THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

CHARLES PRICE

Transcript of an Interview
Conducted by

Leon Gortler

at

The University of Pennsylvania

on

26 April 1979

Charles C.
Price
JH
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ER FOR HISTORY OF CHEMISTRY ORAL HISTORY PROJECT

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Charles C. Price
(Signature)

17 January 1985
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Charles C. Price, interview by Leon Gortler at University of Pennsylvania, Philadelphia, Pennsylvania, 26 April 1979 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0005).

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

CHARLES C. PRICE

1913	Born in Passaic, New Jersey, 13 July

Education

1934	B.A., chemistry, Swarthmore College
1935	M.A., chemistry, Harvard University
1936	Ph.D., chemistry, Harvard University

Employment

1936-1946	Assistant Professor to associate professor, Department of Chemistry, University of Illinois
1945	Visiting Lecturer, Polytechnic Institute of Brooklyn
1946-1954	Professor and Chairman of Department of Chemistry, University of Notre Dame
1947-1951	Member of Subcommittee on Plastics, National Research Council
1951-1955	Member of Committee on Mathematics, Physics, and Engineering Science
	Department of Chemistry, University of Pennsylvania
1954-1966	Blanchard Professor and Chairman
1966-1978	University Professor
1978-	Emeritus professor of chemistry

Honors

1946	ACS Award in Pure Chemistry, American Chemical Society

ABSTRACT: During the course of this interview Charles Price discusses his life and career as a chemist. Initially, he recalls his childhood, early education, and undergraduate life at Swarthmore College. Price then speaks about his teachers and colleagues at Harvard University where he did graduate work. Considerations about his work at the University of Illinois, his first faculty appointment, and research for the National Defense Research Committee during the war, follow. Price explains why he assumed the chairmanship of the department of chemistry at the University of Notre Dame and why he convened the first Conference on Organic Reaction Mechanisms, a landmark in the development of physical organic chemistry. Price elucidates his research at Notre Dame and then at the University of Pennsylvania where, as chairman, he helped to rebuild the department of chemistry. The interview concludes with Price discussing his more recent chemical research, the current state of physical organic chemistry, his family, and his political and sporting activities.

INTERVIEWER: Leon Gortler is a chemist with an interest in history. Born in 1935, he attended the University of Chicago and then received his doctoral degree from Harvard. After doing postdoctoral work at Berkeley for a year, he began teaching at Brooklyn College in 1963. Ten years later, he became professor of chemistry. He has since coauthored two textbooks about organic chemistry. Both his historical and scientific research focus upon physical organic chemistry.

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INTERVIEW: Charles Price

INTERVIEWED BY: Leon Gortler

PLACE: University of Pennsylvania

DATE: April 26, 1979

Gortler: I'd like to start with your childhood and hear about the kinds of things that might have induced you to become a scientist and to lead an extremely active career. While reading about your various activities, I became exhausted.

You were born in New Jersey in 1913. Tell me something about your family, about your brothers and sisters. Were you not the first child in your family?

Price: I was the first of five. I had three sisters and a brother.

While still quite young I felt pressured to become either a doctor or an engineer. My grandmother, Sarah Shoemaker Farley, wanted me to become a doctor. Her husband and son were M.D.'s. When I lost my hand at the age of six, however, she realized that I couldn't become a doctor. I had had no desire to take up that profession anyhow. The second pressure came from my father, who was an engineer. It affected my brother; he became an engineer. Furthermore, I thought that when I went to college I would become a chemical engineer.

Gortler: You already had those leanings?

Price: I was certainly leaning very much towards science. When I was at George School, I took all of the science and math that I could. Those were my favorite subjects.

My grandmother also influenced me. Even though I never became a doctor, she helped me to appreciate the biological sciences. While still in grade school, I lived with her for a year or two and went to all kinds of biological club meetings with her. So, I got exposed to biological science when very young.

As you see, I was naturally inclined to go into science and received encouragement to do so. One person, not yet mentioned, influenced me to become a chemist. He, Jerry Creighton, was my professor at Swarthmore. He was quite an interesting physical chemist who had done a lot of electrochemistry. He had written a major textbook on electrochemistry and had filed the patents that Atlas used to reduce sugars to sorbitols. He said, "There's no sense being a chemical engineer. You might as well study chemistry, and if you still want to be a chemical engineer you'll be a better chemical engineer." So, he steered me into chemistry. Needless to say, I never went back to chemical engineering.

Jerry Creighton had a very interesting philosophy about recommending his chemistry majors for graduate school.

He did not recommend them to the same graduate school. He said, "The best one of you will get an assistantship or scholarship and the other three won't get anything." There were four chemistry majors who graduated with me from Swarthmore. He therefore made us draw lots for the big three, Harvard, Princeton and Yale. I drew Princeton. The fellows who drew Harvard and Yale got assistantships, but I didn't hear from Princeton until a month before the fall semester was to begin. By then I was enrolled at Harvard.

Let me tell you how I finally went to Harvard. At that time, Swarthmore had a system of outside honors exams. Louis Fieser, the other major influence on my career, was my outside organic examiner. He found out that I had no assistantship or any other form of financial assistance to fund my studies at graduate school. Obtaining support was quite important for me because the country was suffering then from the effects of the Depression. Fieser apparently liked what I did for him on the organic exam and at the orals because I was offered a scholarship to go to Harvard two weeks later. I hadn't even applied for it. I decided to accept the scholarship and, of course, to work for Louis Fieser.

