CHEMICAL HERITAGE FOUNDATION

FRED BASOLO

Transcript of an Interview
Conducted by

James J. Bohning

at

Northwestern University

on

1 March 1991

(With Subsequent Additions and Corrections)

THE CHEMICAL HERITAGE FOUNDATION
Oral History Program

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(Revised 17 March 1993)

Fred Basolo, interview by James J. Bohning at Northwestern University, Evanston, Illinois, 1 March 1991 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0091).

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

FRED BASOLO

1920 Born in Coello, Illinois on 11 February

Education

1940 B.Ed., Southern Illinois University
1942 M.S., inorganic chemistry, University of Illinois
1943 Ph.D., inorganic chemistry, University of Illinois
(Mentor: John C. Bailar, Jr.)

Professional Experience

1943-1946 Research chemist, Rohm and Haas

Northwestern University, chemistry department
1946-1948 Instructor
1948-1953 Assistant Professor
1953-1959 Associate Professor
1059-1980 Professor
1969-1972 Chairman of the Department
1980-1990 Charles E. and Emma H. Morrison Professor
1990- Emeritus Morrison Professor

Honors

1954-1955 Guggenheim Fellow (University of Copenhagen)
1961-1962 Senior NSF Fellow (University of Rome)
1964 Award for Research in Inorganic Chemistry, American Chemical Society
1971 North Regional Section Citation of Excellence, American Chemical Society
1972 John C. Bailar, Jr. Medal, University of Illinois (first recipient)
1974 Alumni Achievement Award, Southern Illinois University
1975 Award for Distinguished Service in Inorganic Chemistry, American Chemical Society
1976 Francis Patrick Dwyer Medal, University of New South Wales, Australia
1977 Fellow, American Association for the Advancement of Science
1977 Honorary Member, Phi Lambda Upsilon
1979 Fellow, Japanese Society for the Promotion of Science
1979 Member, National Academy of Sciences
1981 Honorary Member, Italian Chemical Society
1981 James Flack Norris Award for Outstanding Achievement in the Teaching of Chemistry, American Chemical Society, Northeastern Section

1982	Illinois House of Representatives Resolution No. 686 Honoring Fred Basolo as a Resident of Illinois
1983	President, American Chemical Society
1983	Oesper Memorial Award, American Chemical Society, Cincinnati Section
1983	Corresponding Member, Chemical Society of Peru
1983	Fellow, American Academy of Arts and Sciences
1985	Honorary Professor, Lanzhou University, People's Republic of China
1984	Doctor of Science (Honorary), University of Southern Illinois
1987	Foreign Member, Academia Nationale dei Lincei (National Academy of Science), Italy
1988	Laurea Honoris Causa, University of Turin
1988	IX Century Medal, Bologna University
1988	Award for Research in Inorganic Chemistry, Societá Chimica d'Italia
1988	Honorary Professor, Zhongshan University, People's Republic of China
1990	Harry and Carol Mosher Award, American Chemical Society, Santa Clara Valley Section
1991	Padova University Medal
1991	Distincion Bicentenaria Medal, University of Los Andes in Merida
1991	Chinese Chemical Society Medal
1992	Chemical Pioneer Award, American Institute of Chemists
1992	Monie A. Ferst Award, Sigma Xi
1992	Humboldt Senior U.S. Scientist Award
1993	Gold Medal Award, American Institute of Chemists

ABSTRACT

Fred Basolo begins this interview by discussing his childhood in Coello, Illinois, and his elementary and high school education. He attended Southern Illinois University where he studied to be a chemistry teacher but his instructors encouraged him to attend gradute school in chemistry. At University of Illinois, he studied inorganic chemistry with John Bailar. After receiving his Ph.D., he worked at Rohm and Haas in Philadelphia for three years. He decided to return to academia and accepted a positions as professor of Chemistry at Northwestern University. His research interests have included kinetics and mechanisms, and metal carbonyls. Basolo describes the connections he made with Italian scientists and his American Chemical Society presidency and concludes by offering his opinion of how general and inorganic chemistry coureses should be taught.

INTERVIEWER

James J. Bohning, Assistant Director for Oral History at the Chemical Heritage Foundation, holds the B.S., M.S., and Ph.D. degrees in chemistry. He was a member of the chemistry faculty at Wilkes University from 1959 until 1990, where he served as chair of the Chemistry Department for sixteen years, and chair of the Earth and Environmental Sciences Department for three years. He was Chair of the Division of the History of Chemistry of the American Chemical Society in 1986, and has been associated with the development and management of the Foundations's oral history program since 1985.

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INTERVIEWER: James J. Bohning

LOCATION: Northwestern University

DATE: 1 March 1991

BOHNING: I know you were born on February 11, 1920. Could you tell me something about your parents and your family background?

BASOLO: Well, my parents are ethnic Americans. Like everyone else, at the turn of the century they immigrated to the U. S. with very little formal schooling. I think they probably went through the third grade or so. My father was a coal miner in southern Illinois, and that's where I was born, in this little coal mining town [Coello, Illinois] which is sort of a disaster area at the moment. I grew up in this little community which was all foreign laborers. You could go all day without speaking English, and I practically learned how to speak this peasant dialect of Italian before I learned how to speak English. I learned how to speak English when I went to school. Other than that, I spoke what they called Piedmontese. My parents both became citizens and that was a big occasion for them.

My brother and sister were older. My brother was eleven years older; he's no longer alive. My sister is nine years older. They came along during the Depression when they really had to go to work and help out; they didn't even go to high school. In those days in such little mining towns a lot of people didn't go to high school. By the time I came along, of course, everyone went to high school. But not everyone went to college. I was fortunate in being able to go to college, to Southern Illinois Normal University, which at that time was just a teaching school. I got a Bachelor of Education degree.

BOHNING: You were nine years old when the Depression came.

BASOLO: That's right.

BOHNING: Did that have much of an effect on your family?

BASOLO: Well, it did. For all of these immigrants who had come over, it was a tough life. The immigrants in that little mining community managed to buy a little plot of land and they had their own one cow and one pig which they'd butcher every year, and their chickens. They supplemented their income with such things and they always did gardening. It wasn't easy; it was difficult, but these people came from conditions which were much worse than that, so for them it was still quite satisfactory.

BOHNING: What was your schooling like?

BASOLO: Elementary school was just a very small school in this little mining town. It was more than just a one-room schoolhouse, though. It was a building which had about four different rooms in it so that the first and second grades were together, and the third and fourth, and the like. That's no longer true at the moment. But you had to go to the neighboring town of 2,500 to go to the Christopher Community High School. In those days, there were practically no paved roads, and there were certainly no school buses. We had to drudge through the mud and snow and whatever else about a couple of miles or so to get there.

BOHNING: Did you have any teachers at that time who had any influence on you?

BASOLO: Yes, one in particular in high school. There was probably more than one—two or three. But the principal of our high school took an interest in the students. The high school was small enough so that he knew who the students were. He was very much involved in encouraging me to go on to college, because no one from this little mining town had even gone to college before. Not only had my brother and sister not even gone to high school, no one my age had ever gone to college. It was an effort on his part to at least convince me that this was the thing that I should do.

Fortunately, I was able to do this because Roosevelt and his PWA [Public Works Administration] programs had a youth program. I was able to go on the basis of the fact that the tuition for such a school—a state Normal school—was extremely small, practically nothing. This youth program that Roosevelt had set up paid twenty-five dollars a month. For that twenty-five dollars a month, I had to do work-study type things. I also worked in a dry goods store, and stayed in the attic apartment of the Dean of Women's house for taking care of it. In those days, you didn't have gas heat, you had coal. I was stoking her furnace and in the summer mowing her lawn. I even had money left over. We did light housekeeping up in the attic apartment and at the end of the month we'd have a little money left over and we'd go out and have a few beers. It was possible; we managed to do it.

BOHNING: Did you have any science exposure in high school?

BASOLO: Yes, and that's interesting. I'm one who has always pointed to the fact that you really need good science teachers who themselves are excited about the sciences and motivate youngsters into the sciences and into engineering. My chemistry teacher was an attractive blond gal. This was her first job. She majored in home economics, and home economics at that time required at least one year of chemistry, which the home economics majors took begrudgingly. She did likewise. [laughter] But when she got to this high school there was nobody there to teach chemistry. She at least had a year of general chemistry, I guess. So she was given the job, her first job among other things, to teach this chemistry course.

She was such a charming young lady and as honest as the day is long. She just simply admitted that she didn't know all that much chemistry. She did one thing that I think at least in my case had a very positive effect. She said, "Look, here's the laboratory manual. We're going to do our lab on Saturdays. Those of you who are interested in doing the lab part of the course, I will be here to supervise, but you will have to read the lab notebook and come prepared to do the experiments. I'll just be here to supervise and make sure you don't blow yourselves up." And that's what she did. It really gave us a chance to be slightly creative, at least to the extent that we had to read the directions and follow them on our own without being spoon-fed by somebody. I think that, at least in my case, was a stimulus in that direction.

BOHNING: What kind of experiments did you do?

BASOLO: It's a long time ago. I can't remember all that much, but I think that we were doing just the usual things. We were generating oxygen with potassium chlorate and manganese dioxide and collecting hydrogen and such things as that. But they had enough chemicals and enough glassware that you could do a few things. And the class was small enough.

BOHNING: What about physics and mathematics?

BASOLO: I had algebra in mathematics. I had no physics in high school, if I remember correctly. My first physics class was when I went to college.

BOHNING: What was Southern Illinois like when you arrived there? That would have been in 1936.

BASOLO: As I said, it was just a Normal school. People who went there, like myself, went there because that was really in their neighborhood in southern Illinois, and that was probably the only place they could afford to go. But they also went there knowing that when they finished they would teach. They would teach in elementary school or they would teach in high school. If you went two years, at that time, you would get a certificate. You wouldn't get a bachelor's degree. You'd get a certificate which would qualify you to teach in elementary school. If you had four years, you would get a bachelor's degree. Only one kind of bachelor's degree was offered then; that was a Bachelor of Education. You had to take all the education courses, practice teaching and so forth. At the end of that, many of my colleagues went into teaching. Not all, of course, but it was believed that that's where you would end up. Obviously, that's not where I ended up, but that's where I might have ended up.

BOHNING: You thought, then, that you would be a science teacher as you were progressing through this time?

BASOLO: Sure; at that time that's exactly right. I arrived there and this was big time for me, even though it wasn't all that big time. Fortunately, they had what we ended up calling the Four Horsemen. The pictures are up there on my office wall. Of these four faculty, one was an analytical inorganic chemist, one was a biochemist, one an organic chemist, and one a physical chemist. They were dedicated teachers. Professor [James W.] Neckers particularly, who was chairman of the department, took a real personal interest in all of us. I just automatically got put into chemistry when I was enrolled and advised what courses to take the very first year as a freshman. I did well, and with anybody who did well, people in chemistry immediately kept an eye on them. By my junior year or so, Neckers called me in and talked to me about going to graduate school. My first reaction was, what the hell is graduate school? [laughter] My parents are expecting me to get a job as a high school teacher and make a living. What's this with graduate school? So it came, really, not as a planned thing. It just happened.

BOHNING: Can you tell me something about the chemistry courses you had?

BASOLO: We had a very good coverage of chemistry at Southern at that time. Classes were small enough that our teachers were in there with us in the lab. The lectures were small enough that you could even ask questions. I think Neckers did the general inorganic chemistry. In those days it had a lot of inorganic reactions. He did the analytical as well. Professor [Talbert W.] Abbott did the organic chemistry. I'll always remember organic chemistry. In fact, not only will I remember, but I still have J. B. Conant's book right here (1). Every once in a while I reach for it, and right now I'm reaching for it and I don't see it but it's here. That was the book we used. Although I ended up being an inorganic chemist, I remember those lectures and I remember using that book. Physical chemistry was done by a fellow by the name of [Kenneth A.] Van Lente, who was also very dedicated and a very good physical chemist. He was a hard nosed individual and really drove you. We even had biochemistry, so it was a very good curriculum.

BOHNING: Do you remember the text in physical chemistry?

BASOLO: I think it was Getman and Daniels (2). Then, of course, I had practice teaching in chemistry. That was a disaster, because the old codger who was the high school teacher for whom we had to do practice teaching by that time knew less chemistry than we did, and we could see that we knew more chemistry than he did. Anyway, he certainly had a lot more experience in handling classes than we did, but that was a different kind of a ball game.

BOHNING: Did that help you make a decision about not going into high school teaching? How did you react to that experience?

BASOLO: When Neckers gave me this pep talk about going on to graduate school, by that time I had developed a lot of respect for the four of them. They all had Ph.D.s in chemistry and it just seemed to me that if he really thinks highly enough of me and I'm sufficiently interested, which I was by then, why stop and go into high school teaching? Why not take him up on this and apply for a teaching assistantship at various places. So I did and finally ended up at Illinois.

