



JOHN C. BAILAR, JR.

Transcript of an Interview
Conducted by

Theodore L. Brown

at

University of Illinois, Urbana

on

28 May & 17 June 1987

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
RELEASE FORM

This document contains my understanding and agreement with Chemical Heritage Foundation with respect to the participation of John C. Bailar, Jr. in a tape-recorded interview conducted by Theodore L. Brown on 28 May & 17 June 1987.

1. The tapes, corrected transcript, photographs, and memorabilia (collectively called the "Work") will be maintained by Chemical Heritage Foundation and made available in accordance with general policies for research and other scholarly purposes.
2. I hereby grant, assign, and transfer to Chemical Heritage Foundation all right, title, and interest in the Work, including the literary rights and the copyright.
3. The manuscript may be read and the tape(s) heard by scholars approved by Chemical Heritage Foundation subject to the restrictions listed below. The scholar pledges not to quote from, cite, or reproduce by any means this material except with the written permission of Chemical Heritage Foundation.
4. I wish to place the conditions that I have checked below upon the use of this interview. I understand that Chemical Heritage Foundation will enforce my wishes until the time of my death, when any restrictions will be removed.

a. X JK

No restrictions for access.

NOTE: Users citing this interview for purposes of publication are obliged under the terms of the Chemical Heritage Foundation Oral History Program to obtain permission from Chemical Heritage Foundation, Philadelphia, PA.

b. _____

My permission required to quote, cite, or reproduce.

c. _____

My permission required for access to the entire document and all tapes.

This constitutes our entire and complete understanding.

(Signature) _____

John C. Bailar, III

(Date) _____

26 March 98

This interview has been designated as **Free Access**.

One may view, quote from, cite, or reproduce the oral history with the permission of CHF.

Please note: Users citing this interview for purposes of publication are obliged under the terms of the Chemical Heritage Foundation Oral History Program to credit CHF using the format below:

John C. Bailar, Jr., interview by Theodore L. Brown at University of Illinois, Urbana, Urbana, Illinois, 28 May & 17 June 1987 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0073).



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



The Chemical Heritage Foundation (CHF) serves the community of the chemical and molecular sciences, and the wider public, by treasuring the past, educating the present, and inspiring the future. CHF maintains a world-class collection of materials that document the history and heritage of the chemical and molecular sciences, technologies, and industries; encourages research in CHF collections; and carries out a program of outreach and interpretation in order to advance an understanding of the role of the chemical and molecular sciences, technologies, and industries in shaping society.

JOHN C. BAILAR, JR.

1904 Born in Golden, Colorado, on 27 May

Education

1924 B.A., chemistry, University of Colorado
1925 M.A., chemistry, University of Colorado
1928 Ph.D., organic chemistry, University of Michigan

Professional Experience

1924-1925 Chemistry fellow, University of Colorado
1926-1928 Assistant in chemistry, University of Michigan

University of Illinois
1928-1930 Instructor in chemistry
1930-1935 Associate in chemistry
1935-1939 Assistant professor
1939-1943 Associate professor
1943-1972 Professor
1972- Professor Emeritus
1937-1951 Secretary of the Chemistry Department
1941-1967 Head of Division of Inorganic Chemistry

Honors

1946 Foster Lecturer, University of Buffalo
1957 Clark Lecturer, University of West Virginia
1959 Noyes Lecturer, Phi Lambda Upsilon, University of Illinois
1959 Hon. Sc.D., University of Colorado
1959 Hon. Sc.D., University of Buffalo
1959 President, American Chemical Society
1960 Smith Lecturer, Oklahoma State University
1961 Award in Chemical Education, American Chemical Society
1962 Kuebler Award, Alpha Chi Sigma
1962 Merck Lecturer, Bucknell University
1963-1964 Member, President's Advisory Committee
1963-1971 Treasurer, International Union of Pure and Applied Chemistry
1964 Priestly Medal, American Chemical Society

1965	Dwyer Medal, Chemical Society of New South Wales
1966	Werner Memorial Lecturer, Zürich
1966	Werner Gold Medal, Swiss Chemical Society
1968	Dwyer Memorial Lecturer, Chemical Society of New South Wales
1968	Welch Foundation Lecturer
1968	American Cyanamid Lecturer, University of Connecticut
1968	Award for the Teaching of Chemistry, Manufacturing Chemists Association
1970-1971	National Lecturer, Sigma Xi
1972	Award for Distinguished Service in the Advancement of Inorganic Chemistry, American Chemical Society
1973	Hon. Sc.D., Lehigh University
1974	Honorary Fellow, Indian Chemical Society
1976	Honorary Member, Illinois State Academy of Science
1978	Heyrovsky Medal, Czechoslovakian Academy of Science
1982	Hon. Doctor of Humane Letters, Monmouth College
1983	Spicer memorial lecturer, Georgia Institute of Technology
1983	Ferst Award, Sigma Xi

ABSTRACT

The interviews entail a discussion of John Bailar, Jr.'s scientific life, beginning with a description of his family background. His parents had a great influence on Bailar's early education; growing up, he often helped his father with his chemical research. This experience peaked Bailar's interest in pursuing chemistry at the University of Colorado, where he received a B.A. in 1924, followed by an M.A. in 1925. Bailar continued his education, earning a Ph.D. in organic chemistry at the University of Michigan in 1928. He then took a position teaching general chemistry at the University of Illinois. While there, he changed his emphasis to inorganic chemistry, conducting research on isomerism and molecular rearrangements, and later on coordination compounds. By the early 1930s, Bailar had become a member of the graduate faculty, supervising the research of several graduate students. Throughout his career, he was heavily involved with the ACS; in 1959, he was elected president. The interviews end with Bailar's recollections of some of his most successful students, and his reflections on graduate education.

INTERVIEWER

Theodore L. Brown holds the B.S., M.S., and Ph.D. degrees in chemistry. Since 1956, he has been a member of the faculty of the University of Illinois, Urbana-Champaign, where he is now Professor of Chemistry, Emeritus. He served the University in various capacities, including as Vice Chancellor for Research and Dean of the Graduate College during 1980-1986, as Founding Director of the Arnold and Mabel Beckman Institute for Advanced Science and Technology during 1987-1993, and as Interim Vice-Chancellor for Academic Affairs during 1993. He is currently a member of the American Chemical Society Governing Board for Publishing as well as of the Board of Directors of the Arnold and Mabel Beckman Foundation.

TABLE OF CONTENTS

1	Childhood and Early Education	Family background. Educational influence of parents. Early interest in chemistry.
5	College Education	Bachelor's and master's degrees in chemistry from University of Colorado. Graduate work in organic chemistry at the University of Michigan. Thesis research under Professor Moses Gomberg.
7	Teaching at the University of Illinois	First position teaching freshman chemistry. Research interests in inorganic chemistry. Work on stereo-isomerization, coordination compounds. Influence of "Speed" Marvel. Supervision of graduate students, placement work.
19	American Chemical Society	Attendance at national meetings. Involvement in physical and inorganic chemistry division. Creation of separate inorganic chemistry division. Election to office of President.
24	Involvement in IUPAC	Revamping of constitution. Selection as treasurer.
27	Summing Up	Reflections on graduate students. Importance of research freedom.
33	Notes	

INTERVIEWEE: John C. Bailar, Jr.
INTERVIEWER: Theodore L. Brown
LOCATION: University of Illinois, Urbana
DATE: 28 May & 17 June 1987

BROWN: To start our interview, John, I know you were born in Colorado in 1904. In fact, isn't your birthday 27 May?

BAILAR: Yes, I was 83 years old yesterday.

BROWN: I don't know a great deal about your parents and your family. Would you describe them and tell me something about your background?

BAILAR: My father was born in Illinois near Mason City. His father, my grandfather, was in charge of the County School Department there. Then, when my father was two years old, they moved to eastern Kansas. A few years later there was a drought in Kansas. The crops failed and the family moved on to Colorado. My grandfather had what we would now call a trucking business, he hauled supplies to the mines in Leadville. As a result my father lived for several years in Leadville. In 1883, my father was the first graduate of Leadville high school. There were three members in that class actually, but they received their diplomas in alphabetical order. Thus, he is listed on the high school roles as graduate number one. A few years after graduation the family moved again to Glenwood Springs, Colorado. He was working on his father's farm and it was there that he met my mother.

My mother was born in Pennsylvania. Around 1890 her whole family moved to eastern Colorado. She went to school in Greeley. In those days it was called the Greeley Normal School; today it is known as the University of Northern Colorado. After she finished she went to teach in Glenwood Springs which is where my parents met.

