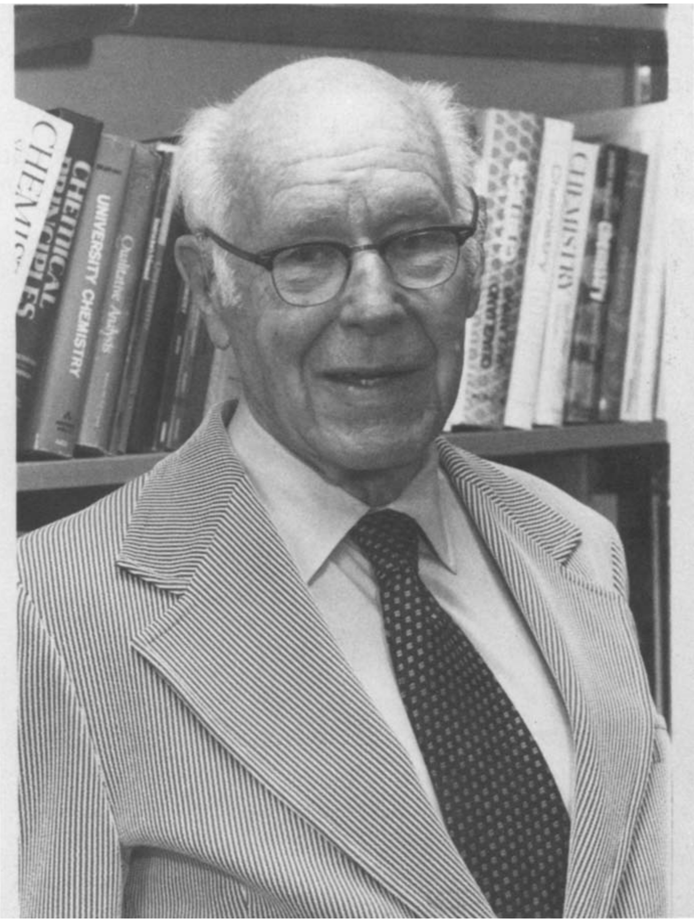
Coordination Chemistry Reviews, 106 (1990) 1–23
Elsevier Science Publishers B.V., Amsterdam



John C. Bailar, Jr.

John C. Bailar, Jr. was born in Golden, Colorado, on May 27th 1904. He was educated at the University of Colorado where he received his B.A. in 1924 and M.A. in 1925. He then obtained a Ph.D. from the University of Michigan in 1928. He subsequently joined the University of Illinois where he has spent his entire career, being Head of the Division of Inorganic Chemistry between 1941 and 1967.

John has received innumerable awards including the ACS Award in Chemical Education, the Priestley Medal of the ACS, the Frank P. Dwyer Medal of the Chemical Society of New South Wales, the Alfred Werner Gold Medal of the Swiss Chemical Society, and the ACS Award for Distinguished Service in the Advancement of Inorganic Chemistry.

He has served on many committees of the American Chemical Society, being its President in 1959. He also served as Chairman of the Division of Chemical Education (1947), the Division of Physical and Inorganic Chemistry (1950) and the Division of Inorganic Chemistry (1957–1958). He has also been active in IUPAC, being its Treasurer in the years 1963–1971.

John has served on numerous editorial boards, and has authored or co-authored 13 books, including multiple editions. He is the author of some 250 research publications in inorganic chemistry.

A CELEBRATION OF INORGANIC LIVES

INTERVIEW OF JOHN BAILAR
(University of Illinois)

A.B.P. LEVER

A.B.P.L. Let's go far back to the beginning of your life with your father and your initial interests in chemistry.

My father was a professor of chemistry at the Colorado School of Mines. He was a very good father, and he spent as much time as he could with me. He often took me to the laboratory where he was doing research. He taught me quite a lot of chemistry as we went along. I "helped" him by pouring things into the funnel, folding filter papers and doing things of that sort. I remember one incident which occurred when I was about in the first grade. I saw a bottle labeled H_2O_2 and I asked him why it wasn't just HO. He explained that the molecule had two atoms of hydrogen and two of oxygen. I understood this alright. Unlike many students, I never had any fear of chemistry. It just came naturally to me.

A.B.P.L. So when you got to university age, you decided to go directly into chemistry?

I didn't really think much about it. I took general chemistry when I was a freshman. I enjoyed it and I did well with it, so when I came into the sophomore year, I signed up for organic and for qualitative analysis. I guess that meant I was a chemistry major. I just never made any decision about it and never thought about anything else. I didn't think any other subject would appeal to me particularly, so there I was.

A.B.P.L. What was it like being a chemistry undergraduate? What were the laboratories like and the teaching? How did they differ from those of today?

I went to the University of Colorado which was about forty miles from home. I earned my own expenses by summer work and what I could pick up while I was at school. The laboratory was not awfully different from what we have nowadays. The ventilation wasn't as good. The odor of hydrogen sulfide and other noxious gases was pretty strong in the laboratories but other than that it was about like the laboratories that we have now for general chemistry. We didn't have anywhere near the equipment that we have nowadays. As I remember, we didn't have any quantitative experiments in general chemistry. It was all qualitative. Otherwise, it wasn't very different from what our freshmen do now.

However, I recall one experiment that indicates that safety precautions were not what they are now. The lab manual instructed me to put a little bromine into a test tube and drop into it a piece of white phosphorus "the size of a grain of wheat". When I did this, there was, of course, a violent reaction. The phosphorus shot out of the test tube and struck the ceiling, where it continued to burn until it was all consumed. Nowadays, it makes me shudder to think of allowing a beginning student to handle either liquid bromine or white phosphorus, to say nothing of mixing them.

A.B.P.L. Then you decided to do graduate work and that again was an automatic choice, I guess, into chemistry? Why did you choose Michigan?

