CHEMICAL HERITAGE FOUNDATION

ROBERT W. PARRY

Reflections on the Gordon Research Conferences

Transcript of an Interview
Conducted by

Arnold Thackray and Arthur Daemmrich

at

Salt Lake City, Utah

on

19 July 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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315 Chestnut Street
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ROBERT W. PARRY

1917	Born in Ogden, Utah, on 1 October

Education

1940	B.S., chemistry, Utah State University, College of Agriculture
1942	M.S., inorganic chemistry, Cornell University
1946	Ph.D., inorganic chemistry, University of Illinois

Professional Experience

	University of Illinois
1943-1945	Research Assistant, National Defense Research Committee, Munitions Development Laboratory
1945-1946	Teaching Fellow
	University of Michigan
1946-1969	Faculty Member
1958-1969	Professor of Chemistry
1960-1963	Founding Editor, *Inorganic Chemistry*
	University of Utah
1969-1997	Distinguished Professor of Chemistry
1997-present	Professor Emeritus

Honors

1972	Manufacturing Chemists Award for College Teaching
1980	Senior U.S. Scientist Award, Alexander Von Humboldt-Stiftung
1985	D.Sc., *honorary causa*, Utah State University
1987	First Governor's Medal of Science, State of Utah
1997	D.Sc., *honorary causa*, University of Utah

ABSTRACT

Robert W. Parry begins the interview with a discussion of his childhood in Ogden, Utah. After graduating from Ogden High School, Parry attended Weber College for two years, where he studied chemistry until his funding ran out. At that point, Parry started performing research for the United States Department of Agriculture Forest Service. When Rudger H. Walker, Parry's supervisor at the Forest Service, became dean of the College of Agriculture at Utah State University in Logan, Parry followed him, and there received his B.S. in 1940. Parry continued his education, earning his M.S. from Cornell University in 1942 and his Ph.D. from the University of Illinois in 1946. Parry briefly discusses his early career, which included positions at E. I. du Pont de Nemours and Company, the Munitions Development Laboratory at the University of Illinois, the University of Michigan, and the University of Utah. Parry then discusses at length his experiences with the Gordon Research Conferences [GRC]. Parry attended his first conference on inorganic chemistry in the 1950s and has attended almost every Inorganic Chemistry Conference since. Parry has served GRC as a conference chairman, as an executive committee member, and as chairman of the board of directors. Parry describes the evolution of GRC through four distinct eras: the Gibson Island Conferences, and the directorships of W. George Parks, Alexander M. Cruickshank, and Carlyle B. Storm. Parry concludes the interview with a discussion of the strengths and importance of GRC.

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 in Philadelphia. He holds a Ph.D. in Science and Technology Studies from Cornell University and has published on biotechnology policy and politics, the sociology of medicine, and pharmaceutical drug regulation. In his research, he brings long-range perspectives to bear on the analysis of globalization, risk, health, and environmental policy. Daemmrich has held fellowships from the Social Science Research Council/Berlin Program for Advanced German and European Studies, and the Kennedy School of Government at Harvard University.

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INTERVIEWERS: Arnold Thackray and Arthur Daemmrich

PLACE: University of Utah
Salt Lake City, Utah

DATE: 19 July 2002

THACKRAY: This is an oral history with Robert W. Parry concerning the Gordon Research Conferences [GRC] on 19 July. The interviewers are Arthur [A.] Daemmrich and Arnold [W.] Thackray. Before we begin the interview proper, Bob, GRC records show that you were chair of the board of trustees from 1967 to 1968. But you have stated that those dates are not entirely accurate.

PARRY: Well, 1967 to 1968 is correct. Usually, when one joins the board on their last year they are asked to serve as chair. Because of the troubles that occurred during my chairmanship, I was asked to stay on an extra term. So I was on the board from 1965 to 1967. I was chair from the fall of 1967 to the fall of 1968. Then I was asked to stay on the board, so I continued on the board again from 1968 to 1971.

THACKRAY: All right. We can come back to that later. Let us begin at the beginning, as it were, with a sketch of your personal biography.

PARRY: I was born in Ogden [Utah] in 1917. I went to public school and I am a graduate of Ogden High School. I went to Weber [State] College, which is in Ogden as well, for two years. Weber used to be a two-year college, but now it's a four-year state university (1). My education was governed by finances and I ran out of money. So, I began working for the [United States Department of Agriculture] Forest Service Research Lab[oratory] in order to make enough money to stay in school.

THACKRAY: When did the Parry family arrive in Utah?

PARRY: My grandfather, Joseph Parry, arrived in Utah in 1852.

THACKRAY: And your father's occupation?

PARRY: My father was a cemetery monument dealer. He had a shop to make and sell cemetery monuments, which is a very interesting profession. He was not fond of the business nor of what he was doing, which is why he insisted that I get an education. My father didn't want me to do the same thing he did. At one point, I volunteered to come home and help my father with the business, but he put his foot down and said, "You will not. You're going to get an education and use that education." I would have to say that he was the one who essentially forced me to get an education.

THACKRAY: Where did your love of chemistry come in?

PARRY: I started to study chemistry because I liked it. After two years, I ran out of money for college. Fortunately, at that very point, I was offered a position with the Forest Service to perform analysis. That's where my love of chemistry started. My immediate supervisor was Lowell Woodward. He encouraged me, and, like my father, wanted me to go back to school. The top man directing the project on which I was working was Dr. Rudger [H.] Walker. At the same time that I was wondering if I should continue with my schooling, Dr. Walker announced that he was leaving the Forest Service to become the dean of the College of Agriculture at Utah State University in Logan. He asked if I would be interested in going to Utah State. I jumped out of my chair with joy. Lowell Woodward and Dr. Walker put me in touch with Professor Don Pittman, a soils professor at Utah State, who offered me a job analyzing soils.

DAEMMRICH: What first stimulated your interest in chemistry?

PARRY: My older brother was interested in physics. After two years in college, as you well know, one can take courses in physics or chemistry. But, what helped me decide on chemistry was that the Forest Service and Utah State paid me for working in chemistry. That was the basis, really, for my choice of chemistry. I have always loved chemistry, but you can love almost anything if you work at it, and that's what I did. I enjoyed working for Professor Pittman. As graduation time approached, the people who were guiding my intellectual advancement suggested that I should go to graduate school. I didn't really know what to expect, but they insisted that I should at least apply to some schools. To my amazement and surprise, I received graduate offers from Cornell [University], the University of Wisconsin, and some others. I selected Cornell, but that was a difficult choice.

DAEMMRICH: How long were you at Cornell?

PARRY: I was at Cornell for two years. I intended to get my degree in four years, but the war [World War II] began toward the end of my second year. I received a letter, not to join the

[United States] Army, but to use my technical skills in the war effort. I learned that DuPont [E.I. du Pont de Nemours and Company] needed assistance in developing a new tetryl plant operation in Kankakee, Illinois, so I went.

Tetryl was a brand new explosive that was useful because it had a very high resistance to shock. Most explosives, for example, TNT [trinitrotoluene] and black powder, explode when hit with a hammer. Tetryl was different. It had a very high shock resistance, a property that was to be used in a fiendish way. The [United States] Navy wanted the tetryl loaded into the nose of artillery shells, so that when the shell was fired at a ship, instead of exploding upon impact, the shell would pierce the ship's steel plating, travel to the interior of the ship, and then detonate. These were very dangerous, and, well, very effective weapons. Tetryl was one of the major factors contributing to navy successes in the war.

Later on, on another project, I was one of the few people hospitalized for mustard gas in World War II. We were testing shells loaded with mustard gas and one exploded unexpectedly. I had the unfortunate job of cleaning up the place. I was put in the hospital for a couple of weeks.

DAEMMRICH: Did your exposure take place on a testing site for DuPont?

PARRY: Yes, I was at the DuPont site. I left DuPont to go to the Munitions Development Laboratory at the University of Illinois. The name of the facility tells you exactly what we did. I worked directly for Professor Edward W. Comings in chemical engineering and Professor John C. Bailar, a famous inorganic chemist, who was a consultant on the project. Both men were wonderful people. At the end of the war, I decided to get my Ph.D. with Bailar. That was one of the best decisions of my entire life. He was a truly wonderful human being. Bailar contributed to all aspects of his students lives. Around the same time as my doctoral research was coming to an end, Bailar went on vacation, but when he returned he said, "Bob, I know where you are going to go." I asked, "What do you mean?" He replied, "You're going to the University of Michigan"—and I did. I loved Michigan. It was and still is a truly great school.

THACKRAY: What year did you go to Michigan?

PARRY: I began as an instructor at Michigan in 1946. After I put in my three years as an instructor, I made assistant professor. After that, the big question became whether I was going to make tenure. Fortunately, I was given tenure and I continued at Michigan until 1968. In 1968, I met a man named Jim [James C.] Fletcher, the president of the University of Utah. Fletcher was a man whom I admired greatly. If he wanted something done, he would get it done. He was a very able man. Fletcher had gotten a Ph.D. in physics and had started his own company of which he was president and ran for a number of years. He eventually sold the

company for several million dollars. So when he was asked to be president of the University of Utah, money wasn't something that worried him.

