CHEMICAL HERITAGE FOUNDATION

NORMAN HACKERMAN

On the Gordon Research Conferences

Transcript of an Interview
Conducted by

Arnold Thackray and Arthur Daemmrich

at

Philadelphia, Pennsylvania

on

12 March 2002

(With Subsequent Corrections and Additions)

ACKNOWLEDGMENT

This oral history is one in a series initiated by the Chemical Heritage Foundation in collaboration with the Gordon Research Conferences. The series documents the perspectives of key individuals who organized and managed the Gordon Research Conferences and records the Conferences' impact on scientists' research, careers, and lives.

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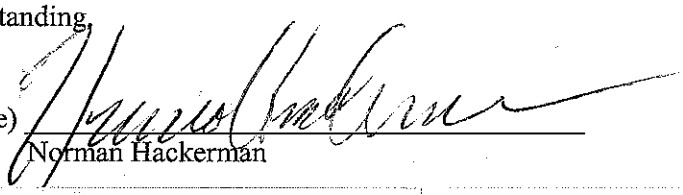
(Signature) 
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NORMAN HACKERMAN

1912 Born in Baltimore, Maryland, on 2 March

Education

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

Professional Experience

1935-1939 Assistant Professor of Chemistry, Loyola College
1936-1940 Research Chemist, Colloid Corporation
1939-1941 Assistant Chemist, United States Coast Guard
1941-1943 Assistant Professor of Chemistry, Virginia Polytechnic Institute
1944-1945 Research Chemist, Kellex Corporation

University of Texas at Austin
1945-1946 Assistant Professor of Chemistry
1946-1950 Associate Professor of Chemistry
1948-1961 Director, Corrosion Research Laboratory
1950-1970 Professor of Chemistry
1952-1961 Chairman, Chemistry Department
1960-1961 Dean of Research and Sponsored Programs
1961-1963 Vice President and Provost
1963-1967 Vice Chancellor for Academic Affairs
1967-1970 President
1985-present Professor Emeritus of Chemistry

Rice University
1970-1985 President
1970-1985 Professor of Chemistry
1985-present President Emeritus
1985-present Distinguished Professor Emeritus of Chemistry

The Robert A. Welch Foundation
1982-present Chairman, Scientific Advisory Board

Honors

1956	Whitney Award, National Association of Corrosion Engineers
1964	Joseph L. Mattiello Award
1965	Palladium Medal, The Electrochemical Society
1965	Southwest Regional Award, American Chemical Society
1972	LL.D., St. Edwards University
1975	D.Sc., Austin College
1975	Honor Scroll, Texas Institute of Chemists
1978	D.Sc., Texas Christian University
1978	LL.D., Abilene Christian University
1978	Gold Medal, American Institute of Chemists
1981	Mirabeau B. Lamar Award, Association of Texas Colleges and Universities
1982	Distinguished Alumnus Award, Johns Hopkins University
1984	Edward Goodrich Acheson Award, The Electrochemical Society
1984	Alumni Gold Medal for Distinguished Service, Rice University
1987	Charles Lathrop Parsons Award
1987	AAAS-Philip Hauge Abelson Prize
1993	Vannevar Bush Award, National Science Board
1993	Doctor of Public Service, University of North Texas
1993	National Medal of Science
1999	Texas Distinguished Scientist Award, Texas Academy of Science

ABSTRACT

Norman Hackerman begins the interview with a description of his graduate experience and encounters with Neil [E.] Gordon while at The Johns Hopkins University [JHU]. After graduating from JHU with his Ph.D. in chemistry, Hackerman became a steady participant in the Gordon Research Conferences [GRC], mainly the Corrosion Conference. In 1950, Hackerman acted as chair of the Corrosion Conference. Hackerman recalls the atmosphere of the Corrosion Conference, as well as the many others that he has attended, as informal, interactive, and informative. From 1970 to 1973, Hackerman served as a member of the GRC Board of Trustees. Functioning in the capacity of participant and Board member, Hackerman has watched the GRC evolve from a fledgling symposium to an international force that unites academe and industry. Hackerman concludes the interview with a discussion of the GRC's role in public education and understanding of science.

INTERVIEWERS

Arnold Thackray is President of the Chemical Heritage Foundation. He majored in the physical sciences before turning to the history of science, receiving a Ph.D. from Cambridge University in 1966. He has held appointments at Oxford, Cambridge, Harvard, the Institute for Advanced Study, the Center for Advanced Study in the Behavioral Sciences, and the Hebrew University of Jerusalem. In 1983 he received the Dexter Award from the American Chemical Society for outstanding contributions to the history of chemistry. He served on the faculty of the University of Pennsylvania for more than a quarter of a century. There, he was the founding chairman of the Department of History and Sociology of Science, where he is the Joseph Priestley Professor Emeritus.

Arthur Daemmrich is a policy analyst at the Chemical Heritage Foundation. He has published on the history of medicine, DNA typing (genetic fingerprinting), biotechnology policy and politics, and comparative work on pharmaceutical drug regulation. Current research projects include a study of scientific instrumentation, regulation of the chemical industry, and the history of environmental policy in the United States and Europe. Daemmrich has held fellowships from the Social Science Research Council/Berlin Program for Advanced German and European Studies, the Kennedy School of Government at Harvard University, and the Chemical Heritage Foundation. He has a Ph.D. from Cornell University (Science and Technology Studies) and a B.A. from the University of Pennsylvania (History and Sociology of Science and German Literature).

TABLE OF CONTENTS

1 Reflections on Neil E. Gordon
Johns Hopkins University. Early days of the Gordon Research Conferences [GRC]. Neil E. Gordon. Samuel C. Hooker Scientific Laboratory. GRC funding.

4 Gordon Research Conferences
Gibson Island Conferences. Herbert H. Uhlig. Acting as chair of the 1950 Corrosion Conference. Conference structure. Evolution of the Conferences. Joining the GRC Board of Trustees. George W. Parks. Alexander M. Cruikshank. Changing role of industrial involvement. The academic and industrial stream. Participant changes. Carlyle B. Storm. Financial issues. Creation of "breakout sessions." The importance of the Conferences. The effect of the World Wide Web on the GRC. Alexander M. Cruikshank. GRC tradition. Start of Conferences in California. Linus Pauling. Alan H. Cowley and chemical education. GRC 50 Years in New Hampshire. Start of European Conferences.

27 Conclusion
GRC's roll in public education and public understanding of science. The Robert A. Welch Foundation. Alan H. Cowley.

