CHEMICAL HERITAGE FOUNDATION

HAROLD J. READ

Transcript of an Interview
Conducted by

James J. Bohning

at

Grove City, Florida

on

22 March 1995
(With Subsequent Corrections and Additions)

ACKNOWLDGEMENT

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.

THE CHEMICAL HERITAGE FOUNDATION
Oral History Program

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(Date) Feb. 22, 1997

(Revised 17 March 1993)

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

HAROLD J. READ

1911	Born in Dubuque, Iowa on 14 February

Education

1934	B.S., chemistry, University of Illinois
1935	M.S., chemistry, University of Illinois
1939	Ph.D., chemistry, University of Pennsylvania

Professional Experience

1933-1935	Chemist, Engineering Experimental Station, University of Illinois
	University of Pennsylvania
1935-1937	Assistant Instructor
1937-1938	DuPont Fellow
1938-1940	Instructor
1936-1940	Part-time Chemist, Graham, Crowley & Associates
1940-1945	Industrial Fellow, Mellon Institute for Industrial Research
	Pennsylvania State University
1945-1951	Associate Professor of Physical Metallurgy
1951-1971	Professor of Metallurgy

Honors

1961	Proctor Memorial Award, Electroplaters Society
1966	Scientific Achievement Award, Electroplaters Society
	Melvin Romanoff Award, National Association of Corrosion Engineers
1986	Honorary Member, The Electrochemical Society

ABSTRACT

The interview begins with Harold J. Read describing his family background and early education in northern Illinois. Read praises his high school education and laboratory training, recalling his thesis verifying Colin Fink's patent on chromium plating. A brief discussion traces Read's education at the University of Illinois during the Depression; intermittent jobs, such as editorial work for the Commerce Clearing House; theses projects in electro-organic chemistry with Sherlock Swann, leading to B.S. and M.A. degrees; and leaving Illinois for an assistant instructor position at the University of Pennsylvania, where his Ph.D. research brought him into the area of metallurgy. The discussion next turns to Read's decision to accept a research position at the Mellon Institute in Pittsburgh and his metal work making specimens for Houdaille Hershey and others, which eventually led to equipment design and manufacturing prototypes for the Manhattan Project. The majority of the interview discusses Read's involvement with the ECS, beginning with his introduction to the Society in 1934 and his election as secretary of the local sections in Philadelphia and later Pittsburgh. Read describes local section meetings and initial Society activities, including planning for the Spring 1940 national meeting, editing work on *Modern Electroplating*, chairing the Electrodeposition Division, and chairing the Publications Committee, where he was influential in broadening the Society's publication activities. He also discusses his work with the Society's monograph series and awards committees, emphasizing the diversity of scientific interests within The ECS. Next the conversation focuses on Read's decision to accept the vice presidential nomination, which eventually led to the presidency; as president his work focused on publications, the discontinuation of *Electrochemical Technology*, and the implementation of the Council of Past Presidents. The interview ends with a discussion of Read's views of the Society and electrochemistry, present and future; Read reflects on the Society's reaction to new developments in electrochemistry and related fields, and comments the growth of membership and national meetings, and comments on the Society's contributions to his career in terms of science and technology, human relations, and his consulting practice. Closing remarks emphasize The ECS's problems with the publication and storage of abundant new scientific information, and the continued diversifying and branching of the Society and the field.

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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INTERVIEWEE: Harold J. Read

LOCATION: Grove City, Florida

DATE: 22 March 1995

BOHNING: Dr. Read, I know that you were born in Dubuque, Iowa, on the 14th of February, 1911. Could you tell me a little about your parents and your family background?

READ: Both of my parents were born in 1875 of farming parents. I can tell you fairly precisely about my paternal background. It's Irish. My paternal grandfather and grandmother emigrated from Ireland as youngsters separately, in different families, and were married in this country in 1857, which goes back quite a way.

On the maternal side, as far as I know, my ancestors welcomed the Asians who came to North America via the land bridge that once connected Siberia to Alaska. In other words, I have no idea of when the family came to America. The family name on my mother's side was Taylor which indicates, of course, English background. Other than that, I know relatively little—in fact, almost nothing about my family background. I've not been much interested in genealogy. What I've learned has largely been through the efforts of cousins rather than discussions with my own parents.

My father initially was a carpenter but later became a plumber and then a contractor. My mother had a very interesting occupation for a woman. She was a compositor for the *Galena Gazette*, a newspaper—probably the only one they ever had as a woman. She was there some ten years or so before she married my father in 1910. She had only a high school education, which in those days—which would be the early 1900s—high school was considered pretty high. Did I say high school education? I was wrong—eighth grade.

BOHNING: Eighth grade. Okay.

READ: At that time, the eighth grade education was about equivalent to our present high school education, because students spent most of their time learning rather than playing instruments in the band or whatnot. Actually my folks left Dubuque when I was six months old, so I'm not much of an Iowan. We moved to Galena which is in the northwestern corner of Illinois, a very

high, hilly area and an old mining area for Black Jack, which is lead-zinc ore. After Galena, we just moved around and around to various towns in northern Illinois.

BOHNING: Well, I'd like to go through a little bit of your childhood and where your interest in chemistry developed.

READ: All right. My father, who had developed a successful country store and plumbing business, suffered financial reverses in the early twenties. He extended too much credit to farmers and got caught in the Panic of 1921. This changed things quite a lot for me. I had gone to a country school where I was the only student in my grade. When my father entered his bankruptcy, we left the town where we were then living and moved to Galena, which is the county seat and big enough to have a fine school system. A whole new world opened up to me there, because there was a library. In the little community where we had lived previously, which was named Woodbine, there was just nothing at all, so the Carnegie Library in Galena was not only a revelation but a grand one. I began more and more to enjoy school.

We left Galena when I had finished seventh grade and moved to Crystal Lake, Illinois, a northwestern suburb of Chicago. This was a wholly different community—about the same size as Galena, perhaps a little smaller, but it was a summer resort town in those days. At the time I was ready to enter high school, the community had built a new community high school to take care of not only Crystal Lake, but Fox River Grove and Cary, two small towns to the south.

The tax situation in Crystal Lake was a little odd. Most of the residents were summer residents but had very considerable properties, some of them quite elegant, on which they paid taxes but sent no students to school. As a result, the community had a plethora of tax money. When the community high school was built, they did it up brown. It was a beautiful piece of work. Most importantly, they engaged a gentleman named H. A. Dean as superintendent. He had what I consider one of the most wonderful ideas for a school superintendent—namely, that all the instructors should have their degrees in the material that they were going to teach, and to hell with education courses. To take care of the law, which requires a certain amount of formal education courses, Mr. Dean had to accept the minimum "education" courses, but he insisted that what was important was the subject matter. We had marvelous instructors, just wonderful.

My interest in electrochemistry started, not surprisingly, in the course in chemistry that I had in this high school, whose laboratory was better equipped than the freshman laboratories at the University of Illinois. We had excellent equipment, and everybody had all they wanted.

We were also encouraged to do a thesis. This was between 1923 and 1928. Colin G. Fink's patent on chromium plating had just been issued, so I decided, for my project, that I would see whether or not this patent worked. I was able to get a copy of the patent. Lo and

behold, I ran some experiments, plated some material, and it worked. I wrote a letter to Dr. Fink telling him of my experience; never had a reply. [laughter] He was able to get along on his own.

BOHNING: How did you come across this patent?

READ: My instructor suggested that I might be interested in it. Just why he did it, I don't know. Incidentally, he's still alive. He's well along in his nineties and on his fourth wife—remarkable man. In any event, that was my interest in electrochemistry. Incidentally, I graduated as valedictorian of my class, which was not a very big one. That gave me a scholarship to the University of Illinois, which was my main reason for going to Illinois rather than to a nearby college in the Chicago area. At that time, of course, there was no Chicago campus of the University of Illinois.

BOHNING: You must have started in college right after the Depression started. When did you go to Illinois? In 1930?

READ: It was before the Depression. I entered as a freshman in the fall of 1928, so I got caught in the Depression. Because my father was still recovering from his financial problems, he could not finance my college work. Although I could stay at home with free board and room, he couldn't offer me anything else. During high school I worked and earned money one way or another. I did one year at the university and ran out of funds. Also, I wasn't quite sure what I wanted to do. I had enrolled in chemical engineering. I had a variety of jobs for a year and then went back to school for two years. I still wasn't quite sure what I wanted to do. I'd changed to chemistry because I early became convinced that I didn't want to be an engineer.

I dropped out again. This time, I got into something quite interesting, after some more or less miscellaneous jobs. As the legislative season of 1933 came on, a friend of our family, John Huling, was responsible for the Legislative Reporting section of Commerce Clearing House, the publishing division of Corporation Trust Company, and needed a great deal of additional assistance when the legislatures started meeting. At that time, almost all the legislatures met in alternate years—almost all of them in the odd years—so when 1933 came along, he needed a great deal of additional help. I was hired for that.

In those days, the legislatures lasted no longer than the minimum salary period for the legislature, which was usually as short as possible—namely, about two months. When the legislators finished their work, our jobs were over, except mine. For some reason or other, the president of Commerce Clearing House decided to try an experiment. The State and Local Tax service in those days had thirteen editors, all of whom were lawyers. He decided that he would like to try somebody who wasn't a lawyer as an editor for that service. Apparently, my editorial

work for the Legislative Reporting Service had gone quite well, so I was invited to become an editor in the State and Local Tax service. Since I needed money—I needed a job—I accepted.