They were married in 1897. Soon after they married they got the idea from various reading material that it would be great to go to college. My mother decided that if they saved their money very carefully for four years then they would be able to go to college. My father agreed with this. They made this decision sometime in the late summer or early fall but they continued to think and talk about the idea. Subsequently, my mother suggested that if they worked and saved their money for two years they could afford to go to college for two years. Then they would drop out for a short while to earn some more money before returning to finish school.

My father was already 32 years old and my mother was 25. One day about Christmas that year, my father was in town (Glenwood Springs) when he overheard a man mention that the second semester of the university started the next week. He ran home and recounted this to my mother, who said, "Well, some day we'll be there." To which he replied, "We're going now!"

They didn't have any money but they packed up their belongings and moved to Boulder, Colorado. They enrolled in the university which was quite a remarkable thing. This was because at that time the university had only 300 students. It was a new school and most of the students were young. For a married man of 32 and his wife to both enroll at the university was really unusual! To support themselves they both got jobs. My father worked in the chemistry store room; my mother was secretary to the dean. (They did not have typewriters so she would handwrite all of the correspondence and other work.) In addition to working, my parents borrowed some money to pay expenses.

They graduated in three and a half years. Actually my mother left a little early and did not get her diploma. She taught high school in either her last semester or final year. My father remained in Boulder and got his bachelor's degree with a major in chemistry. After graduation they moved to Cripple Creek, Colorado, where father had gotten a job teaching high school. My older sister was born in Cripple Creek. However, they stayed there for only two years, I think.

While they were in Boulder and my father was working in the store room, he came into contact with a man named Palmer, who was a professor of chemistry. Just about the time my parents were finishing school, Professor Palmer became president of the Colorado School of Mines. Palmer needed to hire a chemistry teacher and remembering my father, asked my father to come to the School of Mines to be the assistant professor of chemistry.

It's hard to believe that it was only five years between the time my father worked as a laborer on his father's farm, and when he was a college professor. He stayed at the School of Mines for 15 or 16 years. After that, he went to work in the research laboratory at the Great Western Sugar Company. He worked there until his retirement some years later. That's a quick survey!

BROWN: I have some additional questions about your family. How old was your father when you were born?

BAILAR: My father and mother were married five years before they had any children. Fortunately, they did not have any children while they were in school. I was born two years after that when my father was 39 and my mother was 32.

BROWN: Did you grow up in Boulder, Colorado?

BAILAR: Yes, I lived in Boulder until I went to university.

BROWN: When you were growing up, who had the most influence on you — regarding your decisions about what kind of a career you might want to have?

BAILAR: There had never been any question in my mind that I would go to college. My mother and father had been so eager to go that I took it for granted that I too would go to college. I don't believe any of my school teachers had a particular great influence. All of them must have had some general effect but none of them stand out in my mind as being real top-notch teachers. My high school Latin teacher was perhaps the best teacher I had, but I wouldn't say he had any influence on my long-term career. My parents spent a great deal of time with me and my sisters which was important.

BROWN: How many sisters did you have?

BAILAR: Two, an older sister and a younger sister.

Even when I was even a very small boy, I would go to my father's laboratory and "help" him in his chemical work. He was always doing research. I would help him filter things or wash dishes. In this way I became acquainted with chemistry before I ever went to college. I remember one incident from a time before I think I could even read. I had seen a bottle in my father's laboratory labeled H_2O_2 and I asked him, "Why don't they just say HO?" He then explained to me why it was H_2O_2 rather than HO.

BROWN: That was a pretty acute question for someone that young!

BAILAR: Yes. But the point was that I knew some chemistry. My father was a fine teacher. (Some years later he received a certificate from the State of Colorado for "eminence in teaching".) Almost every Sunday afternoon, my father and I would go for a walk and just talk. The conversations were about all sorts of things and not just chemistry. He spent a great deal of time with me.

I also spent quite a lot of time with my mother. I entered high school very young, maybe only twelve years old. In my sophomore English class we read Emerson's essays. (That is pretty advanced stuff for a 13 year old.) While my mother was ironing, I would read an Emerson essay. I would read a sentence and ask, "What does it mean?" She would explain it to me, and we'd go on to the next sentence repeating the process. I think now if I read an Emerson essay I would find it stimulating, very worthwhile, but at that time — without her help — I could never have understood what it was about.

My parents had a tremendous influence on me. However, they didn't try to guide me into a particular profession. Of course, I know people who have said that they knew from the time they were six years old that they wanted to be a chemist, or a doctor, or what-not. I didn't have that experience. Even when I started college, I wasn't sure I was going to be a chemist. I took the introductory course in chemistry and did well in it. I really liked the class, Professor [John B.] Ekeley was a good teacher. The next fall I took another course in chemistry. Pretty soon, I discovered I was majoring in chemistry.

BROWN: You didn't feel there was any particular pressure from your parents to go in that direction?

BAILAR: Oh no, none at all. In fact, I remember one occasion my father talking with me about what profession I might follow. I don't think chemistry entered into it at all. We did discuss the matter of whether I would be a teacher or do something else. I vaguely remember that he said something to the effect that teaching wasn't a bad profession.

When I was a junior in college, I was called into in the dean's office one day. This dean was the same one from the time my parents were in school. He asked me what I was going to do when I graduated. I responded that I didn't know yet, that I hadn't decided. He gave me quite a little talk about why I should become a teacher. I remember him saying that the only people who are really happy are teachers! [laughter] I don't know if that's true, but I remember that conversation quite well. By the time I started graduate school I had decided I would be a teacher.

BROWN: Was your choice of a chemistry major influenced by any particular teacher that you had in college?

BAILAR: No, I don't think so. Professor Ekeley who gave the lectures in general and organic chemistry, which I took in my sophomore year, was a very good teacher. However, I doubt that he influenced me to become a chemist. The fact that my father was both a good chemist and a good teacher may have had the greatest influence on me.

BROWN: To go back for just a moment, you were going to explain why you began high school at such an early age. Was this because you were tutored at home?

BAILAR: I think this was the result of a disciplinary measure. When I was in the fifth grade, a teacher complained to my father that I didn't pay attention in class, I was just sitting looking out the window watching the railroad yard across the valley. (I liked watching the trains switch

back and forth.) My father responded by saying that it was obvious that I didn't have enough to do!

The fifth and sixth grade classes were in the same room with the same teacher. My father's solution to the problem was to put me in the sixth grade, assuming that the challenge would keep me busy. I moved up to the sixth grade and I guess I behaved myself because I finished the sixth grade that year. The next year I went to seventh grade, and again my teacher complained to father that I didn't pay attention. It seems I was always "wool gathering." Again he replied, "Well, he doesn't have enough to do! Give him something to keep him busy!"

My teacher suggested that I take arithmetic with the eighth grade. In that year I did all the seventh grade requirements plus the eighth grade level arithmetic. That kept me busy but it simply compounded the problem of being ahead. The next year when I was in the eighth grade, I had already completed all the arithmetic lessons for that grade. Somebody suggested, (I don't know who), that I should study at home during the summer and then start high school. This is what I did.

I don't particularly remember that I studied a great deal that summer. My parents quizzed me on English, history, civics, and a few other things. The net result was that I entered high school two years early, having passed the fifth to the eighth grades in two years.

BROWN: Let us go back again to the point where you were finishing college. How did you decide to go to graduate school?

BAILAR: Before I graduated college, I decided to go to graduate school. By this time I was getting very interested in chemistry. I spent another year at the University of Colorado, and took a Master's degree. This was largely the result of the fact that I had been elected by the Alpha Chi Sigma chapter to represent them at the Biennial Conference in Pittsburgh. This meant that I had to go back to my own chapter for at least a year. I think the year of graduate school at Colorado was very good for me.

The reason I went to Michigan and continued my graduate work seems like a silly reason now. When I was going to Pittsburgh to the Alpha Chi Sigma conference, I realized that I had never been in the East United States. I thought it might be a good idea spend the summer at some eastern school since I was already going to Pittsburgh. I sent for the catalogues and discovered that summer school at the University of Michigan started a week after the meeting in Pittsburgh. This I thought would work well so I decided to spend the summer at Michigan. I took three courses — all in physics.

I enjoyed Ann Arbor. During the summer I lived in the Alpha Chi Sigma house. I liked the boys there and this caused me to think about going to graduate school and getting my doctor's degree from Michigan. My life wasn't very thoroughly planned and this was the reason I went to Michigan.