When Fletcher came to the University of Utah he was very interested in making the university into a high quality institution, which he did. Fletcher brought in people like Chevis Walling, people whom he felt could do what he wanted to get done. Henry Erving was already there. Fletcher also brought in numerous people in biology, including a gentleman from Holland who was an expert in artificial organs.

I was unsure as to whether or not I wanted to go to Utah. But after talking with Fletcher, I realized that it was very hard to say no to him. After hearing my answer, Fletcher said, "What do I have to do to get you here?" I replied, "Well, Michigan's a very fine school. I like it there. They've treated me very nicely. And I don't see any real basis for leaving Michigan." To which he replied, "Well, what's the matter with Utah?" I said, "Utah is not Michigan. Utah is not there yet. It may get better, but it is not on the same level as Michigan, so why should I go to Utah?" He was very direct, "You should come here for two reasons. First, because we want you to, and, second, because you can help us make Utah a quality institution and a fine research institution." I replied, "How do I know that Utah wants to be a quality institution? How do I know that this isn't just Jim Fletcher talking? How do I know that the state will make the same commitment?" He surprised me completely with his reply: "Let's you and I have dinner with the governor tonight." I said, "Jim, the governor is a busy man. He doesn't need to see skeptical professors." Fletcher smiled and said, "Just a minute." He went into his inner office, came out with a big smile on his face, and said, "You have a telephone call in my office." I went into his office, picked up the phone, and the voice on the other end said, "Dr. Parry, this is Cal [Calvin E.] Rampton," the governor of the state. "Dr. Fletcher tells me you have concerns about the commitment of the state of Utah to education." The governor told me that he couldn't commit the state to any long-term activities, but he thought that the state was very interested in its university becoming a high quality institution. Everything he told me was verified up until about five years ago, when new leadership was instated. After that conversation, I came to Utah, and as you can see, I am still happy here.

THACKRAY: Yes. Bob, if you think about your life outside your own research and teaching, how would you describe yourself? One does not become president of the ACS [American Chemical Society] by not having concerns beyond one's own research and teaching.

PARRY: I'm not a very interesting character, but I work hard and I like people. I've had lots of very good graduate and undergraduate students. I am no athlete; I tried to play tennis for years, but my wife can still beat me. She's good. [laughter] My wife also likes to hike and see interesting parts of the world. I have quite a lot of friends. That's a hard question. I really don't know what the answer to that is.

THACKRAY: What took you to your first Gordon Research Conference? How did you hear about the conference?

PARRY: I had just obtained my Ph.D. under John Bailar and had started at Michigan. Professor [W.] Conrad Fernelius, the head of the chemistry department at Penn [Pennsylvania] State University, was organizing the first GRC on inorganic chemistry (2). Fernelius needed young faculty members, so he called me up and asked me if I'd come. There was no money, in those days, for supporting travel and universities didn't have much money to help either, so I went to my first conference paying much of my own way, as did everyone.

THACKRAY: Did you already have some idea about what a Gordon Conference was?

PARRY: I didn't have the foggiest idea. In fact, my first conference was a rough experience for me. I hadn't been out of graduate school long enough to complete any new research, but I was working on a project involving boron hydrides. I had an idea that boron hydrides would act like coordination compounds with a hydride ion hooked to the boron rather than chloride or fluoride. With this theory, I thought that I could understand boron chemistry. However, the compound called the 'diammoniate of diborane' posed a structural problem. I thought it might be similar to coordination compounds, so I wrote a proposal. But, I didn't have data to prove it! I made the mistake of agreeing to give a talk on what was, essentially, speculation. There was quite a lot of evidence supporting my theory, but my presentation was still all speculation and the other researchers ate me alive. It was a very unpleasant session for me. About four years later, I completed my research and the evidence showed that I was right. Thus, the first conference that I attended was *not* a pleasant experience, but that was my fault. I presented speculation and that was not what the attendees wanted to hear.

DAEMMRICH: How was your presentation structured?

PARRY: I used slides and a chalkboard.

DAEMMRICH: How many people were present?

PARRY: About ninety to one hundred people were present.

THACKRAY: Was this a three-hour session?

PARRY: I talked for a little over an hour. After that I didn't talk; they quizzed me. They *persecuted* me. [laughter]

DAEMMRICH: During the sessions, how long, normally, are individuals allowed to present?

PARRY: Gordon Conferences are unique in the sense that you don't just give a canned speech. The audience can interrupt you at any point. For example, one can make the statement that he assumes that this molecule will act like a hydride ion and another will act like fluoride. The audience will immediately break in with questions like, "How can you assume that? Can you name a case where that's true?" It is extremely hard to answer these types of questions without some evidence. If NMR [nuclear magnetic resonance] had been available in those days, I could have obtained, in two days, all the evidence I needed to prove my point, but we used vacuum lines, made compounds, tried to analyze the compounds and determine what they were, et cetera. It was a different world.

THACKRAY: The first conference that you attended was on inorganic chemistry?

PARRY: Yes. There was a different topic in inorganic chemistry each day. I remember a few other individuals who also gave presentations. Alan [F.] Clifford gave a beautiful talk on non-protonic acids. Clifford had beautiful compounds that he had made. He also had data. If you have data, you can quiet the opposition down; but if you don't, then you're open to criticism and skepticism. That was one of the things that finally endeared me to the Gordon Conferences. You could interrupt one another to ask a question. The audience didn't just pick on one person; at one conference, there were two gentlemen, who, probably, were among the best in the world in inorganic theory. One was from Harvard [University], and the other was from [University of California at] Berkeley. The two men got into a very heated exchange involving an area of research that was difficult to prove. This type of situation happens occasionally. The ground rule is that you're on your own. If you make a statement and someone wants to question you, your answer cannot be, "Because I say so." In all the talks that I gave after my first GRC, I was very sure to have data to back up my theories.

DAEMMRICH: On that point, this is a simple logistic question, how did you, in the early 1950s when you were attending, take laboratory findings in a form that you could present to everyone? In other words, what types of data sets were used during presentations?

PARRY: I used slides. The slides contained diagrams, the apparatus we used, et cetera. You see, in those days, not many people knew about vacuum lines. We were the "descendants" of Alfred Stock. Although we had never seen him, we had constructed his lines using his papers as guidelines (3). I had tremendous respect for him. He was a masterful man and a masterful

chemist. Stock's papers told us how to do the research, and then we had to tell others about our experiments.

THACKRAY: How many other laboratories were doing similar research?

PARRY: There were about six or seven laboratories in the United States. The American center of all this research was in the laboratories of Professor H. [Herbert] I. Schlesinger at the University of Chicago. Schlesinger employed a gentleman named Herb [Herbert C.] Brown, who worked the vacuum lines for a while and then became an organic chemist. Brown won a Nobel Prize, as you probably know (4). Anton [B.] Burg from USC [University of Southern California] was performing this type of research as well. He was a very good chemist. In addition, there was George Schaffer at St. Louis University, Tom Wartik at Penn State, Al [Albert E.] Finholt at St. Olaf College, Riley Schaeffer at the University of Indiana, and Grant [W.] Urry at Purdue [University].

THACKRAY: Inorganic chemistry is a rather wide subject area for one conference.

PARRY: Yes.

THACKRAY: How many boron chemistry researchers were at this conference?

PARRY: There were only ten or so.

[END OF TAPE, SIDE 1]

THACKRAY: There seems to be a built-in mechanism that keeps the conferences focused on what is new in a particular field, what has been done, and what is known.

PARRY: Gordon Conferences are very good at keeping one up-to-date and on their toes. At the early conferences, Richard [A.] Ogg from Stanford [University] always asked questions which made the presenter wish that he weren't presenting. Dick was amazingly able. He was an excellent physical chemist and could ask questions that were very penetrating. Some conferences don't have even a single person like Dick, so they're dull. I've been to some that were quite dull with no aggressive questioners in the audience. However, at most Inorganic [Chemistry] Conferences that I've been to there have usually been one or two people who initiate the questioning.

THACKRAY: If there were one hundred people at the Inorganic Conference, how many of those researchers might have been at ten previous conferences, and how many might have been at none?

PARRY: Well, in recent years, conference organizers have been making an effort to provide money to help young professors who have never been to a conference. Out of one hundred participants, maybe twenty-five haven't been to a conference before. There are maybe as many as five or six who have been to ten conferences. Fred Basolo and I almost always went to the conferences. This year will be the first year that I've missed the conference. I've missed between five and ten since 1951.

THACKRAY: All Inorganic Chemistry Conferences?

PARRY: Yes. I have been to a few other conferences as well, but I have always gone to the Inorganic Conference. I have a letter on my desk from this year's chair stating that he did not see my name on his list of attendees, and that he wanted to be positive about whether I was going to attend. Well, I'm not going to attend. I will be going to another meeting. I wrote back to the chair to inform him that I had a prior commitment and was very sorry that I wouldn't be able to attend.

THACKRAY: Bob, you've been to about forty-five out of fifty conferences.

PARRY: Well, let's say forty out of fifty.

THACKRAY: All right. How many other conferences do you suppose have that level of continuity from its attendees? Is this unique or the norm?