Gortler: Did you decide to work for him because he'd been your examiner, or did you look around when you got to Harvard.

Price: Well, I looked around a little bit, but I was very grateful to Louis Fieser for what he'd done for me, and other things being equal, I was going to work for him.

Gortler: I guess that the other organic chemist, Bartlett, got there about the time that you did and that he was young.

Price: Bartlett and Kohler were there.

Gortler: That's essentially it, I think, because Conant had just become president.

Price: He was president. I was interested in physical chemistry approaches and had an interesting interview with Louis. He gave me all of the synthetic organic things to do and I kept saying, "Yes, yes, what else?" After giving me half a dozen organic synthesis problems, he finally assigned me one on the mechanism of addition and substitution of bromine with aromatics, particularly the phenanthrene ring system. I jumped at that, because it was more mechanistic. Of course, I got a lot of help from Paul Bartlett, because he was working on bromination at that time. Louis was my supervisor, however, and I did a lot of synthesis, to make substituted phenanthrenes in order to measure the effect of different substituents on the reaction. Twice a day, every morning and afternoon, Louis would come into the lab and ask, "What's new?"

Gortler: That's the way he ran his group?

Price: That's the way he ran his research group. He came around twice a day, morning and afternoon, and his first question was, "Well, what's new?"

Gortler: Did you feel that you had to have something for him? I guess you did. There's a story that goes around, maybe a legend, that you bet Fieser that you could finish up in two years.

Price: I don't know whether I bet him, but I did finish.

Gortler: Yes. I know that you did. It was an absolutely incredible feat.

Price: Well, I had an excellent start, being in the honors program at Swarthmore. I also did work pretty hard and I had a lot of luck. You've got to have luck in research to have things fall into place. It all did fall into place very well.

Gortler: Let's go back a little before I return to that period at Harvard, which is the period upon which I want to concentrate. Tell me a little bit about your father's influence upon you, and about your growing up. I guess you grew up in Passaic?

Price: No. My mother and father lived there in an apartment only during the first year or two that they were married. They moved to Elizabeth shortly after that, and then to Summit, New Jersey.

At that time, my father had great ambitions. He was a hard-driving, entrepreneurial type. He wanted to make a million dollars by the time he was thirty years old. He started his own engineering company, T. W. Price Engineering Co., with offices in the Woolworth Building in downtown New York. He designed and supervised the building of steel plants during World War I and one of the coaling stations on the Panama Canal. He was quite a successful engineer and was, I guess, making a lot of money.

He worked so hard, however, that he had a breakdown about the time that I was four or five years old. The doctors told him to quit his hard-driving entrepreneurial company. He bought a two hundred acre farm near New Brunswick, New Jersey, in order to regain his health. He farmed. He had forty or fifty dairy cattle and distributed milk every morning. It did him a lot of good.

Farming was not his calling, however, and, one Sunday afternoon about a year and a half after he started the dairy business, all of the barns burned. We suspected that spontaneous combustion caused the tragedy. That fire essentially got him out of the business of farming.

He went back to being an entrepreneur and started the biggest dog kennel in the United States, Strongheart Kennels. He sold very well-trained dogs to Cal Coolidge and others. That venture was successful.

Eventually, my father got back into the engineering business and into inventing. His inventions improved coal-burning furnaces in homes. They were excellent inventions. He got into that business, however, at about the time that people began to heat their homes with oil. Nonetheless, that's the kind of father I had--a very dedicated, hard-working man with an engineering background from Penn.

Gortler: A hard-working life is natural to you, then.

Price: Yes. I guess the Quaker tradition also stresses that way. Both my mother and father were Quakers.

Gortler: We haven't mentioned that you were at Notre Dame. I always wondered how a Quaker managed that.

Price: How a peaceful Philadelphia Quaker wound up in the midst of the Fighting Irish?

Gortler: Right. This grandmother of whom you spoke, was she on your father's side?

Price: On my mother's side.

Gortler: What kind of role did your mother play in the family?

Price: She was very much a housewife and a mother. She didn't have many outside activities. Although she was interested in many things, she was no intellectual. In her later life she became actively involved in a number of causes, like the World Federalists and things of that sort. Before then, however, she had five children to raise on a farm. The first time I really became aware of her role was when we were living on the farm. She not only cared for her own kids but, from time to time, for quite a few farm workers as well. We had at least one family that lived in a little house on the farm and did a lot of extra labor. Ours was a big farm, two hundred acres, and a fairly active one. So she had a pretty busy life and, of course, Dad kept her stepping pretty well. He was always off doing some extracurricular business activity.

Gortler: What about your own schooling? You mentioned the George School at one point.

Price: Well, before going there, I went to public schools in New Brunswick and Bound Brook. We lived about halfway between Bound Brook and New Brunswick and I used to take a bus to school. I guess that I was always the type who explored, getting myself into all kinds of things. I also guess that I got on my father's nerves quite a bit, like when I painted the family car one day. I guess that I was pretty hard on my father, although not intentionally. I was just always into something. I always had my own ideas of what I ought to be doing, and relatively few inhibitions about doing them.