I decided I wanted to be an organic chemist. At that time there were only three chemistry professors at Michigan who had graduate students — one in organic, one in physical, and one in analytical. This meant that I worked with Professor Moses Gomberg. In other words, I had no choice. If I wanted to be an organic chemist he was the person I had to work with. This suited me fine because he was a very famous chemist. I believed that I could learn a great deal from him.

I spent three years at Ann Arbor. Unfortunately, things didn't work out as well as I had hoped. I wasn't very happy there. In fact, I was quite unhappy most of the time. My research didn't go as well as I thought it should have; I didn't learn as much from Professor Gomberg as I thought I would. He was a very fine teacher but not a very good person to teach graduate students. Instead of teaching us, we were his "hands" doing exactly what he told us to do. He would come into the laboratory five or six times every day and ask each one of us, "What have you done since I saw you last?" Often that meant sometimes two hours earlier. In such a short time, we didn't have any time to pursue our own ideas, all that we could say was, "I did all that you told me to do." He was working on free radicals and I discovered that I wasn't very interested in free radicals. I think this was a large part of my unhappiness. Needless to say the day that I got my Doctor's degree was a great day for me!

BROWN: It doesn't seem that it took you very long to get your graduate work done at Michigan.

BAILAR: I was there three years. I think that was more or less standard at that time.

BROWN: How large was Gomberg's group?

BAILAR: He would never take more than three or four students at a time. He'd take one or two new ones a year. During the two years I was there, he had a "post-doc" and three graduate students. This meant that he only had about thirty graduate students in his career.

Gomberg thought I should take a job in industry. He was quite displeased with me when I turned down an offer with Eastman Kodak when I heard that there was an opening here at the University of Illinois. Fortunately, I got the job here.

It's an interesting story how it actually happened. Professor B. Smith Hopkins, at the University of Illinois, wrote to Professor Hobart H. Willard, who was head of the analytical chemistry department at the University of Michigan, announcing an opening at Illinois. Professor Willard brought this letter down to my research laboratory, while I was out. He left the letter with one of the other boys who showed it to me when I came in. When I read it, I was as high as a kite. I thought it was a wonderful opportunity and just what I had to have.

Then Professor Gomberg came in. When I told him about the job, he said, "There is no use applying there. They don't need any organic chemists." He was quite discouraging. After that I took the letter back to Professor Willard. As I entered his office, letter in hand, he asked, "Are you interested in this job?" I said that I was, but noted that Professor Gomberg had not been positive. Professor Willard replied, "I'll get you that job." This was on a Saturday. There was no secretary there, but we sat down at the typewriter and typed a letter of recommendation. He proceeded to put it in an envelope, seal it, and told me to take it to the post office and mail it. Three or four days later, I got a response from Illinois saying my letter had made a good impression but that they couldn't promise anything until there were other applicants. At that point all I could do was hope that everything would go well.

BROWN: What year was this?

BAILAR: 1928.

BROWN: Can you describe in more detail what the Illinois position entailed?

BAILAR: Teaching freshman chemistry. You had to do your own research before you could have any graduate students to work with you. It was the custom of the chemistry department to bring in two or three young people every year whose primary responsibility was teaching freshman chemistry for two or three years. For most people a promising job in industry would show up and so they would leave the university to pursue it. If I had been in organic chemistry, that would have been my fate as well. There was no place here for me as an organic chemist.

BROWN: In other words, they already had a full staff of organic chemists.

BAILAR: They had Roger Adams, C. S. Marvel, Reynold C. Fuson and Ralph L. Shriner — which was enough. I don't think I was a terribly good organic chemist, anyway.

BROWN: Going back for a moment, you came to Illinois to join the general chemistry teaching faculty which was synonymous with inorganic chemistry. Is that correct?

BAILAR: Yes. The inorganic staff handled freshman chemistry.

BROWN: Who did you report to? What was his title?

BAILAR: Professor Hopkins. He was Professor of Chemistry.

BROWN: Did the department have a divisional structure at that time?

BAILAR: Yes, they had one. However, Professor Hopkins did not officially have the title "Professor of inorganic chemistry". He was in charge of the inorganic chemistry program and the freshman chemistry classes. Now, you should know that there wasn't much inorganic chemistry. There were only two graduate courses. Well, one was an advanced undergraduate class and the other was a graduate course.

BROWN: Who else was on the staff that taught general chemistry besides Hopkins and yourself?

BAILAR: There was Leonard F. Yntema, Harry C. Kremers, Ludwig Frederick Audrieth — who came the same time I did — Miss Virginia Bartow, Miss Rosalie M. Parr, and there was a young man by the name of Donald S. Villars, who came a year or so before I did and left soon thereafter.

BROWN: Were you given a laboratory? Where was your office that first year?

BAILAR: It was in the far corner of the third floor of Noyes Laboratory. I was there a year and a half, at which point I moved around somewhere in the vicinity of this office. Professor Yntema was scheduled to move over to the annex where I had built him a still. Just about that time he was offered the position as head of the department of St. Louis University. Kremers took a job in industry. Audrieth and Lawrence L. Quill (who had joined the staff after I did) both went to Europe on fellowships. There was nobody to sit in the Chemistry annex office but me. Thus, I was sent over there to take charge of freshman chemistry.

BROWN: Noyes Lab was constructed in 1916 so even in 1928, 1929, this was still a comparatively new building.

BAILAR: Oh yes, it was considered one of the buildings of chemistry in the whole country.

BROWN: When did you move into the annex?

BAILAR: In February 1931, in the middle of the 1930-31 school year.

BROWN: Did you become the director of general chemistry then?

BAILAR: Well, Professor Hopkins was the real director but after a while, I sort of grew into that job and he eased out of it.

BROWN: Can you describe how you felt in those early years about the relationship in your career between your functions as a teacher and a researcher? Did you have the idea that you were coming primarily to teach or to do research?

BAILAR: I came primarily to teach. During my last year at Michigan I remember talking to one of the other graduate students about my desire to get a job at a small college where I could teach. Gradually, as the year wore on, I began to think that I'd rather be in a big school located in a small town. I didn't want to be in a big city. As you can see, this job at Illinois was just perfect for me.

BROWN: Was there a sense when you came that you were going to be expected to do more than just teach — to do research for example?

BAILAR: Oh, yes. The first day I was here I was told to meet with Professor Adams. He spent about an hour with me and among the things we discussed an important question was about the sort of research I planned to do. I outlined the program that I had in mind. He said, "Well, that sounds all right." Then he sent me up to Professor Hopkins who also asked me what kind of research I planned to do. Again I had to go over my research. I'm not sure if it was on that day, or sometime later, but Hopkins pointed out to me, that while I had a degree in organic chemistry and it would be nice if I would work on rare earth alkyls, to distill these and get a separation of the rare earths. Personally, I didn't accept that invitation because other people had tried this and ...

BROWN: It took a long time to make the first rare earth alkyl.

BAILAR: That's right, yes. And they're not volatile either.

BROWN: How did you decide on a research program? When did you first get interested in coordination compounds?

BAILAR: Even as a graduate student at Michigan, (remember I wasn't that interested in free radicals), I was interested in isomerism and molecular rearrangements. When I came here, my idea was to work on molecular rearrangements, isomers. One day I was teaching freshman class and we had on the board the equation for the reaction of antimony chloride (SbCl_3) and water to SbOCl . One of the students in the class said he wanted to ask me a question about that antimony hypochlorite [antimonyl chloride]. I said "that's not a hypochlorite". Again he said "OCl.. hypochlorite." "But" I said, "this is not a hypochloride." "Why not", he asked, "it is OCl"? So I explained the difference to him between an oxychloride and a hypochlorite.

While I was discussing this with this student, it suddenly occurred to me that if we had a metal with a valence of 1, and the same metal with a valence of 3, we could form isomers. We would have a hypochlorite of the monovalent metal and the oxychloride of the trivalent metal. As soon as the class was over, I went to the library. I found thallium (valences 1, 3). Then I set out to make these two thallium compounds. Well, you can't make thallius hypochlorite because ... [Some conversation not recorded]

[END OF TAPE, SIDE 1]

BAILAR: No matter what I was doing, mowing the lawn, waiting for the bus, or lying in bed waiting to fall asleep, I'd think about possible inorganic isomers. Most couldn't ever be formed, but I found it a very stimulating hobby.

You can't go very far in inorganic chemistry, and especially with the subject of isomerism, without getting into coordination problems. So I began to read the papers by Alfred Werner and Paul Pfeiffer and other well-known inorganic chemists. The first piece of research we ever did on this subject was a Walden inversion, which happily turned out well (1). You take an inorganic optically active compound and convert it into another compound with the opposite configuration.

