig gave some courses at MIT, but not in the chemistry department; his was in metallurgy. Fontana was in chemical engineering; mine was chemistry. There were certainly a few others but the answer is no. At best, it was two lectures in physical chemistry. It's quite different now. Electrochemistry's big stuff these days. I mean the organic chemists use it, the biochemists use it. It's a great tool. You've got the kind of control that before lasers you didn't have with light, so it works very well.

BOHNING: But if all you had were two lectures in physical chemistry, how did you get people to become electrochemists?

HACKERMAN: Here, I'd given my version of what was a good Ph.D. problem. Now, the ones that I've got, I must say, had a predilection towards applied stuff. I've had about seventy Ph.D.s. I'd say fifteen of them, maybe twenty, went into academic work. Of the physical chemists, it might be the reverse. So most of mine went into industry by preference.

BOHNING: Was the Society able to attract chemical engineers?

HACKERMAN: Yes. Here's the way that they became attracted. Someone would get a chemical engineering degree and go to work for Monsanto or Exxon, and they'd assign him a corrosion problem. He'd be coming to The Electrochemical Society to find out what that was, what to do next. [laughter] They went there or to the NACE or both. Now, back in the earlier days, going to the meetings was a fairly simple thing, in the fifties and sixties and seventies. Now it's not so simple. But, yes, they did it by virtue of the jobs that they picked up.

BOHNING: One of the people I'm going to see next month is Charles [W.] Tobias, who was an electrochemical engineer (6).

HACKERMAN: Right. He coined the term, as far as I can tell.

BOHNING: I'm wondering what influence he had in the Society in terms of attracting people.

HACKERMAN: A great deal. Tobias is a first-rate scholar, and it stands out all over him. So he certainly attracted some very able people at [University of California] Berkeley to work in his lab, and his influence on the Society was big. He was one of my divisional editors for a very long time—in fact the whole time I was there, I think, from the time he came till I left. You'll find that his influence was sizable, particularly in mass transport problems in electrolytic processes. Some of his better students are really high-standing people.

BOHNING: Just as an aside, another person I'll be interviewing is [Harold J.] Read (7). He's in Florida.

HACKERMAN: He lives in Florida now? He was at Penn State at the time I knew him. Read was actually the first technical editor of the journal, now that you mention it. I believe that when Uhlig started that, Read was his technical editor, and then, I don't know what happened there. That's when they brought me in.

Harold had a pretty good influence, I would say. You know, he came from a good school and is a very active and thoughtful guy, but he didn't spend nearly as much time as I did on the Society, for example, not after a while. He was president before me or after me, I can't remember.

BOHNING: I don't remember. Let's see. You were 1957, and he was 1966. He was after you. Then Tobias was 1970. I just noticed a name here; Uhlig was 1955.

But I saw the name of [William Reed] Veazey from Dow. W. R. Veazey.

HACKERMAN: It would have been early.

BOHNING: It was 1945.

I'm assuming that the Dow people would've been very active in the Society.

HACKERMAN: Absolutely. They were.

[END OF TAPE, SIDE 2]

BOHNING: You already said they asked you to run for third vice president. You ran, you said yes, and that's how you became the president.

HACKERMAN: Right up the line, yes.

BOHNING: At that time, what kind of an agenda did you have in mind, in terms of exerting a lasting influence on the Society?

HACKERMAN: Well, I've got to think about that a little bit. I don't think presidents <u>have</u> a great deal of influence on the Society, to get right down to it. The Society has a momentum of its own, and the very important people are the staff who are dedicated to it. But, what you can do in the several years that you have contact—if the Society was getting too theoretical—is to push it back to a moderate position; or if it was too industrial, to push it back to a moderate position. The big thing, during my time, was to make that journal work. If the journal failed, the Society was going to close down. That was <u>the</u> thing that it had going. I spent most of my time on that and trying to make sure that the meetings were of the kind that the members wanted. You could detect that better back then when we had our meetings in places like Buffalo and not Honolulu. In Honolulu you can't tell whether it's the papers that are of importance, but in Buffalo you can. [laughter] I'm not big on Buffalo, but—

BOHNING: No, no. I understand. It's a good point.

HACKERMAN: You know, Birmingham, Alabama, that kind of thing. It was to make the Society as effective and useful to the members as possible. The other problems were all nitty-gritty—"How do you pay for this?" "Are the dues high enough, or are they too high?" "Are three people too much for the staff?" An awful lot of that kind of stuff. So it is a management problem. Philosophically, the Society, I thought, was doing what it was supposed to do. It had its hands full paying for the journal.