30 Index

INTERVIEWEE:	Norman Hackerman
INTERVIEWERS:	Arnold Thackray and Arthur Daemmrich
LOCATION:	Chemical Heritage Foundation Philadelphia, Pennsylvania
DATE:	12 March 2002

THACKRAY: This is an oral history interview with Norman Hackerman on 12 March concerning the Gordon Research Conferences [GRC]. The interviewers are Arnold Thackray and Arthur Daemmrich. Norm, we're interested in starting with your interactions with Neil [E.] Gordon and your knowledge of Neil Gordon, who seems quite interesting.

HACKERMAN: He was a member of the faculty at [Johns] Hopkins [University] while I was a graduate student there. I remember taking the preliminary oral exam with him in the room. My impression was that he was a man of high intellect, but not a great chemist. He used to have to study beforehand, when he asked questions on the orals about chromium or manganese. He always asked questions out of the group of students he happened to be teaching, which was the freshman class, and we all knew that. I did get to talk to him quite a number of times. I found him to be an extremely interesting individual with great ideas in a variety of subjects, the Gordon [Research] Conferences being one of them. I remember him talking to me about them on occasion. I didn't go to the early conferences on Gibson Island, Maryland, but he talked to me about them.

I thought it was a first-rate idea to get academic and industrial research chemists together, talking about the edge of the field, whatever area it was. Therefore, I thought that the idea of the conferences was first-rate also. You'd come and you'd talk—no attribution, no copying, and no pictures. You just took away what you remembered of it and used it in your own work. It certainly worked in the early days of the Gordon Conferences. I don't think it works quite the same way now. I think it's a much more normal kind of conference conversation—not the kind where you let it all out. Of course, you know your ideas and findings are not going to be stolen, used, or whatever. Gordon's idea, I think, was originally just that—talk and walk away.

THACKRAY: He was a professor of chemical education, which was a rather unusual thing in itself.

HACKERMAN: That's correct. He had no research lab. He had started a journal about chemical education already by the time I got to meet him. The conferences themselves were a

form of chemical education—not formal, not low-level, but actually informal and high-level. So, they were an extension of his interest.

THACKRAY: Classically, of course, Hopkins had been such a great research university.

HACKERMAN: Yes.

THACKRAY: How did he sit with his colleagues?

HACKERMAN: He probably was ostracized a bit. It was a little hard for a graduate student to know how these guys mixed. But I've already told you—when he came into the orals, he was prepared for a certain kind of question because that was the material he was teaching freshman at that time. The other people were free-stylers—they asked questions that came out of the blue. You didn't know what they were going to ask. So I would say that I'm sure they got along all right personally, but professionally, they probably didn't.

THACKRAY: Why were you interested in him? Why did you connect with him?

HACKERMAN: I can't remember, as a matter of fact. Well, I do remember one conversation we had. I was about to finish graduate school, and I was concerned about getting a job—this was 1935—and I went to see him. He started to say to me, "Well, I don't know about a job but—" and the plaster began coming off the ceiling and we had to run. [laughter] When we got out into the hall he said, "But we did put you up for [the] Phi Beta Kappa [Society]." I thought that was non sequitur. As it turned out, I did get a part-time job, but not through him. Other than that, I didn't really have much contact with him. It was required that we go around to all the faculty members before we took orals, so I did talk to him at that point. I talked to him about a job. How we got to talking about the Gibson Island Conferences, I don't remember, but I was in his office talking to him. The conferences had already started. I think they began in 1932?

DAEMMRICH: The first conference was in 1931.

HACKERMAN: Yes, 1931. I was still an undergraduate in 1931.

THACKRAY: Gordon got involved with the [Samuel C.] Hooker Scientific Library. Do you know anything about that?

HACKERMAN: Yes, he did. Well, he talked to me a little about that. He had acquired it with the original intent—now this is kind of a guess—of bringing it to Hopkins. I think he was at cross-purposes with Hopkins already though, so he took it with him to—

DAEMMRICH: Central College in Missouri.

HACKERMAN: Missouri, yes. You know, the buzzword was that there had been some talk of bringing the Hooker Library to Hopkins with the help of [Sebastian S.] Kresge.

DAEMMRICH: I think it even got named after Kresge when it moved to Wayne [State University] in Detroit, [Michigan].

HACKERMAN: Is that right? It was originally started by Samuel [C.] Hooker of Hooker Chemical Company up in Niagara Falls, [New York], I think. That was all part of the conversation I had with him.

THACKRAY: When did you get to go to your first Gordon Conference?

HACKERMAN: It would have been in the late 1940s, or maybe 1950. The Corrosion Conference was my steady one. I also was involved with the [Chemistry at] Interface[s] Conference for a fairly long period of time, and also with the Electrochemistry Conference when [John O'M.] Bockris spoke at it in the 1980s. I chaired all three of them at one time or another, but Corrosion was the steady diet.

DAEMMRICH: Just before we move on too far from Neil Gordon, I'm curious, how much did people talk about his ability to raise funds and launch new initiatives?

HACKERMAN: I never heard any conversation at all. "Fund-raiser" wasn't actually a title back in the 1930s. [laughter] First of all, the number of dollars available was absolutely minimal. I guess Gordon was precocious in that respect.

DAEMMRICH: Yes. Funding was limited, but he raised a hundred thousand dollars.

HACKERMAN: I never could understand that. Those were the days of ten-cent corned beef sandwiches, you understand.

THACKRAY: That was an extraordinary feat at that time.

HACKERMAN: Yes, it was. How he separated those people from their money—

THACKRAY: Indeed. The conferences, from their earliest moment, always had a funding aspect to them.

HACKERMAN: But the industry paid for that. Industrial companies were wise enough to know that they might get something out of it. So, I think, they're the ones that supported it. They certainly sent their people. I can't tell you about the Gibson [Island] Conferences. I knew a few people that went there, but I never went to one of them myself.

THACKRAY: What was the buzz about them that you heard?

HACKERMAN: Well, here were some thirty people, fairly high in their fields of catalysis, corrosion, and, I think, hydrocarbons. Those must have been the first three of the conferences scheduled (1). The conferees sat on the sand for a week and talked to each other.

THACKRAY: You had to be invited?

HACKERMAN: Yes, you had to be invited, and you had to be willing to stay on that sandy spit for a week or for five days at least, and it wasn't comfortable. A few people I know went to the first ones—Herb [Herbert H.] Uhlig was one of them—he was from MIT [Massachusetts Institute of Technology] and a leader in corrosion. He said it was, you know, full of sand flies and hot as the devil—no air conditioning—in the middle of the Chesapeake Bay, [Maryland]. But they sat around in their underwear and talked. Apparently, it was very successful. That is, the transmission of ideas really worked. That was because there was nothing else to do. I guess they could go swimming, but that was all.