PARRY: I don't know that much about the other conferences. For some conferences, I think, it is unique. I would imagine that in the Polymer Conferences there are people that have been there for a long time. I would have to review the attendance sheets.

THACKRAY: In 1951, the Inorganic Conference was where?

PARRY: The New Hampton [School] in New Hampshire.

THACKRAY: GRC attendees were the only people at the school during the conference?

PARRY: That's right.

THACKRAY: And last year where was the conference held?

PARRY: We were at Salve Regina [University] in Rhode Island.

THACKRAY: And were GRC attendees the only people at the university?

PARRY: No, two conferences were at the university in total.

THACKRAY: So, there was no need for you to ever know anything about the Polymer Conference as it were.

PARRY: That's right. That used to be somewhat different, when maybe twenty or thirty conferences were held each year.

THACKRAY: But all the conferences were spread among twenty colleges.

PARRY: Yes, maybe fifteen colleges. Some of the sites hosted two conferences. I assume that New Hampton didn't have room for more than one conference at a time, but it held conferences during the entire summer. Schools needed the money that the conferences provided, so changes were made in order for them to be more accommodating to the conference. Some schools stopped hosting the conferences when they became rich and famous, but the Gordon Conferences saved a lot of those colleges from financial ruin, because they provided a utilization of their facilities in the summer.

THACKRAY: Going back to your first conference, of the one hundred attendees, how many were men?

PARRY: All were men.

DAEMMRICH: Were there moderators at the sessions?

PARRY: Yes, there were moderators who were supposed to call on people, but when you've got a very vigorous discussion going, you don't always follow *Robert's Rules of Order* (5). Attendees shouted at each other, but the noise and disorder did not cause a problem because we were interested in hearing different points of view.

THACKRAY: One is only admitted to the conferences by the chairperson's invitation, correct?

PARRY: That is correct. One sends an application to the chair, who has the right to either admit or deny entry. Some conferences do not receive a surplus of applications and will usually admit everyone who applies. There have been a couple of Inorganic Conferences in recent years where 160 people applied, but only 120 were admitted because of the physical space limitations.

THACKRAY: Who chooses the chair?

PARRY: He or she is elected by the conference attendees of the preceding year.

THACKRAY: A vote involving the one hundred or so people in attendance?

PARRY: Yes. The attendees select the chair. Two or three people are presented and then a written ballot is taken. The candidates leave the room, so that the attendees can discuss the candidates' qualifications. Then there is the vote.

THACKRAY: Chairing a conference honor to be competed for.

PARRY: In a sense it is. It is a job, and a hard one at that. It used to be even harder because the chair had to raise money.

THACKRAY: Essentially, one volunteers for the chair and is voted for or against by conference attendees.

PARRY: Yes, but one does not volunteer. You are asked to run by the nominating committee.

THACKRAY: Who are the members of the nominating committee?

PARRY: In the early days, the second conference committee consisted of Conard Fernelius, John Bailar, and Larry [Laurence L.] Quill, who was at Michigan State. These men were the grandfathers of the whole discipline. These three men pushed the envelope of inorganic chemistry.

THACKRAY: When did you join the nominating committee?

PARRY: Well, I didn't become a member of the nominating committee until after I had been elected chair. Now, everyone who has been elected chair is placed on the nominating committee.

DAEMMRICH: At some point, GRC instated vice chairs. Now the vice chair automatically becomes chair for the next conference cycle.

PARRY: Yes, that is correct. It was decided that raising money was a problem and that fundraising does not work if one starts raising money in November for a conference in July. You cannot present invitations to speakers unless you have money in your pocket to pay for the invitations. That is why the vice chair was instituted. All of these things evolved, but, for the first conferences, somewhat older members of the profession, who enjoyed a lot of respect and a lot of support, were needed. The "initiators" did a great job. They rose to the assignment.

THACKRAY: What you are describing sounds very academic. Has the Inorganic Conference always been very academic?

PARRY: Let me address that question a little bit differently. [W.] George Parks was the first director of the conference, and he did a wonderful job. Money was always a problem. How do you get people there? A researcher really has to be dedicated to want to spend his vacation and his dollars to go to a technical conference. George Parks, when he agreed to run the conferences as director, went to the leaders of industry and asked each for a thousand dollars. When he finished he had thirty-five thousand dollars. He was very effective.

The organization of the conferences has been copied throughout the world, which makes the Gordon Conferences, essentially, unique. The organization structure, I thought, came from

Alex [Alexander M.] and Irene Cruickshank, but it didn't. They brought the pattern from the Gibson Island Conferences run by Neil [E.] Gordon. The Cruickshanks were responsible for putting the pattern into the conferences you see today.

On Gibson Island, Gordon said, "We're going to meet from eight in the morning until noon. Then you can have the afternoon off. We'll meet again from seven until nine at night." Attendees loved it—they loved having the afternoons off. The afternoons gave rise to a lot of talk about chemistry. Attendees became involved in the morning discussions, which continued into the afternoon. That was one of the wonderful things about the conference—you had these types of opportunities. You had a "trapped" group of excellent researchers whom you could talk to and argue with. The pattern, put in by Alex and Irene, followed Neil Gordon's pattern almost precisely. But after the first year, there were people who thought that the pattern was not effective. They wanted to work in the afternoon and go home at night, but as time went on, people began to like the model. It has become, really, the signature pattern of the Gordon Conferences.

THACKRAY: Going back to 1951—if the sessions ran from 8:00 am until noon, were you being "eaten alive" for four hours during your presentation?

PARRY: No, there was another person presenting besides me. I was being eaten for an hour and a half.

THACKRAY: They took a break to digest. [laughter] And what happened in the evenings?

PARRY: In the evenings, there were two speakers as well, making the sessions either an hour and a half or three hours.

DAEMMRICH: What went on during the afternoons and after the evening sessions?

PARRY: You could go to the morning session, then play tennis or go sailing in the afternoon. That was the great thing about New England. One of the bad things about the Gibson Island Conferences was that the conferences were held at a very posh country club whose patrons didn't really like scientists using their club. As a matter of fact, ultimately, the conferences left Gibson Island because the club wouldn't host us any longer. The conferences worked fairly well in New Hampshire, but Alex and George still had problems. You've read that George banned one researcher from the conferences for life because he was intoxicated while shooting a bow and arrow; the arrow traveled the full-length of the hallway. He could have killed somebody. There were a few other instances like that, but by and large there was lots of harmless horseplay, and very little truly bad behavior.

THACKRAY: When did women enter the field of inorganic chemistry?

PARRY: Well, attendees didn't like to have women around at first. I didn't know any women inorganic chemists. The first woman who was an inorganic chemist that I interacted with was probably Mary [L.] Good. I met Mary when I was the editor of *Inorganic Chemistry*. Mary sent me a paper. I think she was a professor at Louisiana State [University] in Baton Rouge at the time. I sent the paper back to her and told her that I would accept it provided she made certain changes, which she did. I learned later that she wasn't happy about making those changes, but she wanted to get published.

There weren't very many women in chemistry at that time. There was a Catholic nun at the University of Michigan who came to me and wanted to work with me. Her name was Sue [Susan] Fleming, Sister Sue. At that point, I had not had any female graduate students, which I justified to myself by saying that the work that I was doing involved things like getting loads of liquid nitrogen, moving heavy things around, et cetera. I thought women would have trouble with that type of work—this was a job for men.

When Sister Sue came and asked me if she could work with me, I said I'd have to think about it because hazardous activity was involved. I was concerned that her habit could catch fire. Well, she came back maybe a week later. "Dr. Parry, can I work with you?" I began to hem and haw. She said, "Yes or no—can I work with you?" I said, "Yes," and she was the first woman that I had as a graduate student. She was wonderful. She didn't need to worry about lifting things. My male students lined up to help her. She'd whistle, they'd come, and whatever she needed was done. After that, I had quite a number of female students. One was named Karen Williams Morse. She married one of my other graduate students, Joe [Joseph G.] Morse. They both received Ph.D.'s. In fact, I told them that I arranged the whole thing by putting them in the same lab together. Karen is the president of Western Washington University. She's done wonderful things.

THACKRAY: The early Gordon Research Conferences were comprised almost entirely of men. Did the fact that women were not present become an explicit issue?

PARRY: It never became a formal issue. But, you see, it was becoming something that universities were talking about. Someone said, "We need to invite this lady to give a talk. She is doing good work here." That was the way it started. People never said, "We've got to get a woman." We have always felt, "Here is good work being done by this chemist who just so happens to be a woman."

THACKRAY: Has the conference evolved to include the social pattern of today's families and so on?

PARRY: There have always been families. Attendees could bring their wives and families with them, but they usually lived off-site. There were lots of cabins and semi-resorts around the New Hampshire site where families would frequently stay. The researcher would come over in the morning and go to meetings. He would play with his family in the afternoon, put the kids to bed at night, and then go back to work. A lot of families worked in this way.

THACKRAY: So there were people staying at the college, and people who were not.

PARRY: That's right. You didn't have to stay at the college.

DAEMMRICH: How did that experience work for you?

PARRY: Well, I've taken my wife to some of the conferences, but she didn't really like the conferences very much. So she would go with me once in a while.