BOHNING: Did you do any kind of independent laboratory at Southern?

BASOLO: No. The closest thing, I guess, was when we took the biochemistry course. But even that was not independent. Scotty was always around and we were collecting our own urine and doing tests on it and so forth, but no, they didn't have independent undergraduate research in those days. Not there, anyway.

BOHNING: What about some of your other laboratory experiences? Did you do any inorganic laboratory work?

BASOLO: No, I think the inorganic laboratory work as an undergraduate was what we did in general chemistry. There was no separate inorganic course taught. There was a separate wet-type analytical course taught, where we had a semester at least, if I remember correctly, of titrations and gravimetric and those kinds of things. But that's it.

BOHNING: General chemistry was certainly a lot different than it is today.

BASOLO: That's right. Absolutely. [laughter] Except when I teach general chemistry. I try to teach reactions.

BOHNING: I want to come back to that later on. You've written some things about that.

BASOLO: That's right.

BOHNING: How many chemistry majors were there with you at Southern?

BASOLO: That was a particularly good year. In those days they were quite successful in having several chemistry majors who went on to do graduate work and got a Ph.D. Dick [Richard T.] Arnold, for example, is a very outstanding organic chemist who's a few years older than I am and has been retired now for some time. He was one of the people who we were always being told about. In my class there were a half a dozen of us. We all went to graduate school and we all ended up getting Ph.D.s. Just this past October or November, the Class of 1940 had its half-century reunion. [laughter] Some of us showed up there! We hadn't seen one another for ages, and it was kind of interesting. Not all six of us showed up, but three or four of us did. I guess I'm really the only one who went into academic work that people know about as well. But other people were very successful in industry, and they did very well.

BOHNING: Did others go to Illinois with you, or were you the only one?

BASOLO: As a matter of fact, there were three of us who went to Illinois. There was a guy who did organic chemistry, a guy who did physical chemistry, and I worked with John Bailar in inorganic chemistry. But I was the only one who took an academic position. These other people were extremely successful in industry. They became whatever you become—research directors and titles of one type or another, and ended up being very successful.

BOHNING: Did you apply anywhere else besides Illinois?

BASOLO: Yes, I think I did. I think I applied only to the Midwestern schools, and I think I got offers from all of them. Since I really hadn't been away from Southern Illinois, it was already a big, big jump for me to go to Champaign-Urbana. I can remember when I first arrived there and saw this huge campus with all these huge buildings, and I was really impressed.

BOHNING: What was it like in your first year at Illinois? How was your background from Southern in terms of how you could compete with the other entering graduate students?

BASOLO: There were three of us, and we had no real difficulty competing. We all took the required courses the entire first year. At the end of the first year, you had to take what was called a qualifying exam in each of the four areas, and none of us had any trouble getting through those. I guess I don't recall having any particular course difficulty.

There was one problem that I did have but it was solved in an unbelievable way. You were required to pass one of two language exams before you started your second year of graduate work. That was one of the rules. I had two years of German, but I didn't know all that much German. I never had any French, and usually you took German and French as the two languages. So I put it off until summer, thinking that during the summer I'd bone up on German and take the exam and hopefully pass it so I could start the second year of graduate work. I don't know what would have happened if I hadn't! [laughter]

But what happened is I took that German exam and it was terrible, because it had to do with making hydrogen peroxide electrolytically. My translation was so poor, and I knew it was poor, because the chemistry that I was writing down didn't make any sense. I was almost certain that I had failed. There was a week between then and when the French exam was to be held. I decided to sign up for the French exam and see if I could get ready for it. French is not the same by any means, but some of the words are similar to English and it isn't all that bad. It's not as bad as German, anyway. It's amazing. I boned up with [Maurice L.] Dolt's Chemical French (3), and learned as much as I could in one week. I took the French exam and felt better about the French exam in terms of the chemistry as it turned out in translation, thinking that if I pass I'll probably pass the French exam. A few weeks later the results were out and I'd passed both of them. [laughter] I had gotten rid of both of them. That was really the most frightening thing that happened.

The most comforting thing that happened when I arrived at Champaign-Urbana from a place like southern Illinois, never having been out of southern Illinois, was this big time stuff. These were big time professors; they were giants in organic chemistry. People like Roger Adams and Speed [Carl S.] Marvel. You name it, they were there. Reynold C. Fuson was also there, and he was no slouch.

Reynold C. Fuson taught the beginning course for graduate students in organic chemistry. One of his hobbies was to learn as much as he could about each of the graduate students in this course. Even before you had arrived, he would have gone through your file and saw who you were and where you were from. When you walked into his office to register for his course, he could start a conversation with you. In my case, because of my name—Basolo—and because of his interest in Italy, he immediately greeted me in Italian. (He'd been to Italy several times. There were even stories about his maybe having a mistress there. Whether that was true or not, I never did find out.) I spoke to him, and he said, "You should be able to speak Italian better than that." I said, "You're absolutely right; this is a peasant dialect." Even though I only had this one course with him, he was a bachelor and he had me over to his apartment. He would get out a good bottle of Italian wine and I would listen to some operas with him. He and I became very good friends and continued to be good friends until he passed away. It was very comforting to have somebody, one of the big shot professors, make you feel welcome. When I decided to work with Bailar, he was very outgoing and very pleasant, so I had no problem.

BOHNING: What about some of the other instructors that you had that first year?

BASOLO: Well, I had [Thomas E.] Phipps and [Worth H.] Rodebush in physical chemistry, and I think they turned me off more than they turned me on. Neither one of them were really that good as teachers. In any case, I think I had by that time pretty much made up my mind that I wanted to work with John Bailar and do inorganic chemistry, so that didn't do much.

BOHNING: Did you have Bailar in that first year?

BASOLO: Yes, I think I did. It's either that first year or the second year. I took the course from him in coordination chemistry, which is his specialty.

BOHNING: What was it that attracted you to Bailar and inorganic chemistry?

BASOLO: I really can't put my finger on it other than the fact that looking back, it might be Jim Neckers at Southern who was somebody I warmed up to and had a lot of respect for; he was an inorganic chemist. So I thought that maybe I'd like to be in the kind of chemistry that Neckers was in. That might be what kind of set it up to begin with. Then when I got to Illinois and started looking into it, inorganic was really second class citizenship. As I was saying, at Illinois the giants were all organic chemists. Although they had three hundred or so graduate students, only half a dozen or so were inorganic graduate students. They only had three or four inorganic faculty. Bailar was one, [Ludwig F.] Audrieth was one. [G. Therald] Moeller was another. When I talked to the inorganic faculty, Bailar was clearly the person I knew I would enjoy being associated with. So that turned me on.

BOHNING: When did you start your research?

BASOLO: I think it must have started in the middle part of the second year. The first year was really devoted almost entirely to course work and getting ready and passing those qualifying exams.

BOHNING: And you had a teaching assistantship at the same time.

BASOLO: I had a teaching assistantship at the same time, and then I got some kind of University fellowship. Maybe I started having research a little earlier, because I really did manage to finish in three years, in 1943.

BOHNING: The war started during the first year you were there.

BASOLO: That's right; that's exactly right. That was one of the reasons that the faculty were doing what they could to have people try to finish or get enough done that they could finish. In my case, that's exactly the way it worked out. I was able to get enough done and have an acceptable thesis and take a job. I had two job opportunities. One was at the University of Chicago on the Manhattan Project, where they couldn't tell me what they were doing. The other was at Rohm and Haas, which was on classified research, which they couldn't tell me what it was all about, but they could certainly tell me about their work on resins and everything else that they were doing. I just felt more comfortable because I knew a little bit about something that was going on at Rohm and Haas and I didn't know a thing about what was going on in Chicago. I guess by that time I was beginning to feel like I ought to get out of the state of Illinois for a little while, so I went to Philadelphia.

[END OF TAPE, SIDE 1]

BOHNING: Before we move to Philadelphia, what were the laboratory facilities like at Illinois when you were doing your Ph.D. work there?

BASOLO: By today's standards they were pretty primitive. OSHA [Occupational Health and Safety Administration] would never approve anything that was going on there! [laughter] There was no such thing as the kind of ventilation that you're supposed to have. The kind of sophistication that was available to you to do this kind of chemistry was minimal. Infrared wasn't being used all that much. There was no NMR [nuclear magnetic resonance]. X-ray refractometers were not in existence. In the research that I was doing, and that most people were doing in terms of synthesis and reactions, it was just glassware, the classical organic type that was being used. In my case, it was aqueous chemistry so it was solution chemistry. The compounds were not air-sensitive or anything else. You just used ordinary flasks and beakers.

BOHNING: Did you measure any absorption spectra at Illinois, or was that still too early? You did a lot of it when you came to Northwestern.

BASOLO: No, that was just at the beginning of measuring absorption spectra. I did do some. Just a few spectra of platinum complexes. That's right. But it was sort of a primitive kind of thing.

BOHNING: This year is the fiftieth anniversary of the Beckman DU, which would have been 1941. Did you have a DU? Is that what you were using?

BASOLO: I can't even remember the instrument that we used, and I used it in conjunction with somebody else who was actually doing spectroscopic kind of things. I was not all that knowledgeable about what kind of instrument it was. I don't remember it being a DU, though. My first instrument that I used when I got here was a DU. [laughter]

BOHNING: That's why I asked that question. If you weren't using a DU, what were you using?

BASOLO: I can't recall what predated the DU, but there must have been something at Illinois that some of the physical chemists were using and getting UV-visible spectra.

BOHNING: G. E., for example, put out an instrument that was developed by [A. C.] Hardy. Would your measurements have been in the ultraviolet? Had you gone down that far?

BASOLO: I would have certainly gone in the near ultraviolet, because these platinum compounds are virtually colorless.

BOHNING: There wasn't much available that was simplified before the DU.

BASOLO: No, that's it. I don't think this was, and I think it was the sort of thing where you know your fellow graduate students and you say, "Look, I'd like to have a spectrum of this thing." You go down and bring your solution with you and hang around. My thesis, I'm sure, has two or three spectra in it. But it probably doesn't tell you anything about the instrument. [laughter]

BOHNING: I've noticed that in many papers at that time that frequently authors will say, "We thank so and so for the measurements." That's where it stops, so you get no idea of how the measurements were made. I've been interested in that part of the history of spectroscopy, before the DU.

BASOLO: That's interesting now that you mention it.

BOHNING: It's very frustrating to look at a paper and see "We thank so and so for the measurements" with no indication of how they were made. [laughter]

BASOLO: Maybe that's what is in my thesis, as far as I know. [laughter]

BOHNING: Theoretical inorganic chemistry was certainly in its infancy at that time. How theoretical was your training at Illinois?

BASOLO: Mine was not. Mine was not at all, because of the direction that I went in. I also had to take at least one or two courses outside of the department. Being an inorganic chemist, I actually took ceramics. Some of the people at Illinois as physical chemists would take math or things like that, but I didn't. As far as the theory was concerned, we just simply used seat of the pants kind of valence bond theory to explain structures, magnetic properties and so forth as best we could of these [Alfred] Werner complexes. In those days, that was what you had. In retrospect, that wasn't all that we had, but coordination chemists didn't learn about crystal field theory until later. All of the physicists knew about crystal field theory at the same time that [Linus] Pauling's famous bond theory appeared and even before that. But since chemists were not reading physics journals [laughter] they were not aware of crystal field theory until the early 1950s.

BOHNING: Did Bailar assign the problem you worked on, or did you select it from a list of things?

BASOLO: Bailar suggested that there were two or three things that he was interested in and allowed me to select the one that I thought I would be most interested in. In fact, I think there were two. We ended up sort of doing what we could with one of the problems and it didn't seem to be going anywhere. Then we backed off and worked on the other one. That one did result in a couple of publications.

BOHNING: Yes. You have a paper in 1950 that was based on your thesis, but there was a woman on that paper as well (4).

BASOLO: Yes. That was Betty Rapp Tarr. That's right. She predated me. We overlapped just a little bit, but not all that much, just enough. I think she was in her last year as a graduate student when I was just getting started. But the work that she had done had to be carried a little further before it would be publishable, and that's what I did.

BOHNING: What did she go on to do and were there many other women at Illinois at that time?

BASOLO: No, there weren't many women, and I don't know that I've ever heard any more about Betty Rapp Tarr after she left. I really don't know if she went into industry or just what she did.

BOHNING: What kind of person was Bailar to work for?

BASOLO: I think he was superb. He's just the type of person I think that I would want to work for. He was interested in the work so he would always be available. In those days he was not traveling all that much, so he was there much of the time. When he was there he would always drop by the lab and inquire as to what was going on. He wouldn't necessarily be dropping by the lab to check up on how much you'd accomplished and what you were going to do next and so forth. He would just come by to interact socially and professionally and give you an opportunity to ask questions and to tell him what you've done and what you're planning to do. It was an ideal arrangement.