BOHNING: Did you have to maintain a public interface that year? Did you go out on the road?

HACKERMAN: It doesn't have nearly the number of sections that the ACS has, but I'd say I went to ten of them, concentrated in the East because that's where most of them were. There was a section down here by that time, there was a section in Los Angeles, there was a section at Hoover Dam. There weren't a whole lot of sections, but I went to maybe a quarter of them, maybe a third. That's kind of worthwhile; people see somebody live out of headquarters. But it's not a big thing. The big thing was to maintain the finances so that you could do that which you thought was important. The two things that were important were the journal and the meetings.

BOHNING: What kind of feeling did you sense from the meeting with these local sections? Were the members happy? Did they give you good feedback for improvement?

HACKERMAN: They were not unhappy. I won't say I didn't find any suggestions for change that were of some use, but I don't think there were a whole lot of them. They knew what they felt, but they didn't know anything about the workings of the Society. So I'd tell them how it worked. Then there's what changes would you make, if that were the arrangement that you had to deal with. Basically the problem was, occasionally, that dues were too high. They were 15 bucks at the time or something like that, which was pretty good-sized money. Or, "It's too far to go to get to the meetings. They ought to have the meetings closer to me, say, have them in Cincinnati so I can get to them." Or, "You jokers don't know a good paper when you see one." That kind of stuff. I'm not being derogatory on the business of moving around, because you can't tell when the good idea's going to come, so you have to scrub the place. But by and large, there was no major complaint. "It takes too long to review a paper." It used to take eight months or nine months from taking the paper in to publishing it. Now it takes about a year and a half or a year and a quarter. They didn't like it then and they don't like it now.

BOHNING: Right.

HACKERMAN: So, my answer is: The combination of concern about the length of time always led us to a study of how long does it really take, and whose fault is it that it takes so long, because after all, the authors are involved in it too. If the author holds it four months and it takes eight months, that's not bad. That kind of lecture I'd have to give them once in a while. By and large, it was a nice give and take, and I think they felt they knew more about the Society. I didn't always take away a feeling that I'd learned anything, except that there are certain common complaints, and they are common! It just spread across the whole thing. Most of those complaints you can't do a whole lot about, but you might be able to do something about them.

BOHNING: You've indicated that all of your division editors were volunteers with no pay at this time.

HACKERMAN: Correct.

BOHNING: And you were getting some money for a part-time secretary. What about the year that you were president? Did you finance your traveling and everything?

HACKERMAN: No, the Society paid for a Society trip. I'm pretty sure of that. Back then we had some pretty archaic regulations here at the University of Texas. If you flew, you were on one per diem until you crossed the border, and then the per diem changed. [laughter] Plane or train. But inside the state you were on, say, six dollars a day and outside the state you were on eight dollars a day, [laughter] so travel was not that easy at that time.

BOHNING: How was it working with the Society's headquarters office?

HACKERMAN: All right. We had some nice, able people. In my total period, I went through three Society secretaries; I'm not talking about the staff. They were all good people; they all, I think, did very well. I don't think the Society was competitive in wages and other employment benefits. In fact, they moved out to the country, to Pennington, to sort of compensate for what they couldn't pay to live in New York. In fact, that was a good deal; they got better people who didn't want to live in New York. It's never been a high-paying place. If you wanted to make the most money, that's not the job you'd take. But they're hardworking people.

Henry B. Linford was the secretary. [laughter] I don't know why that suddenly popped in.

BOHNING: Well, we've talked a little bit about accomplishments and problems. Is there anything else you want to add to that?

HACKERMAN: No. Nothing I can think of.

BOHNING: We've talked a little bit about the relationship of the Society to the ACS, and maybe looking at it in its relationship to other societies, because you said there was corrosion engineers had their own organization.

HACKERMAN: Right. I would say that the Society wasn't standoffish, but there wasn't a whole lot of overlap between it and others. I think it looked on itself as being closer to the Faraday Society than anything else. In fact, we once talked about calling it the Gibb Society in order to be more nearly parallel. The Transactions were the same as the Faraday Society Transactions. Both of them switched in the late forties. It has, I think, on one or two occasions, had joint meetings with other societies. I think in Honolulu they had one with the Japanese Chemical Society.

There was this inimical feeling between the ACS and The ECS for a long time, though it doesn't exist now. It didn't have that kind of relationship with any other. I think that the one with the ACS was predominantly the personal animosity between Fink and Parsons, which probably grew out of the fact that Fink wanted to get out.