DAEMMRICH: Was the absence of participants in the afternoons noticeable?

PARRY: When the conferences were at Brewster Academy, families were on the lake most of the time. We had boats, places to swim, tennis courts, et cetera. Families could use the facilities because they were an important part of the conference. An attendee only needed to show his badge and he and his family would be let in. But in New Hampton it was different; New Hampton was a tennis place. People who played tennis would play a lot in the afternoon. There was golf nearby, but tennis was the big thing. There were also tennis matches in the summer at New Hampton, sometimes when conferences were there. In fact, one year Gil [Gilbert] Haight and Harry [B.] Gray won the New Hampshire State [Tennis] Championship. After that, the scientists were barred from participating. [laughter]

THACKRAY: All right. Going back to 1951. You were the young assistant professor. Were there a lot of older more established researchers in attendance?

PARRY: No, there were not a lot of older men in the room. The "elder statesmen" of inorganic chemistry were not that much older than some of the youngest in the group. The leaders included Conard Fernelius, John Bailar, Larry Quill, Roland Ward, Therald Moeller, Lou

[Ludwig F.] Audrieth, and Wendell [M.] Latimer, among others. These people were carrying inorganic chemistry into a new era. In the early days, inorganic was frequently taught with lots of memorization of properties and reactions, but with limited correlations involving structure and electronic interpretations. This pattern changed as people like Fernelius, Bailar, Ward, Quill, Latimer, and Moeller moved away from the old pattern. Ultimately, people like [Henry] Taube, Basolo, [Ralph G.] Pearson, and a lot of others defined a new path of systematic, structure-based inorganic chemistry.

THACKRAY: In last year's Inorganic Conference, what was the age distribution of the group?

PARRY: There were about twenty people that were young. A real effort was made to get them to the conference. Including the younger attendees, there were about 120 attendees last year. The attendees were fairly evenly distributed from twenty-five up to forty-five or fifty years of age. Finally, there were a few attendees who were in their eighties, like Basolo and I. It's not completely a young man's game. People like to attend, and, scientifically, they can frequently be very useful.

THACKRAY: Yes. Conference attendance is like the rest of science in the sense that the conference reflects science. Age brings certain benefits, but youth has plenty of benefits as well.

PARRY: That's right. Very well put! The conferences also have a diverse demographic from academia, government labs, and industry. The industry representation is unique for a scientific meeting and dates back to early years when the conferences raised money from leading companies. Companies said, "We'll give George Parks a thousand dollars." Parks said, "I will give you the following for your thousand dollars: you can send any one of your employees to the conference of your choice."

THACKRAY: To each conference?

PARRY: Well, they gave a thousand dollars to support the conferences in general! Parks wasn't going to put restrictions on their right to choose the conference. Companies had to pay their employee's expenses including transportation, registration fees, and all other expenses. I don't remember restrictions on industrial attendance other than those imposed by the companies.

[END OF TAPE, SIDE 2]

THACKRAY: If there were thirty-five companies paying a thousand dollars each and about twenty conferences—that only amounted to two people per conference. Two people in a hundred aren't going to make the conference into an industrial conference.

PARRY: But they weren't the only ones. Remember, in those days, the only alternatives were the ACS meetings with twenty-minute presentations. Industry felt that they did not have sufficient contact with each other and with the academic community to properly pursue some of their problems. So they sent people to conferences, paying their way.

DAEMMRICH: And those people had to apply by the normal routine?

PARRY: That's right. If the industry could send the person, the chair had to take one person from that company.

DAEMMRICH: All right.

PARRY: After that, industry could apply to as many conferences as they wanted, but they had to do it in the usual fashion. At that point, industry liked the conferences, so a lot of industry people attended. They began to give that up gradually and that is one of the problems. The industrial participation is low now.

THACKRAY: When did industry begin to back out of the conferences?

PARRY: That is a hard for me to answer. Perhaps, gradually over a period of twenty years.

DAEMMRICH: In the 1950s and into the early 1960s, what companies, specifically, were interested in inorganic chemistry?

PARRY: Augustus [H.] Batchelder was from Standard Oil [Company] of California. R. [Robert] W. Cairns was in industry.

THACKRAY: I believe that Cairns was with DuPont.

PARRY: Right. George [B.] Brown was in industry. A. [Arthur] M. Bueche was in industry. Joe [W. Joseph] Coppoc was in industry. [John P.] McCullough was in industry. Dave [David W.] McCall was in industry. John [D.] Hoffman was in industry. [Ernest G.] Jaworski was as well. Andy [Andrew] Caldor was in industry. I think [James R.] Florini was in industry. Kathy [Kathleen C.] Taylor was in industry. I cannot think of all their names now, but there were a lot of industrial scientists. For example, I remember that Earl Muettertie used to come all the time—DuPont sent him. DuPont usually sent a lot of people. At the Inorganic Conference there would be a delegation of at least four people from DuPont.

THACKRAY: The model for raising funds from industry was taken from Gordon and Parks?

PARRY: Yes. Gordon was not as successful as Parks was. Gordon, as I understood it, made the attendees pay to come to his conference. He invited them, but they had to pay their own way, more like an ACS meeting. I'm speaking of an area that I don't know much about since I was never invited to give to the Gibson Island fund.

THACKRAY: In Parks' early days, in the 1950s and 1960s, you stated that you had to pay in order to attend.

PARRY: I did.

THACKRAY: Have academics ever been underwritten?

PARRY: Actually, I got the first grant that was not an industrial grant for the Gordon Conferences. I was playing tennis in the morning with Denny [Dennis] Elliott, who was at the Air Force Office of Scientific Research, Don [Donald] Martin of Harshaw Chemical [Company], and perhaps Fred Basolo, but I can't remember. After we played, we walked back to breakfast. I was going to be chair in 1963; this was 1962. I was complaining that I couldn't do what I wanted to do, which was to invite attendees from all over Europe. There were new developments in inorganic chemistry, like the crystal field theory. The exciting concepts in inorganic chemistry were coming out of Denmark and other European countries, so I wanted to bring those scientists to the conference, but I didn't have the resources. I said, "We will have to do as we have always done before, just take local talent."

Denny said, "right" in a noncommittal way. About a month later I got a letter from him asking, "Would you like me to donate ten thousand dollars to bring people from Europe to the conference?" He continued, "I just had ten thousand dollars returned from a project for various reasons. I don't have a commitment for it. Would you like it?" [laughter] Well, I couldn't get the "yes" out fast enough.

Initially, I had trouble trying to get George to accept the ten thousand dollars because he said he didn't want a tax problem or political issues. So I said, "Dr. Parks, I have a respectful request. Will you ask the board of trustees what their opinion is? If they say that they feel that it would be a mistake to take this money for the conference, I will write to Denny that we cannot accept these funds. But if they can convince you that it's in the best interest of the conference, I would very much like to use these funds." He said he would bring it up to the board. He did and the board said yes. So Denny gave me the ten thousand dollars. I used the money to bring a number of Europeans over, but I only used four thousand dollars of it. The next year I left six thousand dollars for Pete [Peter R.] Girardot, who was one of my students and chair the next year. Pete used four thousand dollars and brought additional Europeans over and some other younger people. The year after, there was still two thousand left for the next chair.

Denny came into more money and began supporting the inorganic conferences along with others every year until he retired. Denny broke the ice and then NSF [National Science Foundation] and many other organizations started to give money. We had to break through the barrier somewhere. George's point of strength was that he had been able to get industrial support to run the conferences, not as plush as we would have liked, paying expenses, and so on. But he had not been able to get support for speakers and similar items. He felt that this would reduce the ability of the conference administration to control the quality of the conference. I can understand now why he didn't want to take the money. He knew that his efforts were working and he wasn't sure what would happen with the new arrangement. But to his credit, when the board of trustees said, "Take the money and run," he did.

We soon found out that federal support provided the much needed fiscal help. Remember, there was very little research money available when George started. The NSF wasn't in existence and there were some agencies, like the Air Force Research [Laboratory], ONR [Office of Naval Research], and others, who had some money but weren't strictly pushing science. Federal help for the conferences has grown so that now the conferences have a sizable amount of federal support. We now have granting agencies that provide money to many of the participants who in turn pay the expenses of their students to come to the conferences. Of course, industry paid a higher fee. Those funds came out of fees and other sources of revenue that the conferences had, but that didn't really permit one to do very much. Try to bring people to the U.S. from Europe with a thousand bucks! That is only enough for one person. Maybe one can fund two attendees for a thousand, if the attendees paid the balance. People were interested enough in the conferences to come. They liked them. The Europeans thought the conferences were great. They liked all the play in the middle of the day and those types of activities.

THACKRAY: Did you feel an obligation, going back now in 1963, to raise money?

PARRY: I felt obliged to raise funds in order to invite foreign attendees. The Europeans didn't have sources for money at the time. Air travel back then was expensive, so if you wanted to

bring someone from Europe, you had to be able to pay all of their expenses especially since these scientists were relatively young and without many resources.

THACKRAY: When you were conference chairman, you received [United States] Air Force grants. Did you receive money from DuPont as well?

PARRY: No, I didn't.

THACKRAY: However, Parks *did* receive money from DuPont.