Well, it was more or less an accident. I had been active in Alpha Chi Sigma, the chemical fraternity, and they had a convention in Pittsburgh. I was elected as a delegate to that convention. I had never been east of Colorado but once, and so I thought "If I am going to go east for this convention I might as well spend some time in the east and see what it is like". So I got the catalogues from several universities and I found that the summer session at the University of Michigan opened just two days after the close of the convention in Pittsburgh. It was close so I thought "Well, I will go to Michigan for the summer". After that summer, I went back to Colorado and took a master's degree. I had rather enjoyed the boys that I met at Michigan and so I went back there for my doctorate. I thought then that I wanted to be an organic chemist. Just why I came to that conclusion I don't remember. I suppose it was because organic was the big field, and I thought I would have a better chance of getting a job. But anyway, I majored in organic chemistry. It turned out that this was not what I should have done. I didn't particularly enjoy the organic. Three or four years after I got to Illinois, I discovered inorganic was much more exciting and fun for me. So I switched over.

A.B.P.L. Do you want to tell us a little bit about life at Michigan before we move on to the Illinois years?

One reason, I think, that I didn't enjoy my work at Michigan was that I didn't enjoy my general life there. The people at Michigan felt that there were only two universities in the United States — Harvard and Michigan. They sort of poopooed my having come from the University of Colorado which they referred to as a "shirt-tail school". Even the Michigan professors felt that there were no schools except Michigan and Harvard. Since I hadn't been associated with one of those, I didn't really amount to much. Also, I didn't enjoy the research which I was doing. Professor Gomberg hovered over us much too closely, I think. He came into the laboratory five or six times every day just to see what we were doing and ask us what we had done since he saw us last. We were just his "hands". I learned some chemistry from him and he was a fine gentleman. I enjoyed him personally but I don't think that he had quite the proper aspect on directing students in research. My own feelings, since I have been directing students, is to give them as much leeway as they can handle. Frequently, I have assigned a student a problem and as he goes along he finds side issues that he thinks are exciting or interesting. I tell him "go ahead". So it may turn out that when he writes his thesis, it's not what we had started out to do at all, but something that he developed on his own. This hasn't been true of all my students but it has been true of a good many of them. I have fostered that. I believe that it's fine for a student to think of things of his own and work on them. He needs instruction only when he runs into trouble or if he is obviously in a blind alley. The teacher needs to tell him it's a blind alley and he'd better start something else.

A.B.P.L. You were saying something about Gomberg taking only one student each year?

Yes, I should have said that Professor Gomberg never had more than three students in his laboratory. He would take one each year. The great majority of them had done their undergraduate work at Michigan. The plan was that these students would work for one year with Professor Schoepfle. If he thought they showed promise and were able, he would turn them over to Professor Gomberg for their doctorate work. Nobody had graduate students for doctor's degrees in organic chemistry except Gomberg. So, on average, they turned out about one Ph.D. in organic chemistry each year. Professor Gomberg, I think, had only 29 students during his whole career. The situation was not quite the same in analytical and physical chemistry. In those areas, most of the graduate students came from small schools in the midwest. If they wanted to do analytical chemistry, they worked with Professor Willard. If they wanted to do physical chemistry, they worked with Professor Bartell. There were other professors but they didn't have doctoral students. Only those three in the department — Gomberg, Willard and Bartell — had doctorate students working with them. I never could quite understand this, because Michigan had some very able people, but that was the way it was set up.

A.B.P.L. After Gomberg, what then?

In the Christmas vacation of my last year at Michigan, I went with another student to Columbus, Ohio to attend the organic symposium. This was quite an exciting experience for me because I met a lot of well-known organic chemists — people whose names I had known for some time. I enjoyed that meeting very much. Among the people I met was Professor Adkins of the University of Wisconsin. He expressed to me the opinion that he would like to have a student from Michigan come to work with him. When I got back to Ann Arbor, I told Professor Gomberg about this. He said that he couldn't send anyone to work for Professor Adkins. I asked him why not. He replied that if it was a good student "we would want to keep him here, and if he were not a good student, we could not recommend him to Professor Adkins". This implied to me, and still does, that he felt there was only one student or perhaps two students a year at Michigan that were worthy of going on for doctorate work in organic chemistry. This seems to me almost incredible because there were 10 000 students at Michigan. They had a lot of chemistry majors and I am sure that there were several every year that could have done graduate work to great advantage.

A.B.P.L. How did you decide what kind of job to get when you were leaving Michigan?

During my last two years at Michigan I was a teaching assistant in general chemistry, and I loved that job. I enjoyed the teaching very much, so I decided that I wanted to get a position teaching in a large university but, hopefully, in a fairly small town. So I began, during the latter part of my last year, to apply for positions in various universities. Professor Gomberg thought that I should go into industrial work. I had an offer from Eastman Kodak and I had to make a decision on that by a certain date. Two or three days before that date, Professor Willard had a letter from the University of Illinois saying that there was an opening for a young man in general chemistry. He brought the letter to my laboratory and since I was out, left it for me. When I came in and read it, I was tremendously excited. It was exactly the sort of position that I wanted. I showed it to Professor Gomberg, but he poopooed the whole idea and said, "Oh they wouldn't want you at Illinois. They have a lot of organic chemists there. You wouldn't fit in there at all", and so forth. So, terribly crestfallen, I took the letter back to Professor Willard. When I handed it to him, he asked whether I was interested. I told him that I was much interested and started to tell him what Professor Gomberg had told me — that there was no point in applying. He said he would help me get the job. That was on a Saturday morning. I explained to him I had to make a rather sudden decision, so he sat down at the typewriter and typed out a letter of recommendation, put it in an envelope and sealed it. While he was doing that, I wrote a letter of application. I took them both to the post office and put them in the mail. On Monday I had a reply from Illinois, saying that my letter had made a good impression but they couldn't give me a final decision until they heard from other candidates. On the basis of the statement that my letter had made a good impression, I declined the position at Eastman Kodak. This didn't please Professor Gomberg at all. He felt that I certainly should have taken it, and said, "Well I don't know whether we can find anything else for you now or not". But happily, a few days later I got the letter from Illinois offering me the position. As a matter of fact, the offer came in for $300 more than the original letter had suggested. I didn't take any time to decide. I immediately wrote a letter to Professor Adams at Illinois, saying that I would accept.