Now, with industrial sponsors it was pretty good, certain industrial sponsors—Dow and some of the others, you know, companies that were interested in this general area. The chloralkali industry obviously had an interest. As I told you, the Aluminum Company of America was interested through Bob [Robert] Mears, who recognized the value of basic electrochemistry to his research and development problems. So I think there were maybe two dozen industrial sponsors who looked on the Society with great interest. They were the ones you'd expect them to be, the ones who were in the field.

BOHNING: What kind of support did they offer, in addition to supporting their employees in attending meetings and things like that?

HACKERMAN: Well, they took out these ads in the journal. What do you call them? Not house ads, but institutional kinds of ads. "The Nickel Company," that's all it had to say. They did that kind of thing. I think they occasionally sponsored symposia of interest to them, perhaps gave some money to help bring speakers in, or sent people to meetings, which was very important to us. Interestingly enough, we published a lot of papers that came out of industrial research labs. I'm sure the publication came after the patent, if there were such. But I'd say—and I can't guarantee that this is true, but I think it is—that the percentage of papers that came from industrial labs in total was higher in this journal than it is in the other journals, including *Industrial Engineering Chemistry* and *Chemical Engineering* and all the rest that publish chemical engineering papers. But we published a lot of industrial papers.

BOHNING: Well, I think this was also a time when industry was changing, and it wasn't this terrible secrecy at the early part of the century that companies had. They recognized that it was important, once you already had protected yourself through patents, that is.

HACKERMAN: That interaction was valuable.

BOHNING: Yes.

HACKERMAN: Yes. Of course that was the root of the Gordon Conferences; not only between academe and the industry, but across the industry. Anyway, I wouldn't be surprised if maybe half our papers—certainly back in the fifties, sixties, and seventies—were industrial.

In the various divisions, there have been lots of cosponsored symposia, and that's about the best way to do that. That is, Industrial Electrolytic would get together with Physical Electric Chemistry, and they'd have a symposium on mass transfer, for example, which was of interest to both sides. As I said, the semiconductor things, I think, stemmed out of Dielectric and Insulation, and there <u>are</u> dielectric materials with which you just displace an atom here or there, and you can get a semiconductor out of it. If you heat a cord, you can get a conductor or something like that. Physical electrochemistry, I think at one time was called Theoretical Electrochemistry, but that word dropped out when Theoretical Chemistry became more powerful. Theoretical or Physical Electrochemistry was very frequently a cosponsor with the other divisions—Corrosion, Insulation, and all the rest of them. When the Society decided to publish in two parts in the same journal, Solid State and Electrochemistry, the interaction was predominantly in each of these divisions rather than across the divisions, although occasionally there'd be some across the dividing line. I'd say internally, the various divisions interacted quite nicely with each other. For a long time they didn't have the disciplinary septa that universities have. They recognized the importance of knowing someone across the fence, so they got together and did it. That part worked really well in this Society.

BOHNING: At first glance, when you look at the various divisions, they almost look so diverse you wonder how they could interact with each other.

HACKERMAN: Some of them do, yes. For example, it's hard to see where Electrorganic would fit, except with Physical Electrochemistry, in terms of reaction rates, orders, and things of that sort. But, you know, by virtue of the fact that it's not a big outfit, and the guys at the banquet sit together and talk to each other, you produce the sessions five years down the line. As you look at the coming meetings, you'll find that there are lots of interactive symposiums—quite a few. It may be that it's now happening in other societies, but not nearly as much as I think it's happening here.

BOHNING: Well, that certainly builds a stronger organization, when you have that kind of thing.

HACKERMAN: Right. Absolutely.

BOHNING: We did touch on question number ten briefly (2), but I think we just skipped over it here; I don't know whether you want to add anything else. The relationship between the academic and industrial contingents—we talked about this a couple of times earlier.

HACKERMAN: Well, in the sense that this Society is a closer knit group, it works out pretty well. They certainly don't just isolate from one another. It may be that it was a lot better in earlier days than it is now, or than it might have been for some years in the recent past. This is because the academic was in the minority in the earlier days. In the sixties, seventies and eighties, they increased to be the majority. There may have been—now, I was going to use the word antagonism, but that's not the proper word. There may have been some <u>indifference</u> one to the other, because cooperation has never been as productive as the cooperators would like it to be. You know, you begin to shake your finger at the system that says cooperate when you don't gain as much as you think you ought to. But it certainly has been as good as any other group, and maybe better because it's small, and the interest is on the field rather than on one's own bailiwick.