PARRY: Yes. In fact, Parks would have objected to us trying to tap his industrial sponsors. They were sponsors of the conferences, not of a single conference.

THACKRAY: That is very interesting, because it's a very centralized model. Yet, at the same time, the conferences were very disaggregated.

PARRY: Yes. Well, Parks felt that there were some fiscal restraints that had to be imposed, so that he wouldn't be nickeled and dimed to death by people wanting this, that, and the other. Parks' philosophy was to distribute funds evenly among all conference chairs. That was a good idea. But the question remained, "Is it a good idea to leave chairs on their own to find additional funds?" You see, I had solicited funds unintentionally. I wasn't asking Denny for a grant. I was simply informing Denny of what I could and could not do.

THACKRAY: Thus, funds were not only obtained from the government, but also by the entrepreneurship of the chair.

PARRY: Yes.

THACKRAY: And *that* was a breach of protocol.

PARRY: That's right. Parks didn't want the conferences to function in that manner. You see, there have been four eras in the Gordon Conferences. The first was the Gibson Island Conferences, about which you have read as much as I have. Then there was the Parks era. Parks made the conferences work because he could raise money from industry. If he said that he needed thirty-five thousand dollars, he got thirty-five thousand dollars. Industry trusted

Parks enough and liked him enough to give him what he asked for. That was his stock-in-trade. When industry sent people to the conferences, Parks hosted cocktail hours with some of them, but the academics were not invited. The Parks era was all about industrial chemistry and industrial participants. When the idea that one could get money from the outside was finally accepted, George felt that it posed a problem for the conference administrator.

The third era came with instatement of Alexander Cruickshank as director. Alex encouraged people to go out and find money. This policy changed the whole pattern of the conferences, because, now, there was emphasis on trying to get money from outside sources with the government as a possible provider. Denny Elliott's donation emphasized the point that we could get outside funding. The era had changed completely. Now, there was government money which came from agencies such as NSF, the air force, the navy, the Army Research Office, and environmental agencies.

DAEMMRICH: You received funding from the EPA [Environmental Protection Agency]?

PARRY: EPA and many other groups.

DAEMMRICH: In that period, was there any conflict or discussion about differences among military, civilian, and governmental support?

PARRY: No. In fact, the people running the military services did their best to use the same rules as NSF. Strict rules detailing the topics of discussion were not imposed on the conferences.

THACKRAY: But in 1970 there was campus controversy about military funding.

PARRY: Yes, and, you know, I was at the forefront of all of that. At one point, there were chanting students trying to force their way into the chemistry building.

DAEMMRICH: Here?

PARRY: No, at Michigan. Michigan had a more vigorous group of students than the group here at Utah. I remember standing in the front door while students chanted that they were going to destroy all military research. I shouted, "Name me one person who's doing military research in this building." There was a young member of the chemistry faculty, originally from Cornell, Julian Goddell, who was the students' "insider." One of the students said, "Tell him, Julie." I

turned to Julian and said, "Yes, tell me, Julie, who is doing research in this building that's military or secret?" Of course, he couldn't name me one name. There were a lot of us who had military money, but it was used on projects that the university had supported previously. That incident took the wind out of the students' sails. I wouldn't move out of the doorway, so they left. It's difficult to comprehend how one person telling lies can get students all worked up. As you know, this was not just one isolated incident—the University of Wisconsin was bombed by student protesters.

THACKRAY: Yes, I remember. Going back to the Gordon and Cruickshank eras and the industrial sponsors of the Gordon Conference. Has that type of funding gradually died away or was it explicitly discussed?

PARRY: It didn't die away, but I think that it faded into the operation of the conference. Industry wanted their researchers to attend, so they paid their way. If they wanted to send ten people to the polymer conference, they put ten names on the list, and unless they were horrible researchers, they got in. I think that it was just a matter of changing the mode of operation.

THACKRAY: Yes. You stated that there were four eras. First there was Gibson Island, followed by Parks, and then Cruickshank, who encouraged individual fundraising. What era followed?

PARRY: The fourth is our current director, Carlyle B. Storm. He was specifically given the job of applying newly developing technology to Gordon Conference operations. The number of conferences continued to grow and the technology of the "information age" was needed to keep track of more and more conferences. When Alex retired after a brilliant career as director, Storm was brought in with the charge to modernize communication operations, record keeping, conference operations, et cetera. We are now in the fourth era and I believe that Carl is doing a good job.

DAEMMRICH: Let us go back to the Parks era, when you first took a leadership role in GRC and began serving on the board.

PARRY: Yes, I started under Parks.

DAEMMRICH: What made you want to be involved in GRC governance? What caused the transition from being chairman of a conference to wanting to be on the council?

PARRY: Well, you get to the point that you like what you see and, in a word, you want to facilitate that kind of an operation. One of the ways to do that is by being on the board. The council was such a big body, and it doesn't really make most of the decisions. The board was a small group that made the decisions—it made the conferences work. The chair was the person who had to the major responsibility.

Let me tell you a bit about the operation when I first joined the board. It was a moment of crisis for GRC because of George Parks' troubles with the IRS [Internal Revenue Service]. When we were having trouble with George, I did not make public statements as to why he was being terminated and what the circumstances were. Instead, I said that Dr. Parks had personal problems, which made him wish to resign as director. Parks later said that was not true—he didn't want to resign as director. But we didn't make that point—we tried to make it as painless as we could for him.

At the time that we first heard about his problems, Parks told us in a board meeting that the IRS was looking at *the* income tax. He didn't say *his* income tax. He indicated that it was the income tax for the Gordon Conferences and we assumed that the conferences had been paying income tax as it was required. Parks didn't say any more than that and he said it was still preliminary and he would tell us about it later. Well, when the next meeting was held, we were told that it was not the Gordon Conferences' income tax, but it was George Parks'.

[END OF TAPE, SIDE 3]

PARRY: The problem with the IRS developed when Parks was reimbursed for travel and other expenses by GRC but then also claimed them as business expenses on his income tax. I don't know how they learned about it, but they started to investigate him. Ultimately, he was indicted, tried, and convicted of a felony. I was told, but I cannot verify whether it's true, that he was not sentenced to prison. The judge who presided over his trial was scheduled to meet with him and sentence him in an open court. The judge stated that he thought that George Parks had been penalized more than enough by what had happened to him. The judge continued, "When all of this started you were a widely respected member of the scientific community. People admired and supported you. Today you have lost all of that. I can't recommend any punishment that would be worse than what you've suffered. I am not going to send you to prison. I think your life outside will be a sentence that is rough enough."

Now I do know that George didn't go to prison. George died a few years later (6). He was very sick when all of this took place. It was too bad—I liked him, even though he was an autocratic character. I didn't have any good connections to industry and I couldn't give him any money, so I wasn't very important to him. But he was a guy who wanted to do what was best for science when he started, and he did it. He set up the whole thing. He got Alex and Irene to do the job. He hired them. They actually carried out the organization. Alex knew all about operations when he took over from Parks. He knew a lot more than Parks did about how to operate the conferences. Alex and Irene, when they took over, saved the conferences.

Alec Jordan, who was a public relations person for the GRC, called me and wanted me to fire Parks. I told him, "I don't think I have that power, and if I do, I'm not going to use it." I said, "I think that this is a matter for the entire structure of the Gordon Conferences. We're going to approach this step by step through the organization and the rules of the conference. We are not going to jump out in front firing." Alec wasn't too happy with that position. He argued with me quite vigorously that I had a responsibility to the people of science to get rid of George.

There was also talk from the board about having someone go to Parks to tell him that he had been relieved of his duties. I said, "I'm not going alone. I need some backup." I also said, "Further, I would like to have as backup someone not on this board, somebody from industry who George respects." Someone responded, "How about A. H. Batchelder? He's respected by everyone." I said, "Fine. If that's your recommendation, I'll call him and see if he will go with me to talk to George." They also suggested Joe [W. Joseph] Coppoc, who was a research director at Texaco. Batchelder was at Standard Oil of California. Both were big wheels in the oil industry. I called Joe Coppoc first, and finally got him to agree to go with me to talk to Dr. Parks.

Then I called up Batchelder. He said, "Dr. Parry,"—I had never met Batchelder face-to-face so I was still Dr. Parry to him—"I've been chairman and a member of the board. I've paid my dues." I responded, "Please. This isn't an ordinary Gordon Conference request. We have a situation where if this is not done right, these conferences will disappear." He said later that after I made that statement he knew this was something important that was very bad. Batchelder said, "All right, you've convinced me." He came with me and Joe to see Parks. That was very good, because Parks couldn't say that the board or I had railroaded him. Batchelder and Coppoc were, of course, as concerned as anyone else about what Parks had done. They were wonderful!

DAEMMRICH: Can you say a bit more about what the threat was to the conferences from Parks' personal IRS troubles?

PARRY: The conferences didn't have any problem. As soon as Parks got out, the conferences were reviewed, and received a clean bill of health. The IRS wasn't interested in the conferences.

THACKRAY: Presumably, the word got back to others in industry as to why Parks had been removed.