Things went quite well. I earned enough money to insure going back to school to finish my first degree. I decided chemistry was still it. Incidentally, I was offered a job with Commerce Clearing House, editing their house magazine, which would have been a stepping-stone to management. It was quite a temptation to continue, but I decided science had more attractions than the law. In fact, some aspects of the law repelled me rather badly.

I went back to Illinois. Of course now that I had a fair amount of money saved, I found there was no trouble getting jobs. [laughter]

I can go back a little bit. Am I being too wordy?

BOHNING: No, no. It's fine.

READ: To go back a little bit, when I was a freshman at college, I took my meals at a boarding club, which was quite the thing to do at that time. One of the group was a senior who was working on his thesis in chemistry with Frank C. Howard and Sherlock Swann. Frank Howard was a nonentity—a pleasant chap, but a nonentity. Sherlock Swann, of course, in his day had a great deal to do with The Electrochemical Society, a very well-known electro-organic chemist.

Well, the chap whom I'm referring to, the senior—I can't remember his name—broke his wrist. Part of the experimental work involved metallographic work, which in turn involves polishing specimens which were held by hand during the various operations. He was righthanded, and his broken wrist was on the right hand and he could not hold the samples well enough to polish them. He was desperate about what to do. Well, I'd had such a good background in high school that college work was never much of a problem to me and I had free time, so I offered to see if I could polish his specimens for him—which I did—and became very much interested in what he was doing, which was an electro-organic reduction, and also very much interested in his reaction to Sherlock Swann, which was very favorable.

When it came time for me some years later to choose a thesis subject for my bachelor's degree—and in those days Illinois required a thesis for this degree, at least in chemistry—I went around to Dr. Swann to see if he had something that would suit me, and whether I could suit him. Well, we came to an agreement, and I did my senior's thesis with him to such an interesting result that I did my master's thesis with him, too (1).

This is how I got into electro-organic chemistry, but I rather soon got out of it. After the master's degree at Illinois, I could have stayed there with an assistantship, but I wanted to go someplace else. Sherlock was very much in agreement. I remember him saying that he would

dearly love to have me stay on and continue the sort of interesting things we were doing, but in all good conscience, he could only advise me to see if I could find something of interest elsewhere just to broaden my background, broaden my experience. Happily, I was able to get an assistant instructorship at Penn [University of Pennsylvania].

BOHNING: Why Penn? Was Edgar Fahs Smith's legacy in electrochemistry there yet? He died in 1928, I think.

READ: No, Hiram Lukens, who succeeded Smith, had a much different chemical perspective, but it still involved electrochemistry.

BOHNING: Okay.

READ: Hiram Lukens was very active in The Electrochemical Society, and Sherlock Swann had already become quite active although he was still a relatively young man. Sherlock felt that I could get along well with Hiram Lukens, and that the atmosphere there would be good for me.

It turned out differently because when Edgar Fahs Smith retired and Hiram took over as head of the chemistry department, he was too busy in his new activity to take on graduate students. As a result, I did my thesis with Martin Kilpatrick on a subject which was really more metallurgy than chemistry—kinetics of the passage of gases through metallic membranes (2). That's how I got more and more into metallurgy.

As I finished my Ph.D. work I also married Catherine P. Braungard, the sister-in-law of A. Kenneth Graham, with whom I often collaborated in research and consulting activities.

When I finished my Ph.D. at Penn, I again decided that I ought to go someplace else, although I could have stayed at Penn. I was already an instructor rather than an assistant—I was offered a good job there, but Hiram told me that much as he would like to, he couldn't promise fast promotion simply because of the way things worked at Penn. You just had to take your chances. The chances didn't look too good. That's why I went to Mellon Institute [for Industrial Research]. Mellon was sort of a halfway measure between industry and academia.

BOHNING: Had you thought about industrial jobs at that point?

READ: Not seriously, no.

BOHNING: Okay.

READ: Except that at Mellon Institute it was really an industrial job because of the nature of the fellowship that I worked with. You know how Mellon works.

BOHNING: Yes.

READ: It was very different than Battelle [Memorial Institute]. The fellowship that I was with was H. H. Robertson Company, and they're makers of heavy builders materials. They were wanting to diversify because the building material industry was up and down. My assignment was simply to get interested in something different than what the company was making, and then see what I could do to develop it. Very interesting. That's why I accepted the job.

I had a very pleasant five years technically, but I ran into problems with the patent attorney. We just didn't see eye to eye. I thought that I was being badly used and neglected, so I was eager to leave when the opportunity came to go to Penn State.

Incidentally, I had a very, very pleasant five years in Pittsburgh. I enjoyed that very much. Part of it came from the place where we lived. We lived right on the edge of the Carnegie Tech [Carnegie Institute of Technology] campus, and my neighbors were a chemist, a metallurgist, a playwright, two mathematicians, that sort of thing. It was a very close-knit community.

[END OF TAPE, SIDE 1]

READ: Incidentally, I'm the last survivor of that community. Just a few days ago I received a death notice about the only other one. Time goes by.

Well, I told you why I left Mellon Institute. Of course, I was there during the war years, and the war led to some activities which considerably affected the course of my life.

BOHNING: I saw your shop in the garage. It's just very impressive, so I'm assuming you've had that interest for a long time. There was a period where you, because of your metal working, did some work for the Atomic Energy Commission during the war. Is that correct?

READ: Yes, but it was for the Manhattan Project, not the AEC. Also, we did quite a bit for Army Ordnance and a little bit for the Navy. The way things went—and I'll try to make it reasonably brief—my next-door neighbor was Max [Maxwell] Gensamer, a professor of mechanical metallurgy at Carnegie Tech. After the war got underway, he got mixed up with Houdaille Hershey and their gun tube extrusion plant out in Illinois. It used to be that gun tubes were made by piercing steel billets. Well, Houdaille Hershey decided to make them by extrusion, at least up to caliber of 75 millimeters. Max got involved in studying the mechanical properties of the resulting steel tubes and, as a result, had a great many tensile specimens shipped to him for testing and evaluation of the results.

In Pittsburgh at that time, if you wanted somebody to machine a great huge roll weighing twenty-five tons or so, down to the proverbial gnat's eyebrow, everybody in town wanted to do it. If you had a little bitty something, like a tensile specimen, nobody was interested, so Max had a lot of trouble. He knew that I had a lathe in my basement and asked me if I could please make a few specimens for him, because he just desperately needed them.

I made about two dozen, and apparently they were all right. Then there were four dozen. Then there were more. My wife was quite adept manually, so I taught her how to run the lathe, and she started making the specimens. Then they came in by the hundreds. She told me one day that she spent too much time watching what was going on. If she had two lathes, she could keep them both going. I found another lathe and she kept it busy. Eventually we started making Charpy impact specimens. I borrowed a milling machine for that, and she was running two lathes and a milling machine, and reading detective stories on the side.

Well, that's the reason for the shop. Incidentally, we did so much of that, that I had to set up a company to handle billing and other business problems. Again, my whole work force was Catherine. I did work in the evening and week ends, particularly grinding tools. All of this was undertaken only after I had obtained the approval of my superiors at Mellon Institute.

When the Manhattan Project came along, Max Gensamer got involved in that. One of the problems was, what happens to the graphite in a pile? The reactors were known as piles then. It was Max's job to find out. He wanted to compare natural graphite with some varieties of manufactured graphite. Where to get the natural graphite? I undertook to take specimens out of the back of display specimens from the Carnegie Museum. I presume that still, if you look at the specimens of natural graphite in the Carnegie Institute in Pittsburgh, you'll find them looking perfectly natural, but if you turn them over, you'd find a hole in the back of every one. [laughter]

That was the sort of thing that I did to help Max. It got to the point where we needed to design equipment that was different from anything available, so I got into equipment design and into manufacturing prototypes. That's why I have so many machine tools.

When I went to Penn State, the equipment for the most part came along with me. For a while as a side-line, I operated an instrument-making company, but as I became more and more

immersed in both professional and consulting activities, which I enjoyed much more than the business that was involved in the instrument-making activity, I closed the business. Also, there was a limitation on what I could do with the instrument business without getting into a good deal of paperwork, which I detested. I kept the machinery.

BOHNING: When you were in Philadelphia, you were associated with the consulting company A. Kenneth Graham & Associates?

READ: Yes. This came about through having offices close together. When I first began graduate work at Penn, I was assigned to an office that was right next door—or nearly so—to Ken [A. Kenneth] Graham's office. We visited back and forth and found lots of mutual interests. This is what got me back into electroplating. You may remember that I verified Fink's chromium plating patent in high school. I had never done any more electroplating after that until Ken got me interested in electrodeposition. Together we devised some experiments on what was happening at the cathode during electrodeposition. I made the gadgetry and did the work, and we published some material together (3).

We got so interested that during the summer recess, he offered me a job at his little research laboratory out in Jenkintown. That worked out very well. We became personal friends as well as more or less scientific friends. I met the girl who became my wife at a Christmas dinner at Ken's home. Our connection got ever closer, and in due course we became brothers-in-law.

That's how I got into the Graham activity. That was completely interrupted by my going to Mellon Institute and the war. After the war, Ken and a chap named Clyde A. Crowley, from Chicago, got together to form Graham, Crowley & Associates, with offices in Jenkintown, which is a suburb of Philadelphia, as you probably know, and other offices in Chicago—Crowley in Chicago, Graham in Jenkintown. They undertook a diversity of activities, and Sherlock Swann and I, along with two or three others, became the associates. We got into some very, very interesting consulting work. One of the associates, as I just said, was Sherlock Swann. John C. Bailar of the University of Illinois was another.