BOHNING: Question number twelve: "How has the Society contributed or reacted to significant scientific events in electrochemistry and related fields?"

HACKERMAN: Oh, it's generally been on its toes. In fact it might have been on its toes too much during the cold fusion business, although that hasn't yet been laid to rest by any manner of means. [laughter] It did get involved quickly in that one, and had a number of conferences and symposia, a number of debates, and sometimes rather hot debates. I did react to the business of semiconductor systems as electrochemical arenas very quickly. Early in the game the business of dealing with microelectronic systems came up. Silicon papers appeared in the very early issues of the Journal. It was a sizable player in STM—in using it as a tool, that is. It didn't help invent it, but it very quickly used it. It was very important, it seems to me, in developing the aspect that if one used solid state electronic instrumentation, e.g., the potentiostat, one had a means of control that hadn't been available before. Thus, you ought to go back and look at problems with the fresh instruments, because you might find that you were wrong with the information you had in the first place. I think it did that pretty quickly and pretty well. The Journal got into the game of sensors right at the beginning. [interruption]

BOHNING: We were talking about point number twelve.

HACKERMAN: I started to say that the usual electronic instrumentation for potentiostatic measurements or for electrochemical inductance spectra, that kind of thing—the Society reacted quickly in publishing papers on these items. Solid state sensors became of interest quickly and, in fact, there's a sensor division now. So I'd say that the nose for new fields is pretty good in the Society. The business of maintaining good potential control, for example, is very important in lots of electrochemical processes and that includes organic electrochemistry. Thus, some organic chemists came back into the field. For a long time, organic electrochemistry was very empirical. But now if you look through the JACS [*Journal of the American Chemistry* Society] or other journals that publish a lot of organic papers, electrochemistry is scattered all through them. They don't show up in this Society very much, but the use of electrochemical procedures in synthetic chemistry is first-rate, and the Society recognized that in a hurry.

The answer to question thirteen is straightforward. It really is a very responsive Society. The number of bureaucratic layers is small enough so that you can get things through. They can't always do something for you, but they hear you and respond. On question fourteen I'd say that the financial aspects have always been a problem.

BOHNING: Okay.

HACKERMAN: I think the Society looks very carefully—it always did in my time with them—at the journal and at the meetings. Those are the two important things. Those are the only two things that are of any consequence. Everything else is maybe not frivolous, but peripheral.

They've always been very careful to keep them in the center of their attention and, on an almost daily basis, worry about whether the meetings are doing what they're supposed to and the journal is doing what it's supposed to. I think that's pretty important.

BOHNING: Well, as you said earlier, the technical orientation is foremost over anything else, as compared to the ACS who goes off and does all kinds of other things.

HACKERMAN: And worries about wages and accreditation and all that kind of stuff, and about their own position in Washington.

Well, question fifteen is a great broad question. But obviously I met lots of people from whom I gathered understanding of the field, and with whom I had sometimes deeply embedded contacts with industry and other institutions, but with industry predominantly. So the Society—although I didn't join it for that reason, and I didn't stay in it for that reason—has certainly been very good to me in terms of making accessible individuals I might not have run across otherwise. I would never have seen Mears just offhand. As vice president of research and development of ALCOA , he wouldn't have come down here to look for people. Here's this director of research at ALCOA—I was a young squirt—chances are I'd never have seen him. So, it was good. Again, it's the smallness, I think, that makes the difference.

BOHNING: Did it help you to place your students?

HACKERMAN: Directly, no. Indirectly, yes. I'm trying to think of a place where it was critical to the placement of a student, and I can't. It was useful, but never critical. I'd have a student who would want to go to a certain part of the country—he wanted to be an industrial chemist in the Northeast, say—it would help them to know that. But it was never the final word on placement.

[END OF TAPE, SIDE 3]

HACKERMAN: The Society was doing what it's doing now, but better. I mean that. The business of expanding responsibility and coverage you do at the expense of the intimacy. Also, it will not always look as to whether there is a future for electrochemistry. The Society broke away when it was readily definable and could be delineated. It now has worked itself around to being an integral part of the field, and maybe it's not necessary anymore. I certainly don't visualize anything which would suggest that if it were part of the ACS, it wouldn't be a division. If it were a division, it would be more like the ACS than it is like The Electrochemical Society. So for the

long foreseeable future, I'd say that electrochemistry will be looked at as a separate field, and this is a good Society with which to do that.