PARRY: In fact, this situation has even been written about elsewhere. What made this situation seem appropriate to everyone was that I had Batch [Batchelder] and Joe Coppoc, who were respected throughout the whole country. Nobody could say that we had railroaded him,

that we hadn't been fair, or that we weren't doing what was best for the conferences. If I had tried to move early in the game, I could have triggered a fight which would have been nasty.

THACKRAY: Yes.

PARRY: Batchelder and Coppoc were wonderful; I have tremendous respect for those two men. They went in with me in front of George, who was telling them that Alex couldn't do it. I still remember Batch. He said, "George,"—he could call him George— "George, this is not a matter of whether Alex can do this or not. That's our problem. The thing is, you can't." [laughter] They were part of a group that, I think, saved the conferences. Batchelder and Coppoc, the Cruickshanks, and many others. I felt that I had to appoint a committee of people to review the functioning of the conferences after that. I had to be part of that group according to the board. The group included Batchelder, Coppoc, Herb [Herbert S.] Gutowsky, Herbert E. Carter, and John [P.] McCullough. The work of those men and others saved the conferences. McCullough and Gutowsky studied all the transactions that had been done, and came back to inform us what needed to be done. They kept us from going downhill.

THACKRAY: When you say transactions, are you speaking of both the financial transactions and how the conferences operated?

PARRY: Right. Money was a part of it, but it wasn't the whole thing. Also important are other factors, such as the way you treat your individual conference chairs and what kind of support they are given.

THACKRAY: Are there correspondence or drafts in the University of Utah's archives that relate to these processes?

PARRY: I don't know. The university asked me if they could have my papers; I said, "Yes. I'd be glad to get them out."

THACKRAY: I would like to review your archives because the Gordon Conference archives themselves are disappointingly blank on exactly these sorts of territories (7). What were Parks' other strengths in relation to the conferences, beyond industrial fundraising?

PARRY: He had many administrative skills. He had the wisdom to treat his staff fairly and that generated employee loyalty. If Alex and Irene were given responsibility for an operation, he let them do it. Parks did not try to micromanage their work. He delegated responsibility and was

very successful as an administrator for many years. In my judgment, his most important administrative achievement was getting Alex and Irene Cruickshank involved with the Gordon Conferences. Let's talk briefly about the Cruickshanks.

Alex obtained B.S. and M.S. degrees from the University of Rhode Island where Parks was a professor. Irene came into Alex's life fairly early. She was the "girl next door" with whom he played as a child. They hit it off really well and were married before they left Rhode Island to go to graduate school at the University of Massachusetts. Alex obtained his Ph.D. in chemistry there under the direction of Professor J. Harold Smith. Alex and Irene then returned to Rhode Island where Alex became a faculty member at the University of Rhode Island. That began a period of collaboration with Dr. Parks. Irene was hired as Dr. Parks' administrative assistant and secretary. Alex and Irene both worked with Dr. Parks for many years and were very knowledgeable about the operation of the Gordon Conferences. In fact, Alex and Irene set up many of the conference operations and were very knowledgeable about the details.

When Dr. Parks' legal troubles appeared, the board of directors asked Alex to accept an appointment as the executive director of the conferences. In my opinion, Alex Cruickshank's willingness to accept the responsibility under very trying circumstances and his ability to do a truly outstanding job, with Irene's help, *saved* the Gordon Conferences.

THACKRAY: Do you think that anyone knew all of the conference chairs?

PARRY: I don't know. At first the number of conferences was relatively small and perhaps Alex and Irene knew all the chairs, but certainly the members of the conference administration, overall, did not know all of the chairs. A burst of expansion developed during Fred Basolo's tenure as chair of the board. That expansion bothered Fred.

THACKRAY: He didn't want expansion?

PARRY: He wanted carefully controlled expansion.

THACKRAY: Why is that?

PARRY: Because Fred felt that the conference subject matter was getting into areas of biology and medicine where the conference structure was not knowledgeable. He wondered where it would lead.

Fred felt, then, that there were good conferences in the physical sciences, so he wanted to maintain the emphasis in our area of expertise. He thought that the biologists would take

over, and, well, they have, but it hasn't done any damage, as near as I can see, to the other operations. That is my personal opinion.

THACKRAY: What were the Cruickshanks' other strengths?

PARRY: First, Alex was a native of New Hampshire. He had the ability to talk with the heads of local colleges and universities. He knew what their finances were like. He knew what he could do, what he was doing for other schools, and he was well liked. Cruickshank was a well-respected and well-loved person by all who did business with him.

Parks was always Dr. Parks to everyone who wasn't in his inner circle. Parks always referred to people by their last name, and not by their title, that is, Dr. Whomever. Sometimes, if he had to, he would say, "Dr. this…" or "Dr. that…" Alex was completely different. He was Alex to everyone. He went to local schools and to the Thursday night meetings, which had a social component, and people enjoyed having him. The last thing Alex brought was an appreciation of the contributions of others in all areas: fundraising, controlling, finances, maintaining conference quality at a high level, seeing that things worked smoothly, et cetera.

One of the jobs that he had to do, and one which he needed help with sometimes, was trying to control out-of-control participants. It seems silly to say this, but some university professors are as bad as everyone else when they are intoxicated. Alex had some problems to face, but he handled them very well. He took the responsibility to do what was required and everybody respected him for that. He never fought; he simply took action to control people and to maintain the decorum of the conferences. He was also wise. He encouraged chairs to use money to bring students and a broad spectrum of conferees. People learned that he had the best interests of the conferences in mind so they supported him.

THACKRAY: That seems to have been a shift from the traditional role of the chair.

PARRY: It was a gradual change, but Alex encouraged it. Parks didn't want to change and Alex couldn't change immediately—he didn't have the resources. But there has been a downside to Alex's gradual change, in that, now, academia, and not industry, is dominant in most conferences. Now we worry about how to get industry to participate more actively.

THACKRAY: Why is that a concern?

PARRY: Industry is a very large segment of the chemical profession and it has a very important function in our society. After all, we are an industrial society. The concern is that we don't want to turn the conferences into an academic appreciation center. We want to try to interact

with people in industry, help them if we can, and get to know their people and their activities, have industry get to know us, our people, and our problems, and maintain a mutually desirable cooperation.

THACKRAY: This is what the board and senior debenture holders like yourself worry about?

PARRY: Yes.

THACKRAY: But it's not necessarily what any particular chair worries about.

PARRY: I don't know if the chair worries about this, but he is asked questions about it. If there's a conference with a sizeable industrial activity and the chair tries to make it an academic show, he will have to answer a few questions. For example, if the Polymer Conference only discussed a series of theoretical presentations on polymerization, the chair would hear complaints.

THACKRAY: Complaints from whom?

PARRY: They would hear complaints from participants, the scheduling and selection committee, and maybe even from the board.

THACKRAY: How would the board know?

PARRY: The conferences are evaluated by qualified observers and by all the participants. Every person who attends the conference is asked to fill out an evaluation form. If sizeable groups of the participants and or the evaluators state that a bunch of pointless gobbledygook, which they didn't appreciate and didn't understand, took place, the conference chair, the director, the scheduling and selection committee, and, in extreme cases, the board, would hear about it. Usually it doesn't go past the scheduling and selection committee. The system works.

THACKRAY: What is an evaluator's function?

PARRY: Evaluators remain at a conference long enough to make a firm evaluation.

THACKRAY: Were evaluators present in 1951?

PARRY: I don't know if they were present in 1951. The procedures were just being set up, but they are, as far as I know, in use at current conferences.

THACKRAY: But evaluators have been in use for a long time?

PARRY: Yes. When I ran a conference, many years ago, I knew who the evaluator was. He was a highly regarded professor from the University of New Hampshire. I had a lot of confidence in his ability to give a fair and highly competent evaluation of the conference.

THACKRAY: Was he an inorganic professor?

PARRY: Correct. And he was a very good man.

THACKRAY: Who's task was it to write a confidential report on whether Dr. Parry knew what he was doing or not?

PARRY: Many people had that task.

THACKRAY: All right.

PARRY: The people involved didn't always write glowing tributes about the conference which they attended. They were reviewing. They felt a professional responsibility to competently and objectively judge the conference.

THACKRAY: And those reports went to the board?

PARRY: First, they went to the executive director and the scheduling and selection committee. If the report was bad enough, the committee would not reschedule them for a year or more. There was a control system which worked well. If controversy were to arise, the board had ultimate responsibility.

THACKRAY: That's good. As board chairman, how much of a demand was there on your time?

PARRY: That position took up a lot of my time, but I did not serve during a normal year.

THACKRAY: Yes. Were the board meetings held in different locales across the country like ACS meetings?

PARRY: No, for the most part these meetings were held in New York at the Roosevelt Hotel. One came to the hotel, stayed overnight, and then started in the morning. If we didn't finish, then we stayed until the business was complete. When I was chairman, I convinced the board members to meet at Snowbird [Ski and Summer Resort] in the Utah mountains. My argument went something like this: "Why do the meetings have to be in New York City at the Roosevelt Hotel? All of us know what the inside of that place looks like. Let's go out to Salt Lake to Snowbird Resort. I'll get you a place where you not only have a fine meeting, but it won't cost any more or even as much as it does in New York City, and you can see a beautiful spot." Well, after a pitch like that the board members who were sitting in the Roosevelt Hotel said, "Great!" So the next meeting was held at the resort. A few meetings were even held in California.