BOHNING: Well, what I'd like to do at this point is to start looking at your Society activities, going all the way back. My indication is that you joined the Society in 1934 when you got your bachelor's degree at Illinois. Was that unusual, to be joining—I'm going to say—a specialists' society after your first degree?

READ: Yes. Quite unusual. Actually I joined May of 1934 before my first degree in June of that year.

BOHNING: That was Swann's influence, I'm assuming?

READ: Yes. Swann proselytized. Also, I should point out that I joined the American Chemical Society at the same time, within a matter of weeks. Interestingly enough, Sherlock Swann, of course, got me into the Electrochem, and Speed [Carl Shipp] Marvel was the one who got me into the American Chemical Society. They both went on to become presidents of their respective societies. I consider myself the president-maker. [laughter]

BOHNING: I was going to ask you earlier why you didn't succumb to organic chemistry at Illinois, with that being Marvel's and Adams' strength.

READ: Well, of course, my two theses were in electro-organic. Also, I was interested in the metallurgical aspects, because really what I was studying was not the reaction as a chemical reaction, but as an electrochemical reaction. The reaction I employed was just an ordinary routine one, reduction of a ketone to a hydrocarbon, but what I studied was the effect of the metallurgical aspects or properties of the cathode on the reduction efficiency of the organic process. So I did not escape the organic aura that pervaded Illinois at the time.

Another reason that I got teed off on organic chemistry in most respects was that during 1934—that would be the graduation year for my bachelor's degree; I went on for a master's degree—I worked during the summer for H. Fraser Johnstone. His laboratory, in which I worked, was directly below the laboratory in which organic synthesis was going on. In that summer they did everything in the world they could to make stinks. When an organic chemist sets out to make stinks, it's really something. Of course the effluvia came down—no air-conditioning—it came down out of their windows and into mine. I've had enough of organic stinks. [laughter] My reduction reaction was with a material that didn't stink.

BOHNING: When you first joined The Electrochemical Society, did you go to any meetings? Was there a chapter at Illinois?

READ: No. The closest chapter, or section, was in Chicago, and I was too poor to go up. Also, I was too busy. It wasn't until I got to Penn that I got interested in Society activities per se. There was just no opportunity or reason to do anything at Illinois.

BOHNING: What did your membership in 1934 entitle you to? What did you get for it?

READ: You got the *Transactions* [*of the American Electrochemical Society*] twice a year. This was a report, of course, of what went on at the semi-annual meetings. Also, you received monthly a bulletin which varied considerably from time to time, but was sort of a news magazine and contained some material of scientific interest alone, that is, apart from any Society activities—mainly patent listings pertaining to electrochemistry and to some degree, reports of industrial activities. It was pretty much whatever Colin Fink could find time to put in, because he edited it. He was a remarkable man. He did an awful lot for the Society.

There were just those two things, that's all, and the meetings, which I got into quite early.

When I went to Penn, the first meeting to be held after I got there was in Washington, which was nearby. I was still too poor to go, so Hiram Lukens loaned me enough money to attend the meeting. I was very, very favorably impressed, not only with the scientific aspects of the meeting but also the people. I'll take one example that I'll never forget. You probably have heard of Charles F. Burgess, founder and owner of Burgess Battery Company?

BOHNING: Yes.

READ: He was still alive and still fairly active in 1935. I remember still, going down to breakfast one morning and standing in line. The gentleman in back of me asked if I were alone. He had seen my little badge, as a member of the Society. I said, "Yes." Well, it turned out to be Charles Burgess. He asked me to join him for breakfast. It was a wonderful experience to have an hour or so with him. Unhappily, he died not too long after that.

That was typical of the way youngsters were treated by the older members of the Society, for the most part. There were some crusty ones too. [laughter] You always have to dilute things.

Hiram Lukens was always a little careful with his money. While I hope he was sure that he would be repaid, he helped things along by getting me jobs translating German patents for a firm of patent attorneys in Philadelphia. I was not only able to pay him back, but also to begin courting the girl who became my wife.

BOHNING: Was there a section in Philadelphia at the time?

READ: Yes, there was. I've forgotten which year I was elected secretary, but it was very soon after I moved to Philadelphia. Apparently I did a good job because instead of putting me up the

ladder to become chairman, they kept me on as secretary, probably for two reasons. I haven't mentioned it before, but I looked much, much younger than I actually was. I suspect that maybe this was one reason they didn't want me to become chairman. The other reason was the good job I was doing as secretary. In any event, they kept electing me secretary until I left Philadelphia. I got involved in quite a few things.

BOHNING: How big a group was it?

READ: I don't remember. I would judge maybe fifty members, of whom perhaps twenty-five or thirty attended meetings quite regularly. It couldn't have been much larger because we met in the old Lenape Club for dinner, and the dining room there was not large. Then we moved to the old Harrison Laboratory for the lecture and discussion. Sometimes the meetings were quite well attended by people other than members of the Society because the section was able to attract well-known scientists through personal contacts by Hiram Lukens, Milton Barbo, and George A. Perley of the Leeds and Northrup instrument company. They had extensive connections with scientists all over the country and helped a great deal in bringing in these people of outstanding ability, men who were interesting.

BOHNING: Were most of the members of this section industrial people, as opposed to the academics like Lukens?

READ: I think that there was probably a majority of industrial people, but not a big one, because there were a few academic institutions in Philadelphia that had some interest in electrochemistry. There was Jerry [H. Jermain] Creighton at Swarthmore and Samuel Heiman at Temple University.

Also, there was a fair amount of interest in coming to Electrochem meetings because we didn't limit ourselves to electrochemists for our speakers. The general idea was that we ought to be interested in science, particularly science that had <u>some</u> connection—sometimes very vague—to electrochemistry. One of the things I'll say now and perhaps elaborate on later in terms of some of your other topics, was the diversity of interests and diversity of activities in the Society. I was a member of the American Chemical Society's section in Philadelphia, but I found it much more rewarding to be active in the Electrochem. To a considerable degree, the same kind of thing prevailed in Pittsburgh where again I chose the Electrochem Society. There was a section of the American Chemical Society in which I never became active. Simply, I enjoyed the diversity of Electrochem.

Now, it's true that there is much diversity in the American Chemical Society, and it is becoming more and more so, but it isn't diverse compared to the fields covered by the

Electrochemical Society. There are times when I wonder whether or not the Society ought not to be called the Society of Potpourri. [laughter]

BOHNING: What was the relationship, if there was any, between The Electrochemical Society's section and the American Chemical Society's section in Philadelphia?

READ: Outside of members who belonged to each, I don't remember any.

BOHNING: Okay.

READ: Remember, of course, this is quite a few years ago.

BOHNING: Well, the reason I ask that question is that The ECS originated because of disagreements within the ACS.

READ: To some degree, yes.

BOHNING: There were some ill feelings, I guess, between certain people. The ACS didn't start an electrochemistry group, and therefore The ECS went its own way.

READ: I've heard different versions of that. [laughter]

BOHNING: That's why I was asking whether any of those feelings still carried on.

READ: Not to my knowledge.

BOHNING: Okay.

READ: I did get quite deeply involved in The Electrochemical Society's activities in Philadelphia, even though I was still a youngster—I guess because sometimes I would do things other people didn't.

BOHNING: What kinds of things were you doing there? As secretary, you sort of controlled things that went on, didn't you?

READ: Yes, pretty much. A specific example was a meeting which was held in Wernersville. The Spring 1940 meeting was hosted by the Philadelphia section. We selected as venue a resort up in the mountains, namely at Wernersville. Wernersville is sixteen miles from nowhere, but it had a very, very nice resort hotel with good meeting facilities, and we thought it was easy to get to because we were really close by. It's just outside of Reading. It turned out that non-residents of the Philadelphia area felt differently about accessibility.

In those days, the host section handled all of the detail of holding a national meeting; things that are now handled by the society's headquarters office. Well, at that time the section chairman was Jim [James L.] Crenshaw of Bryn Mawr College. At the planning meeting, he didn't show up, and the treasurer and I were the only two officers there. The treasurer was Albert Ware. Al knew absolutely nothing about electrochemistry, but he and his brother printed *Transactions* and other Society material. He was the perennial treasurer of the section and a swell fellow. He had never been to a national meeting, had no idea what was involved, so, of course, it devolved on me to run the planning meeting. Apparently I did a fairly good job, since the meeting went off very well. If you want more, there's an article about it which I wrote for *Interface* (4).

BOHNING: Okay.

[END OF TAPE, SIDE 2]

READ: The folks in the Society in recent years have been after me to write reminiscences, which I have done with only a moderate amount of prosaic license. [laughter]

BOHNING: Were you doing anything on the national scene while you were in Philadelphia, or was it mostly local?

READ: We started work on *Modern Electroplating* (5) while I was still in Philadelphia. The meeting at which all of the papers which comprised the book were presented was in Chicago in the fall of 1941, but we had started work at least two years before, so I must have started work nationally then in Philadelphia about 1938, 1939. My specific activity was in working on the

editorial committee for *Modern Electroplating*, which turned out to be the first monograph published by the Society—after some developments which I'll talk about a little bit later.

BOHNING: Okay.