A.B.P.L. This was for general chemistry teaching but did it involve research work?

Oh yes, I was certainly expected to do research. Everyone at Illinois in chemistry is expected to do research. No restriction was put on the research that I would do. I could do anything I wanted. But of course since I was trained in organic chemistry, that's what I set out to do. I had worked on free radicals at Michigan but my chief interest lay in molecular rearrangements and isomerism, so I started doing research and I published several papers on organic rearrangements — the Cannizzaro reaction, the rearrangement of azoxybenzene and the pinacol–pinacolone rearrangement. I anticipated that I would spend the rest of my career as an organic chemist.

A.B.P.L. We should go back to the welcome that you had at Illinois.

I arrived in Urbana the day before registration for the Fall Term and I was instructed to go in to see Professor Adams. Registration time is a very busy time and I am sure he had many things to do, but he took about half an hour to tell me what to do and how to do things if I wanted to get ahead at Illinois. This was not just in my research. He did ask me about what research I planned to do, but he also gave me some personal points — where to eat and who to associate with and things of this sort. After about half an hour of this, he sent me to see Professor Hopkins, who was to be my immediate supervisor. Professor Hopkins greeted me warmly and spent at least half an hour saying the same sort of things that Professor Adams had told me — what to do and how to get along in the department. For example, he said "You can read the newspaper in your office if you want to — nobody will complain. You can do that if you wish but that isn't the way to get ahead." I realized right away that I was welcome here — that people were really interested in my getting ahead in the department and I plunged into work as hard as I could. I don't remember it, but my sister tells me that soon after I arrived at Illinois I wrote to my parents saying "I expect to spend the rest of my life at Illinois". And that is indeed what I have done. I have now been here for 62 years.

A.B.P.L. You mentioned something of the power of the professors at Illinois?

Soon after I came to Illinois, I was impressed with the fact that the faculty had a great deal of power over the lives of the graduate students. If a student wanted to do graduate work in organic chemistry, he was pretty well told where to go. That is, there were deals between universities. Professor Adams had a deal with Whitmore at Penn State that he would send one student there each year and Penn State would send two students to us each year. The professors chose the students and told them where to go. We had a deal with Minnesota — we sent them one, they'd send us one. We had a deal with Wisconsin — we sent them one, they'd send us one. That is not true any more. The professors don't take that aspect into consideration. The students usually apply to a lot of different schools and they go to visit the schools and decide which one they want to join for graduate work.

A.B.P.L. It is interesting that you had the job offer from Eastman Kodak and came to Illinois, and therefore Eastman Kodak's loss is our academic gain, but you also switched ultimately into inorganic chemistry which was a loss for organic chemistry and a gain for inorganic. Why did you choose to do that?

Well, as I have pointed out, I was particularly interested in molecular rearrangements and isomerism. I considered these to be parts of organic chemistry. But one day when I was teaching a class in freshman chemistry, we had on the blackboard the formula SbOCl — that is, antimony oxychloride. A student asked me about "that antimony hypochlorite". I explained to him "It's not a hypochlorite". He said "Well, OCl is hypochlorite isn't it?" I said, "Yes that's true, but this is not a hypochlorite". It suddenly occurred to me that if one formed the oxychloride of a metal with a valence of three and the hypochlorite of the same metal with a valence of one, he would have inorganic isomers. This is the first time I had ever thought of inorganic isomers. As soon as the class was over, I went to the library and began to look for a metal that I might use for the experiment; I found thallium. So I tried to make thallic oxychloride and thallous hypochlorite. Well, of course, you cannot make thallous hypochlorite because the thallous ion is easily oxidized and the hypochlorite ion is a strong oxidizing agent. As soon as you put them together, they destroy each other. But I spent some time on that before I discovered my error. Then I tried to make sodium selenosulfate and sodium sulfoselenate. (I later discovered that other people had tried this.) One can't make sodium selenosulfate. The selenium will not oxidize the sulphur from +4 to +6, so that failed also. But during the course of this research, about which I was so excited, I discovered that there were lots of isomers among the coordination compounds, so I turned my attention to coordination chemistry and have pursued that ever since.

A.B.P.L. How did this chemistry get funded? Presumably there weren't agencies that were giving you money to do research — where did the money come from for the chemicals and did you have people working with you at that time to help you?

The funding was entirely from the University. The National Science Foundation and other agencies of that sort didn't furnish money in those days. There was no great deal of bookkeeping about it. If I wanted a chemical, I went to the store room and asked the store keeper for it and I was given it. The same with apparatus — beakers, test tubes and all those things. You didn't even sign out for them; you just took them. When you were through with them, if they were still usable, you took them back. After the first year, I had some undergraduate students doing research with me. These were for the most part directed to me by Professor Marvel. He turned out eventually to be one of my very best friends and strongest supporters. So, yes, I had help.

The first inorganic student that came to me was a senior. He had taken my course in freshman chemistry during my first year and he liked it so much that he decided to talk to me about doing research. This was Robert Auten, who turned out to be an extremely fine student and very imaginative. He was one of the best that I have had. I suggested to him that we might look for a Walden inversion in coordination chemistry. He was very doubtful about it and didn't think that it would lead to any result, but he agreed to give it a try. Sure enough, we did get an optical inversion in the reaction that we studied; $cis\text{-}[Co\text{-}en_2Cl_2]Cl + M_2CO_3 \rightarrow [Co\text{-}en_2CO_3]^+ + MCl$. Since then, several other examples have been found, but that first one was the most exciting because it opened up a whole new area for me and really established my life as a coordination chemist.