When the conferences moved to New England—Gordon actually didn't have much to do with it at that point—they scheduled morning and night sessions and you played golf or tennis in the afternoons. So it became a vacation arrangement. The first fuss that I remember was because people brought their families. After that, conferees didn't want to listen to anybody talk at night because they had to be with their wives, their kids, or both. I remember that we had a

hell of an argument one night, during which someone suggested that maybe we ought to fix it so that only the participants can come—nobody else. But that was voted down.

THACKRAY: Did the night meetings continue?

HACKERMAN: The night meetings continued, but they became less and less effective. You'd have a good morning meeting, then you'd go out in the sun all day or go swimming all day and you had kids to play with and deal with. After dinner you didn't want to talk anymore because you were tired—you wanted to sleep. So it didn't work as well. I think it became less effective.

THACKRAY: You were talking earlier about being down in Texas in the early days. Was that the time at which you went to your first Gordon Conference?

HACKERMAN: Right.

DAEMMRICH: According to our records, you first chaired the Corrosion Conference in 1950. I don't know if you attended one before that.

THACKRAY: Do you know who invited you? How did that work?

HACKERMAN: Yes, I must have attended one before that, or I wouldn't have been chair. My guess is that I went there in 1947, 1948, or something like that. You were asking about an invitation. We didn't have to be invited. They announced those things, you applied, somebody selected you, and you showed up. I think the chairman in 1947 or 1948 must have been Herb Uhlig, because he was probably the one that got me in there.

DAEMMRICH: We've got a [K.G.] Compton and [I.A.] Denison. [Daemmrich shows Hackerman a list of the Corrosion Conference chairmen].

HACKERMAN: Compton was out of Bell Labs [Laboratories]. Let me see the list. Denison I knew. Compton I knew briefly. Harry [R.] Copson was with the Nickel Company. Burns—I knew Bob [Robert M.] Burns quite well, and he may have been the one that got me into the conference. He was at Bell Labs also.

THACKRAY: Corrosion is a subject with such obvious industrial importance. Did early Gordon Research Conferences have strong industrial involvement?

HACKERMAN: Absolutely, yes. That was exactly what they were set up to do. Organic High Polymers, Catalysis, Petroleum, Textiles, Corrosion, Medicinal Chemistry, and so on—all of those had direct connections to the outside. What happened after a while was that they became a little more esoteric.

DAEMMRICH: So, in those early years, they were able to bring people together from competing companies and competing labs.

HACKERMAN: Absolutely, and everyone talked.

DAEMMRICH: They left the bias behind, so to speak.

HACKERMAN: Well, I don't know if they did that, but they talked. [laughter] The fact is, there were a few people who'd just sit and listen. The attempt was to get everybody to say something. The Corrosion Conferences, generally, had less than fifty participants. It didn't always make expenses. Then we'd get complaints from management saying, "We've got to do something. Invite seventy next time or seventy-five." So, it became money-driven after a while.

THACKRAY: Fifty is a lot of people to have in an informal discussion. How did it work at that time, around 1950?

HACKERMAN: It worked in the formal sessions and it worked in little groups. After the sessions we each had bottles of whiskey that we had stashed someplace.

THACKRAY: Was this the morning session or the night session? [laughter]

HACKERMAN: After the night session. Four or five people with common interests got together and sort of hammered away over a drink, and that worked pretty well. The main session had fifty people total though, and eventually as many as one hundred. It was the job of the chairman to try to drag everybody into it by saying, "Hey Joe, what about this? What do you think?" We knew each other pretty well. It wasn't a big group.

THACKRAY: You chaired the 1950 Corrosion Conference. Did you finger a series of people to speak?

HACKERMAN: No. You started off by asking a question. You asked Bob Burns, "Well, you're an old-timer in this—what do you think about such and such metal in this system? What would happen? What's the chemistry of the metal?" And he'd generally respond. If nobody else picked up you'd call on somebody else—but not to give a dissertation, just to answer a question. So you had to know everybody in there. You had to know what they knew and what they could talk about. That was about it, you know. We had a blackboard, and we might have had a slide projector, and that was about it.

THACKRAY: Where was the conference held?

HACKERMAN: At Colby Junior College [renamed Colby-Sawyer College in 1975] up in New London, New Hampshire.

DAEMMRICH: How much correspondence went on beforehand to map out what topic areas would be covered? Corrosion is potentially a huge field.

HACKERMAN: Well, the several people who were involved with the management of the current session would have a meeting to decide who the chair and the vice-chair—I think that was what they were called—of the next session would be. That person would be given the task of determining the narrow field that they were going to deal with. It became a little more formal later. In one year, the topic would be wet corrosion and the alternate year it would be dry corrosion. That was because wet corrosion sort of over-ran the place and the dry corrosion guys didn't like it. They said, "If you want us to stay here you're going to have to let us talk about our kind of corrosion rather than your kind." And that did happen. In retrospect, it was much more dependent on who was in charge.

THACKRAY: As chairman, how did you get the right people to come to the conference?

HACKERMAN: In part, the same people came just about every year. The trick was to get a few new ones. That you had to do by knowing the field and knowing which new guys were publishing. If they didn't ask to come first, then you called them or asked them, "Hey, don't you want to participate in this great activity?" That was true of all three of the conferences I dealt with.

THACKRAY: So you had to make the discussion work every day during the sessions?

HACKERMAN: Yes. Well, some of them, after a while, began to get field leaders, so to speak. They picked somebody out of the crowd and asked them to chair a specific session. In my case, I did it the whole way through. Ultimately, discussion leaders were designated, which led to a more formal agenda with speakers and discussion.

DAEMMRICH: The companies who helped sponsor the conferences, did they then send specific people? What role did they play in terms of framing questions for discussion?

HACKERMAN: Well, they sent their own people that they knew were interested in the field, but I don't think they tried to frame questions. I think it was a catch-as-catch-can. They depended on their representatives. These individuals were not official representatives, but they obviously were representatives at the same time. They were supposed to be "white knights" talking about difficult problems and extracting from it whatever they could of value to their company. I think that's the way it worked, but without being formal about it.

THACKRAY: If I'm at Union Carbide [Corporation], how do you persuade me to pay for a conference that's going to profit the Dow Chemical Company?

HACKERMAN: You know, just by using the "you're a good citizen" persuasion.

THACKRAY: That went over all right?

HACKERMAN: It went for a while, but then it broke down. I think the current conferences don't do what Neil Gordon had in mind. The current conferences are just another lecture series with more people. First of all, they have well over a hundred of these conferences now, because that's what it takes to break even or put a little money into the general kitty. So you can't have the kind of discussion that we used to have. Our problem, at that time, was shutting people up so we could get others to talk, because it was inevitable that some people just took over the podium and said the same thing at least a dozen times. Currently, you don't have that problem. Most of them come, sit tight-lipped, and probably have a hidden recorder some place. They also take pictures, which was never permitted at that time. It is a formal lecture system in a nice setting. New Hampshire is a nice place in the summer.