READ: I did get involved nationally in that way. There, apparently, things worked out pretty well for me, and *Modern Electroplating* did all right, too. I never made much of a point of this publicly, but I did most of the editorial work, because I had more experience in editorial work than any other member on the editorial committee. Their main job was selecting authors for the various chapters of *Modern Electroplating*, then deciding whether or not the submitted manuscripts fulfilled the directives and whether or not the material there was suitable and acceptable. I edited practically everything as far as the mechanics of editing were concerned, putting down the scribbles in the margins and so on. Also, I did the galleys before passing them on to the authors, because most of the authors didn't know how to work up galleys, and I had done so much of it with Commerce Clearing House that it was no problem.

BOHNING: Now, there was a Division of Electrodeposition that you were involved with. This is still when you were in Philadelphia, according to what I have here. You were chairman in 1942.

READ: Yes. The Electrodeposition Division, of course, was an old one within the Society. I became secretary first in Pittsburgh, I think in 1941, the year after I arrived there. Apparently I get elected secretary anyplace I go. Then I went on to become chairman. This was during the time that I was editing *Modern Electroplating*, because the papers were presented in the fall of 1941. I didn't edit the material for the *Transactions*. That was done by Colin Fink, but I re-edited the papers for *Modern Electroplating*. No, I'll take that back. I edited the papers for the meeting.

BOHNING: Okay.

READ: Colin didn't have to do anything with them except approve them for publication in the *Transactions*. I reworked them to some degree for publication as *Modern Electroplating* and, of course, edited the fore material and what I always called ass-end material. Most of that editorial work was done in both Philadelphia and Pittsburgh.

BOHNING: When you got to Pittsburgh, you eventually became chairman of the Pittsburgh section as well. How would you compare the Pittsburgh section with the Philadelphia section?

READ: It was much more industrial, although there were good leaders in the academic area too. Some of the Pittsburgh section leaders became presidents of the Society: for example, Francis [C.] Frary with ALCOA, and Jake [John C.] Warner with Carnegie Tech. No, when I think back, it was mainly industrial people who were active in the Society, because Westinghouse and ALCOA were the ringleaders of the Pittsburgh section. Incidentally, the reason the Pittsburgh section fell apart is because the Westinghouse and ALCOA companies changed complexion. It was people like Francis Frary with ALCOA, John Marden with Westinghouse, Sidney Barnardt with Westinghouse, and there was Earl Gulbransen who was very active in the Society but never became president.

BOHNING: You had mentioned earlier how good Swann was at proselytizing. Was anybody in the Pittsburgh or Philadelphia section doing that with students?

READ: Jake Warner, yes.

BOHNING: Okay.

READ: To a degree, Paul Fugassi. You won't find much about Paul Fugassi anywhere except in some few, very, very erudite publications. He was a teacher first and foremost, and a thinker—he published very rarely—but he was also a very fine gentleman. Between Jake Warner and Paul Fugassi, we had quite a few students who joined during the five years that I was in Pittsburgh. However, to some degree, of course, we must remember they were war years, and the number of students was small.

BOHNING: Where was Fugassi? Was he at Carnegie Tech also?

READ: Carnegie Tech, yes, in chemistry.

BOHNING: Was there anything else about the Pittsburgh era? You got onto the publication committee, and that was right after you left Pittsburgh, I believe. Is that correct?

READ: No, I was on the Publications Committee while I was still in Pittsburgh, but I became chairman after I moved to Penn State.

BOHNING: Okay. What was the committee like when you joined it? Were you appointed or elected to it?

READ: Appointed.

BOHNING: By whom?

READ: By the then president. All members of committees in The Electrochemical Society, with a few exceptions, are appointed. The exceptions are the Chairman of Ways and Means, and some other committees headed by elected vice presidents. Their chairmanship is mandated by the constitution or bylaws. All others are appointed. Now, these are committee members I'm talking about. The Board of Directors, of course, is quite different. That's elected.

I think you asked about the Publications Committee. It was me, to put it bluntly. [laughter]

BOHNING: Okay.

READ: Nobody else did any work. The committee was relatively inactive. I tried to stir some things up, and maybe some of the things that came to pass might at least have been influenced by that stirring. I felt that the Society ought to be doing more in the field of publication. I had very strong opposition from Colin Fink, who was the perennial secretary of the Society—very powerful. I got nowhere at all. It was only after his departure as secretary when other people took over the day-to-day operation of the Society that we got someplace with publication—although we got our foot in the door with *Modern Electroplating* while Fink was still secretary.

BOHNING: That was the first?

READ: That was the first monograph.

BOHNING: Okay. What kind of charge did that committee have at that time?

READ: It was very vague, and I can't recall it.

BOHNING: Okay.

READ: The chairman was a member of the Board of Directors. Maybe that was the only reason for having a Publications Committee meeting. Actually, it was very weak. I had hoped I could do something about it, but I wasn't able to.

BOHNING: At least you got the monograph series.

READ: Yes.

BOHNING: That was your idea, that monograph series?

READ: No. The idea of the symposium, which led up to the monograph, was Dick Hull's. He was the ringleader and chairman of the Electrodeposition Division at the time—about 1938. The initial publication of the symposium papers presented at the meeting was in the *Transactions*, which was the semi-annual publication of the semi-annual meetings. The volume from the Chicago 1941 meeting contained not only all of the papers of the Electroplating Symposium, but also some other general papers that filled out the meeting agenda. The *Transactions* came only to members and was not offered for general sale, but there was a tremendous demand for only the papers that constituted the symposium on electroplating.

Well, all of us as a group felt that we ought to publish a monograph. Fink and some others on the Board of Directors were very much opposed because the Society would have to risk the cost of publishing with no guarantee, of course, of return. Ed [Edwin M.] Baker, who had been a member of the Editorial Committee for the Electroplating Symposium, was very much in favor of publishing a monograph. He was the president of the Society at the time, and he forced the monograph through the Board of Directors as a Society activity (5).

Well, of course it turned out to be a money-maker—really a good money-maker. The opposition to monographs in the Board of Directors disappeared completely—including Colin Fink—so after that there was no trouble in broadening the Society's publication activities.

The next monograph was Herb [Herbert H.] Uhlig's *Corrosion Handbook*, which was a monumental effort (6). It was published, unhappily, in my regard, by Wiley [John Wiley & Sons]. Companies in the book publishing business had noticed that *Modern Electroplating* was selling like the proverbial hot cakes. They came around wanting to become publisher for The Electrochemical Society. Wiley succeeded in persuading the Board of Directors to enter into an agreement for a series of monographs to be published by Wiley.

When the time came for the second edition of *Modern Electroplating*—because we had always envisioned a new edition every ten years to keep it up to date—Wiley was now the monograph publisher, and they engaged Allen G. Gray to edit the next edition of *Modern Electroplating* (7). He was an experienced editor and became a director of publications for ASM for many years. Notice I said the "next edition."

Well, when they brought it out, there was no mention of the first edition. It was listed as following the *Corrosion Handbook* in the series. The *Corrosion Handbook* came out in 1948—whereas, of course, the *Modern Electroplating* came out in 1942. Well, of course, we were absolutely outraged—although nothing came of the outrage. [laughter] That's why a lot of people were confused about the order of appearance as publications between *Modern Electroplating* and *Corrosion Handbook*, including Bob Frankenthal. I remember that when Bob introduced Herb Uhlig as recipient of the Scheson Award he referred to the *Corrosion Handbook* as the first monograph of the Society. Of course I was outraged. [laughter] He had just taken what appeared on the flyleaf in the fore matter of Wiley's publication. Incidentally, I think that the issue of *Interface*, which will appear this month, has the story of the monograph (8), so you can check what I'm saying now with what I wrote for that. [laughter]

BOHNING: All right.

READ: Are you familiar with *Interface*?

BOHNING: I'm not a member of the Society. I am a chemist, but I'm not an ordinary chemist.

READ: Well, *Interface* is the members' magazine, but quite different from the old, old *Bulletin*. The *Bulletin* disappeared when the *Journal* came along—but that's another story.

BOHNING: You essentially then left the Publications Committee around 1950?

READ: I left it before that because I became only an ex-officio member of the committee as technical editor of the *Journal*. The technical editor cannot be the chairman of the Publications Committee. I've had no connection with the Publications Committee since 1949, really—except once in a while in the distant past they asked me to sit in with them.

BOHNING: I know we talked about this earlier, but I don't know if we had the dates down as to when you were technical editor for the *Journal*.

READ: It was 1949 and 1950.

BOHNING: Okay. That's when you turned it over to Norman Hackerman?

READ: Yes.

BOHNING: What did technical editor mean?

READ: It meant we did everything with the *Journal* except about two pages. [laughter] There was an editor, originally. Really, all that he did was to write the editorial which appeared in the fore matter and edit a small amount of Society news that appeared as end matter. All the rest of it was the responsibility of the technical editor. The technical editor really was the editor. In due course, not very soon but in due course, the Board of Directors realized this at the time Cecil King, the editor for many years, died, and abolished the term technical editor and simply had an editor of the *Journal*.

BOHNING: What kind of acceptance rate did you have on papers at that time?

READ: I can't remember. I just can't remember. That's too long ago. After all, it's forty-four years.

BOHNING: I know. [laughter]

READ: I just can't remember. It might well be that there are records in the Society, but I would hate to try to find them. Maybe I can answer it in a general way.

BOHNING: Okay.

READ: I don't recall that we had very many rejections because there were lots of new things going on, and people were getting into new areas. The papers that were being offered for publication were so often new material, you didn't care how bad they were. You were delighted when they were good, but if they weren't so good, you fixed them up and published them anyway.