BROWN: And that was a cobalt compound?

BAILAR: Yes. I had an undergraduate student under me who did that work.

BROWN: So where did your research go from this point?

BAILAR: I got interested in coordination compounds through isomerism. We synthesized some new compounds — which was not always easy — and did some other experiments in coordination compounds. Mostly it was isomerizations, stereo-isomerization particularly. We did do some work on inorganic polymers at one time, but that didn't amount to very much. I was glad to get back to stereochemistry.

BROWN: When you first came to Illinois it was at the rank of instructor. How long did you stay at that level?

BAILAR: I was an instructor for two years. Then I became an associate for five years. This was during the Depression and there were no promotions of any sort at the university, no raises in pay. In fact, there was a pay cut. Fortunately, prices went down too so the cut in pay didn't hurt me very much. Seven years after I first arrived here, I became an assistant professor. The man who came here after I did was caught in the Depression; there wasn't enough money to keep everybody on, so he was let go.

BROWN: When you were a young faculty member who did you turn to for advice? Who among the faculty members in the department or in the university was your mentor?

BAILAR: The person I went to for advice was "Speed" Marvel. He was ten years older than I and an assistant professor at that time. When I had a problem I'd go and talk with him. His judgment was sound and he gave good advice. I've always felt that my progress here was due more to him than to Professor Hopkins. Speed was a fighter and a pusher and Hopkins wasn't.

BROWN: What was your relationship to Hopkins?

BAILAR: It was a very friendly relationship. I realized that for many years I was, and would be, working under his direction, his guidance. He was the boss of the inorganic group so I tried not to overstep my authority, to confer with him when there were decisions to be made about the freshman program and so on. Clearly we got along well because when he came to the fourth edition of his textbook (2), he asked me if I would work with him on it. We got along beautifully during this project. Two or three times a week I'd go to his office to talk things over. Then we'd do some writing and we'd get together again.

Hopkins was rather a sensitive man. I soon learned that I should not criticize something that he had written. I discovered that I could write something, show it to him and he'd say, "That's fine." But if I had said to him, "Now, what you've written is not very good," I quickly learned that he didn't like it. Early on that project, he said, "Well, I'm retired and I have plenty of time to work on this. You don't have so much time; let's wait till the book is finished before

we decide how we divide the royalties." We worked well over a year together, not knowing (at least I didn't know) how much royalty I was going to get out of this book. However, I trusted him and he trusted me. Finally, when the book was finished, wrapped up and sent off, and he said, "Now let's decide. You write on a piece of paper what proportion you think you ought to have and I'll write on a paper what proportion I think you ought to have and then we'll trade papers." I wrote 20%, he wrote 30%. We agreed on 25% for me. I cite this simply to show how we trusted each other and that we had a good relationship.

BROWN: This was a revision of an existing textbook of which Hopkins had previously been the sole author. Did the book go through several more editions following that one?

BAILAR: It had one more edition. I did the fifth edition by myself after Hopkins died (2).

BROWN: You mention that Hopkins had retired when you were working on that fourth edition. Who became the faculty leader of the inorganic and general chemistry group when Hopkins stepped down? Did you become that person?

BAILAR: Yes.

BROWN: What year was this?

BAILAR: I think 1941.

BROWN: Who was on the faculty at that time?

BAILAR: Audrieth, George Thereld Moeller; Miss Bartow and Miss Parr were still here. Michael J. Copley was gone, I don't remember who was here aside from those people. Quill had gone.

BROWN: How did the other parts of the department's program in inorganic chemistry develop during that time? Wasn't Audrieth a non-metal chemist, for example?

BAILAR: Yes, Audrieth first worked on nitrogen. Later he got into phosphorus chemistry. Moeller was interested in the rare earths, taking over Professor Hopkins' area.

BROWN: When did you take your first graduate student?

BAILAR: 1931 or 1932. The first two students I had were officially signed up with Professor Adams. I was not on the graduate faculty at that time so they assigned them to Professor Adams who subsequently turned them over to me. The first one was an organic chemist, who was given a molecular rearrangement problem. The second one was an inorganic chemist. After working with these students, I was admitted to the graduate faculty and had my own students.

BROWN: How was research funded at that time? Were there external sources of support?

BAILAR: There were some university fellowships and we had a little money from Du Pont. There was a fellowship called the Carr Fellowship, but most of the graduate students were teachers, half-time teaching assistants. In terms of supplies and equipment, if you wanted some chemical, you went to the store room and got it. You didn't have to sign up for it. If you needed a piece of equipment, and the department could afford it, they'd buy it for you. If they couldn't afford it, you did without. There was no "outside" money to speak of.

BROWN: Who made those decisions? Did it all go through Professor Adams' office?

BAILAR: Yes. The business office in those days consisted of one man — Carl F. Miller. If you wanted a piece of equipment, you'd have to go to Adams. If he felt you were justified, he'd approve it.

BROWN: For example, didn't you need a polarimeter?

BAILAR: No, there was a polarimeter here; if there had not been one, I suppose I would have had to plead for one. The polarimeter we had was very good. It was a little hard to use because it was a great big thing. It had a big arc lamp that was later replaced by a big incandescent bulb. To get a wavelength, you turned a knob and moved a prism. Then you had to readjust the whole thing to get to get to zero point back to zero. However, you could read it to a thousandth of a degree once you got it set up.

BROWN: What was the average group size? How many students did you have during the 1930s? What would have been the largest group in terms of the number of students that you would've had at any point?

BAILAR: In inorganic chemistry, probably six or eight, maybe ten — if it was a big group. Organic chemistry had larger groups. Adams would have maybe 20 students. Marvel had almost 20. In comparison, 1940 was a big year for us in inorganic. Audrieth had four students to take Doctor's degrees; and I had four. That was eight Ph.D.'s in one year. We thought we'd really accomplished something.

BROWN: There weren't very many centers of inorganic chemistry during the early part of your career. Illinois seemed to become a center at that time. Hopkins was the leader. Tell us about how Illinois came to be a center.

BAILAR: When I came to Illinois, I think there were only three or four schools in the U.S. where you could get a Doctor's degree in inorganic chemistry — Cornell, Chicago, California and Illinois (it is possible there were one or two others). When I was a student at Michigan, Professor Leff taught a course in inorganic for graduate students. But, it never had more than two or three students and we didn't do any research. Thus when I arrived, I found that Illinois was a very different situation than what I had been used to.

Professor Hopkins was very generous. Some students who had come to Illinois planning to work for him would change their minds after a semester and decide they wanted to work for Audrieth or myself. Professor Hopkins never complained about this. When I was there, he never had a big group and he didn't seem to attract many students. In fact, in his last years, he did not have any students at all. Audrieth, Mohler and I began to have some.

BROWN: Where did you publish your work?

BAILAR: The Journal of the American Chemical Society [JACS]. That was the only place one could go.

BROWN: What literature did you read? Were there international journals that specialized in inorganic chemistry?

BAILAR: Yes. I read the journal literature including Werner's work and people who followed him. I read work from the British Chemical Society and the American Chemical Society. Occasionally, one would find an article in an Australian journal but the big source of information was either the German literature or JACS.

BROWN: It must have been a different situation then because of the small numbers of people who thought of themselves as inorganic chemists. Without a division of inorganic chemistry, how did you communicate with colleagues? What were the major mechanisms of communicating with other schools?

BAILAR: We would give papers at the national ACS meetings. There was no division of inorganic chemistry. Rather we were lumped together with physical chemistry. The Division of Physical and Inorganic Chemistry was where we would present papers but there were never very many people there to hear your talk. Essentially, we didn't communicate very much with each other.

BROWN: Inorganic chemistry was more flourishing in Europe. Did you write to colleagues there very much?

BAILAR: No, I didn't do that at all. I read about the British and German work but I didn't have any personal contact with those people at all. We were on our own here.

BROWN: Before we talk about the way in which inorganic chemistry and international chemistry developed, let's discuss the university here. We have talked about internal aspects of the department of chemistry. Was the department considered to be one of the strong departments of the university?

BAILAR: Yes. Noyes came in 1917. He was a very strong leader. When he retired in 1926, Adams took over and he also was a strong leader. Between the two of them, they had built this up into a very powerful department. Noyes brought in people like Worth H. Rhodabush, Marvel, and George L. Clark. He was very particular, choosing strong people to join the staff.

BROWN: What kind of relationship did chemistry have with the departments on campus? Would you characterize the environment as more collegial?