A.B.P.L. Tell me about your work on the Walden inversion.

The reaction that we call the Walden inversion was named after the famous organic reaction which Paul Walden discovered. Some people have criticized us for referring to it as a Walden inversion for they say the mechanism is not the same as for the organic reaction. (We have an octahedron instead of a tetrahedron, and so on and on.) Of course, this is true, but Walden didn't know anything about mechanisms. All he knew was that he started with the *dextro*-rotatory compound and could convert it to either a *laevo*- or *dextro*-rotatory derivative. That's what Auten and I did with the inorganic compound. But in spite of that, people have stopped referring to this as a Walden inversion and have referred to it as a Bailar inversion. This is alright with me, although I don't think there is any importance to that distinction. In our studies of this inversion one of my students, Don Peppard, wanted to prepare a compound to which he had two chloro groups, two ammonia molecules and one ethylenediamine molecule, all attached to the same cobalt(III) atom. This was quite a problem in synthesis, but we worked it out to get all three of the possible isomers. Two of these have the two chlorines in adjacent positions in the molecule; one of these is chiral, and we partially resolved it. We never did get to studying the Walden inversion in reactions of this compound because by the time Don had finished the synthesis, I thought he had learned enough and had accomplished enough to warrant a doctorate, so he took his degree and left.

A.B.P.L. What kind of equipment was available?

Well, the only pieces of equipment we used were beakers, test tubes, flasks and such. Of course, we had a polarimeter so we could measure optical rotations. The polarimeter we had was a very large one — Schmidt and Haensch — and it was a little hard to adjust. One could change the wavelength on it, but once it was changed, it took a half a day to get the instrument adjusted so one could use it. Hence, we didn't change the wavelength very often. We simply stuck with the D-line of sodium which is in the yellow. Since our solutions were deeply colored — purple, green and red — we had to use very dilute solutions. Happily, these coordination compounds have quite high rotation so we were still able to see the two halves of the field and compare them for their intensity of light.

A.B.P.L. What kind of material were you teaching in those days?

I was teaching beginning freshman chemistry and in those days that was almost entirely descriptive chemistry. As a matter of fact, the first year after I had been here, Professor Hopkins published a textbook in general chemistry. He had a chapter on atomic structure, but it was almost the last chapter in the book, so it was never used in explaining properties of compounds or anything of that sort. It was just a sort of an addendum which the teacher could use if he wanted to when he finished the rest of the course. Professor Hopkins was somewhat criticized for this in a review of his book, but he just laughed it off and said, "Well, we didn't use it and we don't need it". His course, which we all followed since we were under his direction, depended almost entirely on where elements are found, how they're prepared, and what their properties and uses are. In his textbook he had many charts showing the amounts of various compounds that are used in industry and items of that sort. There was little theory to it, as far as I can recall. We knew about the law of Guldberg and Waage and we knew about the Haber process; there was a little theory there, but not much. It was almost entirely descriptive chemistry. That continued for some years before we began to use atomic structure and molecular structure to explain properties. My own feeling is that we've gone a little too far on this because I think many students don't appreciate (don't understand, really) what that's all about. They learn more or less by rote. They can do the problems in many cases, but they don't have any real appreciation of how the atomic structure is important in determining properties of substances. The uses of compounds are much more interesting to many students than the theoretical aspects of the course.

A.B.P.L. How would you see the students of those days compared to the students of today?

Well, the students of those days weren't awfully different from the students we have now. Of course I haven't taught a class regularly now for 18 years so I can't compare students of 1990 very well with the students of 1928. But I think there is not a great deal of difference. The students that I taught when I first came here were a little more conscientious. I think they worked a little harder on their lessons than the students do now. But I may be wrong about that.

A.B.P.L. You presumably were following the work of Werner and Dwyer and others in coordination chemistry?

Well, the moment I discovered Alfred Werner I was excited about him and his work. I read nearly all of his papers and even got hold of some theses that had not been published so that I could follow what he had done. Later, of course, I became interested in people like Dwyer, Lyons, Chatt, and Nyholm, all of whom were outstanding in the field of coordination chemistry. I read many of their papers but haven't tried to duplicate their work or anything of that sort. They have had a good deal of influence on what I have done. Most of my research has been on the stereochemistry of coordination compounds which, of course, came out of my early interest in isomerism. Most of the papers that I have published have been on stereochemical properties of one sort or another.

A.B.P.L. What effect did the Depression have on you?

Yes, I came here in the fall of 1928 and the Depression started two or three years later. The man who came here one year after I did was dismissed because the university couldn't afford to keep him. There were no increases in pay and no promotions for five years, so that I stayed at a low rank for seven years after I came here. Then I became an assistant professor. I was an instructor first and then we had a rank called "associate"; I was an associate for five years.

The Depression didn't have much effect on the graduate student enrollment (or the undergraduate enrollment, for that matter). The pay of the graduate assistants was not increased during that time. In fact, it was somewhat decreased. All of us took decreases in salary, but the university went on about the same otherwise. All of us were glad to have some sort of job to work at instead of being let out because there wasn't enough money to pay us.

A.B.P.L. Can you tell us about your salary in those days?

Yes, I came here at a yearly salary of $2100; by the time the Depression started I was getting $2700 but this was cut back to $2550 or something of that sort and it stayed there for five years. Then I was promoted to assistant professor at a salary of $2800 and after that, salaries went up slowly. In my tenure, salaries didn't get as high as they are now. When I retired, I was still getting only $27 000 a year, whereas they are now starting our assistant professors at that salary or more.

A.B.P.L. Would you like to tell us about what other things you were doing with your time during those early years in addition to teaching and in addition to directing research yourself? What other things occupied your time?