A specific example was the matter of fluorescent materials. The Electrochemical Society was the leading publisher of the work that was going on in this area of ceramics. I was heavily criticized because I published, while I was editor, some papers that were just out-and-out ceramics. They just didn't appear to have any connection with electrochemistry—except that I was advised by Woldemar [A.] Weyl that this was going to become an area which was going to be connected with a lot of things, including electrochemistry, as something very different from traditional ceramics. Woldemar Weyl was at Penn State as director of a research project or activity sponsored by a consortium of glass manufacturers. A very peculiar chap, but brilliant. He supported me. [laughter]

BOHNING: Why would these people be submitting papers of that kind to you?

READ: Well, there was no place else for them. [laughter]

BOHNING: Okay. They were being rejected elsewhere. [laughter]

READ: Also, the fluorescent materials were activated by electron bombardment. That was really the connection with electrochemistry.

BOHNING: Well, as you said earlier, what impressed you about the Society was that it was diverse.

READ: Yes, it always has been. Right from that very first meeting in Washington, that has been my reaction to the Society and a reason for becoming active in it, continuing to this day. I'm not active at all professionally anymore—I haven't been since I retired in 1971—except that I follow things. The Electrochemical Society has so many branches that it's a place to follow things.

BOHNING: I want to wait before we get to your term as president of the Society, but perhaps we should look at the pre-presidential Society activities on this list (9). We talked about local sections and divisions, publications. You were also on several awards committees.

READ: On a lot of them. In fact, I just can't remember them all.

BOHNING: Was there anything particular about those committees? Were the types of nominations you got pretty good nominations, or did you have to go asking for nominations?

READ: Sometimes yes, sometimes no; to some degree it depended on the award. For instance, there was one called the Roeber Research Fund, a rather minor one. You had to fish for nominations for those. A major award, like a Palladium Medal and so on, no problem. We got plenty, at least in those days. I understand they're encountering difficulty nowadays—I think largely because there are so many awards. Forty or fifty years ago there were very few awards that were substantial in terms of both prestige and money.

BOHNING: The Society has a larger number of awards now, is that what you're saying?

READ: I was saying that the scientific community as a whole has, including The Electrochemical Society. Yes, we have far more awards. One of the reasons has been the money derived from publications. Because the various groups within the Society have become rich through royalties from monographs developed by them. Proceeds from book publication accrue in part to the Society in general, and in part to the groups that sponsored them and are responsible for the publication. This has spread not only to monographs but to proceedings and to special publications of one sort or another. Incidentally, I've forgotten how much *Modern Electroplating* brought in. Years ago, the *Corrosion Handbook* had brought in over forty thousand dollars to the Society and to the Corrosion Division. The Corrosion Division takes care of awards running around five thousand dollars and doesn't feel it in their treasury. [laughter] It's been a bit of a problem. You have to watch the IRS.

BOHNING: Yes. They wonder if you're still nonprofit when you're building up those reserves.

READ: It has been a source of considerable planning on the Society's part to keep its nose clean.

[END OF TAPE, SIDE 3]

BOHNING: There are lots of general-type questions. Maybe what we should do now is go to that period in the 1960s when you became president, starting with number seven. Then we can work through the other questions below that.

Why did you want to be president?

READ: Well, I can't remember that I ever wanted to. A little more specifically, I hadn't really thought much about the presidency. I was asked by the Nominations Committee if I would accept the nomination for vice president, which is the first step toward the presidency. Would I accept? That's a part of our Society rules; nominees must indicate their acceptance. Actually I took a fair amount of time in making a decision. I decided finally that I would like to do it, and sort of halfway hoped that I wouldn't win the election. There are always at least two candidates for the vice presidency.

Well, I accepted. I had to justify the amount of time that it would take. I was very busy as a consultant. Also, I was extremely active in university affairs, particularly with the university senate, to which my colleagues repeatedly reelected me. I wasn't sure that I could properly divide myself up amongst these activities.

I finally decided that I could. I'm glad that I did, as it turned out to be an interesting and rewarding experience and not too big a drain on my time. I didn't go into it, let's say, without thinking it over in detail. Of course I knew what was involved because I had been active in this, that, and the other activity of the Society for so long that I knew what the story was. What I didn't know was that our then-executive secretary was going to die while I was a vice president. One of the reasons that I accepted the invitation to be a nominee was that I knew the executive secretary. He was called simply the secretary in those days, but he was in point of fact an executive secretary. I knew him well, I knew he was capable, and I knew that he would do most of the work, in fact all of it, really. Then he died of a heart attack while I was still vice president.

Fortunately, Ernie [Ernest] Enck offered to take over the job as secretary. That was a lifesaver, because I knew Ernie very well. He was a Philadelphia man. I knew his capability, I knew that he had time, I knew he had the interest—so I was saved, but it would have been a very difficult situation if the Society had elected to choose somebody with whom I couldn't work well.

BOHNING: Did you have any agenda at the time? Was there something specific you wanted to accomplish during that period?

READ: I can't recall really that I did. The Society seemed to be going along quite well. There'd been a fair amount of reorganization and things had been changing, but the era of transition had passed. I was interested mainly in insuring that our publication policy, to put it bluntly, was what I liked. Since the Society seemed to be going in that direction, my main thought was, "Well, let's keep it going that way." The direction in which it was going was to make the Society more than a simple reporter of our meeting activities and more a place for publication of material of interest in a wide field. I think that maybe I made a little contribution in this direction, sort of continuing how I had felt as the technical editor in the initial days of the *Journal*. I think that if I had, say, gone on and on in this activity, things might have gone a little differently than they have now, but maybe not too much.

One of the things that I was concerned with was *Electrochemical Technology*. This was a publication which was started to take care of material which was more practical, more industrially oriented than the core of material that came to us in one way or another for the *Journal*. Unfortunately, it turned out that while the publication was a success initially, it suffered from the death of the initial editor, Al Loonam. He was holding it together. When he died, the Board of Directors through the Publications Committee accepted the offer by Norm [Norman] Hackerman, who was already editor of the *Journal*, to take on *Electrochemical Technology* as well. I think this was a serious mistake. Norm did an excellent job with the *Journal*. No doubt about that. I don't think he was equally interested in *Electrochemical Technology*. Norm is not an industrial man at all, quite the opposite. He just wasn't interested in *Electrochemical Technology*, and it fell by the wayside. I was the one who killed it, inasmuch as I was chairman of the ad hoc committee which recommended discontinuing the publication. I hated doing it. In fact, the three of us who constituted the committee hated what we suggested had to be done. We just didn't like it. We didn't see any way out, though. It was running into too much money. There seemed to be no way of finding an editor who would really do the job that Al Loonam did.

The three people who recommended discontinuing it were Charlie [Charles L.] Faust, a long-time member of the Society and past president, and Ralph [A.] Schaefer, again, a long-time member and past president—both of them very active in the Society—and I. Charlie Faust was at Battelle, and Ralph Schaefer was an industrial man—so we had me as an academic, Charlie Faust as an institutional member, and Ralph as an industrial member of the committee. I was the one who appointed the committee with myself as chairman. Intentionally I kept it very small—just the three of us—as I was well aware if we included Tom, Dick, and Harry and all their cousins, we would just go on and on and probably never arrive at any very good solutions.

As things worked out, we just discontinued the magazine with really no serious problems. Oh, we had a very, very small amount of recrimination by those who felt otherwise than we did. That was actually my main activity during my presidency, dealing with *Electrochemical Technology*. Apart from that, I can't really remember anything that was much of a problem. Things went pretty routinely with Ernie Enck taking over as secretary, because he'd been a member of the Society so long and was so good at organizing things. Although he was a

chemist by profession, he was really much more of a businessman by reality. That's exactly what we needed. He took over things—made life very easy for the president, at least for me.

BOHNING: Did you have to travel a lot during that year?

READ: More during the vice presidency. The custom in the Society is that the vice presidents, of whom there are three, will divide up the sections amongst themselves and visit each of the sections once. That means that each vice president doesn't have to do the whole bunch; he does just a third of it. There was that travel. Of course I had to travel from State College [Pennsylvania] to New York for the Board of Directors meetings, but that was easy. It was of course by train in those days, but there was no problem at all there. I traveled so much as a consultant that a little bit more didn't make much difference. As a consultant, I practically commuted between Chicago and State College at that time.

BOHNING: Whom were you consulting for during that time?

READ: Graham, Crowley & Associates for one. Also, I was consulting for Naval Ordnance and for Picatinny Arsenal—but some of my work for Picatinny took me to Chicago rather than Picatinny. I did a great deal of work over the years for Naval Ordnance, mainly on corrosion.

BOHNING: Why don't we look at some of the other questions on this agenda below number seven, unless there's anything else you want to add about your year as president.

READ: What I'd like to add is something that was done during the time that I was the first vice president, while Ernie [Ernest B.] Yeager was the president. Do you know Ernie—or of him?

BOHNING: No.

READ: A very famous electrochemist, theoretical as all get out, at Case—back in his days, the Western Reserve University. He was an extremely active president. In fact, he did so many things that when I followed, I didn't have as many things as I might have had to take care of otherwise.

One of the things that he wanted to do was to bring the past presidents more actively into the Society's activities after they had completed their presidential year. It was he who suggested

the formation of the Council of Past Presidents, which is not a very active group—but it does meet twice a year at each of the semiannual meetings, at a breakfast get-together, and the proceedings are all off the record. The current president will be present as an invited member of the group to discuss Society activities of any kind. I and others thought this was a splendid idea, and to some extent it fell to me to implement it as the successor to Ernie. The council has gone on to this day.