When I was in the eighth grade, my parents sent me to visit my grandmother in Swarthmore. I attended the eighth grade at Swarthmore High School. That's when I got so well acquainted with my grandmother.

After I finished the eighth grade at Swarthmore High School, my parents sent me to the George School, which is a Quaker boarding school in Bucks County. During my first year there, I goofed off quite a bit and got into all kinds of trouble because I was absent-minded. I was always forgetting things that occurred once a week, like assemblies. Often, when my family came to take me home for vacation, they would find me outside walking off hours of detention. I often remembered everyday things, however.

I wasn't one of the hell-raising types, although there were those kinds at school. I just didn't quite manage to do the things I was supposed to do when I was supposed to do them. During the first year I had problems with English and Latin. Eventually, however, I graduated with a pretty good record. I think that I got nothing but A's in my science and math courses.

Gortler: Did any particular teachers influence you at that time? Do you remember any books that did likewise?

Price: Well, Norman Swain was my chemistry teacher, and I did enjoy chemistry there very much. I also liked astronomy. One of our teachers was an amateur astronomer, who used his own telescope to look at the stars. I was always intrigued by astronomy. I'm still writing books, of course, and the last book I wrote had a lot to do with the origin of the universe, cosmic theory, and what not. So, I've retained an active amateur interest in astronomy. That was kindled at the George School.

I've always been a very active competitor in sports. I have a very vigorous competitive instinct that I'm afraid still hasn't been completely subdued. I played baseball and football and wrestled in school. When I went to college, I transferred from baseball to lacrosse, which, in my opinion, is the world's greatest game.

Gortler: That's an eastern game. We didn't know much about it in the Midwest.

Price: It's spreading all over the country now. It was only played here on the East Coast when you were young.

Gortler: I take it that you expected to go to college at a fairly early age.

Price: Yes. I didn't doubt that I would go to college. I think that my parents strongly doubted that I would go to graduate school. I didn't get any encouragement from them. Jerry Creighton visited my father in Plainfield and told him that he ought to let me go to graduate school. My father had a business at that time and I used to spend my summers working with him. He thought it would be very nice if I went into the business with him.

Gortler: Right.

Price: I don't think Jerry Creighton had a great deal of difficulty convincing him, but it was interesting that Jerry Creighton came over to Plainfield to visit my father in order to persuade him that I ought to go to graduate school.

Gortler: He obviously had a fair amount of faith in you. Before going to Swarthmore did you consider other institutions?

Price: Almost none at all. My father had spent a year at Swarthmore when he was fifteen. He goofed off and flunked a couple of courses. His father therefore took him out of school and made him work for a couple of years. Later, my father graduated from Penn. My mother had spent one year at Swarthmore and then married my father. So they both had attended Swarthmore. A lot of the family had gone there. I guess that I never considered going anywhere else.

Gortler: Can you tell me anything else about undergraduate life at Swarthmore apart from the science?

Price: Well, I thoroughly enjoyed it. I met my wife when I was a junior and she was a freshman. I guess it wasn't much more than a month after we met that we decided that we were to be each other's. One reason why I got through Harvard in two years was so that I could marry her when she graduated from college. In those days you couldn't get married while you were attending college. In fact, I would have lost my scholarship at Harvard if I'd married her.

Gortler: That was a stipulation?

Price: It was a stipulation that a person couldn't get married and keep a Harvard fellowship. The authorities probably thought that a married man wouldn't spend evenings in the lab. My wife's a wonderful gal. She's played a very important, inspirational role in my life.

I enjoyed sports. I was lousy at football. Why I played football at George School and in college, I'll never know. I would have loved to play soccer because I loved to run. That's what I loved about lacrosse. When I played, I ran all over the field. I was really handicapped as a football player having only one hand. I don't know why I was so stupid to keep playing football instead of soccer. Nonetheless, I did play football even though I never really enjoyed it. I played lacrosse and I loved it. I didn't play any water sports in college.

Gortler: When did you begin to play squash?

Price: When I went to Harvard. I hadn't intended to play squash, but Stan Tarbell asked me to play squash with him. He said, "Come on down, I'll show you how to play squash."

I liked playing squash so much that I soon bought a racquet. Stan and I were good friends at Harvard and did a lot of things together. I didn't play any squash at Swarthmore because there were no courts there then.

Sports were very important at Swarthmore, as was the honors program. If you knew what you wanted to do, the last two years at Swarthmore provided a stupendous way to learn.

Gortler: Of what did they consist?

Price: Well, there were two seminars per semester. Either advanced physics and organic chemistry or advanced math and physical chemistry. I took an advanced chemistry seminar every semester and either an advanced math or an advanced physics seminar with it. I met once a week for three hours or so with the prof, and had a lot of independence.

During the first semester of organic seminar, for example, we went through Conant's book from end to end.* Additionally, we spent one day in the lab and a lot more time doing independent reading and study. During the second semester, each of us wrote a twenty page paper on the Grignard, or the Friedel-Crafts, or you name it. Each week one of us would talk about what he'd written. The honors students had desks in the back room of the library and we sat there with Beilstein** and Chem Abstracts around us. We literally dove into the literature. It was just a fantastic way to study. By studying with three other guys who were doing the same kind of thing, we shared a lot of mutual education. Whatever any one of us learned would be shared with the others, like in a lab. You always learn more from lab partners than you do from professors.