BAILAR: It's hard to tell. I suppose it was. Of course, the university only had 10,000 or 12,000 students when I came, which meant that it was easier to meet people. I was one of the active staff members and I soon began to get committee appointments which enabled me to meet people from many other departments. I haven't been on a committee outside the department in 15 years so I don't meet new people very often. However, it is hard to compare that period with today. I suspect that it was easier, though, to be with other people at that earlier time.

BROWN: What sort of interaction with industry did you have as a faculty member in the 1930s and the 1940s? How did the department relate to industry in that period compared with the way it does today?

BAILAR: There was very much less contact. Adams and Marvel were both consultants at Du Pont. Adams was also a consultant at Abbott Laboratories. I wasn't conscious of any interaction with industry at all until I was appointed to take charge of the student placement. Then, of course, I met all of the industry representatives when they visited the campus. I learned what their companies were doing as an essential part of my job of placing students. Marvel had handled placement work before me but he did it on a very informal basis. He couldn't do anything on a formal basis and didn't really go even half-way for the students. I was given an extra girl in the office to handle appointments and the related work.

That placement work turned out to be a very interesting job. As I got acquainted with the industrial people I would find myself saying, "This student would be interested in that!" or "This man would like that student!" Then I'd go to the laboratory, find the student, take him aside and say, "Look, I want you to see this man because you'd like him or he'd like you." Frequently, their response was, "I don't even have a necktie!" I kept some extra neckties in my office for just this purpose. One student was in the hospital recovering from an appendicitis operation when a prospective employer was visiting. I took the personnel man in my car to the hospital in order to meet the student.

BROWN: You took your job very seriously!

BAILAR: I did indeed. I thought it was a great job and I loved doing it! However it got to be so big that I finally realized that I could either be a placement man or a chemist, but not both. This meant that I quit the placement job.

BROWN: You were doing that job along with running the general chemistry program and conducting your own research. This must have been a very busy time. What were the years you were doing the placement work?

BAILAR: From the middle 1930s until 1951. At that time I also had the title "Secretary of the Department." That really meant that I did whatever Roger Adams asked me to do. I had administrative charge of the machine shop, microanalytical laboratories, and the summer school. When we were building the Roger Adams lab, I took the architect on a tour, trying to teach him what chemistry labs should be like.

I had a tremendous number of things to do. I used to get over here before 8 a.m. in the morning, stay until noon and be back before 1 p.m., in case some teacher couldn't show up and I had to find a substitute (or teach the class myself). Then I'd come back in the evenings to work. One consequence was that much of my students' research didn't get published. I had time to supervise them but I didn't have time to write up papers. In fact, right now, I'm writing one from a student who took her doctor's degree in 1951. It's still good chemistry and it is work that hasn't appeared in journals yet.

BROWN: I would like to ask you about your family life. When did you marry?

BAILAR: I married Florence in 1931. She had been an undergraduate student at Illinois. She took a Master's degree here in 1928. She then went to Rochester, NY to work in a hospital as a biochemist. After a year, she came back to Illinois where I met her here in the chemistry department. We were married two years later.

BROWN: When were your sons born?

BAILAR: The first son (John C. III) was born in the fall of 1932, and the second (Benjamin F.) in the spring of 1934. They're just eighteen months apart. Florence and I were very happy together.

She had a double major as an undergraduate student in chemistry and math. When World War II started there was a need for teachers. She took a job in the math department where she taught for about 20 years. When she started both the boys were high school age and could get along. She always scheduled her classes in the morning. The boys would go off to school and she would come over to the university to teach. By the time they got home for lunch, she was home too. It worked out very well.

BROWN: Where were you living?

BAILAR: We bought a small house on Pennsylvania Avenue just before we were married. We couldn't actually buy it because a man and a woman who weren't married couldn't buy a house jointly according to the law. We had everything done except for the final signatures. When we came back from our honeymoon, we moved into this house as had been arranged, and on that day we signed the papers. A few years later, after the two boys were born, we decided we needed a larger house. We bought a house almost across the street from where we were living where we stayed until Florence died in 1975. I lived there by myself until I married Catherine. Of course, the boys were grown up and gone by that time. Likewise her children were also grown. It was just the two of us living in this big ten-room house. Catherine didn't really like

the house, I don't think it was her kind of house. After a year or two, we decided to buy a smaller house which was further out from the university. I wish we were closer to the campus, but you can't have everything.

BROWN: I suppose you don't walk or ride your bicycle much these days. Earlier on, you must have walked or ridden your bike all the time.

BAILAR: Oh, yes. I didn't ride the bus much until recent years. The bus comes just a block from our house and lets me off a block from the chemistry building; it's very easy, so mostly I use the bus.

BROWN: I want to ask another question about industry — something I remember reading in the Tarbells' book about Roger Adams (3). It seems that Adams started a chemical synthesis operations during World War I that provided specialty chemicals which could not be imported. The business continued after the war through summer work or part-time work. Were you ever involved in that?

BAILAR: In a minor way. After I had been here some time I persuaded Speed that we ought to have an inorganic chemist and make inorganic chemicals. We did this for two or three years. Our biggest project was making mercuric sulfate for Professor [William C.] Rose in biochemistry. He was using mercuric sulfate to precipitate amino acids. But we made some other inorganic compounds too.

BROWN: Were those then sold to Eastman Kodak?

BAILAR: The organic compounds were sold to a variety of people outside the university. That was the original purpose of course. As you pointed out, during the war one couldn't import these chemicals. This organic manufacturing also had a group of ten to fifteen graduate students who would work all summer, at 40 cents an hour, as well as earn credit in organic preps. Gradually, as the American chemical industry built itself up, the need for our chemicals declined. Eventually, there was a feeling that we were competing with industry, and the whole thing was dropped. It was a very interesting project while it lasted. It was like the image of the days of alchemy when you went down to the lab and watched all these big 22-liter flasks boiling away.

[END OF TAPE, SIDE 2]

[SECOND INTERVIEW — 17 JUNE 1987]

BROWN: Let's start by talking a little bit about the history of your involvement with the American Chemical Society. When did you first join the American Chemical Society?

BAILAR: I joined when I was still a graduate student at Michigan. At that time, I had no intention of becoming highly active in the American Chemical Society. People at Michigan didn't have much interest in the a national organization. Professor Willard attended the national meetings on a fairly regular basis, but nobody else on the staff at Michigan did.

As you might understand then, it just didn't occur to me that this was an important professional thing to do. During the first spring I was here in Urbana, Professor Hopkins came to my office and asked if I was going to "the meeting." I didn't know what he was talking about. "What meeting did he mean," I wondered? Perhaps he meant some local meeting here on the campus. Instead, Hopkins told me he was referring to the national meeting of the American Chemical Society. I sort of stuttered and stammered a response but really I hadn't thought anything about it.

Hopkins was clear that he expected the department people to go. Of course I went and I rather enjoyed the meeting. I met some people that I had known before as well as made some new friends.

BROWN: Where was that meeting?

BAILAR: It was almost 60 years ago, so I do not really remember but I think it was in Minneapolis. I went regularly thereafter because I found it a worthwhile experience. In fact, I think I've missed only two national meetings in the last sixty years.

BROWN: Can you describe what those meetings were like? What was the general character? Also, was a national meeting ever held here in Urbana?

BAILAR: Yes, but long before I came here. In 1916, when this part of Noyes Laboratory was built, the ACS national meeting was held here to commemorate the completion of the new building. In the early part of this century national meetings were very small. At the 1916 meeting the organic section had held its meetings in room 103 of this building.

There weren't any hotels in Urbana to speak of and I remember Virginia Bartow telling me that the people who lived here in town took the delegates into their homes and entertained them during the week. Of course, that was well before I came but even after I began going to

meetings in the early 1930s attendance was still small. I attended one meeting in Chapel Hill, North Carolina, where we stayed in student dormitories. It was during spring vacation and the students had gone home for a week. Mike Copley and I roomed together at that meeting.

However, even if the size of the meetings is much larger today, the nature of the meetings was not very different. In other words, there were sections such as organic, and inorganic, which would meet together but the main activity — people giving talks — is still the same. Because the size of the meetings of course has grown enormously. You couldn't possibly meet in a place like Chapel Hill or Urbana today.

BROWN: Was there a strong divisional character to the meetings as well as the ACS at that time?

BAILAR: Yes.

BROWN: Which group were you associated with?