So one of the things that I was involved in was the Council of Past Presidents. I'm about the only one alive and still attending meetings who attended the initial meetings. Unhappily, Ernie, who is still alive, is in very bad health and doesn't attend the meetings anymore.

We can pass on now. Am I talking too much?

BOHNING: No, no. This is fine. What was the relationship of the Society to other scientific organizations in general, over the time you were associated with it?

READ: During the time that I was president and associated closely with the Society activities, I was not aware of any problems. I know that early in the life of the Society, way back, there had been problems—largely because a number of people wanted to do things differently than the ACS, which is not too surprising because the ACS, after all, couldn't cover everything—but I can't recall if there were any problems.

There was cooperation to some extent with the Society of Chemical Industry and with the American Institute of Chemical Engineers. These activities consisted of appointing joint members to various committees, which still prevails to a certain extent. I don't remember any problems. I think that in general if some society asked us to cooperate, I can't recall that there were any problems. On the other hand, I can't recall that we ever went out seeking cooperation from other groups. We were so broad in our interests that if something new came up, we didn't ask another society to take care of it; we took care of it ourselves—like the fullerenes, presently. There is absolutely no reason whatever for The Electrochemical Society to be the main publisher and symposium arranger for the fullerenes. It's extremely hard to find any relationship at all outside of the electrons involved [laughter], but those working in the field needed a home, and they were doing something interesting, something new; grist for The Electrochemical Society. They're going great guns now.

BOHNING: Now that you mention fullerenes, what was the Society's relationship with the Fleischer-Pons cold fusion type of situation?

READ: Well, that's not an easy one. I have to be a little careful here. First of all, Pons had been a recipient of a Colin Garfield Fink summer scholarship and had been a member of the Society for many years. There was, of course, a good deal of interest because this time the new development was clearly electrochemistry, no doubt about it. [laughter] John O'M. Bockris, of course, jumped into this, so did Ernie Yeager; but most intimately, Fritz G. Will, who was a past president of The Electrochemical Society, became the director of the project on cold fusion out at the University of Utah, which of course brought the Society directly into it. Several symposia were arranged over a period of a year or two by the Society to look into the so-called cold fusion. Happily, the symposia were arranged in such a way that opinions could be expressed without committing the Society to anything, including without committing any publication at all. Those of us who were consulted at the time thought this was the thing to do. It worked out all right.

At least the Society provided a forum for discussion of this without endorsing or condemning any aspect of it. It was simply a forum. I think it was handled very well. This has occurred on some other occasions, too, particularly back in the days of arguments about mechanisms of bright electroplating. Does this answer your question?

BOHNING: Yes.

READ: In other words, we participated as a provider of the forum without putting our fingers too deeply in the pie. I've had discussions with Fritz, whom I know very well, and I'd just as soon not enter into that.

BOHNING: No, that's fine. Question number nine, what was the relationship of the Society to industrial sponsors?

READ: I think on the whole, very good, but not extensive. We look upon industrial sponsors as a means of financing awards—that's one thing—and to a limited extent as a source of Society income for general purposes. I have tried to interest the Society in attracting more individuals, rather than industrial people, to become supporters of the Society through contributing memberships. It hasn't worked at all. [laughter] There are only two of us. I happen to be one. I'm not a rich man, but I do have a comfortable income, and I'm a sustaining member of the Society. The other one is Joe [Joseph] Schumacher out in California.

Now, to be a little bit more serious. I don't think that I have ever encountered any real problem in the Society of conflict between industrial members and members in academia or research institutions. It's surprising, really, to me, how well that has gone on. I think some indication of harmony is a pretty good balance between industry and academic areas in fulfilling the presidency of the Society. I went back 70 years and found that there are 45 past presidents

from industry and 25 from academic areas. I had a little trouble categorizing a couple of the past presidents. Although I have no data on the ratio of industrial to academic members of the Society, I am quite sure that the number of members who work actively on Society affairs greatly favors industry. Hence, the ratio for past presidents seems satisfactory; no problems.

BOHNING: That leads into the next question, which is about the relationship between the academic and industrial contingents within the Society.

READ: This is a very difficult one—very difficult. First of all, just what do you mean by the industrial contingents?

BOHNING: Well, I'm thinking of the American Chemical Society, where the academic chemists and the industrial chemists are frequently far apart, with totally different interests and agendas.

READ: I thought that's what you had in mind. I've tried to formulate some kind of an answer to this, and I haven't come up with anything very good, but let me put it this way. In my experience, when I asked somebody in industry to participate in one way or another in Society activities, I can't recall any problems at all. I didn't always get an acceptance, but if I had a refusal, it was always on the basis of excellent grounds. On the other hand, if I asked somebody in industry to do something and the task was accepted, almost always it was done. In fact, I found that I got better results, as far as getting things actually done, when I went to people in industry rather than in the academic field.

[END OF TAPE, SIDE 4]

READ: I think that's about all I can say about that.

BOHNING: What about the relationships between the various divisions? You come to this great diversity again.

READ: The relationships between divisions are not always without abrasions. There has been abrasion on occasion. Thus far, in my experience, problems have been worked out pretty well. They usually show up at Board of Directors meetings, because the Board of Directors, under the present setup, and for quite a few years past, has, for the most part, comprised the chairmen of the various divisions. It is here that there is likely to be friction, and this has happened

sometimes, but providing the chairman does his job, it usually doesn't amount to very much. The chairman—president, of course—sometimes lets discussion of controversial matter go on too long but usually he steers things to agreement or compromise.

Now, the other place where abrasion can develop is in the scheduling of the presentation of papers to be presented at the meetings. This is done in the Society headquarters at a meeting of people involved as representatives of each of the divisions which has a symposium scheduled. Almost all of the divisions and groups will be represented. Of course there occasionally are fights for time. On the whole, these get ironed out pretty well. For the most part, it's not the responsibility of the president—the president doesn't even show up—to run the scheduling session. This is one of the many places where our executive secretary comes in, and, to a great extent, our director of publications, who is Sally [Sarah A.] Kilfoyle. It is hard to know what'll happen when she retires.

In any event, scheduling is one place where some abrasion has developed, but it's just an immediate matter rather than policy.

I think as far as policy is concerned, any problems have been ironed out pretty well between the sections themselves, or at least in the Board of Directors meetings. I'm surprised there haven't been more problems because of the diversity, but maybe it's the fact that we are so diverse and so used to new things confronting each other—it's odd how this happens from time to time—we're so used to it that there's no problem.

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

READ: Oh, I think this is where we shine. To take some specific examples—as you know, in the last few years, there's been considerable development on the production of diamond surfaces. I'm not thinking of what is put into an engagement ring, but diamond-coated surfaces for abrasion resistance, conductivity, and some other reasons. Here again, there's something very new that the Society has provided a forum for, with great success—both to the Society and to the people interested in this area. This has happened time and time again. I could hark back to the fluorescent materials as another example. When bright electroplating first became an object of development, members of this Society jumped right into the field and really made it. I could think of some other examples. Again, it's the diversity that we've had for so long that makes it possible to do this, but also the fact that we're used to it. When the fullerenes came along, and people were looking for a home, so to speak—"Oh, we've got plenty of room."

BOHNING: What about responsiveness of the Society to members' needs? That's question number thirteen.

READ: Well, I thought you were referring to scientific needs, not to insurance or that sort of thing.

BOHNING: No. Scientific needs.

READ: I've tried ever since reading question number thirteen to think of some example which might be adduced here. Really, I can't think of much to answer to this one. From time to time there have been some requests by members to do something about getting students to enroll in a specific area of electrochemistry. What can the Society do to encourage students to come into some specific area of interest or some specific group? Something has been done about it in the way of providing published material that can be used by circulation in schools and so on. This is one example where there was a positive contribution by the Society to something that the members had felt was a problem or a need. It has been done on occasion, but not very often.

BOHNING: Would you then classify the members as being satisfied with their membership?

READ: Over the years I've heard very, very few objections to the way the Society operates, or to the way that he or she has been treated. I can recall two, but they were screwballs in both cases. One of them was an extremely capable screwball, and the other one an incapable screwball.

I'll never forget Charlie Kasper, a brilliant, brilliant man, offered the Society four papers in a group and a fifth a bit later (10). Extremely theoretical. He had produced a magnificent mathematical treatment of current distribution in electrolytes during electrolysis. The Society published them after a great deal of trouble, and I had quite a bit to do with it. Every time that I met Charlie after that, he would carry on and carry on, accusing me of trying to ruin his career—not only accusing me, but others who had been responsible for getting his papers published—but this is the exception, very much the exception.

BOHNING: I think that, by comparison, the ACS has on occasion gone through periods where there were more than just a few dissatisfied members. In fact, I think one president was elected at one point as a write-in. There was a grassroots movement.

READ: Yes, what was his name?

BOHNING: Alan [C.] Nixon. I think he's the one who was really a grassroots-level candidate.

READ: Yes. Nixon. Yes, it was Nixon. He was a bit of an iconoclast. I never knew him and never paid much attention to his views or actions.

Actually, the Society hasn't been called on too many times to come to the rescue of its members in one way or another, but they've done what they could. Usually travel expenses to meetings were involved.

BOHNING: What about positive and negative changes in the Society?