Gortler: Right.

Price: We literally lived together because we were there working almost every night. It was a tremendous experience.

Gortler: Do you remember the other fellows who were there?

Price: Oh, sure. Charlie Stauffer was the guy who drew Harvard and then went to Harvard.

Gortler: He studied with Tarbell too.

Price: Yes, Stan and Charlie were lab mates...no, they weren't.

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*James B. Conant, Organic Chemistry; A Brief Introductory Course (New York: Macmillan Company, 1928).

**Friederich Beilstein, Handbuch der Organischen Chemie (Hamburg: L. Voss, 1893-99). A supplement was issued in 1901.

Gortler: Was he the fellow who started with Bartlett and then switched to someone else?

Price: Switched to Kistiakowsky.

Gortler: That's right.

Price: He started with Bartlett and then went over to Kistiakowsky. He has since done a lot of compiling and editing for the International Critical Tables. He's done academic work ever since he left Harvard. He was at Worcester for a long time. He's now at one of the colleges up in Maine, or he may have just retired.

Johnny Brod drew Yale and worked very successfully for Procter and Gamble. I think he's vice president or research director or something like that.

Dave Brearly, the fourth student, was not nearly as good a student as the other three of us. I don't think that he completed his honors requirements. He did graduate. He went to Harvard and got a masters's degree and then worked for Du Pont and a few other companies. I have lost track of him.

Gortler: Do you remember anyone else who did or did not become a chemist and was close to you at that time.

Price: There were no special relationships. I still have a lot of good friends with whom I had played lacrosse. I see some of them quite frequently. I was a fraternity member but I don't see too much of my old fraternity mates. I never was enthusiastic about fraternity life.

Gortler: You were pretty much submerged in your science studies during the last couple of years. Who was the organic professor?

Price: Edward H. Cox. He assisted in the invention of hexylresorcinol. He did that in industry where he had spent a few years. He went to Swarthmore and spent the rest of his life there as a professor. He was a very interesting, cocky little guy, a little martinet only about five feet, five inches tall. He became the science attache for the United States in Paris. He always had a close affiliation with French organic chemists. He loved them and spoke French fluently. He was a good professor although not of the same caliber as Creighton. Creighton was a really great man, very, very, bright. He was the kind of man for whom one had a great deal of respect. We worked hard for Ed Cox because he demanded it.

Gortler: He's the fellow who was demanding all these papers, I take it?

Price: Yes. That's right. He exerted a good influence on me, no question about it.

Gortler: Were you particularly influenced there by any other professors or books?

Price: Yes. I think there was another very important influence at Swarthmore. I think the excellent math and physics I got made it very easy for me--even though I'm an organic chemist--to do things mathematically and not to be afraid to look at physics and physical principles. This influenced me to become a physical organic chemist rather than an organic chemist. I could easily have become a physical chemist. If I had gone to Princeton I'm sure I would have become a physical chemist. Incidentally, I finally did get an offer from Princeton. It arrived in August, however, a little too late. If it hadn't been for Louis Fieser I would have gone to Princeton. I'm almost certain I would have been a physical chemist had I gone there, because they had some very good physical chemists. That was my inclination and it was only Louis Fieser who dragged me away from it and got me to be an organic chemist.

Gortler: These are little known tales that will be known only through oral history interviews.

Price: One other thing was very important for me at Swarthmore--the courses I took in the history and philosophy of science. Those courses were fascinating. We read books about the origin of the universe, about evolution, about all kinds of things. The courses impressed me and broadened my outlook about science. A lot of scientists take a fairly narrow view of what they do. A fellow named Holmes, we called him "Ducky" Holmes, taught the history of science. He was a fascinating teacher and he ran for Congress. Brand Blanchard taught the philosophy course and he was a fascinating teacher too.

Gortler: That outlook has stayed with you because you have had a very broad view of science.

Price: I know that was a very important influence on me. The advanced physics and chemistry that I took were extremely useful to me even though I don't remember the people who taught them. The math teacher, Arnold Dresden, was a fascinating character. I wasn't a particularly talented mathematician, although I was able to get an A in all of the courses except one in which I got a B. I took the exam when I had a temperature of 105 degrees and still wound up getting a B in that course. I wasn't theoretically enough inclined to be a good mathematician.

Gortler: You had enough facility to use it.

Price: Yes. I got exposed to enough of it and pushed hard enough in it so that I wasn't afraid to use it. I understood the principles.

Gortler: That comes out in your papers. You always had sufficient facility to attack a problem. I also got the impression from reading your papers that you weren't afraid to attack a problem.

Price: That's certainly what I got out of my mathematical training, even though I didn't pick up enough facility to be a real mathematician. I felt familiar enough with math so as not to be scared to death, but rather to jump in and try it. You do get a feel for the principles, even though you don't have all of the working activity at your fingertips. That makes a big difference.

