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INTERVIEW OF FRED BASOLO

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D. S. Fred, before you discuss your long and distinguished career, tell us how you started in chemistry.

I was born in an ethnic coal mining town in southern Illinois. My father was a coal miner and my brother and my sister were about ten years older than myself. Neither of them even went to high school. During the Depression they went to work. Ten years later, however, youngsters were going to high school in that little town. I had my first chemistry course from a very beautiful young lady who was teaching for the first time and had majored in home economics. She knew little or no chemistry but volunteered to open up the laboratory on Saturday morning and supervised us, so that we didn't blow ourselves up. She let us do what we wanted using the laboratory manual, and that first exposure to chemistry sparked my interest. I probably would have gone to work after high school had there been jobs, but there were none. This was during President Franklin Roosevelt's Welfare Programs, such as WPA, and there was a student program that provided $25 a month support of students to go to college. I went to Southern Illinois Normal where they had only one degree, a Bachelor in Education. My first job was as a janitor in the main building. My second job was in the library, but later as a chemistry major they had me do urinalysis and hemoglobin tests and things of that sort in the health service department. I also worked for my room, so $25 was quite adequate, and I managed to get through the 4 years during 1936–1940. Fortunately for me, I found at Southern Illinois four faculty members in chemistry who were really outstanding teachers — in fact later on we called them the "four horsemen". They were dedicated to motivating students into going on in chemistry. In my case I started out taking chemistry when I arrived and did reasonably well, so that in my junior year Professor James Neckers talked to me about the possibility of going to graduate school. It had been my feeling that since I was in a Normal school and getting a Bachelor of Education degree, that I would go on and teach in elementary school or high school.

I hadn't even heard of graduate school, and I didn't know what he was talking about. But arrangements were made and I did go to the University of Illinois where I met Professor John C. Bailar, a fine human being and an outstanding chemist. I decided to do my thesis work with him, and have always felt very fortunate for having done so. Most of the graduate students were working in organic chemistry, and only about a dozen of us were inorganic chemists. There was very little interest in those days in inorganic chemistry. I arrived there in 1940 and got my degree in October of 1943, at age 23. At that time we were well into World War II and when I finished, I was offered two jobs. One was on the Manhattan Project at the University of Chicago where they couldn't tell me anything about the nature of the job. The other was on some classified research at Rohm & Haas in Philadelphia where they couldn't tell me about the classified research but they could certainly tell me some of the unclassified work that was going on. So I took the Rohm & Haas job and was there from 1943–1946. During that time I kept thinking that when the war was over I wanted to get an academic position.

D. S. What were the origins of your research on mechanisms of inorganic reactions?

My interest in mechanistic work really came about as a result of the 3 years prior to my coming to Northwestern University in September of 1946. I had occasion to read some review papers and books on the subject of physical organic chemistry. I was struck with their investigations of substitution reactions of tetrahedral carbon, applying the Hughes–Ingold symbols of S_N1 for dissociation and S_N2 for association reactions. I had a feeling in the mid-forties that it would be possible to do much the same for octahedral substitution and for square-planar substitution reactions of metal complexes. My experience as a graduate student with Bailar dealt with the syntheses and stereochemistry of metal complexes. On arrival at Northwestern University in September 1946, I was anxious to start a research program on the kinetics and mechanisms of reactions of metal complexes.

D. S. And you and Ralph collaborated on some of that early research.

That is correct. When I arrived at Northwestern University as a beginning instructor in September 1946, there were no inorganic graduate students, there was no funding, and our equipment was largely limited to a Beckman DU spectrophotometer, a conductivity bridge, and a Rudolph polarimeter. Fortunately, there was another instructor on our faculty, Ralph G. Pearson, who was doing research on physical organic chemistry. My initial mention to him of a possible collaboration doing much the same research he was doing, but on metal complexes, at first fell on deaf ears. After all, what was inorganic chemistry in those days; the action was largely in organic chemistry. However, the record shows that Ralph was finally seduced to inorganic chemistry, and the two of us had a most productive several years which resulted in 55 joint publications culminating with our book *Mechanisms of Inorganic Reactions*.