BOHNING: Was there much war-related work going on at the time?

BASOLO: Yes, there was. Particularly as summer jobs. In those days, we had TAs [teaching assistantships], and that was an academic year thing. The salary was sixty-five dollars a month. But during the summer you could take a summer job. During the war, some of that was government-sponsored research. Marvel had all this synthetic rubber work going on. Audrieth and the inorganic chemists had fluorophosphonates and nerve gases and all kinds of nasty things going on.

BOHNING: Did you work on some of that?

BASOLO: I did not. I was able to continue with my research; I think I got a University fellowship or something like that, so that whatever it was, I did not do any of the war-related work.

BOHNING: What was your reaction when you got to the big city of Philadelphia in 1943?

BASOLO: That also was big time for me, anyway. I worked at this research lab of theirs in one of the suburbs called Bridesburg, which is right along the river. It's a pretty ethnic Polish place, with all the taverns and everything else. It was and still is, I guess, pretty much a slum manufacturing area. I wasn't married and there were other people like myself, just really waiting for the war to be over. You know, that was a war that we supported. It wasn't easy for a person my age to be a civilian. My father was dying of cancer and on two different occasions I was called home. I would go down to North Philly and get on the train and take the train to Chicago. Those trains were wall to wall people, and people my age were all in uniform. You'd be in the train sitting next to somebody whose husband had already been killed or son had been murdered or had someone out in Guadalcanal or some such place. So it was an uncomfortable thing. Some of my colleagues actually went in and volunteered and got an officer's rating. I was tempted once or twice, but I finally decided that they must know what they want me to do. I would get greetings to go in for a physical. I would pass the physical, wait to be called as a GI and then at the last minute they'd defer me again. We'd go through this again and again and eventually the war was over. So I never did go to war.

BOHNING: Was the company behind those deferments?

BASOLO: I think it was the fact that the company was doing war research; it was classified and the work that I was on might have been important. We succeeded in doing what we were asked to do. We were supposed to make synthetic mica. We actually succeeded in making a material which could be used as synthetic mica. It never turned out to be commercialized, because the source of natural mica was always available in the U.S. so they didn't have to rely on this. But they could have, if they had to rely on it. So there were those kinds of things, and I guess the deferment would come because of whatever the priority rating of such research was.

BOHNING: Who was your boss at Rohm and Haas?

BASOLO: It was a guy by the name of Loren C. Hurd, who had gotten his degree at Wisconsin. I can't remember who he worked with at Wisconsin, but he was the boss. I don't know what ever happened to him.

BOHNING: Were there many people in the group you were in?

BASOLO: There were two other Ph.D.s from MIT in our lab, and the other people were non-degree people or bachelor's degree assistants of one type or another. The lab probably had eight or ten people but only three of us, plus Hurd, had Ph.D.s.

BOHNING: Did that mica project take the whole three years that you were there?

BASOLO: No. They were in zirconia opacifiers, so they had a lot of zirconium minerals. They were trying to make zirconium derivatives of one type or another for water-proofing and fire-proofing fabric. I did at some stage get asked to work on the synthesis of some zirconium compounds that might be of interest, so I spent some time doing that as well.

BOHNING: As the war was coming to an end, had you thought about what you wanted to do when the war was over? Were you going to stay in industry?

BASOLO: I think I probably knew even when I went there that when it was all over I would want to have an academic job of some sort. I guess it came up through going to the normal university thinking even in terms of high school teaching. I had taken such a liking to Neckers and John Bailar that I thought it would be nice to be in a position akin to what they were doing. So for the three years I was there, actually, it was really a good time for me to think in terms of an academic position and what I might do when this was all over. Although my work with John Bailar was on syntheses, reactions, and stereochemistry of metal complexes, I did have a chance while at Rohm and Haas to go to the library and read some of what the physical organic chemists were doing in terms of their physical organic chemistry—solvolysis, kinetics and mechanisms, S_N1 and S_N2. It just seemed logical to me that the same kinds of things could be done with some of the compounds that I was familiar with. So I really had a pretty good idea of what I wanted to do the day I arrived here. Perhaps that was the reason that things took off quickly. We were able to tackle this problem. It was fortunate that Ralph Pearson was here and after a year or two I was able to seduce him into working with me on metal complexes, rather than on physical organic chemistry.

BOHNING: What kind of things were you reading? Were you reading [Louis P.] Hammett's book (5)?

BASOLO: Yes, that kind of thing, sure.

BOHNING: As an inorganic chemist, why did you pick that kind of book to read?

BASOLO: I can't recall exactly, except that it was a book that seemed to be something about organic chemistry that wasn't just about the Diels-Alder reaction. At Illinois in those days, I remember the end of the first year when we had to take a qualifying exam in each of the four areas. The organic chemistry was being taught in those days not in terms of mechanisms, but being taught in terms of name reactions. I had made index cards of name reactions with the name on one side and the reaction on the opposite side. I must have had a bunch of them that were some inches thick. In preparing for this qualifying exam at the end of the first year, I kept going through these cards, and as soon as the stack that I got correct was bigger than the stack that I got wrong, I decided I was ready for the exam. [laughter]

I think that when I looked at Hammett's book and I looked at some review papers, it looked like there were people doing things other than that kind of organic chemistry, and it struck my fancy. Particularly the solvolysis-type reactions were pretty straightforward and the kinds of things that I could appreciate. It just kind of triggered that kind of interest.

BOHNING: You left Rohm and Haas in 1946. What were the circumstances surrounding that move? Were you in a position to leave after the war was over, did you go out searching for a position, or were they terminating employment at the end of the war?

BASOLO: No, I don't think they were terminating employment. I think we could have stayed and some people did stay. But a few of us who really wanted academic positions did leave. In my case, as in my colleagues' cases who were there and also left, at this point the GIs were coming back and they were all going to the universities. The universities had such enrollments that they had to get out and get more faculty. So there were an abundance of faculty positions available. I think I applied to a few places.

The one that I really looked at seriously was this one, because by that time I felt it would be nice to get back to the state of Illinois again. I didn't know all that much about Northwestern. When I came here, there was really no inorganic chemist other than P. W. [Pierce Wilson] Selwood. He was an inorganic chemist who had been trained by B. S. [B. Smith] Hopkins at Illinois. Selwood was doing heterogeneous catalysis and that kind of chemistry. The fellow who was the inorganic chemist here had just accepted a job at the University of Colorado-Boulder. The junior/senior level course that he had been teaching in inorganic chemistry was something that they needed a faculty person for. It sounded exciting to me to have an opportunity to step in and teach that course. In those days there was not even a textbook of inorganic chemistry. Moeller had not yet written his book (6).

BOHNING: I used Moeller. I remember that book.

BASOLO: I made up my own notes, using [H. J.] Emeléus and [J. S.] Anderson (7) and several other books that were around and did the best I could. I enjoyed teaching that course. I think that course was also very helpful in getting graduate students here to go into inorganic chemistry.

BOHNING: Was it required of them?

BASOLO: It was a course that was taught at the undergraduate level, and it was taught in the fall quarter. We had students coming as we always do, from all sorts of places, and from schools where they did not in those days have such a course. They just didn't have an inorganic course at the junior/senior level. Inorganic was just general chemistry. So those students were really required to take this course, or they did take this course. In this course, as a result of their taking it when they first arrived, I was able to brainwash a few of them and get them into inorganic chemistry.

BOHNING: What was the situation in the department? How large was it and what kind of facilities did they have?

BASOLO: The department was certainly smaller than it is now, but not all that much smaller in terms of the number of faculty. In terms of university enrollment on this campus, it wasn't all that much different than it is now. But in terms of the number of graduate students, it was smaller. In terms of inorganic graduate students, it was zero. The situation that I found at that time was that at least in my particular case, I did have access to a Beckman DU spectrophotometer. By then we were doing absorption spectra and following spectral changes in order to follow these reactions. We had a conductivity bridge that we could also use to follow these reactions. We had a polarimeter that we could use (if the organic chemists weren't using it) if we had some optically active compounds that we wanted to monitor. We had practically no funds, because there was nothing other than the Research Corporation. I think I got a Research Corporation grant after a year or two. Most of our people were on TAs, though we don't have that many TAs. So you couldn't support large numbers of graduate students.

BOHNING: Who were the stars of the department of the time?

BASOLO: Charles Hurd was the senior person, and very well known in organic chemistry and pyrolysis. The very active group was Herman Pines and Vladimir Ipatieff and their catalysis lab, not only because they were doing good work but also because of the fact that catalysis was important in industry. In the universities, no place other than Northwestern was really doing heterogeneous catalysis in the same sense that it was being done here. Most of that kind of work in those days was being done in industry. Practically no work of that type was done in universities. Northwestern has had this long association with heterogeneous catalysis. Now, a lot of universities are doing it.

In physical chemistry the guy who was really well known and became even better known was Malcolm Dole. He was a fabulous person. He did a variety of things in terms of his research and the glass electrode. He even wrote a book on the subject (8). He did electrochemistry and wrote a book on the subject (9). He worked on oxygen isotopes and got our standard changed from oxygen to carbon-12. Finally, even within polymer chemistry, he did radiation of polymers and branching and calorimetry of some of these things. So those were the people here who were the most productive in terms of research.

BOHNING: I think Ipatieff died in the early 1950s. Did you have any contact with him?

BASOLO: Practically none. His English never really was all that good, and his research interest was different from mine. I would see him and knew that this was the Russian professor, but we really didn't have much contact.

BOHNING: Who was chairman of the department?

BASOLO: It was a fellow by the name of [Robert K.] Summerbell when I arrived. Just before that it was a guy by the name of Ward Evans. Ward Evans was a physical chemist, and in fact he was the mentor of Ralph Pearson. Pearson did his thesis work here with Ward Evans. Then Ward Evans left and went to Loyola in Chicago. I think it was because he'd reached the magic age of retirement, so he went there and Summerbell became chairman.

Ward Evans' claim to fame was not his physical chemistry so much. His claim to fame was that he was on the Oppenheimer tribunal. He wrote the minority report that they should not strip Oppenheimer of all his classified things, and that he should still have access to all of this. That was his big achievement.

BOHNING: Was [Frank C.] Whitmore his predecessor?

BASOLO: Yes, Whitmore was prior to that. That's really before I got here.

BOHNING: Did you teach anything else besides this junior/senior level course when you first came here?

BASOLO: General chemistry. I didn't start right out giving lectures in it. Summerbell, who was an organic chemist and chairman of the department, liked to teach general chemistry. He took on most of the bulk of the teaching. The rest of us were sort of his assistants and would take the quiz classes. We weren't using graduate students in those days. One reason was that Summerbell thought that graduate students were not capable of doing this and doing it as well as he would like. He kept pointing out that we are a private institution charging these large tuitions and competing with state schools; at least we ought to give students a little more for their money than when they go to a state school. [laughter] We ought to have regular faculty doing the quiz classes. During his tenure as chairman, that's exactly what was done.

[END OF TAPE, SIDE 2]

BOHNING: How long did it take before you got your first graduate student?

BASOLO: My first graduate student was Bill [William S.] Castor. I think it was a couple years. [retrieves document] Let's see what it says here. I got here in 1946. It says here that he arrived here in 1946 and left in 1950. I probably got him a year after I was here. He was my very first graduate student. He probably started doing his research in 1947.

BOHNING: Did he take your course? Is that how you were getting yourself introduced?

BASOLO: That's exactly right. He did some zirconium chemistry because at that time I had just come from Rohm and Haas. I'd been doing this zirconium work there and there were a couple of things that I thought might be of interest to him. I myself was doing the kinetics and mechanisms. I was in the lab with my own two hands for my first two or three papers. I was the single author.

BOHNING: Yes, your first paper was in 1948 (10). That was even before your Ph.D. thesis was published.

BASOLO: I think that's right. It took a little while to get all this stuff done with Betty Rapp Tarr and Bailar.

BOHNING: Another paper in which you were the single author was in 1950 (11). So at that point, in addition to the zirconium work you were still looking at synthetic work, but your interests were more in mechanisms.

BASOLO: That was right at the beginning of when I was getting into doing solution kinetics and mechanisms and getting Ralph Pearson to come on board.

BOHNING: I was going to ask how you seduced him.

BASOLO: [laughter] In those days the department had hired about half a dozen of us coming in as beginning instructors. Three or four of us survived; the rest didn't survive and went elsewhere. But when we arrived, some of us started having lunch together. We met in the office of Carroll [L. Carroll] King, who was an organic chemist. In an office of this size or even smaller, when you get three or four people in there, it's pretty full. But we would bring our lunch and we'd sit and talk about whatever topic happened to come up. It didn't necessarily have to be chemistry. It could be football or basketball. It could be anything—baseball, the Cubs, or what have you. But chemistry was often times the topic of conversation as well. On those occasions, day in and day out, if we came in with something that was puzzling us or something we were thinking about in terms of our research and about to do something, we might bring that up and talk about it.