BAILAR: I was in the physical and inorganic chemistry division. When the divisions of the American Chemical Society were first set up in the early 1900s, physical and inorganic chemistry were lumped together as one division. By the 1930s, the physical chemists far outnumbered the inorganic chemists in the division so that practically speaking, the entire meeting focused on physical chemistry with hardly any attention paid to inorganic chemistry. Once in awhile, the physical chemists would allow a meeting which had some programs, or talks, on inorganic chemistry. The inorganic chemists were not very happy with this arrangement and wanted to establish their own division.

About this time I was elected secretary of the physical and inorganic chemistry and this seemed very strange because though I was working in inorganic chemistry, I had been trained as an organic chemist and still tended to think more in terms of organic preparative, descriptive chemistry. It was the plan to put an inorganic chemist into office but because of my background, I had even less in common with the physical chemists.

BROWN: What year were you elected?

BAILAR: It was in the early forties. The important thing to know is that, as the secretary, I arranged the programs. The plan, or at least the custom, was for the secretary to advance to the position of Chairman-elect and then become Chairman the following year. Over the period of three years, I went through this series of offices. [Chairman, Division of Physical and Inorganic Chemistry, 1950; ed.] During that time it became more and more evident to me that this

particular organizational structure was not a situation where inorganic chemists could be very happy. I talked with Therald Moeller about the matter and suggested that perhaps we should contact more inorganic chemists to see if there was any strong feeling for the creation of a separate division of inorganic chemistry. The upshot of our conversation was that we made a list of all the inorganic chemists in the United States that we knew. Together we compiled a list of about a hundred names.

In order to start a new division in the ACS there had to be fifty people who would agree to join. We sent a letter to these one hundred people. When I went to the Gordon Conference in New Hampshire that year I took a copy of our letter with me and laid it on the hall table with the suggestion that anybody interested should sign up. As a result of this and other efforts, we produced a list of about 150 people who said they would join the division if it were started. We met at a national ACS meeting and decided to form a division and I was appointed president. All new divisions are probationary in the ACS. Therefore, they do not elect their own officers that first year. Rather, the president of the ACS appoints division officers. I was appointed president. Although Jake [John C.] Warner, as the president of the Society, appointed the steering committee, I gave him the list of names which he agreed to without any question. With that we were off to a good start and the next year, about 1958, we became a full-fledged division.

BROWN: It sounds as though several years transpired from the time you first talked to Therald about it and the division first met.

BAILAR: Yes, that is right, it did take awhile. I didn't want to upset the apple cart by splitting the physical and inorganic division. However, the longer I thought about it, the more I realized that inorganic chemistry was not going to thrive as a subdivision of physical chemistry. It had to be on its own. Of course now, it's one of the larger divisions of the Society, and one of the most active. Inorganic chemistry is not the same today as it was at that time. It's expanded to include bioinorganic chemistry, and the metal clusters, or rather metallic chemistry.

You asked about becoming president of the Society. I was elected a counselor from the local section. Later I was counselor to the inorganic and physical division. After that I was counselor for the inorganic division. At one time I was also counselor for the division of chemical education. What this meant was that I went to the council meetings quite regularly. I usually didn't say anything in the meetings but, at least I was there and voting. One day, Byron Riegal, who was a member of the committee on nominations and elections, came and told me that the committee wanted to nominate me for the presidency of the Society. Well, I was just amazed! It never occurred to me that I could be president.

BROWN: What year was that?

BAILAR: Probably it was 1951. Anyway, I agreed to be a candidate. I was not elected. A couple years later, I was nominated again, and again I was not elected. In 1957, I was nominated for a third time. This time I was elected. I became president of the Society in the beginning of 1959.

It was a very interesting experience. People told me that by the end of October of that year, I'd be tired of the job. This turned out to be pretty much the case. The job takes quite a bit of time. You do meet interesting people and do interesting things but these are piled on top of your regular job. The workload gets a little heavy.

During the year I was president-elect, I got a letter from the New York section stating that it was a long-standing custom for the president to give the address to the New York section at its January meeting. I didn't want to upset any long-standing customs, so I went to New York and gave a talk. About the same time I got another letter from the Philadelphia section, again stating it was a long-standing custom for the president to speak to the Philadelphia section at its February meeting. Off I went to the February meeting in Philadelphia. These sorts of activities filled my time.

I don't think I was a very strong president. Frankly, I was told that the Chairman of the Board would be the person who really ran the society. My job was more or less an honorary position. Even so, it took quite a bit of time, but nonetheless I was glad to have it. I met some very interesting people and the job was very interesting.

While I was president, I was automatically a member of the board. It was then that I began pushing for a journal of inorganic chemistry. I believed there were enough inorganic chemists and enough research in the field to support a journal. Al [W. Albert, Jr.] Noyes, who was editor of JACS and a past-president of ACS, rather sided with me. One day he said, "Well, I guess you're going to get your journal." Soon after the Board of Directors voted to start this journal. Just at this time Captain Robert Maxwell came to see me from Chicago. He told me that he was just starting a journal of inorganic chemistry — The Journal of Inorganic and Nuclear Chemistry. He asked if I would delay the starting date of our journal by one year. This way he could get his journal started. He believed that if we started the same time, it would simply kill his journal. I agreed but told him that the board had the final "say-so". The board did agree to a one-year delay which allowed him to get his journal off the ground and going.

BROWN: Was it a terribly competitive journal?

BAILAR: No, I'm afraid the standards weren't very high. I remember talking with him later about this. What I told him was that I didn't think his journal was as good as it should be because it accepted practically anything that was sent in. Maxwell said he would talk to the editor about my concern. I don't know if he ever did so but the journal didn't improve much.

BROWN: I would like to talk more about the start of your journal. It seems to me that you had a pretty strong hand in its founding. Wasn't one of your former students the first editor?

BAILAR: Yes, Bob [Robert W.] Parry.

BROWN: Were you influential in his selection?

BAILAR: Well, yes. The Board of Directors, of course, made the selection. We considered several people. When Bob's name was mentioned, I pushed for him because I felt he would do a good job. Bob's a very meticulous person and has high standards for work. However, I didn't suggest his name.

Bob was selected and got the journal off to a very good start. I've always been proud of the fact that I had played a role in starting the journal and in the selection of the first editor.

BROWN: When you finished your term as president of the ACS was that the end of your deep involvement with the Society?

BAILAR: Anyone who serves as president becomes a member of the council for life. I still go to the council meetings quite regularly. Not long ago I was a member of the committee on divisional activities for four or five years. I found that there were many things that I wanted to do when I went to the meetings, even though I found the work interesting and I liked to keep my hand in. I have finally asked not to be reappointed to the committee on divisional activities.

BROWN: Have you noticed any significant changes in the character of the Society over the last 20 years? For example, it seems that presidents are much more active today.

BAILAR: Yes, this is certainly true, especially for the last few years. The current president, Dr. Mary L. Good, has assumed a very active role in organizing symposia and presiding at meetings. Her picture is in the paper very frequently. Compared with the past, this is quite a change. For example, I was told quite bluntly when I assumed the presidency, that this position was an honorary one, and that the chairman of the board actually ran the Society.

BROWN: Is this because chemistry has become more socially conscious? In other words, that chemists are concerned about the image that "chemistry" has in our society.

BAILAR: Yes. We didn't worry much about the image of chemistry back in the early days, that is, my early days. I am conscious of the fact that even since I was a boy chemistry has had a bad image in some ways. For example, during the First World War, when I was still in high school, the U.S. government funded a laboratory at the Colorado School of Mines (where my father was a professor) to do war research. I remember hearing people speak about these chemists as "warmongers". For many years I've been conscious of the fact that many people think of chemists as the people who create explosives or poisons for the destruction of other people. I've tried to point out that while chemists may do these things, they also have done tremendously good things which increase our comfort and the length of life. I've tried to give a more balanced picture of the chemistry profession which has, in fact, accomplished a great deal of good for society.

BROWN: How did you get involved in IUPAC [International Union of Pure and Applied Chemistry]?

BAILAR: I was selected to attend a couple of their congresses. At one of these congresses, there was a decision to revamp their constitution. Copies of the existing constitution were given to many people. We asked to go over it and make suggestions. I spent quite a bit of time working it. I thought it was fun.

I had revamped the arrangement of the constitution and sent it to the secretary general, Dr. Rudolf Morf. Apparently, he was reasonably impressed with my work because I was invited to visit him in Europe. Morf introduced me to the man who was then the IUPAC treasurer. He wasn't really treasurer, rather he represented the bank and was more like a super bookkeeper.