THACKRAY: Was that change a gradual shift over time?

HACKERMAN: Yes.

THACKRAY: When did that happen?

HACKERMAN: The shift was certainly complete by 1970. It was already evident in the 1960s though. Then they spread the conferences out, you know. They're now held all over Asia and Europe as well. It's a big business, in a sense.

THACKRAY: Going back to 1950, were you expected to make the conference break even? How did that work?

HACKERMAN: Well, they called you when they didn't get enough people in the house, because somebody had to make that money up.

THACKRAY: What about the industrial funders? Who recruited them? Did you have to do that?

HACKERMAN: No, I don't think so. I don't remember doing it. I don't remember going to any industry and asking them for dough, so it must have been the staff. That staff also had problems for a while. When was I on the board?

DAEMMRICH: You were on the board from 1970 to 1973.

HACKERMAN: That late? There was a fiscal problem back in that era. In fact, there was an abrupt shift of director.

THACKRAY: [W.] George Parks was the director who resigned just before you came on the board (2).

HACKERMAN: Yes, and he's the guy that sort of ran it into the ground.

THACKRAY: Can you talk about George Parks?

HACKERMAN: Well, I don't really know a whole lot about him, except that the place was almost financially busted. Whether it was bad management or what, I don't know. [Alexander M.] Cruickshank cleaned it up, got it straight, and made it work. He was a good manager. I think Parks just wasn't. Parks probably thought it would run itself—if he let it alone it would be all right. That's not the way it works.

DAEMMRICH: In some of the early years, the conferences were affiliated with AAAS [American Association for the Advancement of Science].

HACKERMAN: Yes, that's right. It did have an affiliation.

DAEMMRICH: That somehow drifted?

HACKERMAN: Well, AAAS didn't add anything, so there wasn't much point to it. Also, at that time, AAAS was not really an operating entity. It is now, but it wasn't then.

THACKRAY: It's interesting that just as you were getting interested in the conferences, they were called the Chemical Research Conferences (3).

HACKERMAN: Yes. And there was another set of conferences that the AIChE [American Institute of Chemical Engineers] ran. I remember talking to AIChE because they were interested in corrosion. These conferences didn't last long; they disappeared after about five or six years.

THACKRAY: When was that?

HACKERMAN: In the early- to mid-1950s. They held their conferences in different parts of New Hampshire. I remember going to two or three of them. I can't remember the names of them. They had the same kind of financial problem that GRC had because they'd only have thirty or forty people there, so it probably didn't carry its weight. I guess the AIChE was beholden to make up the difference and didn't want to do it anymore. That's probably why it disappeared.

[END OF TAPE, SIDE 1]

HACKERMAN: Maybe that was the problem with AAAS. The conferences were interested in AAAS as a money-backer, and AAAS was not interested in providing money for the conferences (4).

THACKRAY: Were those AIChE conferences similar in format?

HACKERMAN: Yes. They were copies. I said AIChE, but I'm not sure it wasn't—wasn't there a general engineering society?

THACKRAY: There's the United Engineering Foundation.

HACKERMAN: That doesn't sound right. It was an engineering group though, I know that.

THACKRAY: You went a couple of times. Did you go back to the Gordon Conferences every year?

HACKERMAN: Yes. I was there just about every year all through the 1950s and most of the 1960s.

THACKRAY: Even though you had a lot of other things going on by then?

HACKERMAN: Yes. I'd drive my family to Baltimore, [Maryland], where I come from, drop my wife and kids there, and then I'd go up to New Hampshire. Because I was one of the vigorous opponents of bringing families to the conferences, I couldn't exactly bring them along. [laughter] I'd go almost every year.

DAEMMRICH: During the mid-1950s, the Gordon Research Conferences were incorporated as an actual nonprofit.

HACKERMAN: Yes.

DAEMMRICH: Was that a noticeable event?

HACKERMAN: Not to the attendees. That formality was important, because they were handling enough money that the feds [federal officials] were interested.

THACKRAY: In the 1950s and 1960s, was it self-evident that there should be a continuing Corrosion Conference?

HACKERMAN: Jumping forward to my board experience, there were discussions as to which conferences we should drop, and which ones should go on alternate years. In the case of Corrosion, we did the alternate years by wet and dry, so they were two really different conferences. People who came to the wet years didn't come to the dry years, and vice versa.

THACKRAY: Was there a significant difference in industrial attendance?

HACKERMAN: Yes, very different.

THACKRAY: In what way?

HACKERMAN: Well, the people doing high-temperature work were interested in the dry stuff. People researching ambient temperature generally had an interest in wet corrosion. They were all fluid, but some were condensed and some weren't.

THACKRAY: How did the industrial involvement begin to change and why?

HACKERMAN: Well, if you look at what has happened in the last few years, you can extrapolate back. The interest of research in the industry, which peaked in the 1950s and 1960s, began to diminish rather rapidly in the 1970s, when it became evident that you couldn't make a direct connection between an outcome and the research that preceded it. It was a very convoluted path that got you to the outcome. I guess people decided that if it was so convoluted, it may not even exist, so why spend 10^6 bucks on industrial research laboratories.

So, companies cut out their research labs. General Electric [Company] probably still has some kind of a lab, but I don't think many others do. [E.I.] DuPont [de Nemours and Company] certainly doesn't have the same kind of lab it once had, and the American Cyanamid [Company] and many others are all gone. I don't think Dow has the same kind of lab it used to have. It used to have a three hundred fifty- to four hundred-person, relatively pure, research lab where

people did what they wanted to do, and somebody learned how to fit what they found to Dow's principal occupation of making money. I think they now outsource it to universities somehow.

DAEMMRICH: That means that the interface of academic to industrial becomes even more important, right?

HACKERMAN: Yes. Well, as an extracting device, it becomes more important. In fact, one of the last reasons for maintaining research laboratories in large companies is to have people who understand what is going on in science and can do some extracting, without having to do the research themselves. That doesn't work either. The academic institutions aren't a good source of the kind of research that the companies need; but they're a good source of the start of the long stream that becomes the technology. It's like a river. It starts up in the mountains where a little stream jumps around, goes this way, that way, and the other way. You've got to be down where the river is fairly straight before you can apply it in technology. That is what's seriously missing. In the golden years of science, companies put big money into the top part of this system. It didn't often pay off notably enough for accountants to relate it to the outcomes. That was the problem. So, most industrial research labs have disappeared. The system still goes on though, and transfers between science and technology still occur.