So little by little, after trying to convince Ralph and his listening to me telling him what was happening, he finally came around to deciding, yes, there are things here that can be done and this is what we should do and how we should do it in terms of kinetics and mechanisms. I could always come up with the complexes that should be used. We should use this for the following reason or we should use that. In those days, I could still go in the lab and actually make these compounds. In fact, even when Harry [B. Gray] was here, who is our most notorious graduate, Harry was busy working with me and Ralph doing solution kinetics and mechanisms of platinum compounds. These were platinum compounds that I had actually made and knew how to make. I told Harry how to do it and gave him the recipes. You know, the first or second time you try to do something you don't get the yields of a person who has done it several times. Harry would get discouraged, and finally I said, "Okay, just let me make them and you can run the kinetics."

It was a slow process before Ralph really got totally into it. He always maintained a certain interest separate from this, but this began to take up the bulk of his time. In fact, not only that, but with the outside chemistry community he finally became recognized as a physical inorganic chemist. His ACS award is in inorganic chemistry, not in physical chemistry. So he really did make a complete change.

BOHNING: As a student, my recognition of him was through Frost and Pearson.

BASOLO: Sure, the kinetics and mechanism book (12).

BOHNING: I never at that point would have thought about his association with inorganic chemistry.

BASOLO: Sure.

BOHNING: When you started doing this work, this was pretty new in terms of the rest of the world. With regard to [Sir Christopher K.] Ingold, I know there's some disagreement with Ingold's work. [laughter] We can talk about that if you like.

BASOLO: Okay.

BOHNING: Did that occur early on?

BASOLO: Yes, and fortunately it turned out to be quite a turning point as far as Ralph and I were both concerned. Ralph and I were just beginning investigators in this area of physical inorganic. Ingold was also just beginning in physical inorganic. That's why things happened the way they did. He was really one of the very best people as far as physical organic chemistry was concerned. But Ron Nyholm, who later became knighted, Sir Ronald Nyholm, joined the department there at the University College-London and started talking to Ingold about his metal complexes.

He showed Ingold some of our initial papers, particularly the base hydrolysis reactions where this all happened. The reaction that takes place with cobalt amines is first order in cobalt complex and first order in the hydroxide ion. However, hydroxide ion seems to be specific. It turns out that there are other nucleophiles that you might use but the compound doesn't respond this way. So Ralph and I came up with the acid-base pre-equilibrium type mechanism with the conjugate base formed undergoing a dissociative reaction (S_N1CB), which all fit the second order kinetics. This mechanism was consistent with all the facts and it made more sense to us and that's what we published.

When this was pointed out to Ingold, I guess by Nyholm and people at University College London, he looked at it and thought, "Those two guys don't know what they're doing. This is just an ordinary second order displacement type reaction, just as with alkyl halides and the hydroxide ion." So he came out very loud and clear and strong on this. It took some doing. As I have always told students, it is impossible to prove a mechanism is right. A mechanism is just like any other theory. It's a theory and it's just not possible to prove it's right. But it <u>is</u> possible to prove that a mechanism is wrong. So then it was up to us to do what needed to be done, to prove that the mechanism that Ingold had proposed is not correct, which fortunately we were able to do (13). If it had turned out the other way around, you probably never would have heard of Basolo and Pearson! [laughter] And Northwestern would never have become famous in inorganic chemistry.

It was kind of amusing because all of this was happening in the late 1940s and early 1950s when I had gone on a couple of ACS [American Chemical Society] lecture tours. I remember going out to the Pacific northwest on lecture tours and people would come to my lectures and I would be all excited about what we were doing. At the end of it all somebody would ask me, "What is a chelate? What is a ligand?" [laughter] It really was a little depressing. But about six or eight months later, I went on a similar lecture tour out east someplace and the attendance was better. I could see that organic chemists were there in addition to inorganic chemists. But the organic chemists were there because they had read Ingold's paper. They wanted to find out what the hell was going on and what Pearson and I were up to and why Ingold was even bothering to take the time of day to pay attention to us. So they would come and it was an entirely different audience, because they were physical organic chemists who would ask the questions that needed to be asked. As it turned out, of course, we were eventually able to show that the mechanism that Ingold had proposed was not correct. That went a long way in terms of helping both Ralph and myself.

BOHNING: How did Ingold react to that?

BASOLO: Like the English gentleman that he was. I remember back in 1961 or 1962 (it was almost over by then but it was still going on), I was a guest of his department there with Ron Nyholm and Jack Lewis and all of the guys who I knew. I gave a talk and afterwards I met with him in his office. We had just done what we thought was a very definitive experiment in dimethylsulfoxide, not in water. We were able to show that this preacid-base equilibrium hybrid reaction was responsible for the first order dependence on hydroxide. When I discussed this in one or two of my slides in the talk, the first thing Ingold wanted to point out to me was, "But Fred, dimethylsulfoxide is not water. It doesn't say anything about what happens in water." Fortunately, Henry Taube was doing this kind of chemistry. Taube was still at Chicago at that time and Henry and I would keep in touch. I knew that Henry Taube had done analogous experiments in water using oxygen-18 labeling. The results were consistent with the results that we had gotten in dimethylsulfoxide. So I was able to tell

Ingold about this and he polished it off.

But I found out later that Ingold was notorious for doing this kind of thing among physical organic chemists. There had been times in physical organic chemistry where he had not been right. But he would never admit in print that he was wrong, and he never did admit in person that he was wrong either. He just simply looked the other way and went on to something else. [Martin L.] Tobe has published several papers where he uses the conjugate base mechanism, which is the one that we proposed.

BOHNING: As you began this work, how did the rest of the inorganic chemists respond to what you were doing? The Basolo and Pearson book didn't come out until 1958 (14). How did the rest of the inorganic chemists respond?

BASOLO: First of all, there weren't that many inorganic chemists in those days. There were some, and there were beginning to be more and more because a lot of people had gone on the Manhattan Project and had become interested in transition metals and actinides and lanthanide-type metals. So there were more and more inorganic chemists, but not all that many. Our papers were being read, finally. In the early days when I worked with Bailar and not long after, you'd publish a paper and never hear any reprint requests or anything else. But there was a cadre of physical inorganic chemists out there such as Taube and others who had worked on the Manhattan Project who really were interested in solution chemistry kinetics and equilibrium mechanisms; they were reading our papers.

I think the first symposium on the subject of mechanisms of inorganic reactions was held at the University of Chicago and was set up by Henry Taube. I presented a paper there and later on we had a symposium here. I think it was 1958, just about the time our book came out. We must have had about sixty or seventy people who attended that symposium on mechanisms. So all of a sudden it took hold. The book, of course, was a winner from the day it arrived because it was the first such book and particularly it was the first such book that not only had mechanisms in it, but likewise had crystal field theory and made use of crystal field theory in trying to rationalize some of the results. The timing was just absolutely perfect.

BOHNING: How did you pick up crystal field theory?

BASOLO: I'd never even heard of it until I went to Copenhagen in 1954 and 1955 on a year's leave of absence. I went there with professor Jannik Bjerrum. Jannik Bjerrum is well known by coordination chemists. He did his thesis on the measurement of stepwise stability constants of copper ammonium complexes. This marked the beginning of the many measurements of stability constants of metal complexes. His father was Niels Bjerrum, who was even more famous. He almost got the Nobel Prize, and there are stories about why he didn't. When I got there in Jannik Bjerrum's lab, he had people like Carl Ballhausen, who was outstanding in theory and crystal field theory; [C.] Klixbull Jorgensen, likewise; and Claus Schäffer. He had four or five people, just young assistants, in his lab at that time. What these people were doing was crystal field theory, which I hadn't even heard of. Also what they had that I didn't have was the number 1 Carey spectrophotometer which was not gray, it was black, actually. The first ones they made were black. Later they made them gray.

The reason they had their instrument is, in Copenhagen at that time, the Carlsburg (beer) family had finally reached only one member who hadn't married and didn't have any financial responsibilities but was interested in the arts and sciences. He set up the Carlsburg Foundation for the support of arts and sciences in Denmark. As you go around the museums and any place in Denmark, you usually see "Courtesy of Carlsburg Foundation". They had bought themselves the very first Carey spectrophotometer. There was Klixbull Jorgensen—he was really a terror in the lab—taking the spectra of almost all of the aqua transition metal complexes. Every spectrum that he took was another paper interpreted in terms of crystal field theory, because the valence bond theory—which I grew up on—could not even begin to interpret those spectra. But crystal field theory did it very well.

The Danish people at universities certainly handled English quite well, but their written English needed some polishing. So almost every Monday, almost every week, Klixbull particularly, and Ballhausen, but mostly Klixbull, would come in with another manuscript which he would give to me to help him put it in better English. So I got my exposure to crystal field theory this way. I immediately let Ralph know, because he was more physical and more mathematical than I am. He looked into the crystal field theory, and by the time I came back he had looked into it sufficiently and he was all excited about it.

So the book was written in part by me while I was on leave of absence, and when I got back I again had to convince Ralph that he should join me as a co-author and that we should complete the book. He was less than enthusiastic until he had read a few of the chapters that I had written. Then he got excited, and he saw how he could fill in the crystal field aspects of all of this and really bring it all together. So he jumped in with both feet and finished the book. Together we wrote the book. The first book was easy to write because at that time, the kind of work that was going on of this type—kinetics and mechanism—either we were doing it, or Taube or a few other labs and we knew exactly what everybody was doing and what the status of it was. We didn't have to conduct a large literature search. We pretty much knew everything that needed to be known. So we sat down and wrote the book as we did without all too much effort.

Later, when we revised it in 1967 (15), it was much more of an effort because by that time the whole field had grown by leaps and bounds. You had to be selective of what example you would use to illustrate whatever principle you wanted to illustrate. Subsequently, even now, people are still banging on our door wanting us to revise it one more time because it's gone out of print. In many countries, they still hustle up enough copies to use it. They'd prefer to use it to some of the more recent books that have been written on the subject. I can understand it, because it covers the area in sufficient detail, with comprehensive coverage. The ones that are written now are written by someone whose interest is specifically bioinorganic chemistry or specifically homogeneous catalysis or something else. So our book is still being used, although it's out of print.

BOHNING: Did you have a commitment from the publisher before you started?

BASOLO: No, it was not written with any intent of making money or that it would make money. It sold like hotcakes and it did make money, but not huge sums of money because it was never written as a textbook, although it was used at the graduate level as a book by many people. I gave the course in coordination chemistry and I never really required that it be used, although I put it on reserve at the library and told people that the book exists. Some of them took advantage by having a look at it.

BOHNING: You mentioned that it was easy the first time because most of the work was being done by you and Taube and others. Was there any work going on in Europe, or was it mostly in this country?

BASOLO: Mostly in the U.S. In fact, I have a story that I always tell when I give a talk on carbon monoxide substitutions of metal carbonyls and substituted metal carbonyls, about how we got into that. When I was in Copenhagen 1954/1955, in May my wife Mary and I left the two kids with a babysitter and people in the neighborhood there who we'd gotten to know by then and we took off. We'd never been to the continent before. One of the first stops was at Amsterdam, where I attended an international conference on coordination chemistry. One of the plenary lectures was given by the late Walter Hieber. Walter Hieber did not discover metal carbonyls, but he's really known as the father of metal carbonyl chemistry because [Ludwig C.] Mond, who did discover metal carbonyls, was in industry and he never published much about it. He didn't continue the work, except he did discover it and he did patent the discovery.

Walter Hieber had an institute at the Technical University in Münich with never less than forty or fifty people working, and they were all working on the synthesis and reactions of metal carbonyls. He was a bachelor; he was just wed to metal carbonyl. chemistry. So in this plenary lecture he had slide after slide after slide that he showed of the work that they had done. He had given this in German because he didn't know English, but you could tell from the slides that he had what was done. When it was all over with I complimented him in English (I don't know any German) and said, "Gee, this is all very nice. Can you tell us a little bit about how some of these reactions take place?" This was asked of him through an interpreter.

He said, "Young man," (in 1955 I was a lot younger) "in my institute we do real chemistry. We don't do the philosophy of chemistry." [laughter] What he really said was, "We're interested in reactions and synthesis and we don't give a damn about theories of bonding or mechanisms of reaction." Well, naturally, at my age at that time, being involved in research with the classical Werner complexes, I just couldn't wait until I got back to Northwestern to start talking to graduate students. "Look, there's all these damn compounds that have different kinds of coordination numbers. They have bridging COs which might react differently from terminal COs. Anything you do, as long as you do it carefully and get good data, we'll be able to explain one way or another. It would be original work which will be publishable and certainly will be acceptable for a Ph.D. thesis. You can't miss. It's not an open ended thing; it really has to pay off."