Gortler: Do you recollect your perceptions about being a chemist as you were finishing at Swarthmore? That is, now that you had chosen to be a chemist, what did you think being a chemist meant?

Price: That's an interesting question because I suppose that I was about as naive and unthinking about my future as you could imagine.

Gortler: Probably no more than any other graduate.

Price: Well, when I look back at my own kids, they did a lot more thinking about what they wanted to do and how they wanted to do it. I really hadn't faced up to what I was going to do when I got out of college. Jerry Creighton told me that I ought to go to graduate school. So, I went to graduate school.

Getting my Ph.D. at Harvard in twenty-one months didn't give me a lot of time to think about what I was going to do when I completed my studies. I never took an interview for a job, even though I was sure that I was going to finish. I just didn't bother.

I did apply for a National Research Council Fellowship. Either Fieser or Kohler suggested that I ought to do that. I applied in order to do some more research. One day Kohler called me and said that he had received a letter from Roger Adams. Roger was on the committee that chose the National Research Council Fellowships. Apparently Roger liked what they were saying about me. He had written to Kohler asking whether I would be interested in working with him. I was so naive that I didn't even know who Roger Adams was at that point! Kohler told me about him.

Gortler: He apparently was a good friend of Kohler's.

Price: Yes, they knew each other. Kohler esentially said, "You go out and work with Adams." So I went out and worked with Adams. That was a great move. I was delighted.

Gortler: Yes. It obviously was. I want to get back to that, because the comparison of Harvard and of Illinois is very important to me.

I don't think you need feel embarrassed about not knowing what you were getting into. I have not spoken to anybody who went into chemistry with any preconceived notion of what it was like. I'm still waiting for somebody to tell me that he knew what he was doing.

Price: That he'd figured it out, had it all planned?

Gortler: Yes, that's right. Did you ever consider being anything but an academic chemist?

Price: As I say, I really didn't think about it. I didn't consciously say, "Well, if I do this and then do that, I'll become a professor." I just liked what I was doing so much that I guess I wanted to keep doing it. I always have thoroughly enjoyed chemistry. It was a little wrenching a few years ago to give it up, for a variety of reasons. But I definitely have given up the active pursuit of chemistry in favor of some other things. My motivation was to keep on having fun. It was exciting fun to solve problems.

I think I could easily have gone into industrial work, because I've been very successful as a consultant. I've had quite a few offers to go into industrial work, but liked the academic life so much that I never seriously considered becoming an industrial chemist.

I like to feel that the things that I do have some practical importance--in contrast to many of my colleagues in the academic world who think that if what they're doing has some useful application there's something wrong with it. I've never felt that way. I've always felt, gee, it's a bonus if what I'm doing has also got some useful application.

Gortler: What about the courses that you took at Harvard? You must have decided to do research very quickly.

Price: I started to do research almost a week or two after I got there because I didn't have to take the first year graduate courses, having done honors work at Swarthmore.

Gortler: I see. So you didn't take Kohler's course?

Price: I took Kohler's course.

Gortler: You did?

Price: That's a course I wouldn't have missed. That was a gem of a course.

Gortler: I'm very interested in Kohler.

Price: He was a great teacher, a great teacher. I thoroughly enjoyed his course. I took Kisty's course on thermodynamics. I took a course by Fieser, the course he was giving in natural products, from which he wrote his book, Chemistry of Natural Products Related to Phenanthrene.* I don't think I took Paul Bartlett's course. Those are the only three courses I can remember for sure. There might have been a fourth. I think a chemical engineer came from MIT and offered a course that I audited. I don't think I took it for credit.

Gortler: There was a course offered in sewage disposal or something like that for a number of years.

Price: No. This dealt with chemical processes or something like that. He talked about how you did chemical processing although I can't remember for sure. I'm sure that I took a course from Louis Fieser and I know I took Kohler's course and Kisty's course.

Gortler: Do you remember the orientation of Kohler's course at that time?

Price: He talked a lot about how to do chemical reactions. He picked examples from the literature and gave us the details of different kinds of reactions such as, addition to alpha, beta-unsaturated ketones. That was, of course, one of his research interests. He did a lot of work on the Michael reaction. He gave us a lot of the details--which reactions worked and how, and what changes in the structure caused it to go 1,2 rather than 1,4. He discussed how structure affects the reactivity of organic compounds. He approached mechanisms classically and we had a lot of discussion.

Gortler: He was raising problems for you.

Price: Oh yes. I can still remember his course and Fieser's course. There wasn't a lecture to which I went that I didn't write down in my notes a couple of research problems that I thought would be interesting to undertake. Both Kohler and Fieser stimulated their students. They raised questions. They said: "Here's what we know; here are a lot of things that we don't know." Meanwhile, I kept writing all kinds of ideas in my notes. So, they certainly did give lectures that stimulated me because I can distinctly remember that I would think about an interesting new research problem about which they had talked.

Gortler: Your first problem seemed to be very physically oriented for Fieser. You obviously waited for the right problem to come along. What was his feeling about the way the problem then proceeded? Obviously, you were doing a very physical analysis of the problem as well.