After I had been here several years, I was made Secretary of the Chemistry Department. This involved doing all sorts of things that Professor Adams didn't have time to do. I had charge of the micro-analytical laboratory (which was new at that time), the machine shop, and the summer session, but the big item was that I had charge of the placement of all of our graduates at all levels — bachelors, masters and doctors. I spent a great deal of time at this and I enjoyed it very much because I love working with students. When you're helping a student find his first job and starting his career, he tells you what sort of job he wants, why he wants that job, who he is planning to marry, why he wants to stay close to his parents and all sorts of other things, so you become sort of a father-confessor to the whole group. For 15 years I had charge of that placement work and placed about 200 students every year in academic and industrial positions. I finally gave it up because I came to the conclusion that I could either be a placement man or I could be a chemist, but I couldn't be both indefinitely. I decided that between the two, I'd rather be a chemist.

A.B.P.L. You have mentioned Dwyer and Werner but in terms of personal contacts with people, who influenced you in the directions you were going?

Well the people that had the biggest influence on my life here at Illinois were probably Speed Marvel, and to some extent, Professor Hopkins. I worked directly under Hopkins, though as time went on I assumed more and more authority on my own and by the time he retired I took over his position without any hitch at all. I didn't have any official connection with Marvel but he turned out to be a very important person in my life because he sent me my first undergraduate students and, indeed, later, some graduate students. He had a very sound philosophy of life, especially academic life. I used to talk to him a good deal about how I should run my program. From a chemical point of view I was on my own, but I got advice from him on how to get things done, how to get along in the world, and to get ahead.

A.B.P.L. Presumably you were working very hard in those days in order to get things done and get results but were you spending any time outside the lab at all?

I had very little time outside the laboratory because with my teaching and research and the administrative duties which I have already mentioned, I was working about 12 hours a day at the university. Since I had administrative charge of the general chemistry, I tried always to be at the office before 8:00 in the morning. If a teacher should call and say he was sick, couldn't come in or something, I could quickly find a substitute or teach the class myself. I stayed until noon, I returned before 1:00 and stayed until 5:00. After an hour or two at home (usually a couple of hours), I came back to the Chemistry Building. I did my reading or worked in the laboratory because in addition to having to direct research students I did laboratory work myself. I would stay until 10:00 or 11:00 at night — sometimes later. So I had very little time for my family or my outside activities. I did spend some time on church work. I was an elder in the Presbyterian church and I had some other activities. I tried to spend some time with my two boys but most of the training of the children was left to my wife. Happily, she did a very good job with it and the boys have turned out extremely well.

A.B.P.L. Perhaps we should go back to the chemistry following the Walden inversion. Where did your life take you? What do you remember as the more important contributions in those days?

Another subject in which we got involved concerned the properties of complex ions in which the ligands contain chiral groups such as propylenediamine, that is, 1,2-propanediamine. It turns out that when you make a complex with a ligand of that sort, not all the possible isomers are prepared. Some have preference over others. We became involved in studying the reason why that was so. The Dutch chemist Jaeger was both an inorganic chemist and a crystallographer; he thought that the differences in stability were a matter of symmetry, but it turned out that that is not the case. Eventually, I built some models and I looked for symmetry elements but couldn't find them. I took the matter to Dave Curtin, who was interested in stereochemistry, to ask him about it. He didn't see any immediate answer to the problem. He said, "Why don't you show it to E.J. Corey?" So I took it to Corey and he immediately saw that this was a problem which could be solved by conformational analysis; within a few days we had this all worked out. I must say that Corey did most of the thinking and planning on this; I furnished the experimental observations and he did the theoretical calculations. We published a paper which turned out to be very important — that is, a lot of people have read it and studied it and have based some research problems on it. Corey was quite interested in it. I think that had he stayed at Illinois, he and I might have collaborated in some other inorganic work, but very soon after that event he went to Harvard and so he continued in pure organic chemistry.

When Elmer Wymore was here with me, we wanted to do some research on complexes containing phosphines; he prepared a diphosphine. Nowadays everybody is working on phosphines or phosphites but at that time, this was quite unusual. In fact, I think we were among the early pioneers on making phosphine complexes. We published a paper under the title "Uncommon Coordinating Agents" but we never followed up on the series, although we had originally planned to do so.

Another piece of research that always has interested me was the research we did on the optical activity of diaminobiphenyl. If this is put on platinum(II), the two rings of the biphenyl molecule are forced out of planarity so that the complex becomes chiral and so resolvable into enantiomers. A postdoc named Habu did that work and it came out very well. If one could put two diaminobiphenyls on a platinum(II) ion, one should get a *meso* form as well as *dextro* and *laevo* forms, but we didn't do much work on that. It didn't turn out well, so we abandoned it at that point.

A.B.P.L. What happened to you during the Second World War?

During the war years, we had a lot of war-related research going on and I had a small group of people working with me on two different problems. One was the development of smoke screens. When you fire a phosphorus bomb you get a column of smoke but you don't get a real screen. A young man by the name of Woyski who was working with me discovered that you could mix white phosphorus with synthetic rubber. When this was fired in a shell, it scattered very broadly so you really got a good smoke screen out of it. The other problem which we worked on concerned a nerve gas which causes temporary blindness. It causes the pupils of the eye to contract so that you can't see well. If you get a heavy dose of this, of course, it is definitely poisonous and can actually kill a person, but in smaller amounts it simply causes a contraction of the pupils of the eyes. One of the boys in the laboratory commented one day that it was getting very dark. The other students said no, that it wasn't — it was a bright day. We immediately took him out of the laboratory and home. He was alright the next morning, but apparently he had got a little whiff of this gas and was temporarily blinded. We worked on those research problems and some other things for several years during the war, but those were the main ones. The research which most of the students had, went along in about the usual way. Of course, we lost some students who went off to other war jobs or joined the army, but it didn't have a big influence here in inorganic chemistry. In the organic field, it had a much bigger influence. Nearly all of the students were doing war research.