Well, it was a little while before I could sell them, because everybody would talk to me and I would also have problems of the normal type that was going on in my lab at the time, and they would go off and talk to graduate students and other people in the lab and come back to me and say, "That metal carbonyl problem sounds exciting but I'd just as soon work on such and such a problem like Joe's doing or like somebody else is doing in the lab."

Finally, Andy [Andrew] Wojcicki showed up. He's now a professor at Ohio State. Andy decided, "Okay, let's have a go at it." So he started this, and things worked very well. He was a gregarious guy and he could sell other graduate students on the project. So then we had all kinds of people, including Bob [Robert J.] Angelici, who's now a Professor at Iowa State University. In fact, the group that was here at that time is a group I have a picture of here in my office, with Harry Gray, Angelici, [John L.] Burmeister, etc. Wojcicki is the only one missing. He had just left a couple of months before. I regret that he's not in the picture, because he is the person who got us started with the metal carbonyls. That was the height of my research activity in those days, with good students, and the number of research people that I had was as big as it has ever gotten.

BOHNING: Was the reluctance on the graduate students' part because of the potential hazardous nature of the materials?

BASOLO: I'm sure that was part of it. The compounds we were dealing with were just ordinary compounds that were water soluble and not air sensitive. They could be handled freely. Whereas, if you mention carbon monoxide and metal carbonyls, people would have to be more careful. They're air sensitive and toxic. That was one of the reasons, because in those days we didn't have the ventilation that we have now. We did the best we could. One of the first things that Harry Gray and Andy Wojcicki did when they started working with such compounds is that they bought a canary. [laughter] They had a canary in the lab which they named Linus, [laughter] after Linus Pauling. The canary finally died of old age. Then they bought a parakeet. The parakeet apparently nipped at Harry Gray's finger one day and later he ended up noticing that he had temperatures every day. It turned out to be psitacosis that he got from this parakeet. [laughter] But that's the way we protected ourselves, with the canary.

[END OF TAPE, SIDE 3]

BOHNING: Let's go back for a moment to your sabbatical in Denmark, because you did publish two papers out of that work (16). You've already told me about how much absorption spectra was being measured there.

BASOLO: That's right.

BOHNING: Did you introduce mechanisms when you were there?

BASOLO: Oh, yes. In fact, one of the papers that I published there was with Arthur Adamson.

BOHNING: That was my next question.

BASOLO: Adamson and his family happened to be there just at the same time, unbeknown to me. Dave [David N.] Hume, who's an analytical chemist at MIT, and his family also happened to be there. So the three of us showed up with Jannick Bjerrum that same year for a year's leave of absence. It was kind of nice, particularly for the spouses and families that they got together and could keep in touch. Dave did some writing but no experimental work. Arthur and I did experimental work and we were in the same laboratory that was used by S. M. [Sophus Mads] Jorgenson, who was really the pioneer of making these metal complexes that now we refer to as Werner complexes.

BOHNING: He's in the dedication of your book (14).

BASOLO: Yes, he's in the dedication of the book. If you're interested in seeing some of his compounds, I have samples I took from the lab of compounds he made that are now a century old. All that's changed now. They have a brand new chemistry building. But Arthur and I were together in that particular lab. As a physical chemist, Arthur was particularly interested in electron transfer type reactions that Henry Taube was doing. But he also pitched in and did a piece of work with me in connection with this conjugate base mechanism.

One thing that we needed to know was that the hydrogen deuterium exchange would be fast enough to permit the proposed S_N1CB mechanism to be valid. The way to do that was to do the hydrogen deuterium exchange using heavy water. In those days, techniques of monitoring this were pretty primitive, but there was a technique that was used at the Carlsburg laboratory at the brewery. The brewery was a real cosmopolitan place, knowing they make the best beer in the world there is no need to do beer research. At that time, [Kai Ulrik] Linderstrom-Lang, who was a very famous protein chemist, was the laboratory director. He had devised a gradient technique to measure H/D ratios of water, because of hydrogen/deuterium exchange being done on some biological studies. It was a gravity type technique, where one put a little bead in the water and depending how far down it went in the gradient, you could get the hydrogen deuterium content.

We asked whether we could come and use his setup. He said, "Oh, you just come; give me a couple of days notice." So we had all our solutions and compounds ready to go and went over. When we got there, he had his white lab coat on with his assistant, who was there with her white lab coat on. They wouldn't let us touch the apparatus. [laughter] They said, "Oh no, it's too much trouble. We'd have to teach you how to do it. You give us the solutions and we'll do the actual experiments. So here was the director of this fancy lab, an internationally known guy in bioinorganic physical chemistry, Linderstrom-Lang, who was—for that day at least—our assistant making the measurements. He was also a delightful person who had numerous stories to tell. He was telling jokes all day and had a terrific sense of humor. So Arthur and I did publish that paper jointly, which was a definitive paper. Somebody had to do that experiment, and we did it.

BOHNING: How did you decide on going there?

BASOLO: Well, as a coordination chemist I wanted to go someplace. At that time it had become known that people were measuring stepwise stability constants of metal complexes and that this was becoming a fashionable thing to do in this country. [W. Conrad] Fernelius, for example, at Penn State at that time had good people who had a bunch of pH meters and were making such measurements. The person who I knew from reading the literature and the person who had really done the seminal work on this was Jannick Bjerrum. I just decided that here was a coordination chemist who has done something that seemed to be very timely and significant. Through correspondence with him I arranged to go there. I had no other reason, and it turned out to be a very delightful year.

Even before we went I guess an announcement that I'd gotten the Guggenheim Fellowship showed up in the campus paper. One of the graduate students came around and said, "Look, my girlfriend lives in Copenhagen and if you need any help with housing, I can write to her." He did, and she put an ad in the Politicken, the Copenhagen daily paper. Not long afterwards, we had her arrange to rent a house for us. It was a nice small house in a nice residential area built right after the war, with people our age, with children of similar age as our two children, who were six and four at the time. The husband and wife who owned the house were a crystallographer and a physicist at the university. They were going to be on leave for a year at MIT and at Harvard. They were delighted to have us show up just about the time that they left, so they left all the linens and all the dishes and everything in their house. The neighbors were waiting for us with open arms, because they knew enough English and they wanted to be able to practice their English on somebody from America living in their neighborhood. So they were there waiting for us with tea and goodies. It was a delightful year, particularly for my wife, because later we went to Italy for a year and it was an entirely different ball game. [laughter]

BOHNING: You talked earlier about this group of graduate students that you had that was so productive, and which you assembled shortly after this year in Copenhagen. I wondered whether we could talk a little more about some of them. Harry Gray, of course, being as you said—I think the word you used was the most "notorious".

BASOLO: [laughter] That's the way it turned out. He's certainly done exceedingly well and he's been very kind to Northwestern and to me and Ralph.

BOHNING: What was he like as a graduate student?

BASOLO: I think he was no different as a graduate student. He arrived here from Western Kentucky [State College]. He wasn't as polished as he is now and wasn't as mature, by any means. We used to tease him about not even having shoes and coming here in his bare feet. But he was a very dynamic graduate student. He got involved not only in his own research but through talking to other graduate students he would sometimes get involved in some things that they were doing as well. Not necessarily just in my group and Pearson's groups, but for example, Carroll King, an organic chemist, was making some sulfur compounds and the guy who was doing this was a friend of Harry's. Harry said, "We ought to make some metal complexes with sulfur as the ligand atom. They made metal complexes and they published a paper (17). I guess King's name was on it, but certainly our names were not.

Harry was that kind of a guy. He was a real winner. He didn't like to lose. He liked to keep things moving ahead. In fact, it was interesting because it was just at the time that Joe [Joseph] Chatt, who was an outstanding organometallic chemist (who in my opinion should have shared the Nobel Prize with [Ernest O.] Fischer and [Geoffrey] Wilkinson), had a person by the name of Bernard Shaw, who's done real well on the faculty at Leeds, as a graduate student. I had Harry Gray as a graduate student. Chatt and Shaw were making some platinum organometallic compounds, and Harry Gray was measuring their kinetics and substitution reactions by conductivity methods. At one of these international conferences on coordination chemistry, Chatt and I got together on this and I said, "If you send us the compounds, we'll do the experiments." He said, "Okay," and when he got back he got Bernard Shaw to bottle up some of these samples and send them. Harry would do the experiments and we'd send the results back and ask for more compounds. I didn't realize what was happening and I'm sure it was just Chatt's way of making a good story. He was invited to contribute a general article (18) to the hundredth volume of the Journal of Organometallic Chemistry, in which he pointed out that Bernard Shaw couldn't make the compounds fast enough and was saying, "Oh, how fast these guys are doing the kinetics." Harry Gray was saying, "Gee, I can't keep up with this guy Shaw who's making these compounds so fast." [laughter] Chatt was explaining how he conned them into thinking that they had to work harder to make more compounds or work faster to get more kinetics done because the compounds are coming. As a result, we published jointly a long paper on the trans effect (19).

Angelici was very good. He was not Harry Gary; there was only one Harry Gray. Andy Wojcicki was outstanding. It was just at a time when an awful lot of very good people showed up here wanting to do graduate work in inorganic chemistry. That's understandable, because if you think back, even at that time there weren't all that many departments that had exciting things going research-wise in inorganic chemistry. Inorganic chemistry, in most departments, even in some of the very better departments, was largely just teaching general chemistry. There were only a dozen or fewer departments doing research in inorganic chemistry, and we happened to be among those. In fact, probably at that point, with the research that Ralph and I were doing and our book, our department ranked near the top of departments doing research in organic chemistry. So we really did have access to good graduate students in inorganic chemistry. We still do. The whole precedence that was established then in inorganic still pertains and we continue to have one of the strongest departments in inorganic chemistry.

But now, of course, there's lots of good inorganic chemistry going on in a lot of different places, so we're competing with a lot more places. But that wasn't true then. It was also true that these people who came at that time, if they were interested in an academic position, they had multiple offers at very good places, because in those days when the universities were expanding and had an opportunity to have an additional spot, they would look at their existing faculty. They had quite a few organic chemists, quite a few physical, maybe an analytical or two thrown in, but a lot of places didn't have a single inorganic chemist. So they were all really writing to us like mad wanting to hire inorganic chemists. Fortunately, we had some and we had some good ones, and they turned out to go out and do well for themselves and as a result also reflect well on the department, naturally.

BOHNING: But isn't it that you created your own competition?

BASOLO: Well, that's right. [laughter] But we've managed to keep up. I'm pleased to say that the department and the university here has had enough foresight to allow the inorganic group on different occasions to really make a strong enough pitch for the fact that we've got a good strong thing and let's keep it competitive. I think even now, we are certainly having to compete with a lot more places, but our department is still right among the better departments. For example, in 1969/1970 when I was asked to do my tour of duty as a chairman—at that time there was a five-year rotating chairmanship—I said, "Oh, God, I can't do it for five years. I may not enjoy it, or I may not do it well. Let's go for three years." They reluctantly agreed and I said, "If I do it for three and I do it well and you people want me, maybe we can do it for three more. That will be six instead of five." So we went along on that.

The other demand that I had before I took it was, "Look, we still get a lot of inorganic chemists (this was in 1969) and if I'm going to be full-time chairman I'm really not going to have all that much time to take on additional students and take care of them. So we really have to have a young inorganic chemist as an additional spot." The department, of course, agreed. They didn't hesitate. But then it was time to convince the College of Arts and Sciences at the university that they should add an additional spot, because in some cases they were not even taking care of attrition. But they finally did. I said, "It's either that or I don't take on the chairmanship. We do get inorganic chemistry students and I feel obligated to make sure that that's covered before I do anything else."

It turned out that spot came along rather late in the year, so I made a bunch of phone calls. One of the inorganic chemists I called was [F.] Albert Cotton, who was still at MIT. He said, "Oh, God, I've got just the guy. He's done so much already he could leave tomorrow, but he doesn't want to get his degree right away because of the Vietnam War. As soon as you get your degree you're susceptible to the military draft, and if you guys are flexible and can hang in there for awhile, then it would work out." We were flexible and we hired Tobin Marks. Tobin Marks is just a world leader. He is here now and has done such a fabulous amount of inorganic chemistry and was recently elected to the National Academy of Sciences. So we went out and got [James A.] Ibers from Brookhaven and Marks, and of course we had [Duward F.] Shriver. We've always been able to have good people and maintain our strength. If anything, our strength has even improved. It's improved, but it's improved along with improvements in departments at other universities so it's certainly more competitive now.

BOHNING: Coming back to that group again, you had mentioned that Harry Gray came from Western Kentucky. Did the others come from small college undergraduate backgrounds?