THACKRAY: Well, there's a very considerable concern today in the classical chemical industry about the lack of innovation.

HACKERMAN: Yes, but the industry squelched in-house innovation.

DAEMMRICH: One of the interesting things about the university/academic and industry divide, or stream as you're depicting it, is that a lot of the time, it's not so much products, but new research methods, that come out of universities. I wonder how much of the conferences were about the methods of doing research, versus talk of specific projects.

HACKERMAN: I don't think there was much. I think it was, you know, "What do we know that's different?" I actually attended about five different conferences, and that's the way it was in those five.

DAEMMRICH: Those were each three to five days long?

HACKERMAN: Five days. I came in on Sunday night and left Friday after lunch.

THACKRAY: If you think about Catalysis as opposed to Corrosion, was there a very different kind of ethos in the room? Was there a different mix of people?

HACKERMAN: No, they were about the same. The ones I went to had roughly the same format: somebody would say something, somebody would ask a question, somebody would say that's wrong, somebody would make a comment. It was very much a round-table discussion with fifty people, which was not easy.

THACKRAY: That's an art.

HACKERMAN: Yes, it is an art, and it's gone. It does not exist now.

THACKRAY: But it became acculturated for a while.

HACKERMAN: For a while.

THACKRAY: In the 1950s, what was the ratio between academic and industrial scientists at the conferences?

HACKERMAN: Not far from half-and-half, with the industrialists being slightly less. The latter got smaller and smaller even in the time I was there until about 1970—although I chaired an Electrochemistry Conference in the 1980s (5).

THACKRAY: When you were on the board, did you worry about that?

HACKERMAN: Yes. That was a major problem. We had money problems, but the major problem was the future of this outfit, since the number of industrial people was diminishing slowly, the openness was diminishing rapidly, and it was becoming a lecture series rather than a discussion. I think that is a big problem now. I'm guessing, because I haven't been involved with it recently.

DAEMMRICH: What sorts of things were you trying to do to adjust that and change things around?

HACKERMAN: Well, we rooted around and asked the Dows and the Carbides, "Why don't you send more people up?"

DAEMMRICH: What were their responses?

HACKERMAN: "We'll think about it," more or less. I think they were beginning to get the feeling that they didn't need the scientific establishment.

DAEMMRICH: They thought they could pick it off the shelf?

HACKERMAN: Yes, that they could find it some other way. They may not be wrong, as a matter of fact. But that's why I spoke of the golden age in the 1950s and 1960s. Those research laboratories were big, and they used up a lot of university output. A similar thing is happening with the pharmaceutical industry now. What's different is that the pharmaceutical industry is just trying to get new compounds with characteristic properties.

DAEMMRICH: Right.

HACKERMAN: None of the companies are researching methods of how to do it—they get that from the university guys. They know what they want because it has this kind of folding, or unfolding characteristic, or whatever it is. They call on guys like [J. Fraser] Stoddart at UCLA [University of California at Los Angeles] and say, "This is what we want. How do you make it?" And he tells them.

DAEMMRICH: Their end of pipeline product is always the same—it's a medicine, an injection, or whatever. The end of pipeline product for Dow could be anything from a fabric to a huge array of items.

HACKERMAN: Or Agent Orange.

DAEMMRICH: Right.

HACKERMAN: So it's quite different now, which is probably one of the reasons that the number of people going for the Ph.D. in chemistry is diminishing.

DAEMMRICH: Well, the main employers of people with chemistry degrees are pharmaceutical companies.

HACKERMAN: Yes.

DAEMMRICH: Was there a push to move into a medicinal area at GRC?

HACKERMAN: Yes. I'm sure that you'll find some of these titles, but I don't see anything off-hand here. [Hackerman is looking at the listing of conferences from 1947 to 1986.] Microbiological Deterioration—now that was a materials problem. That was 1951. I don't see anything here.

DAEMMRICH: Yes, they're just the older ones. You came to the board just after Parks stepped down and Cruickshank took over. What can you tell me about Parks, in terms of what he was like as a person?

HACKERMAN: Well, the system was failing and he was pretty defensive about it. He probably was the cause of it—I don't know. I mean, you've got to start with the chief and work down. Cruickshank was his assistant.

DAEMMRICH: From about 1947 on, right (6)?

HACKERMAN: Yes, and he stopped the leakage after Parks resigned. That's the best I can tell you. I think Parks didn't pay attention to the activity or didn't think about it very much. I don't know. But Alex was able to fix it in a hurry. In my term, that problem appeared and disappeared. They haven't had any trouble since. Cruickshank stayed on for a long time. I think there have been a couple or three directors since he left.

DAEMMRICH: He was on until 1993. It has been Carlyle [B.] Storm ever since.

HACKERMAN: Has he been there since then?

DAEMMRICH: Yes, and he still is (7). Parks brought a lawsuit against GRC at one point, right?

HACKERMAN: Yes, there was strife. Everybody pointed fingers at everybody else. I'm not sure the money was well accounted for, for one thing. I don't remember whether there was a formal accounting firm working for us or not, but if I had to guess, I'd say no. Since this was an academic affair run by academics, it had the looseness of the way academics manage things. That meant that Parks was in full charge. The board probably didn't say much.

DAEMMRICH: Right.

HACKERMAN: But when it became evident that there wasn't enough money to run the conferences, the board woke up. We were lucky because Cruickshank was an important character in GRC.

DAEMMRICH: If the institution was—I hate to use these words—all but bankrupt circa 1970 when Cruickshank stepped in, whom did he turn to?

HACKERMAN: Well, first he stopped the leakage wherever it was. The conferences were well established up in New England, and I think there were some in Wisconsin at the time, or some place in the Midwest. So, what they probably did was run the conference up to one hundred ten instead of eighty-five and got a little dough to plug the leak. My guess is that's what they did for about two or three years.

DAEMMRICH: Right.

HACKERMAN: I know we complained about the increasing attendance numbers, but there was no way around it because you had to have money. Going to industry would not have done it—they were not that interested in just putting dough out to keep it running. I don't think they felt they would get enough out of it. So, my guess is that they did it by increasing the size. That, in turn, impacted the quality—it diminished the quality. Now they have several hundred of these conferences each year. The conferences keep bringing people in, so scientists must like them. I don't think it's just a matter of being used to it. I shouldn't really comment on the more recent ones, because I haven't been to them in the last ten years or so.

DAEMMRICH: As it started growing in the early 1970s, did they institute smaller breakout sessions or were people just going off on their own?

HACKERMAN: Not formally, no. They did it on their own, which was a good way to do it. It was informal and that meant that people with similar interests got together. Again, I'm talking about Corrosion but I would guess it was the same with the other conferences too.