A.B.P.L. Which of your earlier students do you especially recall?

I had been very fortunate in having top notch students. Among the early ones who have achieved considerable success are Fred Basolo, and a few years later Daryle Busch. Bob Parry was one of my students and later Clayton Callis. I have already mentioned Don Peppard, who was an extremely capable student and Bob Auten, who was an undergraduate student with me but who went into industry and became a top man. At one time, three of my former students were directing research in the three big chemical companies; Du Pont, Monsanto, and Dow. Callis was at Monsanto, Bill Drinkard was at Du Pont and Elmer Wymore at Dow. Wymore unfortunately was killed in an accident, the other two men are still working at various and sundry things though they have retired. Three of my former students have become Presidents of the American Chemical Society — Fred Basolo, Bob Parry, and Clayton Callis — and there are a couple more that I am hoping will be Presidents before I die. I don't know whether anybody else has had that many students who have become Presidents.

A.B.P.L. You started getting involved with national aspects of chemistry and ultimately became the President of the American Chemical Society. Would you like to tell us more about those?

The first spring that I was here at Illinois, Professor Hopkins asked me if I was planning to go to the Meeting. I didn't know what meeing he referred to. I supposed he meant some faculty meeting or something of that sort, but he said "Oh no, the Spring Meeting of the American Chemical Society." I didn't know about such meetings because the people at Michigan had never gone to them. Once in a while Professor Willard went, but he was about the only one. It didn't occur to me that many people at Illinois would go, and so I told Professor Hopkins that I didn't know anything about such a meeting. "Well", he said "we expect our people to go". So, of course, I went and I have been going to those meetings ever since. I think I have missed only two or three in all the years that I have been here. I have always encouraged my students to go to the meetings and for the most part they have joined the society while they were still students or very soon thereafter and have attended the meetings quite regularly. Of course, if you go to the meetings, you eventually become a member of the council and then you get put on committees and so forth. I went through all this committee work and eventually was elected President. I might say that I was not elected President the first time I was nominated. In fact, I think that it was the third time I was nominated that I was elected. The third time was in the fall of 1957. In 1958, I was President-elect and in 1959, President. Since then, I've continued to go to the meetings and have been on a committee or two. I still enjoy the contacts that I make there.

A.B.P.L. You were also Chairman of the Division of Chemical Education and at one time Chairman of the Inorganic Division. In all those interactions of the ACS, can you tell us about some of the more important things that you think you affected?

I don't know how much influence I had. I don't think I was ever a very strong Chairman although I was Chairman of three different divisions: Physical and Inorganic, Inorganic, and Chemical Education. Of course I helped to found the Division of Inorganic Chemistry. When I was an officer in the Division of Physical and Inorganic, I felt quite out of place because most of the members of that group were physical chemists and my background was far from physical chemistry. So it seemed to me that we should have a separate division for Inorganic. I talked about it with Therald Moeller, who was one of my colleagues here at Illinois. We decided we would sound out some other inorganic chemists on the matter, so we wrote a form letter to all of the inorganic chemists that we knew (about 100 of them) asking them if they thought it was a good thing to found a new division. The news got around and the response was immediate and enthusiastic. We got about 150 names of people who said they would join if such an organization was formed, so we petitioned the Society for a charter for a Division of Inorganic Chemistry; this was granted. We were put on a probationary status for the first year, as is always the case with a new division. We had good papers that year and we had an enthusiastic response to the papers, so after one year we were given full status and now the Division has become one of the large divisions in the society. We have something over 4000 members. The number of papers given in the Inorganic Division is greater than the number given in any other division, so we feel that it was a good thing to get the group started. We now have some subdivisions; the Subdivision of Bioinorganic Chemistry and Subdivision of Solid State Chemistry are parts of the Division of Inorganic Chemistry. I have recently written a history of all this and it has been published in the Journal of Chemical Education.

A.B.P.L. What were the issues that were important during the time you were President of the ACS?

During my tenure as the member of the board and President of the American Chemical Society, I urged the formation of a journal of inorganic chemistry and this soon came to pass. That journal was first edited by one of my former students (Bob Parry) and it's now published twice a month and has a good circulation. I think that I have had a considerable influence in the development of inorganic chemistry in the United States.

A.B.P.L. You have been involved in various publishing endeavors including Inorganic Syntheses. *Can you tell us something about the books and things that you have edited and written?*

During one of the meetings of the ACS, a group of inorganic chemists was talking about one thing and another and came to the conclusion that there should be a series of books called *Inorganic Syntheses* just as the organic chemists had a series called *Organic Syntheses*. The people in that group were Raymond Kirk, H.S. Booth, Conard Fernelius, L.F. Audrieth and Warren Johnson. I was not there, but they thought about me and asked me if I would like to join the group. I did so, and from the beginning of the first issue of *Inorganic Syntheses* I have been a member of that group. There was a long period after Volume 1 before Volume 2 appeared because the war years intervened and we didn't get enough manuscripts submitted to make a new volume. Since that time, the volumes have appeared fairly regularly and in recent years they have come out every year. We are now up to Volume 28, I think.

Professor Hopkins authored a general chemistry textbook and when he got to the 4th edition, he thought that he should have a little help on it. He asked me if I would help, so on Volume 4 and Volume 5, I was a co-author. After Professor Hopkins' death, Therald Moeller, Jake Kleinberg and I published a book on general chemistry and later with some other people, we have published three editions of a general chemistry text. In addition to that, I was the editor of a large ACS monograph called *The Chemistry of Coordination Compounds*. All of the chapters in this book were written by my former students. I wrote two chapters and I edited the whole book. It turned out to be quite a successful book and sold several thousand copies. It was translated into Russian, and much to the surprise of many people, I received some royalties from Russia. All the U.S. royalties from the book were distributed among the co-authors so that personally, I didn't get any financial reward.