BASOLO: Andy Wojcicki came from Brown University, which wasn't a small school. Angelici came from Saint Olaf [College]. Burmeister came from F & M [Franklin and Marshall College]. As is true of all places, we get an awful lot of our people from good liberal arts colleges. As you well know, they do a damn sight better job of teaching than we do at the universities. They really get people motivated in the direction of chemistry and take a personal, professional interest in making sure that these people are qualified and interested and want to go on and have an opportunity to go on. That's where these people were from. It's always a bit of a mixture, but by and large it's from liberal arts colleges. We have so many of them here in the Midwest.

BOHNING: I'm jumping ahead here, but how does your undergraduate chemistry major population compare to your graduate student population? A number of large universities don't produce that many undergraduate majors.

BASOLO: We're no different from anyplace else. I think that we are suffering much like anyplace else. In fact, at the moment it's almost a little embarrassing. As I tell my colleagues—they don't like to hear me say this—it is absolutely true and it's even more so at big state schools, but at our place we start out with about eight hundred or so freshmen taking either an honors course or whatever, taking beginning chemistry. We're lucky to end up with four or five—it's a good year if we have six—chem majors going on to do graduate work in chemistry. You can end up with only two or three. As I point out to them, "Damn it, if you start out with eight hundred and if you even just intentionally try to turn people off, turn them away from chemistry, you couldn't be more efficient and effective than we are. And here we are telling ourselves that we're just trying to do the opposite, we're trying to get them all fired up to go into chemistry." I don't know; we all must be doing something wrong. It's not just Northwestern, it's across the country in the universities. Liberal arts colleges are still doing what needs to be done and doing it well.

BOHNING: You made the comment earlier in your own experience that you certainly had the attention of the four horsemen.

BASOLO: That's right.

BOHNING: They cared about you, I'm sure, a lot more than anyone here could begin to.

BASOLO: Absolutely.

BOHNING: That's where the motivation comes from, as you said. I had the same experience.

BASOLO: I'm sure of it. At the moment, graduate programs in science and engineering would really be scraping the bottom of the barrel if it weren't for foreign students. In our department we still have an awful lot of nonforeign students. But we do have several Asians and several other foreign students. But if you look at schools of engineering, the number of foreign students is beginning to exceed fifty percent in some of their departments. Even some of the faculty are beginning to be foreign rather than Americans. It's talked about a lot, and there are all sorts of things that are said, but it really hasn't been possible to turn this around. At the moment, across the country fewer American undergraduates choose to do graduate work towards a Ph.D. in chemistry.

BOHNING: In a little bit I do want to get to your term as ACS president and a lot of things that happened in those years, because you had some very strong views on certain subjects.

BASOLO: [laughter] Okay.

BOHNING: Let's move ahead a little bit through my notes. Your work continued on the carbonyls for a long time with a number of people, but then you also made an Italian connection somewhere along the line. How did that come about?

BASOLO: Well, that came about because my name ends in 'o', [laughter] for one reason. I've always felt that connection. As I said, I learned how to speak this peasant dialect of Italian before I learned how to speak English. I lived in this little mining community where you could go day in and day out and all the people there were foreigners and many of them were Italians.

In terms of chemistry, there were two or three Italians who were publishing in coordination chemistry, the kind of chemistry that I was doing. One in particular was Professor Luigi Sacconi. He was not professor at the time, and his first professorship was actually at Palermo. I could tell you a lot of stories about Sacconi, but I won't bore you with the details. In any case, his English wasn't that good. On this one occasion in 1955, when Mary and I drove around Europe, we went to Italy. I still have second and third cousins there, and we visited the cousins in northern Italy. My Italian was perfect. I could speak Piedmontese with them [laughter] and they understood me and I understood them.

But then we took off and we went to the universities. In Milan, [Lamberto] Malatesta was an outstanding inorganic chemist. There was [Aldo] Turco at Padua and Sacconi at Florence and [Vincenzo] Caglioti at Rome. So we purposely went to these places as tourists and they didn't know I was coming. When we got there I'd get on the phone and call them up. If they were in town, they would immediately drop everything and come and look after me.

The one thing that Sacconi really wanted in 1955 to warm up to me for was that he wanted to publish his papers in English journals, like The Journal of the Chemical Society in London or the Journal of American Chemical Society. He was fed up with the Gazzetta chimica Italiana, the Italian journal that nobody would read. He said, "I do all this good work and put it in the Gazzetta and nobody reads it." [laughter] So he wanted to publish in English. He conned me into helping him write these papers in English. He would send me a manuscript and I would edit it, not changing the chemistry but changing the words and putting it in better English. He would submit these papers and he got an awful lot of visibility because he started publishing his papers in English journals. That was really the thing that established the Italian connection, because he was very influential.

But the big power in inorganic chemistry back in the 1960s was Vincenzo Caglioti, who was really the "God Father" of inorganic chemistry in Italy. Even Sacconi and Malatesta and all of the other outstanding faculty would come there and he would dole out whatever monies from the Italian NSF equivalent, Concilio Nacionale de Research. Together, they would decide on who should be promoted to professorships and who would not. Caglioti was chairman of this group of inorganic chemists. On one occasion when Sacconi set up a lecture tour around Italy for me, Caglioti felt it probably should have been he who had done this. Caglioti didn't speak any English, but my Italian was good enough, so we could communicate. He said to me confidentially, "The next time you go on leave I want you to come here."

He made such a nice offer that we did go there in 1961 and 1962. By that time we had two additional children. That's when we lived in the suburbs of Rome. He had arranged for us to stay at this big villa in Montesacro outside of Rome. But there were no English speaking people there, even in the shops, so Mary and the kids had a little bit of a rough time. I used to enjoy it because my Italian was good enough and I could at least communicate. People recognized right away that I wasn't an Italian, but we could converse. But then Caglioti, because of our being there all year and my knowing a lot of coordination chemists who he had invited to come specially because I was there during the year, saw to it that I got into the Accademia Nationale dei Lincei, which is the oldest Academy of Science in existence, founded by Galileo. Likewise, in Turin—Turin is the northern part of Italy—they were doing some carbonyl work at the same time we were and they knew about our work and I visited them and we exchanged ideas. A few years ago they gave me an honorary degree, so they've been more than kind. I have all these invitations to go to Italy and I never turn them down. [laughter]

[END OF TAPE, SIDE 4]

BOHNING: Weren't the Italians the early people to do inorganic photochemistry?

BASOLO: Yes.

BOHNING: You only have two papers on it (20).

BASOLO: [G.] Ciamician in Milan did organophotochemistry and some inorganic as well with some cyanide complexes. But it was mostly organophotochemistry, by putting things out in the sunlight.

BOHNING: But later on the big move into the photochemistry of inorganic materials began.

BASOLO: I really never got involved.

BOHNING: Why?

BASOLO: I don't know. Arthur Adamson was the guy who really did that. [Vittorio] Carassiti in Italy was the guy who started it there. In our case, I think by that time we were well into the metal carbonyl stuff and we were just doing thermal substitutions. Although these things are known to be photochemically active with very high quantum yields, we didn't get involved with the photochemistry. There was no real reason that I know of why we didn't. Things were going so well without getting involved in photochemistry [laughter], so we didn't bother. It was being taken care of very nicely by other people like Harry Gray and Mark Wrighton. Mark Wrighton at Penn State wrote a book on the photochemistry of organometallic compounds (21).

The little bit that we did was because we stumbled on some chemistry that came along as a result of the discovery of dinitrogen as a ligand. That was discovered in the mid-1960s by [Albert D.] Allen and [C. V.] Senoff (22) with the ruthenium compound. If you think of dinitrogen, it is isoelectronic with carbon monoxide. Even when Andy Wojcicki was here and we were doing metal carbonyl chemistry, we put some dinitrogen and dicobaltoctacarbonyl in one of Herman Pines's bombs and revved up the pressure of dinitrogen, hoping to make a dinitrogen compound. I'm pretty pedestrian in my theory. I just count electrons. If it's isoelectronic, it ought to have some tendency to react. But it didn't.

Later, when it was discovered that N_2 can behave as a ligand, everybody jumped on the bandwagon. Taube, us, everybody wanted to really exploit this as much as possible. Particularly the bioinorganic chemists wanting to understand nitrogenase and how it works, and the inorganic chemists hopefully wanting to activate the nitrogen-nitrogen bond. I don't know whether they were hoping to compete with the Haber process or not. We decided we knew how to make some $M-N_2$ compounds using azido compounds of iridium and rhodium, which we got to work nicely to give nitrene derivatives. In the process of doing this, it was noticed that even in a solid state the white crystals of these compounds would take on a gray appearance. Clearly, we immediately began to suspect that they were photochemically active because the azido groups can be photolyzed to give off dinitrogen. So that's how we ended up doing a little bit of photochemistry just as it pertained to that system, which we didn't continue. We did publish a few papers on this research.

BOHNING: Was there anyone here doing photochemistry at the time?

BASOLO: I think [Robert L.] Letsinger had done some photochemistry with some organic compounds. Fred [Frederick D.] Lewis had arrived on the scene as an assistant or associate professor at the time that we were doing it. So we had some people to turn to. But more than that, I was fortunate in Arthur Adamson sending me a postdoc by the name of [Harry D.] Gafney, who is now at City University [of New York]. Harry was the guy who was able to do this photochemistry for us and do it properly and take care of the one graduate student that I had who was working on it.

BOHNING: One of the things that I've noticed, and I commented to Dr. [Charles D.] Hurd yesterday about the same thing, that his work was varied since he had a number of different collaborators from the department faculty. In some respects you've done the same thing because you had a paper going way back with Gordon Barrow (23). Hurd attributed some of that to just the interaction of the faculty within the Northwestern department.

BASOLO: Clearly that's true, and you may see some of it as we have lunch today, because that's been going on and it just grew. It's not done by intent, it just happened. It happened really when we all arrived at the same time and started eating lunch together. But as a result of that Pearson and I started collaborating right from the beginning, having joint students and joint projects and applying for joint grants. Our names would be on grants as joint PIs [principal investigator]. That kind of environment which grew within the department has now extended outside the department and it's very prevalent. There's a lot of interdisciplinary research going on now.

We have this catalysis group with Wolfgang Sachtler as the Ipatieff Professor. We managed to get him a new building just adjacent to this annex; it's a nice laboratory. But in that laboratory he has not only chemists (he himself is a chemist), but he has chemical engineers, he has material scientists, he even has a physicist or two thrown in. It's a very interdisciplinary group. People doing solid state work end up wanting to examine electrical properties, so there's collaborative work with people in electrical engineering. So that is something that's kind of unique at Northwestern that was going on even in the days of Charles Hurd and certainly in the days when we were getting started.

I think it's just because the size of the department is small. It's not like a big state school where inorganic chemistry is autonomous from organic and everything else. From the very beginning, we were small enough that we could interact, and we do interact as a department. We have departmental meetings, the chairman is the chairman, not a head, and we decide policy on the basis of a democratic approach. You have your yeas and nays, and if the yeas exceed the nays then that's the way it is. A beginning assistant professor who has just arrived on the scene and doesn't even know where the bathroom is, at such a meeting his vote counts exactly the same as my vote, and I've been here forty years. That's fine, that's perfectly all right. We do, of course, have tenured people meeting separately from non-tenured people. So the department has always had that kind of thing. I think it began when a lot of beginning instructors were brought in at the same time and began to interact with one another.

BOHNING: You were away from the carbonyl work for a long time but you got back to it.

BASOLO: Yes, that's right. So much research now is driven by funding. When I first arrived here there was no place to go for funding, so you didn't have to worry about it. But that's no longer the case. Later, I was getting NIH support because of the fact that we were doing solution chemistry of metal complexes. So I would write a research proposal to indicate what we were doing in terms of our studies, and then in one or two sentences only, I would indicate that this fundamental understanding of these kinds of principles and all of this just has to be of importance to people who are looking at biological systems, whether they be metalloenzymes or metalloproteins, and so forth. And that was enough. NIH would support it. Not only would they support it, they did support it for about twenty years.

I was on an NIH panel years ago. Taube and I happened to be on it at the same time, just at the time that Senator Mansfield proposed a bill in Congress that the federal agencies should not fund research unless it was closely and directly related to their mission. So it was clear to me that the writing was on the wall. The next time around the chances of getting money from NIH were going to certainly diminish because our research was not that close enough to health. Because the NIH mission is health, what they really wanted was something that would cure a disease or at least would be much closer to being health-related.

I had always had in mind something that was of interest to me but I had just never gotten around to suggesting it to anybody or getting anything started on it. That was synthetic oxygen carriers. Years ago, Melvin Calvin had done a lot of this kind of work. But his work was solid-state gas-phase type interaction. That was supported during World War II because the Office of Naval Research had money for this purpose of fixing dioxygen. In submarine warfare, they had to come to the surface to collect oxygen. When the enemy comes you can go down and release the oxygen. It's been known for a long time that there are certain kinds of cobalt chelates that are capable of doing this. After the war, Calvin published a lot of papers on these cobalt complexes that he and his research group had investigated.