D. S. In the 1940s and 1950s was funding the big issue it is today?

In those early days, there was no funding. My first four publications have no acknowledgment to funding. Northwestern University was not known for its inorganic chemistry, so my first two publications from Northwestern were done by myself. Pearson and I received an Atomic Energy Commission Grant in 1950 of $7000 for two years. That was followed in 1952 by a grant to me from the National Science Foundation, one of the very first grants from NSF to Northwestern University. That was for $6100 for $1\frac{1}{2}$ years and later on in 1960 I also got an NIH Grant for $45 000 for 3 years. In subsequent years, the AEC, the NSF, and NIH continued to support our research as well as some support from PRF and the AFOSR. I have already commented on the instruments available at the start of our research.

D. S. One contribution that you and Ralph made was the application of ligand field theory to mechanistic coordination chemistry.

Yes, that had its origin, where I was concerned and to some extent where Ralph was concerned, in the academic year 1954–1955. I had a Guggenheim Fellowship and spent a very delightful and useful year in Denmark at the University of Copenhagen where my host was Professor Jannik Bjerrum. Some of the people in his group were C.K. Jørgensen, C.J. Ballhausen, and C.E. Schäffer. These people were all very much involved in research having to do with crystal field theory, which I heard about for the first time. At that point I was still using the valence bond theory, in much the same way it was used by Taube in his classical *Chemical Reviews* article of 1952. However, crystal field theory came through to me “loud and clear” that year in Bjerrum’s laboratory. That year I also saw my first Cary spectrophotometer, which Jørgensen was using on a daily basis. He determined spectra of all sorts of metal complexes, and almost every Monday he arrived with a manuscript of a new paper on the spectra of some metal complexes explained in terms of crystal field theory. He had me help with the English of his papers, and he provided me with my first exposure to crystal field theory. By the time I returned to NU, Pearson and his students were all excited about crystal field theory. Ralph was beginning to make estimates of crystal field activation energies (CFAE) to account for ligand substitution reactivities of metal complexes.

D. S. Your influential book Mechanisms of Inorganic Reactions *appeared at the end of the 1950s and it covered so much ground: bonding through mechanisms.*

Yes, the first edition of the book was really not all that much effort for Pearson and me to write. We enjoyed writing (1956–1958) the book, because much of it had to do with our research, and the work of others with which we were familiar. The timing on the book was perfect, this often being the difference between success and failure of books. It was the first book on the subject of inorganic reaction mechanisms, at a time when research in this area was beginning to make its mark. It was also the first book to make use of crystal field theory in the chemistry part of metal complexes, rather than just explaining the spectra and magnetic properties of metal complexes. Fortunately, reviewers liked the book and Professor Daryle Busch even dubbed it as "The Bible" of mechanistic inorganic chemistry. The book was translated into several languages and used worldwide; it is still occasionally referred to by people doing research in the area.

D. S. The famous controversy with Sir Christopher Ingold also brought attention to your work.

Yes, the polemic we had with Sir Christopher Ingold, and to a lesser extent with Sir Ronald Nyholm, was a great help to Ralph and me in the early stages of our research. We collected considerable evidence in support of an S_N1CB mechanism for the base hydrolysis of cobalt(III) ammines (eqns. (1)–(3)):

$$[Co(NH_3)_5Cl]^{2+} + OH^- \overset{fast}{\rightleftharpoons} [Co(NH_3)_4(NH_2)Cl] + H_2O \qquad (1)$$

$$[Co(NH_3)_4(NH_2)Cl]^+ \xrightarrow{slow} [Co(NH_3)_4(NH_2)]^{2+} + Cl^- \qquad (2)$$

$$[Co(NH_3)_4(NH_2)]^{2+} + H_2O \xrightarrow{fast} [Co(NH_3)_5(OH)]^{2+} \qquad (3)$$

In spite of this evidence, Ingold published that we were in error, and that the reaction was a simple S_N2 displacement of Cl^- by the strong base and strong nucleophile OH^-. Since one can never prove a mechanism is correct, we finally had to prove the S_N2 mechanism wrong. This attracted considerable attention to our research, even including that of organic chemists. Had we lost the polemic, that may well have been the last to be heard of Basolo and Pearson, and Northwestern University would not today have one of the best inorganic chemistry departments in the country. We are extremely grateful to Ingold and Nyholm for their help, and we thank our lucky stars we were not wrong.

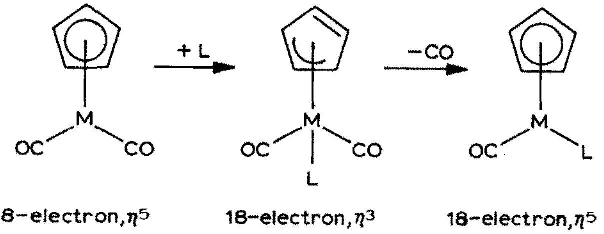
D. S. One thing that characterizes your work is the shift to new areas in timely fashion. When I came to Northwestern in the early 1960s, you were shifting strongly into organometallic mechanisms. What prompted that move?