It may be of some interest to tell how I got a royalty from Russia. They wrote to me that they were publishing a translation of my book. I replied that I planned to be in Russia in a few weeks and would like to have royalties. I knew that Russian publishers didn't usually give royalties, but I thought I might as well try. Much to my surprise, when I arrived in Moscow, they approached me and told me if I were to call Mr. so and so... (I have forgotten his name) he had the royalty money. I called him and later went to his office and had a little ceremony where he called in several people, had tea and cookies while we sat around and talked and they gave me the money, of course in rubles. I couldn't bring them home so when they asked "Well, what are you going to do with the money?" I said I didn't quite know.

I didn't know how much it was going to be, or anything about it. Incidentally, it covered only the part of the book that I had written. They made it quite clear that they were not giving me any royalties for editing the book, because according to the communist doctrine, one man cannot hire another. The authors have to be collaborators, so to speak. Eventually, I bought a fur coat for my wife with the money and when we got back to New

York, we had a little trouble with the customs man. He thought the coat might have come from China and the United States wasn't dealing with China in those days. If he had been able to establish that this fur had come from China, he would have confiscated it, but he eventually gave up and so we brought it home. Later, Bob Parry was in Russia and he received royalty money also. It turns out that if you tell them far enough ahead of time, they can make arrangements and give you the royalty money that's due, but if you tell them after you get there that you want your royalty money, it's too late.

A.B.P.L. You have told us about some of the early chemistry in your life that was important to you. Can you tell us about some of the chemistry more recently in the fifties and sixties that you particularly remember?

One period of my life was devoted to an attempt to prepare inorganic polymers. This didn't turn out very well. We obtained some polymers but they weren't of any particular use or importance so I don't need to say much about them. What we hoped to do was to link coordination compounds together through organic groups. We discovered that the organic material close to the metal ion is often stabilized against heat and this is what we were looking for — a heat stable polymer — but if you have too much organic matter that isn't very close to the metal, you don't get much heat stability. We did publish some papers on this subject and a lot of formal reports to the Department of Defense but I wouldn't want to say that we were very successful in this project.

A.B.P.L. You were also involved in soybean research?

I got involved in the soybean oil problem through a strange series of circumstances. The U.S. Department of Agriculture has a big laboratory in Peoria, Illinois and they were working on the hydrogenation of soybean oil to reduce one of the three double bonds and leave the other two. They discovered that iron carbonyl and one other compound — I've forgotten now just what it was — would catalyze this hydrogenation. Someone in their group commented that both of these catalysts are coordination compounds, and added "If you want to know about coordination compounds, you should talk to John Bailar". So they came to Urbana and asked if I would be willing to study this problem; I agreed to do so. They furnished a considerable sum of money for it, so I hired a Japanese postdoc to come and work with me. I had read a paper by some people at Du Pont (Lindsay and some others) on the reduction of hydrocarbons using a mixture of tin chloride and platinum chloride. I suggested to Dr. Itatani that we try this, and sure enough it worked very well. It did reduce one double bond and leave the other two but they were not really unchanged because we discovered later they were changed from *cis* to *trans* which was bad, but at least it produced one double bond and left the other two. This happened very early in Dr. Itatani's stay with me but we did a lot more work on the problem in attempting to refine the process and learn more about it. We published 18 or 19 papers on the use of this mixture of tin chloride and platinum chloride in the reduction of soybean oil. It turns out that the platinum chloride and tin chloride combine to form a complex of their own and this is actually the catalyst.

A couple of other pieces of research that have always interested me, though they aren't major events in my career, were the preparation of an optically active zinc complex and the preparation of an optically active aluminum compound. These two metals normally do not give compounds that are stable enough to be resolved into optical isomers, but with the ligands that we had, we were able to get quite stable optical isomers. The zinc compound was resolved by a Chinese girl named Jenny Liu and the aluminum compound by an Indian postdoc named Basudeb Das Sarma.

A.B.P.L. You have done a fair amount of visiting lectureship work around the world, in São Paolo and Japan, and Colorado and Guanajato in Mexico. What do you remember of those trips?

Yes, I have been fortunate to have been invited to lecture in a variety of places. In São Paolo, I lectured in English, but the students there were almost all well-trained in English and things went along very well. I was invited to go to the University of Guanajato in Mexico and I went there every summer for five or six years and lectured on coordination chemistry. The students there were not as well trained in English and frequently I would have to call on someone in the classroom who knew English pretty well to explain to the class what I was trying to say. I set out to learn Spanish so I could do a better job there but my sponsors told me not to do that because they wanted the students to learn to speak English. So I continued to lecture in English with the help of members of the class or someone else who was there who could translate for us. It was an interesting experience and I enjoyed it very much.

I have done some other invited lecturing, too. In fact I've given about 150 lectures around the country for the American Chemical Society. I rather enjoy this, because I get to see new people, to talk about coordination compounds and inorganic chemistry, and to see new parts of the country. A year and a half ago I was asked to lecture in New England in the spring. I thought this would be a very pretty part of the country in the early spring so my wife and I flew to upper New York State where we rented a car and drove from there through New England, giving a lecture each evening and driving onto the next place the following day.

A.B.P.L. You have been honored with a large number of prizes during your career, are there any that come to mind as ones that you are particularly proud of?

Yes, the Priestley Medal I think is the one that thrilled me the most. This is the highest honor that the American Chemical Society has to offer. I was at a National Meeting of the American Chemical Society and I was asked to report to the office. When I went there, they told me that I had a call and I was to wait for it. So I waited and waited and waited, and finally I was called to the phone and the man said, "I suppose you know why I am calling". I replied, "No, I have no idea". He answered, "You have been chosen for the Priestley Medal". It was a great surprise to me. I had no idea that I had even been mentioned for the Priestley Medal. It overshadows the other prizes which I have received, I suppose, because it is the highest honor that the Society has.