The next thing I knew, I was the treasurer of IUPAC. I think Al Noyes had a good deal to do with this, although he never told me the story. He did tell me that they had deliberated on the treasurer position. He named a couple of the people that had been considered but said it was decided they would never become treasurer.

BROWN: Was this William Albert Noyes, Jr., who was the editor of JACS?

BAILAR: Yes, but his father had also been the editor of JACS. Al, Jr., was then at the University of Rochester. He had been president of the American Chemical Society [1947] which is how I knew him.

But to return to IUPAC, being elected treasurer sort of burst upon me. I had no expectation that I would be chosen to be the treasurer for the organization. I was treasurer for eight years. As treasurer, I was also a member of the executive committee. We had executive committee meetings about twice a year. This meant that I traveled a great deal.

I found IUPAC to be a very stimulating organization. It covered such a wide range of things. My job, took a great deal of time. I approved all the bills, and helped make the budget. Perhaps you remember that I described the previous "treasurer" as a bookkeeper. I discovered that the secretary general, Dr. Morf, had been the organization's actual treasurer even though he was not elected to do this job. I went to see the man who had been treasurer before me as soon as I was elected. I wanted to ask him to give me some instructions. He suggested we meet for lunch. We had a very pleasant lunch but nothing was said about the treasurer's job. Finally, I asked him what I was supposed to do. He gave me the checkbook and told me that was all there was to it.

Well, I discovered he had let Dr. Morf do all the treasurer's work. I didn't want to continue that arrangement. If I was treasurer, I was going to be treasurer! However, Dr. Morf, I think, didn't like this very well. He was a man who loved to have power. We got along pretty well, anyway.

BROWN: Was that your first trip to Europe?

BAILAR: Yes. It was 1957 and both I and my wife went. This was the first sabbatical leave I had had as well as the first time I had ever been out of the United States. During the fall semester we visited almost all of the countries in Western Europe. I especially visited chemical laboratories and talked to chemists, inorganic chemists in particular. The IUPAC meeting was in Rome. I gave a paper there, but I found the entire experience very stimulating. There were so many interesting people from Europe and Australia and South Africa, and all sorts of places, that I met during the meeting. It really opened my eyes to what a broad field chemistry is. I think IUPAC is a very important organization. I think if we had international organization in other fields we would learn a lot more about how foreign people think and act. As a result I attended every IUPAC meeting for several years.

BROWN: While we are discussing the topic of international relations between chemists, could you describe the program developed by Lou Audrieth during the fifties to bring German chemists to speak at Illinois?

BAILAR: Lou was born in Austria and his native language was German. He came to this country when he was still young. I guess he was just a baby, but his folks always spoke German at home. He joined the staff at Illinois the same time I did, the fall of 1928. He traveled a great deal and had been to Europe as recently as the summer of 1928. He visited Germany several times. He was late getting here because on the ship coming back somebody got scarlet fever and so when the ship came into New York harbor it was quarantined. They wouldn't let anybody off until the quarantine period was over, so he was maybe a week late getting here. In 1930, he went back to Germany for a year on some sort of a fellowship. During that year he

met a good many German chemists. Audrieth became interested in their work. He also spent some time in Italy and became interested in the Italian inorganic chemists.

Audrieth was always interested in European chemistry, especially German chemistry. In fact, I'll digress for a minute to say that he got himself into some trouble in the early thirties after he had spent the year in Germany. Hitler was just coming into power then and Lou was under the impression that Hitler was doing some good things for the country. He changed his mind later, but he gave some talks around here about Hitler's work in Germany. Even then, some of the Americans were very suspicious of Hitler, and Lou was questioned and later criticized for his opinions. Of course, it turned out that his favorable impression of Hitler was a little bit wrong.

BROWN: Didn't he translate a German text? Was it Walden's book?

BAILAR: That was before he came here. When he was at Cornell as a post-doc...it wasn't Walden.

BROWN: It may have been a book on non-aqueous solvents.

BAILAR: No, it was the Baker Lecture Series. George Fisher Baker left a lot of money to Cornell to bring And they had this German chemist, whose name I can't think of just at the moment. Lou became well acquainted with him and translated the book into English.

Lou was two years ahead of me in rank and experience. He had had two years of post-doctoral work and a National Research Council fellowship. The chemistry annex was built soon after we arrived and Yntema was supposed to take over the work in the chemistry annex. Just then he got an offer from St. Louis University to be head of the chemistry department. Kremers took a job with Rohm and Haas. Lou took that fellowship to go back to Germany for a year. Quill, who was older than I and had more experience, also got a fellowship in Germany. When the chemistry annex was finished, I was told to go over and assume responsibility. I was the only one left.

[END OF TAPE, SIDE 3]

BAILAR: I stayed in the chemistry annex for 35 years although my research lab was in the Noyes building. I probably didn't see my students as often as I should have. On the other hand, I'm not sure that is a bad thing. When I was a graduate student and Professor Gomberg came into the laboratory four, five, or six times everyday to check on what I was doing, I don't think I

learned independence as I should have. I just did what he told me to do. By not seeing my students too often, they had to think on their own and decide what to do next.

BROWN: I would like to talk about your students. You have had some students who've gone on to become quite eminent chemists, such as Fred Basolo, Daryle L. Busch, and Bob Parry. How many students have you had altogether?

BAILAR: I've had 90 graduate students who finished the Ph.D. degree. I had only two or three who started and didn't finish. I did have a few who took terminal Master's degrees. I don't know exactly how many undergraduate students have done research with me but I guess the number would be around 40.

BROWN: Have you had post-doctoral students over the years?

BAILAR: Yes, I've had 25 or 30. Twelve or so from Japan. I found the Japanese to be very well trained and hard workers.

BROWN: Do you have a certain kind of philosophy in approaching graduate education, research and teaching?

BAILAR: I've always felt that a research student should be given as much independence as he can take. Usually a student doesn't know what to start on, so I assign him a problem. After a while, if a student sees a side road that he thinks he's interested in, I encourage him to try it. This is how Daryle Busch got involved in the polycyclics that he's been working on for such a long time. He started that work while he was a graduate student. What got him started was something I said in the lecture of a course that he was taking with me. His interest was caught and afterwards he came up to me and asked, "What if we did so and so?" I suggested he try it, which he did and it developed into that whole area he's explored for many years.

Bob [Robert E.] Sievers was a very interesting student to have. He clearly wanted to do it on his own. I would stop at his desk and ask him how things were going. He would reply that everything was fine. He never would say he was having trouble with this or that. When he wrote his thesis, he thanked me for letting him do it his own way. I think this expresses my philosophy pretty well. I have tried to give the students every opportunity to do what they feel is interesting and what they feel they can do. Maybe this is because I haven't really felt very competent to direct them. I let them do it themselves.

I remember when Carl Rollinson took his degree he commented to me, "I'm just getting to the place where I can do research, and now you tell me I've got to go!" I said to him, "Well,

after all, you came to learn how to do research and now you've acquired those skills!" He wasn't quite as sure as I was at that moment.

This was quite different from anything I had been through as a graduate student. All of Professor Gomberg's students worked on free radicals. Even if a student thought he wanted to work on molecular rearrangements, Gomberg wouldn't permit him. Frankly, I wasn't very much interested in free radicals. Thus, my graduate work wasn't as happy as it could have been.

BROWN: Who were some of the other students that were memorable?

BAILAR: Perhaps Fred Basolo has become the most prominent. Panchenison (?), who was extremely interesting to have around, is another one who's done very well. Incidentally, he was here only two years. He took a master's degree at Tulane. His wife had a job down there in Texas and she didn't want to come north. Naturally he was eager to finish as quickly as he could. So he worked day and night, summer and winter, vacations and everything. And in two years he was gone. He went back to Tulane and taught there until he retired.

The first research student I had was an undergraduate, Bob [Robert W.] Auten, but he did an outstanding piece of research which set me off on a track that I've followed for a long time. James P. McReynolds was a very good student. He taught at Ohio State after graduating. Unfortunately, he died when he was still a young man. Eugene H. Huffman was one of my early students. He did very well. He went to the University of California eventually, where he worked there until he retired. Several have gone into industrial work. Dick [Richard L.] Dalton is vice-president of one of the Du Pont divisions now. The first graduate student I had was an organic chemist. This was Theopoulos Parsons. Speed Marvel didn't like that very much. The department was "divisionalized" and he told me "out and out" that he did not like me working with the people doing organic chemistry. In other words, he wanted them under his direction. It was about that time that I was becoming more interested in inorganic chemistry and willing to call myself an inorganic chemist. The rest of my students were inorganic majors. Jim [James V.] Quagliano was another who worked with me and he was a very good student.