Price: You have to give Fieser credit. You know, I wound up measuring rates of reaction and equilibria. The

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*(New York: Reinhold Company, 1936).

bromine addition was an equilibrium reaction and we could measure the equilibrium constants. Fieser had done his Ph.D. thesis on equilibrium constants. The oxidation-reduction potentials of quinones is a thermodynamic study. A lot of work is still being done on that. That was an area about which Fieser knew a lot.

Fieser had a better grasp of physical chemistry than most people realize, because he liked the synthetic. He did an awful lot of good synthetic chemistry, but he also did a hell of a lot of work on synthesizing quinones, all kinds of quinones with all kinds of substituents, and measuring the effect of substituents on the oxidation-reduction potentials of quinones. I don't want to give the impression that Louis was not interested in and not able to do physical chemistry. At the same time, it was very helpful to have Paul Bartlett in the lab right next door.

When Louis presented the problem to me, the older German literature suggested that the substitution of benzene went through an addition reaction. Here was a case where one could isolate the addition intermediate and convert it to the substitution product. Louis wanted to see if we could prove that that was in fact the way it went. We proved that that was not the way it went, although we didn't establish at that time whether it was a free radical or an ion. Nothing I did could establish whether the thing with one bromine added to it was an ion or a radical. I left that very noncommittal in my thesis and in the paper that I published.

Gortler: Yes. There was a hint of a radical mechanism at that time.

Price: I'm sure that we knew that light catalyzed the reaction but there's no question in my mind now that the addition reaction was photocatalytic and could go by a radical. We just didn't have enough evidence to be sure that it went through the electrophilic attack which, of course, is the way it does go. That was just a little before the best evidence for that came along from Ingold.

Gortler: Yes. You spent quite a bit of time afterwards working on electrophilic substitutions. That was not the last of it.

You were talking to Fieser, Bartlett and Tarbell. To whom else were you talking, as if you had time to talk to anybody.

Price: I thoroughly enjoyed Kisty's course in thermo, even though most of the organic chemists hated it. I think that it was a very, very, useful course. I did well in it. I learned a lot from him.

Gortler: Do you remember books from that time, or people or papers that you were reading that you felt were particularly important?

Price: You know, one of the reasons, not by any means the most important one, that I decided to retire early was that I was losing my memory. I was having a terrible time remembering details and people and names. (laughter) But that's a good question.

I certainly thought Conant's book on organic chemistry was a gem, for that time.* He had a different approach to organic chemistry and his textbook was very good. What else? Darn, I must have read a lot of them.

Gortler: Did you know anything about the German literature? Huckel for example? Or was that not required at that time?

Price: Yes, I'm sure we got exposed a little bit to Huckel.

Gortler: The organic chemist, Walter, not the physical chemist, Erich.

Price: Well, I just don't know. I finished my courses in '35. My last year was entirely research. When was Huckel's work?

Gortler: Well, he had a book out about 1930 or '31. That was Theoretical Organic Chemistry.** I think it was a two-volume set. It covered the foundations of organic chemistry. I'm trying to figure out whether that book had much of an impact in the United States. People seemed to know about it, but I'm not sure many read it.

Price: Well, I guess you're well aware of the fact that I was never one of the ardent, overzealous supporters of the resonance theory.

Gortler: That's understated.

Price: I fought those guys very vigorously. Saul Winstein had a public burning of my book on carbon-carbon double bonds, because I didn't genuflect enough to the resonance theory.

Gortler: In that 1941 Reviews article, you spent a lot of time trying to do mainly inductive effects.*** I mean, you weren't calling them inductive effects.

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*James B. Conant, Organic Chemistry: A Brief Introductory Course (New York: Macmillan Company, 1938).

**Walter Huckel, Theoretische Grundlagen der Organischen Chemie, 2 volumes (Leipzig: Akademische Verlagsgesellschaft, 1931). Translated as Theoretical Organic Chemistry by I. S. Leibiger (New Haven: M. H. Davidson Company, 1951).

***Charles Price, "Substitution and Orientation in the Benzene Ring," Chemical Reviews, 29 (1941): 37-67.

Price: I called them, "Direct electrostatic effects," and they were damned important. The resonance theory tried to give all kinds of hocus-pocus explanations for simple electrostatics. It's ridiculous. I'm perfectly willing to admit that there are resonance effects. An allyl radical is stabilized by resonance. A cation, however, is stabilized a hell of a lot more by electrostatic effects than it is by resonance effects.

You know, because I was willing to say that there were electrostatic effects and that everything wasn't due to resonance, the resonance zealots, and I think they were zealots, castigated me. I've often wondered if that's the reason why I never got into the National Academy. I don't know. You just never know about some things. A lot of those zealots were very, very vigorously opposed to my ideas about the importance of simple, little old plus and minus electrical effects. Although perceived as an aspect of fundamental physics, electrostatics is an extremely important part of chemistry.

Gortler: The last paper that you suggested that I read, since I gave you a cutoff date, was the '51 paper, which contains a reevaluation of the electrostatic effects.* There too, you were making a strong argument for these effects.

Price: Louis Hammett reviewed that paper and castigated it. He said that this is the kind of stuff that a poor senior might do, or something like that. It was a blistering statement. I'm not sure that it was Hammett, but I suspect that it was.