THACKRAY: Mentioning California—you were one of the people pushing to set up California operations. Was that an easy or hard sell?

PARRY: It was an easy sell. Alex didn't need to be pushed by anyone. He had used almost all of the small colleges in New England, particularly in New Hampshire, that could do an effective job at hosting a conference. Today, the conferences use schools around the country. Some are not in top shape yet, but they are improving. Some of the schools, like New Hampton School and Brewster Academy, did a fine job for many years, but they wanted to use their facilities in other ways during the summer. We had to go elsewhere. Today, the Inorganic Chemistry Conference meets at Salve Regina in Newport, Rhode Island. The facilities are very nice.

THACKRAY: I want to go back to the board for a minute. As I read the minutes, I see what appears to be a sort of curious issue—whether the fee could be raised. But, in fact, the main problem seems to be that the reserves were always growing. Is that your sense of the background reality?

PARRY: Well, not at that time. When I was chairman, we weren't so plush. The base has been expanded tremendously. The conferences did not want to give chairs unlimited resources. If a conference required additional funding to achieve a given goal, it was the responsibility of the

chair to raise it. I don't even remember money as a serious problem. One must remember, however, that if the conferences generate money, the IRS becomes very interested.

THACKRAY: But, first with Parks and then with individual conference fundraising, it's the charging of fees that is central. One way or another it seems to me that these are very successful financial mechanisms, if you will.

PARRY: I don't understand why. I find it very hard to raise money from industry or anyone else, but all things related to the Gordon Conferences have been well supported, even in the Gibson Island Conference days. If it was said, "We need forty thousand dollars to pay for the house on Gibson Island," and forty people were asked to give, then forty thousand dollars was raised. Neil Gordon did just that.

THACKRAY: Yes.

PARRY: How he did that is a mystery to me, but he was a master at it. Parks was pretty good at it as well.

THACKRAY: But Cruickshank shifted the direction somewhat.

PARRY: Cruickshank didn't feel that he had to control the money, but he felt a responsibility to see that all conferences were adequately funded. The financial picture was quite different in Cruickshank's time. Support from government agencies was the rule, not the exception, and conference support was available from many areas. This is even true today. Everyone should do their part. Some people would like broader support from the industrial community. This can be obtained by attention to the programs.

THACKRAY: What else can you say about the characteristics of the present era, the Carl Storm era?

PARRY: Well, Storm has taken the international conferences and embraced them. Alex's efforts to promote expansion abroad were opposed by some people, but Storm has embraced it and opposition has not been strong. The fact that people from abroad like the conferences has contributed to some of the success of the conferences. The operation is getting bigger and stronger everyday.

THACKRAY: Would I be correct in assuming that every Gordon Conference these days has participants from all over the world, but the majority of the participants may come from within relatively close range to wherever the conference is held?

PARRY: Two hundred miles to five hundred miles is the usual radius. I think that your statement is probably true with the majority of attendees. For example, if we have a conference in New England, we'd get a lot of people from DuPont and other companies that are five hundred miles away. Most of the attendees can drive up in their cars—it doesn't cost them much. On the other hand, if you hold conferences on the west coast, there may be only a few attendees that DuPont sends because it's very expensive. Economics, I think, controls attendance. Our major concern is that industry is not attending as it once did. It's becoming more of an academic "we-love-you" session.

THACKRAY: Has the Inorganic Conference been overseas?

PARRY: No, but we've had a lot of overseas people come here for the conference.

THACKRAY: What differentiates whether a conference is overseas or not?

PARRY: Frequently, there is someone overseas who wants to organize a conference, so he will persuade someone in the United States to be a co-sponsor.

THACKRAY: Today's chair of the Inorganic Conference has to raise money, isn't that true?

PARRY: I don't think there's anything in the written documents that requires that. However, if he or she wants to present a program that will attract a sizeable audience, will present very good science, and will be well-evaluated by the conferees, I think he or she has to raise money. That is important.

THACKRAY: There seems to be—and this speaks to the academic side—a peer-review or peer-esteem aspect built into the conference.

PARRY: Yes, there is.

THACKRAY: One doesn't want to be chair and a failure.

PARRY: That's right. In fact, we've had a couple of chairs who didn't do anything until the end. Conferences had to be canceled and that was no good.

THACKRAY: But canceling a conference will not harm my industrial career quite as much.

PARRY: I suppose you are right. The two people that I know about who had canceled conferences were former industrial scientists, but I believe that they were in academia when they didn't produce a good conference.

[END OF TAPE, SIDE 4]

PARRY: I believe that if a man or woman—let's say a person from DuPont—did not put on a very good show, there could be some career problems. Fortunately, recent conferences with an industrial chairperson have been very successful.

THACKRAY: Over time, much has come to depend upon the internal traditions of particular conferences. From the board level, could you point to the central conferences, the great conferences? For example, is the Physical Organic Conference always a success?

PARRY: Anything having to do with organic chemistry is successful, because there are so darn many organic chemists. They vary by the year. Using chemistry to generate nanostructures is something which in the past few years has become hot because people are interested in making smaller and smaller devices. Non-Linear Optics and Lasers is also popular because that's an area being used. Organic Reactions and Processes always had a good-sized turnout because a lot of the chemistry is organic. Physical Organic has a good turnout. Stereochemistry has a good one. Biology and Chemistry of Tetrapyrroles may not be very interesting to some chemists, but is important to others. I have never been to a biology-orientated conference, so I don't know how many biologists come. That is a focus is outside of my expertise.

THACKRAY: I'm wondering about the internal ecology of the conferences. If you think by way of analogy with the university system there are three thousand colleges, but you'd better worry about the health of Harvard, Michigan, and Colby College. Can you make an analogy with the Gordon Conferences?

PARRY: The system is such that Harvard and Michigan are, and will remain, strong. The success of the Gordon Conferences must be carefully maintained, but they are not tied to the host schools for their quality. The system described earlier works.

THACKRAY: Are there a certain number of conferences that are extremely important?

PARRY: I think the most important one these days is Polymers. It has always been very big, as well as Coatings and Films. I think the conferences on Drug Carriers in Medicine and Biology is always a big draw. The fiber science area is important because it is very important to industry. Natural Products is always a big one; Organic Reactions is a big one also. Thin Films has become quite important in recent years because it covers crystal growth. There are two conferences today in crystal growth, because that is the name of the game right now in many fields. I don't know about conferences on staphylococcal diseases. That could have a limited group of people. I think Theoretical Biology and Biomathematics has a very limited attendance. There was a conference that used to be called Polymers. It may have been given a fancier name now, but it was chemistry involving olefins from petroleum. That would cover some of the polymer work. The Inorganic Conference has had its ups and downs. Two women ran the Inorganic Conference in successive years. One of them was from Michigan State at the time she was elected, and the other one was from DuPont. Both of these ladies developed programs which had to turn away crowds. They turned away forty or fifty people each. They couldn't take them—the conferences were oversubscribed!

THACKRAY: There is a limit of one hundred, plus or minus, correct?

PARRY: That's right, each conference will take about 120 to 130 participants.

THACKRAY: And if a conference only draws eighty participants, it becomes a candidate for scrutiny?

PARRY: That's right.

THACKRAY: Please say more about the scheduling and selection committee and the territory of defining boundaries.

PARRY: The scheduling and selection committee is a very important committee, which operates almost independently of the board. The board has authority over the committee in the normal sense. But the board is smart enough to know that when the committee selects top-flight

experts in various phases of the disciplines for review, to let the committee do its job. There are some biologists, some organic chemists, some inorganic chemists, some physical chemists, some M.D.'s, et cetera. It is a fairly large committee. They meet separately and make recommendations to the board. The board can always overrule the committee's recommendations, but it doesn't.

THACKRAY: The committee controls both the quality and subjects of the conferences?

PARRY: That's right. I can tell you what their tools are. They study, in detail, the reports that people turn in after a conference. If they receive enough reports stating that, "The presenters were arrogant, self-centered academics who we couldn't get anything out of," they may not schedule those presenters next year. If they are rescheduled, then they are very closely monitored.

DAEMMRICH: What if the committee gets a report stating that the presenters are a bunch of self-serving industrialists?

PARRY: If they got such a report, and it was clear that the industrialists had been indulging in self-serving activities, and if they were carrying their industrial pattern of limited communication at the conference, they would not be asked back. The conference organizers would try to get some other industrialists the following year that would be more communicative. They want industry, but not at such a high price. It is well-recognized that industrialists have definite limitations on what they can or cannot say. The scheduling and selection committee knows this very well, and, in my experience, acts wisely.

THACKRAY: Dr. Parry, you've been around. You know a thing or two. I'm a new assistant professor here at Utah and I'd love to end up as you did, as chair of the Gordon Research Conferences. What moves do you recommend I make to get myself there?

PARRY: First thing you do is go to a conference or two.

THACKRAY: All right. [laughter] I'll do that. Then what?