BROWN: You have another student at Du Pont — Bill [William C.] Drinkard.

BAILAR: Yes, Bill Drinkard was one of my students. He was a very quiet sort of boy. Not as impressive as some of the others at first glance but he's done tremendously good work. [Alfred L.] Oppenard and [Joseph H.] Balthis went to Du Pont. [C. Elmer] Wymore went to Dow; he was a very good student. He prepared a diphosphine, and made complexes out of it. We published a paper on unusual coordinating agents (4). Of course, diphosphines now are very common and everybody does diphosphine. In his time, this was rare. [Leonard V.] Interrante was another of my students who has done very well. He worked for General Electric for some

years and now he's teaching at Rensselaer. He tells me he's writing a text book on descriptive inorganic chemistry. Gene [Harold Eugene] LeMay was another one who did very well.

Bob [Robert C.] Brasted and Lealyn B. Clapp both became very prominent for their teaching. They weren't research people particularly, but they both contributed a great deal through their educational activities. Henry Holtzclaw worked jointly with [Herbert A.] Laitinen and myself. He wants to retire now. [Donald F.] Peppard was a good student. He did some extremely nice synthetic work with me. He spent most of his career at Argonne Laboratories. Many of my early students have not only retired, they've died!

BROWN: How many of these people have gone into academic life?

BAILAR: About half. I don't know how much influence I had on this. Most of Lou Audrieth's students went into industry; lots of mine went into teaching. Whether they chose to work with Lou because they were interested in the kind of thing he was interested in, or whether he influenced them, I'm not sure. The same question applies to me. It may be that these students saw that I enjoyed teaching and academic life, and subsequently thought that is what they would like to do. I don't know.

BROWN: The choice of problems may be influential too. I sat in on Lou Audrieth's course on non-aqueous solvents. There was an emphasis on industrial application of these things. He had several patents. He was more oriented toward that approach.

BAILAR: Yes, this is true. Whether the students went to him because they felt that or because he influenced them that way, I don't know. You never know about these things. Almost exactly half of my students have become teachers. They haven't all continued. Two or three of them quit chemistry entirely. That always disappoints me a little, but they have to do what they want to do.

BROWN: Have we neglected to talk about some things that you think are important?

BAILAR: We might mention a little bit about the conferences on coordination chemistry. I've been very active in that group. The first meeting of that group that I went to was its fifth one. The meeting was in Rome in 1957. I've been to every one since then except two which occurred in London and Toulouse. The London meeting occurred during my year as ACS president and I had to be at an ACS meeting instead. I was sick for the French meeting. I've been to all the rest, however.

BROWN: How often are they held?

BAILAR: Originally, they were supposed to met every other year. Then the Japanese invited us to come to Japan for a meeting. The European chemists objected rather strenuously. They said it was too far, and they couldn't afford to go. That meant it would have been four years between meetings for them. I suggested that we stick the Japan meeting in between years, in a year we wouldn't normally meet. I believed it wouldn't do any harm to meet more often, and if the Europeans couldn't go then it would only be two years between meetings. They bought this idea. It turned out that there were a lot of "far distance" countries that wanted to have meetings. What it really has boiled down to since is that we meet one year in a Western part of the world and the other year elsewhere. It was in China this year. Next year it'll be in Portugal. Thinking about the China meeting, I recall that after some years of having these meetings it was voted not have any more meetings in far distance countries. It was presumed that all the far distance countries had had meetings. Then, almost immediately, the Chinese showed up and said they wanted to have a meeting in their country. So the motion was rescinded and we will be meeting in China next month.

BROWN: Has the character of these meetings changed much over the years?

BAILAR: The host organization that arranges the meeting has complete control. Some of the meetings have been restricted to a certain number of people. Others have tried to get as many people as they could. So it's hard to define a change over time. Every meeting is different. In general, they've grown larger. It seems to me that they haven't had a meeting for several where the number of people has been restricted. A small meeting means you get more discussion, more crossing of international ties and so on. A big meeting with more people there means that you spread the word a little further. The organization, if you can call it an organization, is run by a permanent secretary, Stan [Stanley] Kirschner. He is one of my former students. He doesn't really "run" the organization; rather, he helps the host organization get things put together. At each meeting they have what is sometimes called the advisory meeting, or sometimes the executive committee, which plans future meetings. I've been a member of that executive or advisory committee every year since the first meeting in Rome. That was only the fifth meeting of the organization and the group was less formal. I met Joe Chapman one day and he asked me to have lunch with Selane and Merriman the next day. I agreed and asked what was up. He said he just wanted to plan future meetings. So the four of us had lunch and decided where we'd meet the next time. This caused some trouble because Madam Chekutasky from Poland had come with an invitation for us to meet in her country. When she found that we'd already made the decision without even knowing about her invitation, she became quite upset. Since that time, and because of that incident, all of the countries represented have been represented at the meeting of the executive committee. A year ago there were some thirty people on the executive committee.

BROWN: That's quite a large group.

BAILAR: The truth of the matter is that Stan Kirschner organizes things so well that the committee simply "okays" what he's decided and lets it go at that. When we met in Boulder, Colorado several years ago, he had just had a heart attack and a bypass operation. I told him, "Stanley, you ought to have somebody here as your assistant." "Oh no, no, no," he said. I said, "maybe you'll have an accident, or another heart attack, then what?" He said, "I already have somebody in mind." So he asked someone from Holland. We now have an assistant permanent secretary.

BROWN: He's the heir apparent to Stan's job?

BAILAR: That's right. But I don't think he has taken any great part in it so far. Stan just runs things himself. He does it very, very well. I'm very proud of Stan.

BROWN: You've had a lot of very good students.

BAILAR: Yes, another of my students that I should have mentioned is Ronald Archer. I'm very proud of this group of people. I'm sorry that I don't still have graduate students doing research with me. When the second of them was elected president of the American Chemical Society, Herb Butovsky mentioned to me that that was quite a record — two of my students [Parry and Basolo: ed.] as president of the ACS. I said, "Well, if I had the chance, I would do it all again." He just shook his head. One of my students is a nominee this year — Clayton F. Callis. Of course Bob Brasted was a nominee. Alan McClelland was chosen by the Committee on Nominations and Elections, but he was not chosen by the council. The Committee on Nominations and Elections chooses four people and the council votes on those four and chooses two of them. I'm hoping that some of my other students will get to this exalted position and be president of the Society.

BROWN: I see you have on your wall this family tree of chemists. It shows the legacy of your genealogy back to Lavoisier through Gomberg and Bailar. There are already several generations after you.

BAILAR: I sometimes thought if I had time, I would try to assemble a list of the students of the students of the students; it's down to the fifth generation now, I know.

BROWN: The turnover is pretty fast.

BAILAR: An academic generation isn't very long. There must be hundreds of them. Basolo, Parry, Busch, Archer and Sievers have all had lots of students. And they've had students and they've had students, so it goes. I wonder how far my influence runs down into there. I think I've influenced my own students, whether that influence has carried down to their students I don't know.

BROWN: That's an interesting question.

BAILAR: You never could find out. However, it's been a wonderful career, I've had a great time. I think the University of Illinois has been a very fine place for me to work. I've numerous opportunities to go elsewhere and be professor or head of a department or a dean. Florence used to complain that I didn't even think about them. Sure I thought about them, even visited the places but I decided I didn't want any of them. Sure, they offered me a better job than the one I have, at least superficially, that is — more money, more authority, but in the long-run, I knew I would be happier here. This is where I want to be.

BROWN: We've covered a lot of ground on these tapes, is there anything else we should touch upon?

BAILAR: I don't think so.

[END OF INTERVIEW]

NOTES

1. J. C. Bailar and R. W. Auten, "The Stereochemistry of Complex Inorganic Compounds. I. The Walden Inversion as Exhibited by Diethylenediaminocobaltic Compounds," *Journal of the American Chemical Society*, 56 (1934): 774-776.
2. B. S. Hopkins and J. C. Bailar, *General Chemistry for Colleges* (Boston: Heath, 1951 [4th edition]; 1956 [5th edition]).
3. A. T. and D. S. Tarbell, *Roger Adams: Scientist and Statesman* (Washington, D.C.: American Chemical Society, 1981).
4. C. E. Wymore and J. C. Bailar, "Uncommon Coordinating Agents. I. P,P,P,P'-Tetraethylethylenediphosphine," *Journal of Inorganic and Nuclear Chemistry*, 14 (1960): 42-54.