Gortler: It's funny, because I've been talking to Hammett quite a bit recently. He lives just twenty miles from here, and he seems like such a nice, gentle man. I have heard rumors here and there, however, that he could be very harsh.

Price: I have a great admiration for him, but I'm not sure that it's reciprocated at all.

Gortler: Do you remember any other graduate students?

Price: Sure I remember Stan Tarbell very well. He's always been a good friend. He visits me occasionally when he comes to Philadelphia.

Gortler: He was here recently and I interviewed him.

Price: He comes here frequently because he's interested in the history of chemistry.

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*Charles C. Price, "A New Evaluation of Electrical Effects of Groups on the Benzene Ring," _Journal of the American Chemical Society_, 73 (1951): 5833-6.

Gortler: That's right. He has a sister who lives nearby.

Price: Charlie Bradsher was a lab mate. I see a fair amount of Charlie. He's a professor at Duke. Elmore Martin, who worked all his life at Du Pont was another lab partner. A third very interesting guy in our lab was Arnold Seligman, the guy who synthesized methylcholanthrene. He later became an M.D. He died recently of cancer, after having spent most of his career working on cancer research. I remember Warren Lothrop who worked in Boston for Arthur D. Little. There were five of us in the lab together. Those are the four lab mates with whom I got particularly well acquainted.

Max Tishler was up there. I got acquainted with him in those days. He worked for Kohler. I still see a lot of Mel Newman. He was a post-doc with Louis Fieser; he worked there in the basement lab. Mel was the post-doc with whom I got best acquainted. A few weeks ago he gave a lecture at Bryn Mawr and played golf with me. Hirschfelder was also there, and he too worked in the basement. Hirschfelder helped me with some of my synthetic problems. He went on and worked with one of the drug companies.

I did talk a modest amount with some of the analytical chemists. I've always had close contact with them because whenever you measure the rate of a reaction, your number one problem is an analytical one.

Gortler: Right.

Price: I got acquainted with G. P. Baxter, an organic and analytical chemist. I talked quite a bit to G.S. Forbes who had done photochemistry. Since we were measuring reactions that were exposed to light in a photometer, I had to get interested in photochemistry.

Gortler: Yes, I remember there was a comment in one of the papers about how you let the light run all of the time in certain reactions but switched it off in others.

Price: I remember Forbes. He was an interesting character. That's about all that I can remember of people up there.

Gortler: Do you have any general impressions about Harvard? Compare it, if you can, to Illinois.

Price: They both had one thing in common, a work ethic. You worked hard and you enjoyed it.

The social climates were totally different, however. I can illustrate that with a little anecdote. After I arrived at Illinois I played squash with a geologist. We played several games and then began to talk about where we'd gone to graduate school. He remarked, "Well, you've gone to Harvard graduate school, that's interesting. When were you there?" I responded, "From '34 to '36."

"We were both there at the same time," he said.

I asked, "Where did you live?"

He answered, "Conant Hall."

"On what floor?" I asked.

"Fourth floor," he said.

We had lived for two years on the same floor of Conant Hall and hadn't met each other until a few years later on a squash court of the University of Illinois.

That's typically the atmosphere of Harvard. I mean, it's very aloof and unfriendly. When you go out to the midwest, you get to know everybody. The milkman, everybody, says hello to you. You just have a totally different kind of atmosphere.

When Fieser came around to talk to me at Harvard, it was always about business. He'd ask, "What's new?" At Illinois, however, when I worked as a post-doc for Roger Adams, he'd come around and spend an hour talking about his latest trip to China. It was a very much more relaxed and friendly atmosphere at Illinois.

Science-wise, Harvard was super. I mean, I don't know, there must have been other good places to attend, but I just can't imagine that there are any places where I would have learned more than I did at Harvard. Very few places would have let me earn my Ph.D. degree in twenty-one months. Most universities have strict rules about students matriculating for at least three years. Harvard didn't.

I was always extremely pleased with my experiences at Harvard, and not just in science. I did a few other things up there. I played lacrosse for two years with the Boston Lacrosse Club and I coached Harvard's JV lacrosse team. Additionally, although I had no interest in classical music before I attended Harvard, I became interested in it while there. It happened in the following manner.

The Boston Symphony used to perform at old Mem Hall about once a month. Stan Tarbell used to sit in the little peanut gallery up over the stage. It cost him only a dollar to do so. One night I didn't want to work and Stan was going over there. He asked me to go along with him and mentioned that it would only cost me one dollar. I said that I wouldn't mind spending a dollar. I don't think that I missed another concert after that. Ever since then I've been very much interested in symphonic music. I'm no performer, but I sure enjoy listening.

Gortler: When you left Harvard, what kind of chemist did you consider yourself to be?

Price: An organic chemist with a strong interest in physical organic mechanisms. Physical organic chemistry was hardly born at that time.

Gortler: Yes, that's right.

Price: I was interested in the mechanisms of reactions, but I did not get much moral support at Illinois for that sort of endeavor.

Gortler: That was my next question.

Price: I had to fight for that at Illinois where all of the profs were classical organic chemists. They were not at all sure that this new way of looking at mechanisms was all that great. I got along fine with them, but I definitely had to earn my way out there on that.

Gortler: Yes. How were you supported at Illinois?