DAEMMRICH: Right.

HACKERMAN: The current GRC is probably not the same, except superficially, as when I went to it to begin with. Then, it was purely driven by the interest of the four or five dozen people attending. Now, I'm sure it's much more formalized. As I've already said twice, I'd be very surprised if everybody participates in terms of saying something, and I would have to extrapolate the suggestions that the GRC is no longer needed. However, the fact is that they *are* needed and they seem to be prospering.

DAEMMRICH: Yes, they're in a period of rapid growth, if anything, in terms of the number of conferences. As I understand it, they follow the same mode of allowing people to propose new topics.

HACKERMAN: Yes, they do that.

DAEMMRICH: Now in Corrosion, which you're most familiar with, to what extent would you—in conversations and during meetings—map out subfields by saying, "Here's an area that nobody's really done anything on—go into it."

HACKERMAN: What we generally did was take one or two afternoons during the session for the senior guys to get together and map out what the next conference should consider. The next conference might be two years away, because we had that alternating year system. Then, we'd try to identify some younger guy who would be capable of handling the conference, put him in a corner and see if he would do it, and then start on it, but with the understanding that in two years there could be some major shift, some real change in direction that had to be watched for.

DAEMMRICH: Right.

HACKERMAN: That's about the way it ran—fairly informally. We'd always put it in the hands of the seniors, which was not necessarily the best way to get innovative stuff done, but it was a way.

DAEMMRICH: Coming out of some of the meetings, did you ever think, "That was exciting! I should change my research direction and start working on something else?"

HACKERMAN: Absolutely. I guess the best example of that is the first time I heard about a good solid-state electronic potentiostat, where you could reliably set the potential and keep it constant. One of our big problems in trying to do electrochemistry was maintaining a constant potential—not constant on a voltmeter, which was insufficiently sensitive to see the oscillation, but a truly constant potential. That was very important, and in fact, oscillation was the reason that a lot of changes took place and we didn't know it. So the answer is, yes, it was pertinent to my passivity studies.

DAEMMRICH: You also had a list of people that you could talk to about it, right?

HACKERMAN: Yes, and people who had the capacity to tell you something about good electronic systems. Actually, there were a lot of smaller things you'd take away—little tips like how best to use a probe, and what kind of system you could you use that minimized the resistance better. And you'd come away with questions like, was it really possible to hold the probe electrode within a micron of a solid surface without any motion at all? In fact, I guess you got lots of tips on how to do experiments just by sitting around talking to somebody. That was a great outcome of the Gordon Conferences. When you get right down to it, it may be that the small group talks were most important. Although the big group was important too—when the attendees were handled well and the discussion flowed around, it turned up lots of little nuggets—I'm sure of that.

The question now is: suppose the Gordon Conferences disappeared overnight—would it make much difference to the scientific community? With the change in the way people transfer understanding with the use of the World Wide Web, for example, there might not be a noticeable change.

DAEMMRICH: From 1950 through the mid-1970s, when you were deeply involved in GRC, you could make the argument that people transferred findings through publications.

HACKERMAN: Yes, they did, but it was always a year late. It took a year to get a paper out, at best. So, if you wanted to be on the real forefront, you had to talk to the people who were doing it. Today, that's not entirely true.

[END OF TAPE, SIDE 2]

DAEMMRICH: During that period, if you wanted to present new findings at a conference, you did not use slides and overheads, or bring someone in to do a demonstration, right? Instead, you described the new idea or method without visual display or demonstration.

HACKERMAN: It has always been that way at the Gordon Conferences. The change now is that papers can appear the day they're accepted.

DAEMMRICH: Right.

HACKERMAN: Now, you may not be able to find them though—that's one of the problems with the current system. But you don't have to wait a long period of time before they're available to read. Of course, there are also other things. There are papers on the Web that have never been reviewed by anybody.

DAEMMRICH: Yes.

HACKERMAN: If they haven't been reviewed, then your chances of being misled are greater. Getting back to the Gordon Conferences, the conversation was not reviewed either. You had to take your chances on being misinformed. So, you had to depend on your own gut to tell you what to do, but that was all right. I guess that was legitimate. At the present time, the difference is that there's nobody actually discussing what the person's lecturing about. I think the discussion was a form of peer review, which did exist in the 1950s and 1960s, but doesn't now.

DAEMMRICH: Yes. I mean, if you stood up in the mid-1950s and said, "The way in which I can get this probe to be so many microns from a surface is by doing X," someone would stand up in the room and say, "That's bull. I've tried that."

HACKERMAN: Sure.

DAEMMRICH: How were you able to leave the conferences and implement new things in your lab without having been shown visual displays? Was it based on just the description?

HACKERMAN: The description must have been thorough enough, because the few things I can think of off-hand, we did. During the sessions, researchers described things the way you would use your hands to describe a gizmo. So, there must have been enough insight transferred to permit me to understand and reproduce the idea. That's the only way I know it could have been—because there were no papers transferred. Nobody ever scribbled on a piece of paper. The board was always erased. Pictures were never taken. I mean, that was the catechism that we got when we came in. Cruickshank would come around and give us the word about what we could and couldn't do.

DAEMMRICH: Would he come to each of the conferences when it was opening and announce it?

HACKERMAN: He came to the ones that I went to, yes. When they got so numerous, he obviously couldn't do all of them.

DAEMMRICH: Right.

HACKERMAN: The board members went out. I don't know exactly how they did all that.

DAEMMRICH: What exactly would he say? Do you remember the sequence?

HACKERMAN: Well, he'd set down the rules of engagement—you know, no pictures, no attribution, that kind of stuff. He'd take about twenty minutes to do that.

DAEMMRICH: And the sequence—there were morning sessions with the afternoons off?

HACKERMAN: Yes. He announced the timing and housekeeping stuff—when you could have meals, what you could do in the afternoon, places to go, and so forth.

DAEMMRICH: Did everyone eat together for meals?

HACKERMAN: Yes. The conference locations were generally colleges and they'd use the dining rooms for meals. We had breakfast from 8:00 am to 8:30 in the morning.

DAEMMRICH: Did people interact across different conferences they were attending at the same time at the same school?

HACKERMAN: Well, they didn't do that. I think each conference went to its own location.

DAEMMRICH: Right.

HACKERMAN: They had about twenty conferences in New Hampshire when I was active. I'm sure there are well over a hundred now around the world. They had an air of informality about them, which probably still exists somewhat, but it's not as informal as it used to be.

DAEMMRICH: At what point did they start having more formal talks?

HACKERMAN: I think it started in the mid-1970s, when the industrial people lost their verve for this kind of thing. There was certainly lots of informality when I was on the board. We had a rendering of how much money was coming in and how much was going out, but we didn't have an accountant or an audit committee. I'll bet they do now.