Well, I decided we should do some solution chemistry on these systems. It would not be all that different from what we were doing. We would do kinetics and mechanisms of oxygen uptake and oxygen release. My first student on this project was Al [Alvin L.] Crumbliss, who's now a professor of chemistry at Duke University. One reason I wanted to work on this problem, of course, was that then I could write a proposal to NIH and say, "Look, this is now quite closely related to things of interest to the Institute of Heart and Lung." A lot of the old codgers on the Hill were dying of heart attacks and lung cancer, so I was pretty sure that this would probably have a much better chance of flying than earlier work on metal complexes. Sure enough, it did fly and they supported it.

By that time, we had added two or three other inorganic faculty so I decided to devote all my time to just the synthetic oxygen work and stop the metal carbonyl work. Then I became president of ACS, and it was time to write a renewal proposal for the oxygen work. I did it, but I could have done it better had I really taken the time to do my homework properly. So I got a priority rating that was not fundable. It was high enough that they encouraged me to resubmit, but I decided to hell with it. I'm far enough along that I'm not even going to bother to fight my way to that extent. I stuck with just doing carbonyl substitution.

By that time there were several other people around here. Bill [William C.] Trogler was across the hall as an assistant professor and he and I used to talk about things of mutual interest. The inorganic group here has a very interesting, unique thing. Every Saturday morning we get together informally and have a chalk talk kind of research conference. We do have a seminar, but on Saturday two or three students or postdoctorates tell us briefly about research in progress. It's something Pearson and I started many years ago. Saturday mornings when I'm in town, I'm always there. The name of the game, which is good for the students and postdocs, is that they have to present what they want without slides or transparencies and they have to be able to respond to questions and comments on their feet. So there are a lot of interruptions.

We ask a lot of questions and make a lot of suggestions. In fact, I have a little plaque that was given to me at Basolo 70, which comes as a result of this. [A quarter is mounted at the top of the plaque. The transcription reads, "Presented to Professor Fred Basolo on the occasion of his 70th birthday to remind him of all the times that he was right."] The student who did this is now a professor, Steven H. Strauss, at Colorado State University. He was not a student of mine but he was a student of Shriver's. He was doing some work and he was suggesting that such and such was going to happen. I said, "No, that's not the way it'll be. I'll bet you a quarter." My limit is always a quarter. [laughter] Over the years, I've lost a few quarters, but by and large I've won most of them. The reason is, the students don't realize what they're doing, and they're usually specific about what's going to happen. Usually my bet is that it isn't what's going to happen. I don't limit myself to the fact that <u>this</u> is what's going to happen, but something else other than what they suggest is what's going to happen. Usually I have in mind what's going to happen, but my chance of winning is much better than theirs, because in order for them to win, they would have to have specifically that one thing that they say is going to happen, happen. Anyway, the kids know that Basolo is going to ask a lot of questions on Saturday morning at these sessions.

BOHNING: I remember that I found that to be the most valuable experience I had as a graduate student.

BASOLO: Giving seminars?

BOHNING: Yes, and knowing you had to be responsible for what you were saying and be prepared.

BASOLO: Absolutely. I think our students are very prepared when they leave here, whether it be industry or what. We don't try to embarrass them; we just try to engage them in conversation and challenge them a little bit.

BOHNING: Yes, when I was at Valparaiso we had to start giving seminars in our junior year as an undergraduate. We had four full semesters. When I got to graduate school, giving a seminar in front of my colleagues was an old experience.

BASOLO: An old hat, sure.

BOHNING: Whereas my peers had never given one in their life.

BASOLO: It makes a difference, absolutely, yes.

BOHNING: Why don't we move into your reluctant candidacy as president of the ACS. [laughter]

BASOLO: Okay.

BOHNING: I guess I could use that term correctly.

BASOLO: Well, whatever.

BOHNING: I mean, it was not something that you sought out specifically.

BASOLO: Well, that's true. I had never been really active at the national level in the American Chemical Society, or not even at the local level, but I had been chairman of the inorganic division of the American Chemical Society, so that is at the national level. I'd always expressed my views when I had views and I'd always have some views [laughter], and they're usually not mainstream views.

On this one occasion there were two candidates who had been nominated for president-elect. There were some people out there who didn't want either one of them to win, for whatever the reason. I had two or three phone calls from people who I had a lot of respect for saying, "If you're serious about some of the comments I've heard you make and if you really want to be a help, why don't you let us petition you for president-elect?" I said, "Oh my God, I don't know that I want to get involved to <u>that</u> extent." It's a lot easier to stand on the sidelines and make comments than it is to grab the ball and start running with it. But I said, "Let me think about it over the weekend and you can get back to me." Eventually they talked me into it. I said, "I'm not going to raise a finger to get myself elected but if you think it's possible and want to go ahead, I'll go ahead." And they did, and I did get elected without a second election.

So I came on the scene as a novice in terms of understanding the bureaucracy in the ACS, and understanding the good-old-boy type attitude. There are guys who have been working at the national level, and it's their professional life. Some of them spend all their time doing it and back home they do very little. But there they do a lot and they do it well. The only thing that bothered me was that they do it so well that they insist on being reappointed and reappointed, and it takes up a spot that other people cannot get. One thing that they won't do is step aside and let somebody else in. I've never known any organization where you can perpetuate yourself forever except the ACS. Other appointments that you have are term appointments. They might be five years with a renewal for five, but then it's understood that after that, that's it. This was one of things that I wanted to do in the ACS. I didn't care how long; they could decide how long, whether ten years was the right number of total years or whether twelve or fifteen. But <u>some</u> kind of total years, because the first thing that you have to do when you become president of the ACS, the first thing they ask you to do, is to reappoint people on these various committees.

I immediately got into hot water because there were three or four people there that I just wouldn't appoint, because I knew there were some people that I thought should really be on some committees like publication, or whatever it was. So I didn't appoint them. Then these people couldn't understand why they weren't appointed, because they'd done such a good job for twenty or thirty years, first one committee and then another and then another. I was told to go before a committee on committees to explain my position; it was unbelievable, like going into a lion's den. These committees are so huge because everybody gets their way paid to the meeting; they make these committees as big as you could possible finance. There must have been fifteen or twenty members in this committee on committees. When I walked in there to explain where I was coming from and why I was saying this, I could look and I could see all these people, all of them, had already been there twenty or so years, and they knew they'd done a good job, and wanted to continue. They didn't want to stop, and that was the whole point of it. It was kind of a useless exercise on my part.

My idea was we should have a by-law which would limit the total number of years, and that would then make it easier for the president-elect who is doing the appointing, because people would understand that after a certain ten or twelve years or however much, that's it. If you can't do what you think needs to be done in ten or twelve years, then you damn well ought to step aside and let somebody else try.

That was one thing. The other thing that really blew them out of the water was my insisting that we certainly don't need two national meetings a year. We only need one. By this time, in 1983, it was obvious that scientists, chemists in this case, are spending so damn much time at symposia, national, international and so forth. There are so many meetings. The big job of someone arranging a program for a meeting is to find a date that doesn't conflict with another meeting! [laughter] I'd been going to ACS meetings and I knew that the inorganic chemists and others have done a real good job at organizing symposia; they're very well attended. Everybody wants to go hear Harry Gray or Roald Hoffman or whoever. But with those general fifteen-minute papers, it was embarrassing. They had the person who was talking and the person who was about to follow him, and the projectionist (when they still had a projectionist); there was nobody there. You couldn't justify it on the basis of the science being reported, and you couldn't justify it on the business, because the board of directors and people like that could meet at other times if they needed to and could meet whenever they needed to meet.

One meeting a year is what most chemical societies have. They don't have multiple meetings. ACS now is gradually going towards three meetings a year, with the Pacific coast meeting and the Canadian one. Well, that didn't fly, even though there was a questionnaire that was put out by the ACS, asking people whether they would be willing to try this on an experimental basis. The response was not very great, but of the response, it was two to one that it should be tried; at least we could try it. But when it came down to the board decision, there was only one vote in favor of trying it, and that was mine. [laughter]

My idea, of course, was that we could have one meeting a year and we could be more supportive of the regional meetings, because it's getting so expensive to go to these big national meetings, at least for the graduate students and postdocs. The regional meetings are places where it could be much more accommodating and it seemed to me that more could be done there. The staff also was worried. They've got a huge staff, and they spend so much time on these national meetings. No sooner than one's over, they're already working on another one. My point was, I don't think you need to worry about it because you could spend that time on the regional meetings. Another reason is, if you have only one national meeting, and if you're really serious about wanting to meet some people at a meeting, and there's only one, then the chances of meeting that person are far greater than if there are two or even three meetings, because you may go to one and that person may go to another one. So there were reasons for doing this, but the bureaucracy was such that it didn't even get off the ground. The whole point is, these people are voting with their feet. As long as we have ten to twelve thousand, or even eight thousand, whatever the number is that come to these meetings, then the meeting is deemed necessary. My idea is that the meeting is not necessary if people come just because there is a meeting. It's wasteful of time and energy and the cost of whatever to go to meetings which are not necessary. We already have far too many meetings. I still feel that way.

BOHNING: I was involved with MARM [Middle Atlantic Regional Meeting] for a while, and there's another whole audience out there of people at small liberal arts college, which I think is a reasonable segment, who just don't have the wherewithal financially to come to national meetings.

BASOLO: Of course.

BOHNING: So, once every five years, if it's within a hundred miles away, you might get there for a couple days.

BASOLO: You ought to break into the national governance of the society and committees. Once you get into that category and make friends, you get kept on and on and on and on, and you can go to all the national meetings. [laughter]

BOHNING: I have seen this in local sections, where counselors are self perpetuating for decades.

BASOLO: Absolutely.

BOHNING: That also leads us into another thing that you are rather strong about in print, and that is the whole mechanism of ACS governance.

[END OF TAPE, SIDE 5]

BOHNING: It's interesting that you thought the number of committees could be substantially reduced and still accomplish the same thing.

BASOLO: I'm sure that's still true.

BOHNING: These were obviously very strong viewpoints. I was wondering, in addition to the people who were on the side you were looking at, what kind of response did you get from your colleagues?

BASOLO: If you're talking about colleagues who I come in contact with other than people who are ACS active, they were very supportive. "Gee, that's a great idea. That's wonderful. That's my dues, and we don't have to have all those committees. That's wonderful." But that's the group that's the silent group, who never really expresses themselves or get involved. Sometimes they don't even bother to take the time to vote for president-elect and people on the board of directors. Those people were supportive and those people seemed to be sympathetic.

But the minute I started talking to people who were in the governance of the ACS, and had been for some twenty years, they couldn't appreciate at all what I was saying. It was obvious to me that there are committees that are absolutely necessary, such as the Chemical Abstracts committee, the public relations committee, the education committee. There are all sorts of committees that are absolutely necessary and people are doing a good job. I'm not faulting the whole organization. But there are committees that are set up and maybe they were necessary when they were set up, but they don't go away. They're struggling to keep themselves busy and to justify their existence. That's ridiculous. Not only are there too many committees, but all of these committees have a tendency to then set up task forces on this, that and the other. So they compound one another. I'm sure you could get rid of half those committees and task forces and the ACS would be properly governed and the governance and everything else would be run as smoothly as it does.

BOHNING: Have you seen any change at all in the last seven years?

BASOLO: I finished one year as president-elect and one year as president. I was on the board of directors during those two years and then one more year as a member of the board of directors. At the end of that third year I asked to be taken off of all the committees that I was on because if I had gone around talking the way I did, and wanting to make room for other people to get on committees, I wanted to at least do the right thing, and I did. I haven't really darkened their door since then. I haven't even gone to very many national meetings since then. I should really go to this one, because Harry Gray is getting the Priestley Award, and I just feel badly, but I have another commitment. I'll be in Mexico for two weeks, and this was arranged before I even paid attention to when this national meeting is scheduled. There are so damn many meetings, but I've got to be at this Congress on Inorganic Chemistry giving the opening plenary lecture in Mexico City. Then, because I'd never been to Mexico before, the people wanted me to give a lecture tour at various universities. They've got it all set up, and it conflicts with the national meeting. So I'm not even going to be able to go the time that I really wanted to go. I haven't really been to many of them. I don't know what's going on in terms of governance and the like.

BOHNING: There was another area that you were also vocal about, and that was the academic-industrial interface.

BASOLO: Yes, I've always felt strongly—and my colleagues here can tell you as well—that we should have closer interface and should get the two to work together much more so and make a real effort in that direction. But it's always been a very difficult thing. When I became chairman here, during the 1969-1970 academic year, Henry Taube and I had been on an NIH panel together and went out to dinner after one of our sessions. He was very enthusiastic about their industrial affiliates. One reason was, of course, that it was a nice lucrative thing for the department, because the industry's members would pay so much a year.