That happened primarily as a result of my attending the International Conference on Coordination Chemistry in Amsterdam in May of 1955 and listening to a plenary lecture by the late Professor Walter Hieber. He gave the lecture in German (and I don't understand German) but his slides were very well prepared, and it was apparent that he was presenting some very elegant work on reactions and syntheses in metal carbonyls. At the conclusion of his lecture I complimented him on all that beautiful chemistry and asked if he knew how some of these reactions took place. We had to talk through interpreters, but he looked at me and said, "Young man, in my laboratory we only do real chemistry," meaning that they only did reactions and syntheses. "We don't do philosophy of chemistry," meaning he wasn't concerned about bonding or about mechanisms of reactions. Well, I just couldn't wait to get back to Northwestern University to talk to the graduate students about this wonderful virgin area of mechanistic inorganic chemistry. Much was known about metal carbonyls but almost nothing was known about their kinetics or mechanisms of CO exchange. This seemed to me like an ideal Ph.D. thesis problem, because anything that was done would be new original research suitable for a thesis and acceptable for publication.

In spite of the suitability of such a problem, graduate students initially chose to work on some aspect of our on-going research. This dealt with the aqueous solution chemistry of Werner complexes, not toxic volatile metal carbonyls foreign to our laboratory at the time. Andy Wojcicki was the first to accept the challenge to investigate the kinetics and mechanisms of CO exchange with several metal carbonyls. He did such an excellent job, that soon there was no difficulty in interesting students and postdoctorates in research on organometallic compounds.

D. S. About that time you started collaboration and exchange of students with E.O. Fischer.

In 1964 I started a collaborative program for the exchange of predoctoral students in their final year with Professor E.O. Fischer, who shared the Nobel Prize with Professor Geoffrey Wilkinson. Fischer's laboratory was doing outstanding research on the syntheses and reactions of organometallic compounds, and our laboratory was studying the kinetics and mechanisms of reactions of such compounds. This collaborative effort was of benefit to both our research programs and particularly worthwhile to the students involved. The German students brought to our laboratory, for the first time, some of their special techniques of handling air-sensitive compounds. They often came with compounds and studied the kinetics and mechanisms of their reactions. For example, H.G. Schuster-Woldan had just prepared the compounds $(\eta^5\text{-}C_5H_5)M(CO)_2$ ($M = Co, Rh, Ir$), so he came to study the kinetics and mechanism of their CO substitution. His work resulted in two extremely important observations. One was that the relative rates of reaction vary in the order cobalt < rhodium > iridium. Today it is well known that for a given transition metal triad, organometallic complexes of the 2nd row metal are the most substitution labile. Since rapid ligand substitution is a must for the use of these compounds as homogeneous catalysts, we can appreciate the fact that most such processes make use of 2nd row transition metal compounds. He was also the first to observe the phenomena of ring slippage (eqn. (4)) now known to be extremely important in organometallic chemistry.



(4)

D. S. There was another shift soon thereafter to oxygen binding in metal complexes.

Yes, I had read with interest the papers of Professor Melvin Calvin on synthetic oxygen carriers of cobalt chelates published immediately after World War II on work done during the war. They had made detailed studies of solid state gas phase reversible interactions between cobalt Schiff base complexes and dioxygen. I wanted to investigate the solution kinetics, mechanisms and thermodynamics of O_2 uptake as a function of systematic changes in the cobalt chelates. Besides my scholarly interest in this, I also felt it might receive support from the National Institutes of Health—which it did. In 1966 Professor Alvin Crumbliss, my graduate student at the time, made the important observation that his solutions were taking up one equivalent of O_2 per equivalent of cobalt(II). He went on to discover, isolate, and characterize, for the first time, a series of monomeric O_2 complexes of cobalt. These systems have an end-on-bent Co–O–O structure, and they generated much interest in different laboratories. For the first time, synthetic oxygen carriers had been isolated that related directly to the natural proteins MbO_2 and HbO_2. Professor David Petering, then my postdoctoral student, prepared coboglobin (cobalt replacing iron in myoglobin or hemoglobin) and showed that it behaved similarly to the natural proteins. Since oxycoboglobin has an ESR spectrum while MbO_2 and HbO_2 are ESR silent, Petering worked with our colleague Professor Brian Hoffman to initiate the series of elegant papers then published by Brian and his research group.