DAEMMRICH: There were certain traditional nighttime activities, right? Ringing a bell at midnight?

HACKERMAN: It depended on where you were. Some conferences had such things, some of them didn't. Some of the locations had good beds, some of them didn't. Some of the beds were those hard cots that college kids sleep on. Some of them had plush quarters that people could pay extra for. Colby was all vanilla—all the quarters were the same.

DAEMMRICH: Right, they were dorms.

HACKERMAN: Colby Junior College isn't a junior college anymore. I think it's now called Colby-Sawyer College. [H. Leslie] Sawyer was the headmaster when I first started going up there.

DAEMMRICH: Right. At some point in the early 1960s—about 1963—they started holding conferences in California (8).

HACKERMAN: Yes, and I went to a few of those, as a matter of fact.

DAEMMRICH: Can you compare them to the New Hampshire conferences?

HACKERMAN: They were spread out over several years. The conferences I went to in California were the Electrochemistry Conference held in Santa Barbara, and the Science Education Conference, which Linus Pauling led and I gave the second paper. The Education Conference was about ten years ago (9).

DAEMMRICH: Do you know who organized it?

HACKERMAN: Alan [H.] Cowley had something to do with it. He was a member of the board at the time.

DAEMMRICH: And the main topic was chemical education?

HACKERMAN: Chemical education, yes I remember that Pauling's main complaint was that textbooks had great margins. If you'd just get rid of the margins and have smaller textbooks, it would be better. That's what remains with me, but I'm sure he said more than that. The reason I don't remember much of what he said is that I followed him and I made the statement that some high school chemistry teachers were failed chemists, and man, was I booed! I mean, really loudly. There were a lot of schoolteachers in there. They just raised holy hell. They were right though—that was not a good thing to say. I forgot what Pauling said, of course, because of that.

That conference was held during the start of the movement to try to do something about high school science—teaching high school science, and chemistry in this case. The conference was on the West Coast at Oxnard, [California] or some place like that, at one of the standard hotels there—Holiday Inn or something.

DAEMMRICH: When the conferences started in California, hotels were used, right?

HACKERMAN: Yes.

DAEMMRICH: Did that change the atmosphere or the feeling?

HACKERMAN: Well, I didn't go to many of them, but I'd say not drastically. The hotel that was used in Santa Barbara was a motel with a courtyard kind of thing. It was pretty good, except it had a railroad train running down the middle of it—in the middle of the night, too. [laughter] That wasn't very pleasant.

DAEMMRICH: Was the format of the Education Conference the same as the others?

HACKERMAN: Yes, it was the same as the others: meetings in the morning and evening, with time off in-between. It was big. There were probably a hundred twenty or thirty people in there. Again, it was a modern one, so it was not losing money. Alex was out there at that one. He managed to get around to quite a number of them. So it must have been before—when did you say he retired? 1993?

DAEMMRICH: Yes.

HACKERMAN: So it must have been before that.

DAEMMRICH: Right. Do you know Carl Storm?

HACKERMAN: I've met him, but I don't know him. I went back about three or four years ago when they had an anniversary.

DAEMMRICH: There was the "GRC 50 Years in New Hampshire" anniversary some years back.

HACKERMAN: Well, this was in Boston, [Massachusetts], at Logan Field [Logan International Airport] at a Holiday Inn or some other hotel. We had a retrospective, and some of us talked about Gordon and some of us talked about the other conferences.

DAEMMRICH: Right.

HACKERMAN: Is there a history of that?

DAEMMRICH: Yes. I mean there's this [Daemmrich displays the accompanying publication, "Gordon Research Conferences; 50 Years in New Hampshire," that includes the program of GRC's 50th anniversary celebration (10)]. Is this the one you mean? Look at that front page. I think it says where it was held.

HACKERMAN: Yes. I spoke about GRC from 1950 to 1955.

DAEMMRICH: You're right, it was held at the Ramada [Hotel] at Logan.

HACKERMAN: [Hackerman reads down the list of the speakers at that anniversary]. Yes, and [Robert W.] Parry was on the board with me. Parry's at Utah State [University], and [Simon H.] Bauer's at Cornell [University]. The program for the evening started with reflections on GRC in the 1970s. [Hackerman continues to read down the list.] [John P.] McCulloch was a Mobil Chemical Company research chemist.

DAEMMRICH: Have you stayed in touch with these people their whole careers?

HACKERMAN: On and off. I know [John P.] Fackler, [Jr.] because he's at [Texas] A&M [University] now, and Cowley is the guy I told you about that was running that Education Conference.

DAEMMRICH: Right. The first conferences outside of the United States were in 1990, right?

HACKERMAN: Yes, in Europe.

DAEMMRICH: In Volterra, Italy (11). Have you been to any of the overseas conferences?

HACKERMAN: No. Cowley goes to a lot of them. He had a lot to do with setting them up. He's at the University of Texas now. He's my squash partner. He would be a good source of information, especially from the 1970s on.

DAEMMRICH: Let's discuss some more general issues about GRC's role in public education and public understanding of science.

HACKERMAN: This was not a goal in its original charter.

DAEMMRICH: So what is it doing in that area?

HACKERMAN: Well, there's the Science Education Conference I told you about (12). As far as participating in the public's understanding of science, I don't know what they're doing. The education part, of course, moved away from industrial involvement.

DAEMMRICH: Right. How do you see that mission fitting into GRC's purpose?

HACKERMAN: Well, it's just like the [Robert A.] Welch Foundation. We find that you have to go back into the educational background to produce both public understanding and a steady, thin stream of people to do the work.

DAEMMRICH: Right.

HACKERMAN: It's important that the thin stream not be broken. And it *is* a thin stream—it doesn't have to be thick. The only problem with the line being thin is that it's easier to break. The involvement of the conferences in this area, I think, is legitimate. It is true that you have to have it, and the way you get around the industrial involvement is to substitute industry people for teachers. So you have a meeting between teachers and chemists. How does that help you get better chemical education? That's the question.

DAEMMRICH: Other than Alan Cowley, whom would you recommend we sit down and chat with?

HACKERMAN: Have you talked with Alex?

DAEMMRICH: Cruickshank? Yes, we have one interview scheduled with him. Do you stay in touch with him?

HACKERMAN: I haven't talked to him in about a year.

DAEMMRICH: All right. Do you have other comments, suggestions?

HACKERMAN: Well, if I do, I'll give you a call. I can't think of anything off-hand.

DAEMMRICH: All right. Thank you very much.