But more than that, according to him, it brought into the department sort of an educational aspect that was not there before. You bring people from industry there who would give talks. This was the kind of thing that students just never had access to before and he thought this was very good. So it just strengthened my feeling, and when I became chairman, I did everything I could to try to get something like that started. Of course, this was an ongoing thing and had been at MIT and had been at Caltech for years, and still is. But that was during the Vietnam situation with students protesting; it was kind of a recession period. It never got off the ground. I still blame my colleagues for not being supportive. Their argument was, "If we tried now, Fred, and we failed, that would make it more difficult later. "But I think if we had tried, we would have been ahead of the pack and we probably would be better off. We do have one now and everybody else has one now, but it doesn't mean much. What I did salvage from that was the Charles D. Hurd lectures. Did he give you a pamphlet on his lectures?

BOHNING: Yes, that was in part of the information you gave me (24).

BASOLO: That came out of my trying to do something, so that I almost unilaterally insisted that at least we ought to bring somebody here for three days. We've been very successful; that's been very good. And Charles was our logical faculty person; he was our senior member. He had a lot of consulting experience with industry. I was able to salvage our interaction with industry to that extent.

In the ACS, I guess I was a little concerned at one point, and disappointed when the Council for Chemical Research started. I read all the things that they put out, that universities should become members and you'd get so much money for each Ph.D. Chemical engineers would get a little bit more, but they'd get so much money. It just didn't make any sense to me, because it seemed that these industries that then were going to be asked to join and give the money, which would then be passed on, would prefer to deal directly with the departments that they themselves would like to be supporting. They wouldn't like to put their money in a pot and let somebody else decide what to do with it. So I felt a little bad and I was a little negative about that, even though that had to do with industry. I might have said something to that effect, but I don't know how successful CCR has been. I know it's still in existence. I think our department is still a member. You almost can not be a member; you almost have to be in self-defense. [laughter] But it does bring industry and academic people together, and to that extent, I think it's worthwhile. The other thing I worried about is that it would set up another bureaucratic bunch of people on the staff and otherwise, and consume a lot more time and energy. If there was something of that sort that could be done, why couldn't it be done through PRF [Petroleum Research Fund], or something in the ACS?

BOHNING: During the time that you were in the top office of ACS, what was your greatest achievement and what was your greatest frustration?

BASOLO: [laughter] You've already heard about my greatest frustrations. What I thought was worthwhile (whether it was or not can never really be assessed) were those occasions when I was called upon to represent the American Chemical Society, whether it be on the Hill in Washington or in front of the TV tube or something else. I thought that gave me an opportunity to make the point that chemistry is doing, has done, and will continue to do a great deal for mankind. I know we are all saying that to ourselves. All of us are convinced that's true, and we know it's true, but it's like preaching to the choir. What the hell, it's pointless.

But at least this gave me a chance not to just to preach to the choir. I remember the very first time this happened. I think it was in 1983, the 250th anniversary of [Joseph] Priestley's birth. It must have been in January and February, because I'd just become president of the ACS. I had to go to his Northumberland home in Pennsylvania. The Postmaster had issued a special stamp, and one of the Postmaster's people was there. Somebody from the Unitarian Church was there, because he had been very much involved as a founder of the church. It was a beautiful day, and we were looking at the house and everything that's there. It's a national museum kind of thing. Then those of us who were given about five minutes to say something got up on the podium. We were all sitting there, and obviously pretty soon I was going to have to say something. [laughter] What I had decided to say was how Priestley was very important in the discovery of oxygen, which turned out to be really the beginning of chemistry as we know it. But most of the people out there were not chemists. Very few chemists bothered to attend. The people who attended were mostly theologians; they were Unitarians and church people. They must have had about one hundred people or so.

Before it became my turn to talk, I decided, damn it, I'll just tell them that if Priestley were here now, he'd be proud to have been a chemist, because chemistry has done so much. He'd be pleased as punch to see what chemistry has accomplished over all these years and what it is accomplishing. I used as a vehicle, energy, agriculture, medicine, and so forth, pointing out that all of us trust our physicians, and if they diagnose an ailment properly and give us the right medication, things improve. The person that really makes these wonder drugs happen to be usually research people in the laboratories, biochemists, and not the physician who does the diagnosis and prescribes the medicine. It was just an off-the-cuff talk that I decided at the last moment to try on an audience of largely nonscientists.

After it was over with, we had goodies and a little social hour. It was just breaking up when two or three of the older Unitarian fellows came up to me and said, "We are sure glad you were here and able to say what you did. First of all, we were surprised to see anybody here from the American Chemical Society. We knew that Priestley was a chemist, but we thought people who were chemists these days weren't making public appearances." [laughter] According to them, all of chemistry (and that was true in those days and it is still true to a great extent), like dioxin and PCBs and all those things, was either toxic or carcinogenic. So they were glad that I had come along to represent the American Chemical Society and to make a point of all that chemistry has actually accomplished. So I think it was that, and it was being on the Hill a few times to speak in support of education and science research and all those things. As I said, you never know how much good it does. But at least you have a feeling it may be worthwhile. You come away knowing that you're not just talking to other chemists.

BOHNING: Were you involved at all with funding for the Beckman Center?

BASOLO: Yes, I was involved at least at the time that it was just called the Center for the History of Chemistry. My very first meeting, I think I told you at the beginning, was not as an official of the ACS. I was not on the board of directors. However, I was asked to attend a board of directors meeting that was being held in November or December, and I became a member of the board and president-elect on January 1. So they had asked me to come to this board meeting, just as an observer. I didn't have a vote, I didn't have anything, but I was observing. The board of directors met; it's usually about a two day session and there are lots of things to discuss and vote on. I sat through all of this, because I felt that's the reason I came. I wanted to sit through it and see what the problems were and what things were being discussed.

Finally, at some point, Arnold Thackray had made his presentation and there was some discussion about whether the ACS should come through with however much it was, maybe $50,000. Then it was opened up for comments. Even though I probably shouldn't have made a comment, I did make a comment. The comment was, "After sitting through the last couple of days of discussions, this is the first thing I've really heard that's worth talking about. It sounds like something that really needs attention and it would be a wonderful thing if the ACS could come up with the funding and be supportive of this." Of course, they did, and they got the AIChE [American Institute of Chemical Engineers] involved.

So at least initially, to that extent, I was involved, which was no involvement at all other than just making a comment. But then I was put on the advisory board for a few years. Because if you look at some of those original CHOC News, my name was there because I was part of the ACS.

BOHNING: There's another area which you were also very vocal about, about ten years ago. That has to do with the teaching of inorganic chemistry and where it should be and what general chemistry should be like. I'd like you to comment on that. Did you continue to teach general chemistry?

BASOLO: Oh, yes, I insisted on teaching one quarter of general chemistry each year. We were on a quarter system, and the general chemistry was taught for one year for the regular group. The honors people only have two quarters, but the regular group has three quarters. All the years that I've been here, except the first two or three years, I have always insisted on teaching one of those three quarters. Even when I was president of the ACS, I managed to do that. I've enjoyed doing it, and the student evaluations speak for themselves. Students are very enthusiastic about the course that I teach. I used a lot of lecture demonstrations. I teach reactions and syntheses. I use trends in the periodic table. I point out certain kinds of classifications which allow you to make educated guesses. I point out they should look at the forest instead of the individual trees. This is a pine forest and that's an oak forest; don't worry about the trees. They'll take care of themselves. You don't have to memorize any reactions, not a one. Any reaction I give you in lecture or on the homework I'll never ask on the exam, so don't bother. But learn the classifications I give and then from that and the use of trends in the periodic table, you should be able to make educated guesses and be right ninety percent of the time. If you're right ninety percent of the time in this class, you get an A and that's the highest grade you could get anyway.

Some of the students are totally lost and I don't know that anybody could do much for them. Some of the better students really do catch on, even more so than some of the graduate student TAs that are helping grade the midterms. The graduate student TAs haven't really come to grips with having to learn some of these things in the same sense. So there are some times during our grading when it's obvious that the students know more than the graders.

I have been outspoken. I have written articles in the Journal of Chemical Education and pointed out that I think that we've gone too far in teaching bonding and general principles in beginning chemistry (25). In teaching general chemistry, at one point you were teaching only bonding. People knew all about bonding this and bonding that and rabbit ears this and orbitals that. They didn't know whether sulfuric acid was a solid, liquid or a gas but they sure as hell knew that the sulfate ion was a tetrahedral type, orbital hybrid of this type or the other. Then we were teaching baby physical chemistry, which is kind of pointless. If you're going to teach physical chemistry, why don't you let the physical chemists teach it later on? But it turns out, of course, that a lot of general chemistry is taught by physical chemists and they feel more comfortable teaching baby physical chemistry than they feel teaching reactions and things.

So, yes, I have been rather outspoken. I do think that part of our problem is in part the fact that we're not teaching enough reactions and thing at that level for those kinds of people. It might excite somebody to see colors change or see precipitates form or an explosion now and then. If you blow up a hydrogen balloon by itself and then blow it up with a mixture of oxygen and hydrogen, you're going to wake up the class! [laughter].

BOHNING: That's true.

BASOLO: Students take three quarters. Year in and year out, over all these years, my quarters always got the highest rating, I think partly because of the course content.

BOHNING: Do you use a text?

BASOLO: Yes, but I don't follow it very much because texts aren't approaching this in much the same way. But fortunately now, you will see authors even admit that they read the article that Bob [Robert W.] Parry and I wrote (26). I actually wrote much of the article and I got Bob Parry to be a co-author, hoping that they'd pay a little more attention. He agreed with it, so that was fine. Now I notice that in revision, some of the texts are devoting a little more time to reactions and syntheses now than they had in the recent past. But it's still difficult, because people who are teaching these courses are people who have not learned that kind of chemistry and they really are, oftentimes, physical chemists who feel much more comfortable with physical chemistry, principles, solving problems, and so forth.

BOHNING: I think it might be in that article with Parry, I was intrigued with the way in which you classified mechanisms as a way of making predictions. I'm not stating the question very well, but for example, when I used to teach some organic chemistry to nursing students, I always pointed out that when you have a reactive site in a molecule, if you know what this does in general, it doesn't make any difference what else is around it. You can just make a prediction about it.

BASOLO: Sure, that's the nice thing about organic chemistry. That's why it's always been so much easier to teach organic chemistry. If you're going to make esters, it doesn't matter whether you're making them on a huge molecule or a small molecule.

BOHNING: But weren't you in essence trying to relate the teaching of inorganic chemistry in a similar fashion? I know you can't classify things as neatly as you can in organic chemistry.

BASOLO: Sure, and I did it in a very crude, pedestrian way in terms of using language that's everyday language. For example, combination reactions. There's a whole pile of reactions that are combination reactions. If you take a metal and you add oxygen to it, you make a metal oxide. Then you can talk about some metals being more reactive than others; the noble metals wouldn't react, and that's why you have platinum jewelry and gold jewelry. After combination reactions you do decomposition reactions. You explain to them that there are certain kinds of things that will decompose when you heat them. If it's just an ordinary water hydrate like barium chloride octahydrate, you heat it and you just drive off the water and you make anhydrous barium chloride.

Then you can also tell them that if it's aluminum trichloride hydrate, heating it surely does not give anhydrous aluminum trichloride. [laughter] You get aluminum oxide. Then you can explain to them about size and charge at the level at which these people can grasp it. It is a classification that is not very sophisticated, but it is a classification that these kids can understand and use. As I say to the students, by the time we're finished, you ought to be able to write thousands of equations without having memorized a single one of them. And some of them can.

BOHNING: I know in the last years that I was teaching, I was amazed at what students could memorize. It was just incredible what they could memorize.

BASOLO: Well, that's it, but that turns them off, if they have to memorize things.

BOHNING: Yes. But while I know it was a product of their earlier training, I found that many of them had no concept of studying any other way but by memorizing.

BASOLO: But you know, if you're given a lot of these inorganic reactions and it's just a reaction and they can't tie it together in anyway, there's nothing more deadly than that. That's the way general chemistry used to be taught.

BOHNING: Yes, exactly.

BASOLO: And that was not the right way to teach it either. I think you can do a better job than that.

Look, we're going to have to go upstairs, if we can wind it up. How much more do you want to ask?

BOHNING: I've skipped over some things, but you have covered a lot of your scientific work in some of these other publications (27).

BASOLO: Yes, I gave you a lot of information (28).

BOHNING: Yes. I was trying to go behind the scenes, as it were.

BASOLO: Sure.

BOHNING: Thank you for spending the morning with me; I've really enjoyed it.

BASOLO: I appreciate your dropping by. Tell Thackray I'm glad that he put me on the list. We have more people here if you ever need more. [laughter] Everybody gets older all the time, and I'm not even the oldest. There's Hurd and Pines, for example.

[END OF TAPE, SIDE 6]

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