The fact of the matter is that's what happened to the ACS, the APS [American Philosophical Society], the American Institute of Biological Scientists, et cetera. They've spawned small groups which are more homogeneous. Now, that can be bad in the sense that interdisciplinarity is not as clear. You dig deep trenches, and it is hard to see over the top.

Electrochemistry itself is an interdisciplinary activity. It's not a single item. If it is part of anything, it's part of interface science. The only difference between over-all interface science and electrochemistry is that with the latter it is necessary to deal with transfer of charge across an interface. That's what makes electrochemistry. It functions as an oxidizing or a reducing agent, just like any chemical oxidizing or reducing agent except that you can control the potential better. Under the right circumstances you can meter the electrons in and out better. You can tell how fast to do that before you saturate the system, and all that kind of stuff. But it's got enough roots in enough different places, so I don't see any reason to believe it'll disappear as a specialty for a long time.

BOHNING: Did it ever attract physicists?

HACKERMAN: Yes. It doesn't attract high-energy physicists, except to fuss about something like cold fusion. [laughter] It does attract the atomic, molecular, and optical spectroscopists. We used to publish papers by people doing electronic work, that is, solid-state research. For example, those who do solid-state conductivity are generally physicists rather than chemists.

In a sense, superconductivity would fall into this also—charge carriers in solids. It hasn't done it. Of course, I think maybe the people who are involved with this Society don't have sufficiently theoretical backgrounds, i.e., solid-state theory backgrounds. But the field has spread and has tentacles all over. It is a great part of analytical chemistry and it is an increasing part of organic chemistry. It is concerned with maybe the only law that is exact, i.e., Faraday's Law. It is simply the product of charge on an electron times the number of electrons. Someone said there were exceptions to Faraday's Law. This was a paper from the University of the Witwatersrand many years ago, around 1950. It violated the Law by getting more electrochemical change than the number of electrons passed warranted. As usual, it was a measuring problem, not a violation. This is a simple law—Avogadro's number times the charge on the electron. Thus, it simply describes a unit counting process and is as exact as are the two values which are multiplied.

BOHNING: I never thought about it that way, but yes.

Well, we've finished this list. Is there anything else you wanted to say in a general sense about The Electrochemical Society?

HACKERMAN: Well, I certainly wish it well. I think it does an important and good job which for the foreseeable future should be continued. I'd hate to see it amalgamate with anything. I'd hate to see it expand too rapidly or too unnecessarily. It ought to always look around to see what it is that belongs to it, or that could be served by it. But that's about all. It's a good template for other small societies, because it's got the longest history, I think.

BOHNING: Yes. Almost a hundred years now.

HACKERMAN: Yes. You see, the others are all much more recent; you know, begun in the sixties, seventies. It's nice to have a system that you can look back on and see that it works for almost a hundred years.

BOHNING: Well, I thank you very much for spending a couple of hours with me today.

HACKERMAN: Yes, it's nice to see you again.

BOHNING: I appreciate it.

[END OF TAPE, SIDE 4]

NOTES

1. Norman Hackerman, interview by James J. Bohning at The University of Texas at Austin, 8 February 1995 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0083C).

   Charles W. Tobias, interview by James J. Bohning at Orinda, California, 15 May 1995 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0146).

   N. Bruce Hannay, interview by James J. Bohning at Seattle, Washington, 28 December 1995 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0137B).

   Harold J. Read, interview by James J. Bohning at Grove City, Florida, 22 March 1995 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0145).

   Frank J. Biondi, interview by Arnold Thackray at Philadelphia, Pennsylvania, 19 March 1996 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0147).

2. James J. Bohning, *Chemical Heritage Foundation Oral History Project, The Electrochemical Society Project, Interview Agenda-Society Presidents*. See Chemical Heritage Foundation Oral History Research File #0083.

3. D. I. Marshall and Norman Hackerman, "Corrosion Studies on Electrolytic Chromium," *Transactions of the American Electrochemical Society*, 89 (1946): 195-205.

4. Robert M. Burns and A.E. Schuh, *Protective Coatings for Metals*, American Chemical Society Monograph Series, No. 79 (New York: Reinhold Publishing Corp., 1939).

5. Herbert H. Uhlig, ed., *The Corrosion Handbook* (New York: J. Wiley, 1948).

6. Charles W. Tobias, interview by James J. Bohning at Orinda, California, 15 May 1995 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0146).

7. Harold J. Read, interview by James J. Bohning at Grove City, Florida, 22 March 1995 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0145).

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