D. S. You paid your dues to your discipline both as department chairman and as ACS President. Would you describe those years.

I've always been concerned with trying to be helpful to chemistry as best I could, because chemistry has been very good to me. I was chairman of our department for 3 years during 1969–1972. As you know we have a rotating chairmanship, and we have never had to fill it with an outside person. Our faculty is very loyal and responsible, so when asked to serve, one grits one's teeth and does it. At the time I was asked, the chairmanship was on a 5 year rotation; I would only take it for 3 years and we have since been on a 3 year rotation. My 3 years were the worst possible 3 years to be chairman. The country was in a recession; there were no jobs for Ph.D. chemists and most companies were even terminating employment of research chemists. Funding of academic research was at a low point, and students were protesting against the Vietnam war. Perhaps since my first position was in industry, I have always felt it important that our schools and industry work closely together on concerns of mutual interest. The economic climate at the time made it impossible for me to start an Industrial Associates program. I did manage to start our Charles D. Hurd Lectureship, which brings an industrial chemist to our department for 3 days to give three lectures. This has continued over the years; it has been very worthwhile to us and to industry, and finally we do have an Industrial Associates program.

The American Chemical Society Presidency is yet another scenario. Other than being Chairman of the Inorganic Division of the ACS in 1970, I had not been very active at either the local or the national level of the ACS. Naturally I was surprised when some of my friends asked if I were willing to run as a petition candidate for election to become the ACS President in 1983. I said I would think about it, but later agreed if they were willing to do what had to be done to get me nominated and elected. I won, although I took no active part in the election campaign. My year as President of the

ACS in 1983 was both good and bad. The good was that I had to represent chemistry on behalf of the ACS, both nationally and worldwide. It was necessary to appear before congressional committees on matters of the support for basic research, for education, and for the environment. It is difficult to assess how effective this was, but I had the feeling it had a positive effect. This was a particularly difficult time for chemistry, with the news media and lay public viewing chemicals as only toxic and carcinogens, some even making ridiculous statements such as "chemicals are bad, we must get rid of all chemicals". I had an opportunity to respond to this on invited talk shows on radio and television. Always I told my host I would come and respond to questions about harmful chemicals, but I would also want to have equal time to address the fact that 99.9% of all chemicals help increase the quality of life worldwide. This was permitted, so I would give examples of the importance of chemicals in medicine, in agriculture, and in energy.

The down side of being president of the ACS was that I could not do what I felt, and still feel, is needed for the ACS. In these days of scientific meetings explosion, I firmly believe one national meeting of the ACS is sufficient. This would bring more attention to regional meetings, with their larger student participation. The other thing needed in the ACS is a by-law change limiting the years of service for an individual at the national level. At present some people make a life career of ACS service. They work hard and do a good job, but are not willing to step aside and allow a place for new people. Surely one has made his contribution of ideas and innovations during 10–15 years of service to an organization, so it is time to stop and give someone else a chance. Also the ACS has far too many committees, task forces etc., some just creating work to justify their existence. Yes, not being able to help the ACS move in the direction of lessening such problems made for a frustrating 3 years on the Board of Directors.

D. S. At the end of the 1970s you turned back to organometallic mechanisms. Tell us why you changed direction once again.

I guess there were a couple of reasons why this happened. You were one of them along with Professor William Trogler, and both your students. Perhaps because of my asking questions and making comments at our inorganic seminar and our Saturday morning BIP, some of you decided the "old boy" still knows some chemistry and came around to suggest we might collaborate on some problems of mutual interest. I was delighted, because my primary interest has always been reactions of metal complexes (with me this includes transition metal organometallics). The second reason, a more pragmatic reason, is that during my presidency in the ACS I had to write a renewal proposal to the National Institutes of Health on our work on synthetic oxygen carriers. I didn't give it the loving care that it deserved, and it didn't get the priority that would merit funding. I was asked to rewrite and resubmit, but I decided that maybe I should finish up my career in an area that I really am more concerned about and did not reapply to NIH. At present, as you know, my small research group devotes full time to the kinetics and mechanisms of carbon monoxide substitution reactions of metal carbonyls.