[END OF TAPE, SIDE 3]

[END OF INTERVIEW]

NOTES

1. The first conferences held on Gibson Island, Maryland were Heavy Hydrogen, Surface Phenomena, Structure of Solids, Liquids, and Gases, and Thermal Energy and the Structure of Molecules in 1934. See "Research Conferences on Chemical Physics," *Science* 79, no. 2043 (23 February 1934): 176-7.

   The Catalysis Conference and the Frontiers in Petroleum Chemistry Conference were held at Gibson Island in 1940. See "Special Research Conferences on Chemistry," ibid. 91, no. 2366 (3 May 1940): 434-435.

   The first Corrosion Conference at Gibson Island was held in 1941. See Forest R. Moulton, "The American Association for the Advancement of Science; Special Research Conferences on Chemistry," ibid. 94, no. 2434 (22 August 1941): 179-180.

2. W. George Parks resigned in 1968.

3. The Gibson Island Conferences were called the Chemical Research Conferences in 1947, their first year at Colby Junior College.

4. Neil Gordon persuaded AAAS to manage the conferences starting in 1938 on the condition that the conferences would remain financially independent. See "Gordon Research Conferences; 50 Years in New Hampshire," (see note 10): 4-5.

5. Hackerman chaired the Physical Electrochemistry Conference in the summer of 1986. See Alexander M. Cruickshank, "Gordon Research Conferences," *Science* 231, no. 4742 (7 March 1986): 1163-1198.

6. In 1947, W. George Parks became director and Alexander M. Cruickshank became assistant director.

7. Carlyle B. Storm retired as director in 2003 and Nancy Ryan Gray became director.

8. The first two conferences held in California in 1964 were Polymers (West) and Electrochemistry. See W. George Parks, "Winter Gordon Research Conferences," *Science* 142, no. 3594 (15 November 1963): 984, 987-988.

9. See Alexander M. Cruickshank, "Gordon Research Conferences," *Science* 254, no. 5029 (11 October 1991): 302-308.

10. "Gordon Research Conferences; 50 Years in New Hampshire," (program for GRC's 50th Anniversary celebration, held 8 August 1997).

11. See Alexander M. Cruickshank, "Gordon Research Conferences," *Science* 247, no. 4946 (2 March 1990): 1100-1124.

12. The Science Education Conference (see note 9) was held only in 1992. The Innovations in College Chemistry Teaching Conference has been held nearly every year since 1995. See Alexander M. Cruickshank, "Gordon Research Conferences," *Science* 262, no. 5131 (8 October 1993): 262-272; Carlyle B. Storm, ibid. 266, no. 5183 (14 October 1994): 302-306; ibid. 271, no. 5250 (9 February 1996): 826-846; ibid. 278, no. 5336 (10 October 1997): 312-315; Gordon Research Conferences Web site, "Innovations in College Chemistry Teaching," (http://www.grc.org/programs/1999/ innovat.htm; http://www.grc.org/ programs/2001/innov.htm; http://www.grc.org/programs/2002/ innov.htm) accessed 9 December 2004.

    In 1994, the Innovations in College Chemistry Teaching Conference was renamed 'Chemistry Education Research and Practice.' See Gordon Research Conferences Web site, "Chemistry Education Research and Practice," (http://www.grc.org/programs/ 2004/chemedu.htm; http://www.grc.org/programs/2005/chemedu.htm), accessed 9 December 2004.

INDEX

A
Agent Orange, 16
American Association for the Advancement of Science [AAAS], 10-11
American Cyanamide Company, 13
American Institute of Chemical Engineers [AIChE], 10-12

B
Baltimore, Maryland, 12
Bauer, Simon H., 26
Bell Laboratories, 6
Bockris, John O'M., 3
Burns, Robert M., 6, 7

C
California at Los Angeles, University of [UCLA], 16
Central College, 3
Chesapeake Bay, Maryland, 5
Chromium, 1
Colby Junior College. *See* Colby-Sawyer College
Colby-Sawyer College, 7, 23-24
Compton, K.G., 6
Copson, Harry R., 6
Cornell University, 26
Cowley, Alan H., 24, 27-28
Cruickshank, Alexander M., 10, 17-18, 22-23, 25, 28

D
Denison, I.A., 6
Detroit, Michigan, 3
Dow Chemical Company, 9, 13-14, 16
DuPont, E.I. de Nemours and Company, 13

E
Europe, 9, 27

F
Fackler, John P., Jr., 27

G
General Electric Company, 13
Gibson Island, Maryland, 1-3
Gordon Research Conferences [GRC], 1-6, 9-14, 18–28
"Gordon Research Conferences; 50 Years in New Hampshire" 26

becoming a non profit organization, 12
board of trustees, 10, 13, 15, 17-18, 22-24, 26
Catalysis Conference, 15
Chemical Research Conferences, 11
Chemistry at Interfaces Conference, 3
Corrosion Conference, 3-8, 13-15, 19-20
Electrochemistry Conference, 3, 24
Gibson Island Conferences, 1-3, 4
Microbiological Deterioration Conference, 17
Science Education Conference, 24-25, 27
Gordon, Neil E., 1-5, 9, 26

H
Hackerman, Norman
as a GRC trustee, 10, 13, 15, 17
as chairman of the Corrosion Conference, 3, 5, 7-8, 10, 19
as chairman of the Physical Electrochemistry Conference, 15
as chairman of various conferences, 3
at Johns Hopkins University, 1-3
at the American Institute of Chemical Engineers conferences, 12
children, 12
conference attendance, 3, 14-16, 23-27
wife, 12
Holiday Inn, 24–26
Hooker Chemical Company, 3
Hooker Scientific Library, Samuel C., 3
Hooker, Samuel C., 3

J
Johns Hopkins University, 1, 3

K
Kresge, Sebastian S., 3

L
Logan International Airport, 26

M
Manganese, 1
Massachusetts Institute of Technology [MIT], 5
McCulloch, John P., 26
Missouri, 3
Mobil Chemical Company, 26

N
New London, New Hampshire, 7
Niagara Falls, New York, 3
Nickel Company, 6

O
Oxnard, California, 25

P
Parks, W. George, 10, 17-18
Parry, Robert W., 26
Pauling, Linus, 24
Phi Beta Kappa Society, 2
Potentiostat, 20

R
Ramada Logan Hotel, 26

S
Santa Barbara, California, 24-25
Sawyer, H. Leslie, 24
Stoddart, J. Fraser, 16
Storm, Carlyle B., 18, 25

T
Texas A&M University, 27
Texas, University of, 27

U
Uhlig, Herbert H., 5-6
Union Carbide Corporation, 9, 16
United Engineering Foundation, 12
Utah State University, 26

V
Volterra, Italy, 27

W
Wayne State University, 3
Welch Foundation, Robert A., 27