READ: Well, this is one that I could talk on all afternoon, I guess, but I won't. The changes that I have witnessed have been tremendous. When I first started going to meetings in 1935, there was just one session at a time compared to the dozen or more that now meet simultaneously. All of the papers had been reviewed and approved prior to the meeting, and preprinted, so that when you came to the meeting, you had preprints of everything that was going to be presented. Thus, more of the presentation time was devoted to discussion, and the discussion was recorded and published in the *Transactions*. Of course it was edited by Colin Fink, but at least it was recorded. [laughter]

Everything was much more intimate because the number of people at the meeting would be two or three hundred, as opposed to fifteen hundred, eighteen hundred now. In a way this was a fine thing, particularly for a young man. You got to know people in a way that today would be very difficult. On the other hand, it limited rather severely what the Society could publish, because the only venue for publication was the *Transactions* of the meetings. As I indicated earlier, this was a consideration which led to the establishment of a monthly journal.

The Society changed—particularly when the *Journal* was established in 1949 to publish articles independently of what was presented at meetings, and papers presented at meetings were accepted for presentation without peer review and with no assurance of publication. This of course was a tremendous change, and I think on the whole for the good. Although I must admit that at the time I was pretty hard to persuade that this was the way the Society should go, I'm glad it did. On the other hand, the negative part of this great increase in attendance is that it became more and more difficult for young people to meet the older, more experienced members of the Society. This is the negative side of it. Another part of the negative side—I think the most serious one—is that the quality of the papers presented at the meetings has deteriorated tremendously.

BOHNING: Any reason for that?

READ: Yes. The acceptance for presentation is on the basis of abstracts rather than a complete paper. It is true that the abstracts are two in number, a short one and a long one. Often they don't reflect what is really in the paper. All too often it would seem that people are offering papers for presentation only to justify expense accounts. There are people who argue with me on this account, but I'm pretty hard to convince otherwise.

BOHNING: It's very easy to write an abstract.

READ: Yes. Also, you can talk about something for twenty minutes, which is the length of time that is allowed for most papers. It used to be that when you were allowed one hour, you needed some meat—maybe not all meat, but at least you needed some meat.

Here I think is a very negative side of present meeting procedures. Some of the papers are absolute stinkos. They don't make it for publication at all. They just won't get by peer reviewing, but they take up time at the meetings. Sometimes I get pretty seriously annoyed. On the whole, I think that probably the way we operate now is still an impractical way, but after all, things have changed a lot in every society, and not always for the better.

BOHNING: It may be that after you reach a certain number in your membership, you're going to lose certain things. It doesn't make any difference what the upper number is, but at a certain point, passing over that—although it's great for the Society in some respects—causes other problems. For example, you might never have met Charles Burgess if there had been fifteen hundred people.

READ: I very likely wouldn't have. In fact, it's highly improbable.

BOHNING: That's a very interesting point.

READ: Incidentally, Burgess was not the only one by any means. For instance, Lawrence Addicks, a past president of the Society and quite an elderly gentleman when I was a graduate student, was one of those who helped me a great deal, principally because I met him at an Electrochemical Society meeting—maybe it was the one in Washington, I'm not sure. In any event, he was interested in what I was doing. In fact, he was able to provide me with several pounds of silver for my doctoral research project because he was consultant for a silver

producers' association. Not only did he present me with the silver, but he followed up and always wanted to know how things were going with me, not only on the project but also in my other activities.

To some extent the Society recently has done something that may help a little bit. It's an idea that I've had nothing to do with. They've organized poster sessions, in conjunction with the semiannual meetings, at which the posters are prepared and presented by students. The students are present at the session where the posters are presented. This gives an opportunity for people to become acquainted. I'm a little bit concerned about this. Of the two sessions held so far, one was very well attended by presenters and the other one not so well. There will be a third one in Reno in May of this year. I'll be watching very anxiously to see how it works out. The one that wasn't well attended was in Miami Beach. Transportation difficulties may have been a factor.

BOHNING: Miami Beach is not a very nice place to have a meeting, at least I've never found it to be so.

READ: I think it's absolutely stinko, and I've said so [laughter].

BOHNING: Oh, yes, yes. Is there more you want to add on question fourteen? You said you could go on for some time there. I don't want to cut you off on that.

READ: No. Well, I could go on because I could reminisce on so many things, going from one year to another, but I think I've summarized it—I hope—reasonably well.

BOHNING: Okay. Question number fifteen, how have your professional work, your career, and your contributions to science and technology been influenced by the meetings of the Society and your contacts with the Society?

READ: Well, this is one on which, again, I could talk at considerable length. I just can't overemphasize how much the Society has contributed to whatever career I may have had—not only to science and technology, but also to human relationships. Working with people in the Society over so many years, with so many people and in so many different activities, has been really a very contributing factor to whatever I may have done. It's been the contact with people, I think as much as with science and technology.

To be specific, as far as academic work is concerned, the Society has not been much of a factor really; but in my consulting practice, which was quite extensive, it's been an enormous

factor in that I was able to cover so many fields as sources of information in the Society's meetings, but also in the variety of people whom I met, and either just contacted or actually worked with, it's been quite a factor. My consulting activities were as diverse as the Society. They were diverse because of my background in both chemistry and metallurgy and way of thinking of things that came out of the diversity of Society activities. Although I consulted mainly in the field of corrosion and various aspects of electrometallurgy, including electroplating, I also got into quite a few other things just because I had always been interested in variety; unusual batteries were one thing, torpedoes another, and various aspects of photography. This one I don't talk much about. I never published anything in this field—but strangely enough, there are some fascinating metallurgical problems in technical photography. [laughter]

BOHNING: Okay.

READ: I've sort of summarized things here. It's difficult to do much of anything else without going into details which I'm sure would not interest anybody. I hope I've pointed out that it has been a tremendous influence. I've had just tremendous rewards for the time that I've spent working with Society activities.

BOHNING: Question number sixteen, what should the Society do in the future?

READ: The easiest way to say it is, just keep on doing what you've been doing. I shouldn't use the term we as much as I have in talking about current activities because I have nothing to do now with the operation of the Society, except to criticize occasionally and compliment once in a while, but I still feel that I'm a part of the Society's activities, in spite of the fact I'm not active. If that's confusing, that's not meant to be.

BOHNING: No, I understand.

READ: We're facing a very serious problem in publication, the same problem that other scientific societies face. It's becoming extremely expensive to publish the kind of material we publish. More seriously, what we publish is getting buried deeper and deeper all the time in the welter of new information. The problems of publication are really twofold. One is the means of putting information into storable form—be it printing, magnetic tape, or whatnot. The other is what to do with the stored form: traditional library, data bases on disks, et cetera. I can't offer any solution to either of these problems, but I see them as problems that the Society must deal with, and in the not very distant future, either.

Happily, this is really being addressed positively by the Society. The present president is James [A.] Amick, who is very conscious of communications problems of all kinds, including publication. He is very actively doing whatever can be done currently to try to anticipate what's going to happen to us in the future in this area. I think this is really the thing that is of major concern to the Society for the future. We can continue doing, with no immediate problems, what we have been doing in other areas. What we are doing in publication, which is really a critical part of the Society activities, is where something has got to be done. There's just no way out, although I don't know what it's going to be—but I'm so glad that the Society, and its current officers, are not only aware of this but have the courage to begin addressing it.

I'm looking a little bit ahead to the coming presidency because we can always look three men—or in this case two men and a lady—ahead. I'd rather not evaluate what I think are the capabilities of those who will follow Amick. First of all, I don't think I have the background. Secondly, it's too early in their careers as officers of the Society for me to formulate any feelings. I feel, however, that the continuity that exists in our Council of Past Presidents will keep publication problems alive. I can only hope that if the individuals involved are themselves not too critically interested, they will be nudged by others into doing whatever can be done. I'm hopeful that things will be all right, and very, very convinced that at least the current attack on this problem is about all that can be done at the present time.

As to the second of the twofold problems in publication, I'm very much concerned about the burial of information which seems to me is occurring. Present-day graduate students don't do a literature search. They get their sponsor to buy databases which are often very inadequate, largely because of indexing problems. I can remember, my own experience in literature search was that I often had to change course in midstream as to how I chose where to go next. If a computer is doing this, I'm not sure it does it well.

[END OF TAPE, SIDE 5]

READ: Understand, I have no objection to the electronic storing of information in one way or another. It's something that we just have to face—but I have my concern on whether or not it will always do what we would like it to do, in part, because your computer doesn't put information into itself. We still have to put it in. Even more seriously, we have to know how to use what comes out, and perhaps at least for a time, we have to know when to abandon what a database can give us, and search the literature for ourselves. That the Society is going to be involved in this, I think, is inevitable. After all, publication in one form or another is our reason for being. We've got to keep track of what has been done. Let's hope for the best. I wish I could participate, but I can't. This area I can only view from the outside—perhaps wrongly. I hope not.

What else besides publication? Well, as I said before, what we can do is just continue doing what we've been doing. When something new comes along and needs a place, as long as at least electrons are involved, give them a home. Let's see what they can do.

They don't always do well. There have been some new sections of the Society which flopped. Most of them have come along—sometimes only for a period, and sometimes as permanent divisions or groups of the Society. The Electronics Division of the Society is, of course, an example. It's been a very strong part of the Society, and it could well have been missed if the people in the Society hadn't been willing to accept some new things. The transistor, of course, is an outstanding example. It was largely because those who were responsible for most of the work, either in the laboratory or in management, were active Electrochemical Society members and persuaded the Society to start publishing more actively in this area, in opposition to electrical engineers.

BOHNING: Was [N.] Bruce Hannay involved in that?

READ: Yes. He was a past president, of course. Even more strongly, although not so obviously, was Bob [Robert M.] Burns.