PARRY: Talk to people about research you are doing and have done. Publish on that research and do things that are attractive to your colleagues. I can give you the exact point at which I became a candidate for moving ahead in the Gordon Conferences. In my early days, I was intrigued by boron compounds. These seemed to defy conventional chemical rules. My

coworkers and I thought that they might be related to metal coordination compounds where boron replaced the metal. This suggestion was met with scorn and derision. Still, we had a lot of data which supported our belief. In 1958, Herb Brown organized a symposium on boron hydride chemistry and he asked me to take part in that San Francisco meeting (8). I accepted.

At that time, the inorganic division was interested in stimulating discussion of each paper. They had a commentator for each paper who had the responsibility of raising serious questions about the paper. Hopefully, the commentator and the author would get into a debate or fight, which the audience loved. The commentator for my paper was a man who had given me lots of trouble at my first Gordon Conference. His job was defined for him—he was to shoot me down. Those were the rules of the game. When the word got out that I was going to talk and that he was to be the commentator, people loved it. They wanted to see a fight! When I went to give my talk in a large hall in San Francisco, I couldn't get in the door.

I gave my talk, but this time I had all of the evidence I needed. I had the answers, unlike my first Gordon Conference appearance. After my talk, my commentator did what he was supposed to do. He said he couldn't believe this and he couldn't believe that, which were fair comments, and then he sat down. At that point, Professor H. I. Schlesinger arose and asked if he could make a comment. The answer, of course, was yes, but I trembled in my boots. He was respected throughout the world and I didn't know what he was about to say. He walked slowly to the front of the room, through all of the people. The place was silent. When he was about four feet from me, he extended his right hand and said, "Congratulations. That is beautiful work. You are right." After that, people listened. If I had not been able to convince people at that point, I would have been through.

Let me give you another example. Fred Basolo and Ralph Pearson got in a squabble with two very famous English chemists. The English chemists were top authorities in the field while Basolo and Pearson were younger upstarts.

DAEMMRICH: What did the controversy revolve around?

PARRY: Well, it revolved around a reaction mechanism. Basolo and Pearson said the mechanism went by one route and the English gentlemen said that the mechanism went by a completely different route. Remember, mechanisms are very hard to prove or disprove. The two sides exchanged veiled niceties in the literature for sometime. Finally, they confronted each other at a conference in England. This time, Basolo and Pearson had a definitive answer which proved that the English proposal was wrong. Like their American counterparts, the English researchers congratulated Basolo and Pearson when they saw their data. If Basolo and Pearson had been wrong, their careers would have taken a sudden nosedive.

THACKRAY: These cases that you're describing came before you were asked to be chair of the Gordon Conference?

PARRY: Correct, these events did take place before my chairmanship.

THACKRAY: You are telling me that I should be a star in my field, and go to the conferences.

PARRY: And you'd better be right! [laughter]

THACKRAY: I'd better be right. Then I'll become chair of a conference, and get on the council. But how do I going become chair of the board?

PARRY: You just have to con your colleagues on the board into thinking that you know what you're doing, even if you don't. [laughter]

THACKRAY: The universal principle.

PARRY: If you don't let them in on your level of incompetence! [laughter]

THACKRAY: Yes, because the conference has had a stellar list of board chairs.

PARRY: There are a lot of very fine people listed among the board chairs.

THACKRAY: Of course, some of the industry chairs were not necessarily as well known.

PARRY: They were just as good though. I think that Bob Cairns, Batchelder, Brown, Bueche, Coppoc, and John McCullough are people with credentials—just as good as any of the academic people.

THACKRAY: Yes.

PARRY: Dave [David W.] McCall, E. [Ernest] G. Jaworski, Kathy Taylor, and Francis [J.] DiSalvo are people that would stand up well in comparison with any of the academic people.

DAEMMRICH: It's striking to me, looking back at some of the board minutes, that you hit a point in the 1960s where people in the scientific community knew who you were and what you did. Do you want to be known beyond that? Do you want the general public to know who you are?

PARRY: You are perceptive to see that. It was in 1963 that the controversy over expansion came forth. Sides were taken, and it wasn't resolved until the early 1970s. Then in the late 1970s the conference was whole hog, full-speed ahead. We wanted to get others involved.

THACKRAY: Why the change?

PARRY: We instated a new director.

THACKRAY: All right. Parks was fairly comfortable with the size of the conferences.

PARRY: I didn't know Parks well enough to say he was comfortable. I think that Parks did not want to give up his element of control, which was raising money from industry. Now that may be an unfair statement to make, but to me, that was the basis for him not wanting to expand. Expansion would take the power of the operation out of his hands and distribute it to the chairs, whom he didn't have the ability to control.

THACKRAY: Which is essentially what Cruickshank did.

PARRY: Yes.

DAEMMRICH: How did you feel about the issue of expansion and the notion of more public awareness via a PR [public relations] campaign?

PARRY: Well, I've always felt that a PR campaign for science is crucial. We're suffering from the fact that people don't have the foggiest idea what we do and what our motives are. Many think that scientists are trying to fool them. I think we've sort of put that one to rest, but there are a lot of other issues that people are afraid of, such as radiation, and all that goes with it.

THACKRAY: Alec Jordan was with GRC when you arrived on the board.

PARRY: Jordan was the public relations man for the conferences.

THACKRAY: What was your view of Jordan and PR?

PARRY: My view centered, in part, on my view of colleague interactions. The trouble with Alec, for me, was that he was a very good friend of George Parks. His position on an issue was pretty closely tied to George's, as much as I could see, up until the time that George was convicted.

THACKRAY: Bob, can you say some words about the importance of the Gordon Conferences to science?

PARRY: I can tell you what the conferences have meant to me and to the science I've seen. I haven't been to a very large number of conferences across all the areas of science—I've been mostly to conferences involving three or four disciplines, one of them in particular.

THACKRAY: Inorganic. What other conferences have you been to?

PARRY: I went to one on the building of electronic devices and the development of materials for that. I went to a few biology conferences, which were so far out of my range of expertise that I was useless. I've been to an organic conference or two, and I can understand those. I thought that in these areas, which were not so completely foreign to me, the chairs were doing what they were supposed to do.

In Inorganic Chemistry there have been a few substandard conferences. A substandard conference is one where the topics are such that there's no controversy, there's no trying to thrash out where the truth lies. You can tell the difference. If people are jumping up and down, yelling to be heard, and arguing after the sessions in loud voices, you know that you've held a good conference. It's a good conference because people are not simply listening and trying to capitalize or take something back to the company or lab. Instead, attendees are trying to thrash out basic scientific issues. There are a lot of those people involved in science and without them, science suffers. Every conference isn't like that. I can't even tell you whether a majority of the conferences are like that or not, but from reading the reports of the scheduling and selection committee, I gather that a lot of them are. So, in that sense, I think the conferences do an important job.

If you go to an ACS meeting, you spend ten minutes talking to a presenter after he's given a talk and then you've got to run off and give a talk or something like that. You don't get to know him or his science. On the other hand, if you spend an afternoon in a boat, the presenter is in the front end and you're in the back, so you talk to each other about what you're interested in. More science is developed at the GRC than there is in the first case.

THACKRAY: Yes.

PARRY: That is the strength of the Gordon Conferences. They put you in a different vein. You're in a pleasant situation, unless you're eaten alive. You are in a position where you get to know the person as an individual. That advantage is illustrated by the story I told you about Denny Elliott. If we hadn't been playing tennis together because we liked each other, then that situation would never have happened.

THACKRAY: That's the most successful fundraiser of all.

PARRY: I just told Denny how sad I was that I couldn't bring the Europeans to the conference. And then I got his letter.

THACKRAY: I thank you very much. You have provided an excellent perspective on the Gordon Research Conferences.

[END OF TAPE, SIDE 6]

[END OF INTERVIEW]

NOTES

1. Weber State College became Weber State University in 1991.

2. The first Inorganic Chemistry Conference was held in 1951 at the New Hampton School in New Hampton, New Hampshire.

3. See for example: A. Stock, "Die Gefährlichkeit des Quecksilberdampfes," *Zeitschrift für Angewandte Chemie* 39 (1926):461-68; Ibid., "Die Gefährlichkeit des Quecksilbers und der Amalgam-Zahnfüllungen," *Medizinische Klinik* 24 (1928):1114-17, 1154-58; Ibid., "Die Gefährlichkeit des Quecksilberdampfes und der Amalgame," *Medizinische Klinik* 22 (1926): 1209-12, 1250-52; *Zeitschrift für Angewandte Chemie* 39 (1926):984-89.

4. Herbert C. Brown won the Nobel Prize in Chemistry in 1979. Retrieved from <http://www.nobel.se/> on 23 July 2004.

5. Henry M. Robert, *Robert's Rules of Order Revised for Deliberative Assemblies* (New York: Scott, Foresman and Company, 1915).

6. W. George Parks was born 30 December 1904 and died 9 October 1975. *American Men of Science: The Physical and Biological Sciences*, 11th edition (New York: R. R. Bowker Company, 1967), 4061. University of Rhode Island.

7. Records of the Gordon Research Conferences. Chemical Heritage Foundation, Philadelphia.

8. The Division of Inorganic Chemistry met at the 133rd National Meeting of the American Chemical Society in San Francisco, California in April 1958.

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