I also received the Dwyer Medal from the Chemical Society of New South Wales. For this I went to Sydney, Australia and of course, as is the custom, I gave a talk when I was awarded the medal. I knew Dr. Dwyer fairly well. He had been here and I had seen him at other chemical places and so it was a great honor to speak about his work and how it had impinged on mine. In fact, it was quite important to me because when we did the original work on the Walden inversion, we thought that whether one got an inversion depended on whether one used silver carbonate or potassium carbonate. Dr. Dwyer found out that that was not the case. It depended on the basicity of the solution, and from that point on we were able to produce inversions about as we wished. The Werner Medal is also one that gave me very great pleasure. It is a high honor because it was issued on the 100th anniversary of the birth of Alfred Werner and it is the only Werner medal. It isn't a medal that they give once a year, or at some other regular interval. It's maybe once in 100 years, but their intention was to give just one medal. This also was a very great surprise to me. I went to a meeting one morning when we were in Switzerland and someone commented: "Now when we give you the medal...". I replied, "Medal, what medal?" He said, "Didn't you know, we are giving you the Werner medal?" I had never heard of the Werner medal. "Well", he went on, "you're to receive it this morning". My wife wasn't there. She was still back at our hotel, so I called her quickly to come and see me receive this medal. She got there just as it was handed to me.

A.B.P.L. I notice that we should congratulate you for a medal this year, the Order of Lenin Jubilee Medal. Can you tell us something about that?

No, I can't really, because there was no letter which accompanied the medal. There was a certificate which came in the mail a few days later but no mention of why it was given to me except as it says on the medal, "For Excellence in Coordination Chemistry". When we were in Australia this summer, I met a Russian and I asked him about it. He didn't know about it for he's not in that field of chemistry. He told me that when he got home he would look it up and let me know the details of it, but I have not yet heard from him.

A.B.P.L. You retired almost 20 years ago and there was a big symposium in your honor here at Illinois to celebrate that retirement. What have you been spending your time on since then?

When I retired in 1972 I didn't want to quit. I still had some research ideas that I wanted to work on and I have had postdocs since. The rules of the university say I should not have students who are working for degrees but there was an undergraduate student who was eager to work with me. I called the head of the department to see if this was possible. He replied, "Well, it is against the rules but I will ask the Dean." The Dean said it was alright and since that time I have never asked. I have had a whole series of undergraduate students. I have not had graduate students, but have had undergraduate students and postdocs. We have worked on stereochemistry of one sort and another and on some solid state reactions which rather interested me. I have finally come to the conclusion that if you are going to do solid state work, you have to have much more sophisticated equipment and more sophisticated knowledge than I have, so I have about given that up and gone back to purely stereochemical work. I have one undergraduate student who is attempting to prepare a complex of platinum(II) in which there are two *trans*-spanning ligands, one on each side of the plane. How that will come out I don't know, but we're very hopeful of it.

So, through the years, I have continued to have students and to work and to write papers, both on the work we have done here and on reviews. I might mention one recent piece of research which I had with an undergraduate student. My wife was a weaver, and one day she was dyeing some yarn and I noticed a lot of dye was going down the sewer. I asked her "What is this all about?" "Well", she replied, "it's just excess dye". I questioned this because I felt that since the wool she was dyeing is itself chiral, it might absorb one optical isomer of the dye and not the other. So I set an undergraduate student to study this matter. It's not a new problem. Way,

way back in the early twenties or before, Professor Stewart at California and Professor Adams here at Illinois did research on the adsorption of dyes on optically active fibers, particularly silk. They didn't seem to reach any sound conclusions on it. They were particularly interested in whether it was physical adsorption or chemical combination that caused the dye to cling to the fiber. We were interested only in the practical aspect and so we prepared a simple azo dye and metallized it. We put cotton, or wool, or silk individually into a column and poured the dye solution down through and sure enough the liquid which came out was optically active. This indicates quite plainly that for this particular dye, at least, one form is preferentially absorbed over the other form. This may have some practical application because if a dye maker makes an azo dye and half the dye doesn't stick to the fiber, the other half is lost. Even worse, if one half sticks tightly and the other half sticks less tightly, it's apt to happen that they'll sell the dyed cloth but at the first washing or soon thereafter, one form of the dye washes out. That's even worse than if it hadn't stuck at all. We haven't followed up on this but I hope to do some further work on it to determine how general this is and whether it operates at high temperatures or only at room temperature.

A.B.P.L. Tell us about your genealogy.

My chemical genealogy? I have already commented that I worked for Professor Gomberg at Michigan. He took his doctor's degree with Professor A.B. Prescott at Michigan. Prescott was not a Ph.D. He was an M.D. but he was interested in the caffeine from coffee and Gomberg worked on that as a graduate student. Later he published another paper or two on the structure of caffeine. After his doctorate, Gomberg went to Germany and worked for a year with Victor Meyer and he always thought of Victor Meyer as his real mentor. If you follow that line, Meyer had worked with Bunsen, Bunsen worked with Strohmeier, Strohmeier worked with Vauquelin, Vauquelin worked with Fourcroy and Fourcroy with Lavoisier. So my ancestry really goes back to Lavoisier. This is not startling because most of us go back to Lavoisier if we follow it that far.

Coordination chemistry has changed a good deal since I first started in that work. We followed Alfred Werner and his publications very closely and as other people got into the field, they did the same thing. More recently, the field has broadened a great deal and even organometallics are now considered to be more or less coordination compounds. I think there is a great field still open in coordination chemistry, both from a classical Werner point of view and from the more recent work in physical properties, theory, organometallics, inorganic biochemistry and in other aspects.