D. S. You mention the Saturday morning BIP seminar. How did that fine tradition begin?

That started really when it was just BP with Basolo and Pearson. Ralph and I were collaborating on research and sharing students. We decided to have joint informal research conferences on Saturday morning, hoping students would view it as a working day. The speakers were not scheduled and they could only use the blackboard—no slides or transparencies. Always two or three students would make short presentations of their work, or of what they planned to do. Years later Professor James Ibers joined our faculty. He performed X-ray structure determinations on important inorganic compounds provided by other inorganic chemists. He soon realized that his graduate students should make compounds in their own area of chemistry. He felt it would be helpful for him and his research group to join our Saturday morning gathering. Ibers then had some joint students with Pearson and with me. One of our joint students, now Professor Kenneth Raymond, gave our informal group the acronym BIP. It continues to be used even though Pearson has been gone for several years, and even though now all the inorganic faculty and research groups meet together at BIP. This is unique to our department, and it is very helpful to us all in a way that has to be witnessed to be understood. It is good experience for the students to think on their feet, to make such informal presentation of their own work, and it brings all of the inorganic chemists together to ask questions and make suggestions about the ongoing research. The tradition is now well established, so it is my hope it will continue well beyond the demise of the three individuals BIP.

D. S. You are highly regarded here at Northwestern for your teaching, and you have received national teaching awards. Give us your thoughts on the teaching of chemistry.

I told you to start with that I went to a Normal school, where the only degree one could get was a Bachelor of Education degree. As you know, I've pointed out to you and the rest of our faculty colleagues that I'm the only qualified teacher among you. I had to take all the necessary education courses which qualify me to teach in an elementary or high school in the state of Illinois. None of you could say as much. Teaching has always been important to me. It is important to do scholarly research, but one must also be able to teach and to motivate students. As you know, I enjoy teaching at all levels: general chemistry, Junior–Senior inorganic, and special topic graduate courses. My teaching enjoyment seems to rub off on the students who respond positively to my classes and I feel strongly about teaching general chemistry. I think that we are losing the battle with medical school, computer sciences, MBAs and law schools in terms of fewer students selecting chemistry as a career. All across the country we teach hundreds of thousands of students general chemistry, yet so very few of them choose to go on in chemistry. I think our general chemistry textbooks and our teachers are in large part responsible for this. They teach principles and problem solving, largely ignoring the excitement of real chemistry with the excitement of lecture demonstrations and wet laboratory experiments.

Beginning students need to see, hear, and smell chemistry in action in order to want to pursue it. My experience tells me it is possible to teach beginning students reactions and syntheses, with the use of the periodic table, such that they can correctly write reactions 90% of the time without having memorized a single equation. I have written an article with Professor Robert Parry on how this can be done by teaching classes of reactions, not individual reactions. It is gratifying to see that many recently published (including revisions) general chemistry textbooks are making use of this approach. This represents a big improvement over the past 20 years, and I hope it does not turn out to be too little too late.

D. S. You and Johnson also wrote a very timely book for freshmen on the subject of coordination chemistry.

Yes, this is a book I started during my year's leave of absence at the University of Rome. Again the timing on this book was perfect. In 1964 some general chemistry textbooks did not even have a chapter on coordination chemistry, and certainly not one with crystal field theory and reaction mechanisms mentioned. When I returned from Rome my graduate student, Professor Ronald Johnson, did an excellent job helping me finish the book. This little paperback book sold like hot cakes in the U.S., and it was translated into nine different languages. Over the years in my travels about the world, I have often met inorganic chemists, very well-established and well-known inorganic chemists, who come up to me and say "Gee, I want you to know that I became interested in coordination chemistry or inorganic chemistry as a result of having read your book when I was a beginning chemistry student". I am always delighted to hear this, but in recent years, they also look at me as if to say, "How do you happen to still be alive after so many years?"

D. S. As we come to the end of this interview, the big question is "Do you plan to retire?"

Doesn't everyone retire or just fade away eventually? You know, we bought a new home a couple of years ago. My wife's stipulation was that the house all be on one floor, and mine was that it have a large yard so I could do a little gardening. I am doing a little gardening and I still manage to play a little golf, so I plan to enhance these activities and slow down my chemistry. I reach age 70 next year which will be my last teaching year, after which I become Emeritus. I do have the opportunity to stay until age 75 doing research, still having my laboratory and office. Beyond that depends on my health and my desire. At the moment we are still very much interested in the research that we are doing, so surely I will work beyond the age 70 till age 75. Then I will retire in comfort, knowing the department is orders of magnitude better today in inorganic chemistry than it was when I arrived in 1964.