BOHNING: Wasn't Hannay's predecessor at Bell Labs Bob Burns?

READ: Yes.

BOHNING: Okay.

READ: Yes. Bob was the secretary of the Society, and very much instrumental in changing the Society's meeting format and publishing activities—very, very much so. Of course he was a past president, but he was also a very influential secretary and a gentleman, a very fine gentleman. I can remember a meeting of the Board of Directors which I attended as, I think, Publications chairman, and Bob told us about the transistor invention. Jake Warner was at the time president of the Society. Jake and I always went to Keen's Chop House for dinner after the meetings. Our principal item of conversation on this occasion was the transistor that we had heard about for the first time. This was before it was published. It was interesting. I must say that we didn't predict very accurately it's future [laughter]—no idea at all of what it would develop into.

The Society in the future should continue to do what it's been doing and not be afraid to have at whatever may be developing.

BOHNING: The last question is, what do you see as the future for electrochemistry? Is it alive and well?

READ: I was going to say that I don't see any end to it. [laughter] After all, electrons are pretty well established, even though quarks and whatnot come along. [laughter] Seriously, electrochemical phenomena certainly do, in one form or another, prevail always. I think that the only thing that could really prejudice the future of electrochemistry as a sort of separate science is, there will get to be so many branches that will establish themselves rather strongly that electrochemistry as such, as a term, might disappear—but only as a term. I think actually the field will always remain, but it may get even more diverse—branched—than it is now.

There are some factors to growth which make it self-limiting, at least to a degree. If we get too many people on the Board of Directors in too many fields, there's going to be a time when the divisions will not be as active in some fields as in others, and there will be a shift in actions of the Board to favor active divisions. Our Board of Directors is dictated, as far as numbers of directors are concerned, by our membership. We're incorporated under the laws of New York State, and there are certain restrictions, which I don't remember in detail anymore, on the number of members on the Board of Directors. It's mandated in certain ways. Now, one of the effects of this, I think, is to sort of limit overexpansion—simply because if you get too many fields, some of the new ones can't find a place on the Board of Directors for representation.

To some extent, that's occurring now. It's been sort of semi-solved, in a way, by establishing not only sections and divisions but also groups. The groups have no representation on the Board of Directors simply because the Board of Directors is filled. For those groups to become more actively represented in the affairs of the Society, they just have to wait for an opening. One way to do this, of course, would be to enlarge the membership so as to provide additional seats. This is not likely to happen. I think our membership is going to stay pretty constant. It runs about five thousand, and has for some time. I think this is about the limit. I'm not sure why, but apparently this is what happens.

BOHNING: What's the possibility of ECS spawning a new organization if one of its divisions gets big enough and specialized enough to go out on its own?

READ: I think there's every possibility of it. I'm trying to remember if this has ever occurred.

There's a group—I'm not sure what it's called—but it's a materials group. This group comprises as ringleaders quite a few Electrochemical Society members. There's no friction at present between the new group and the Society, but they do encompass an area which The

Electrochemical Society has been active in, that is, material science. I'm sorry to be so vague about this. Is Erik [M.] Pell on your list of prospective interviewees?

BOHNING: Not at this point, and I'm not involved in selecting who goes on the list.

READ: Erik is an industrial man, and this group is largely composed of industrial people. Unfortunately, I have to be vague about it. I know that this group is to some extent an offspring of The Electrochemical Society, although they may not realize it or claim it, because it's the same people who comprise both groups. I think this is an unusual situation for the Society.

I think more frequently a field has petered out and thereby relieved The Electrochemical Society of an area which is no longer very important. I can give a very specific example of this, and that's the field of electrothermics. When I joined the Society, this was the ringleader group. They were mainly interested in carbon electrodes either for electric furnace operation or for industrial electrolysis such as chlorine-caustic production. They were very powerful. Today they are as nothing. They have not established any new sub-area of interest—actually because there's very little going on in that field, and very likely not to go on, particularly in the making of electrodes for electrothermic operations.

It's rather different in chlorine-caustic production. This is a very, very important field for electrode development, but not the graphite and carbon electrodes of traditional electrothermics. This is a new area entirely which has been very strong in the Society, but it is not electrothermics. Electrothermics is steel-making, abrasive-making, silicon carbide, and graphite itself. Those were strong, very strong, people who supported the Society financially as well as scientifically, but they have just faded away.

I think this has happened to some extent in practical electroplating. There's hardly anything now in the activities of The Electrochemical Society having to do with actually producing an electroplated product. This has been taken over pretty well—and properly—by the American Electroplaters Society. It's been taken over, but not well done.

Here was an area within the Society's divisional set-up which was lost in one way, but still is with us in a new way. This has happened in other fields also. Of course I've been very closely associated with the electroplating or the electrodeposition field, and perhaps this has made me a little bit more conscious of what has happened there. It used to be that the Electrodeposition Division papers were largely concerned with actually electrodepositing metal. Today the Electrodeposition Division people rarely deposit any metal. All they do is rig up their galvanostats or potentiostats and their other stats, and publish curves, which is fine. I'm not objecting at all—but it has shown what can happen within a division, without losing the division itself. With the change in scientific emphasis there has come a change in people. I'm getting to the point where almost all of the people whom I knew well in my own field have either died or

completely retired and never attend meetings. I'm getting to that point, so some of the things I say are pretty well dated.

BOHNING: Well, that brings us to the end of my list. Is there anything else that you would like to add at this point?

READ: It's been a very complete list. Really, there's nothing much that I can add to what I've already said, perhaps at too much length. I tried to think of what I might do currently that would be of some value to the Society; unhappily there isn't very much. I'm a consultant to the group that's planning the one hundredth anniversary celebration, and all I'm doing is criticizing a little bit and making a few suggestions. That's about as rewarding as I can get at the present time.

BOHNING: You said you were also writing these things for *Interface* too, which I think is very useful and very helpful.

READ: I'm not sure that it's helpful. [laughter] I said, I think, that some of the things were accompanied by a bit of—not poetic license but prosaic license. Also, occasionally I indulge in what I like to call the Pooh-Bah syndrome. You know what I'm referring to?

BOHNING: I think so.

READ: Pooh-Bah, *The Mikado*? [laughter]

BOHNING: Yes.

READ: In the worst case, my contributions to *Interface* contain a little bit of decoration—as Pooh-Bah said, "To lend verisimilitude to an otherwise bald and uninteresting story." [laughter]

One of these things, as far as the Society, that I'm concerned about, frankly, is a successor to Sally Kilfoyle. She's the director of publications, and an extremely capable and very interesting woman. It'll be hard to find somebody the equal of her. I'm continually astonished at what is done in the way of physically publishing material with such a small staff. I've had, in one way or another, a fair amount to do with publication. I just sometimes can't believe how much material of good quality she turns out with such a small staff. I'm always interested in comparing the staff for *National Geographic* with the staff for the *Journal of The*

Electrochemical Society. Sally and her small staff get out far more pages of more complicated material per month than the huge staff of the *Geographic*.

BOHNING: I can appreciate that.

READ: In any event, I think we've covered things pretty well. At least, I hope so.

BOHNING: Oh, absolutely! I certainly appreciate your spending the time with me this afternoon. I think we've got some very good information, and I certainly appreciate your sharing your experiences and history with me this afternoon.

READ: Well, it's the least I could do. [Paul C.] Milner approached me about participating in this. I must say that I took a day or so to consider it, because I was wondering whether it would be worth not only my time but more importantly, the time of whoever did the interview—in other words, you. I decided at least they had to keep somebody busy. [laughter]

BOHNING: Well, I think it's been very good, and I really do appreciate it.

[END OF TAPE, SIDE 6]

[END OF INTERVIEW]

NOTES

1. Read's senior thesis with Sherlock Swann, at U. of Illinois, was published as:

   Sherlock Swann, Jr., H. J. Read, and F. C. Howard, "Electrolytic Reductions of Organic Compounds at Alloy Cathodes. I. Reduction of Aliphatic Ketones to Hydrocarbons at Cadmium Bismuth Cathodes," *Transactions of The Electrochemical Society*, 69 (1936).

2. Harold J. Read and Martin Kilpatrick, "Silver Membranes," *Transactions of The Electrochemical Society*, 74 (1938).

3. Harold J. Read and A. Kenneth Graham, "Electrolyte Films in Acid Copper-Plating Baths," *Transactions of The Electrochemical Society*, 78 (1940).

   Harold J. Read and A. Kenneth Graham, "Further Studies on Electrolyte Films," *Transactions of The Electrochemical Society*, 80 (1941).

4. Harold J. Read, Article on the principal planning session for the Spring 1940 meeting at Wernersville, PA, *Interface*, 3, No. 2 (1994): 19.

5. Edwin M. Baker, William Blum, Robert T. Gore, A. Kenneth Graham, Richard O. Hull, Harold J. Read, Leon Westbrook, and Gustaf Soderberg, editorial committee, *Modern Electroplating* (The Electrochemical Society, 1942).

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

7. Allen G. Gray, ed., *Modern Electroplating* (New York: J. Wiley, 1953).

8. Harold J. Read, "Modern Electroplating: The First ECS Monograph," *Interface*, Vol. 4, No. 1 (1995).

9. 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 #0145.

10. Charles Kasper, Five papers on current distribution in electrolytes during electrolysis, *Transactions of The Electrochemical Society*, 77 (1940): 353; 77 (1940): 365; 78 (1940): 131; 78 (1940): 147; 82 (1942): 153.

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