Price: Roger Adams had his own money for post-docs. I suppose that he received it from the university.

Gortler: Yes, he did manage to pick up money. Every time he'd get an offer from another university, he'd pick up another post-doc.

Price: I've used the same gimmick.

Gortler: OK. You were listed as a research assistant, but you were a post-doc.

Price: I was a research assistant to Roger Adams. I guess I was paid by the University of Illinois.

Gortler: Do you have any idea how much you were paid at that time?

Price: Eighteen hundred dollars.

Gortler: Go on, tell me more about your stay at Illinois.

Price: After I completed my post-doc with Roger Adams I was appointed to the teaching staff as an instructor for a year or two. I then taught the elementary organic course for pre-meds and home ec students. I also got started with a few senior research students and taught qual organic. I'm not sure that I taught it during the first year. My main responsibility was to teach the elementary organic course for pre-med and home ec students.

Gortler: Was there a qual organic book by Fuson at that time?

Price: Fuson and Shriner?

Gortler: Shriner was already there?

Price: They were both there. Fuson and Connor had written the book that we were using for our one semester organic course. Harold Snyder and I later became coauthors of that book.*

There were lots of seniors in chemistry at Illinois and the faculty at Illinois encouraged them to work with young faculty. Harold Snyder started the same year that I did. We went there as post-docs and became faculty members the next year. The senior faculty helped to push seniors, and then later Ph.D. students, our way.

Gortler: That's very helpful.

Price: A wonderful group of men; they were just great. Roger and Speed were very different. Roger is an eastern aristocrat; Speed, a farm boy from central Illinois.

Gortler: I understand that he always tried to con people into thinking that he was a backward farm boy. (laughter)

Price: That was the image he liked to project. He'd then beat the hell out of you at poker.

Gortler: Great. (laughter) For some reason American Men of Science says that you were an instructor, then an associate, and then an assistant professor. What was the distinction?

Price: "Associate" was an academic rank at Illinois that I don't think any other place had. It was just one extra step up the ladder.

Gortler: OK. How much were you making as an instructor? Did your salary change?

Price: It might have increased by a few hundred dollars, maybe to twenty four hundred dollars. I don't know precisely. My salary was either two thousand dollars or twenty-four hundred dollars.

Gortler: That helps me to understand how much people were making in those days. How did you decide on your first problems? These aren't papers that you recommended to me, but I know from the titles that you were doing alkylation of naphthalene, some periodate oxidation, and some cis-trans isomerizations. Is there any particular reason why you opted to do any of those?

Price: Well, the Friedel-Crafts reaction was developed some in my thesis, of course. I had to make tertiary butyl naphthalene. I got very much intrigued by some problems that hadn't been solved, about alkylation of naphthalene by the Friedel-Crafts method. Some people had reported pure compounds that were eutectic mixtures and things like that.

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*Reynold C. Fuson, Ralph A. Connor, Charles C. Price, and H. R. Snyder, Organic Chemistry (New York: J. Wiley and Sons, Inc., 1944).

So we had to straighten out a few of these things. They weren't earthshaking problems, but they were good things to get started on. They provided good experience for senior students. I had to pick something fairly straightforward for the seniors to do.

I really don't remember how I got interested in the periodate problem. Maybe I read about it in the literature—a very specific cleavage of glycols--and thought that it would be interesting to do the kinetics of it. It's an easy reaction to measure; it's fast and the periodate to iodate conversion seemed easy to follow by titration. I had a senior do that work.

I remember how I got started in polymers.

Gortler: Yes, that was one of the things I wanted to ask you. How did you?

Price: Well, Speed Marvel had made optically active secondary butyl alpha-chloracrylate with d-secondary butyl alcohol to see if he could make optically active polymer. He came around one day and said, "Charlie, if you want to study kinetics of reaction, why don't you study one that's useful, like polymerization?" He said, "I've got this optically active monomer and we found out that the polymer has a very different optical rotation than the monomer. It ought to be pretty easy for you to study it." I jumped at the chance. He gave me some of the stuff to work with. In fact, he even gave me the student, Bob Kell.

I think that Kell wanted to work for Speed. Speed had a big group, and he pushed Bob my way and suggested this problem. It turned out to be a very good suggestion. I don't think Speed could have known this, but many peroxide catalyzed polymerizations have fairly complex kinetics. You can either have the first order decomposition, the peroxide initiation, or you can have a catalyzed decomposition of the peroxide. That gives you rather complex kinetics.

Gortler: Right.

Price: This was perfectly simple kinetics. Half order peroxide, first order monomer, just fit absolutely beautifully with the first order initiation, the second order radical monomer reaction, and the second order radical-radical termination. We were able to show that that particular one fit those kinetics beautifully, and that that was the mechanism. That led us immediately to see that peroxide was not a catalyst, but rather a chain initiator. That led us right into proving that end groups were present in the polymer. I think we were the first to show that the catalyst fragments were at the chain ends of the polymer.

It was a simple logical conclusion from the mechanism. Actually, about a year previously, Flory had proposed that mechanism from the literature data. We got much cleaner kinetics than the typical kinetics in the literature.

Gortler: Had you started consulting at that particular time?

Price: No.

Gortler: That was just a totally academic problem at that time.

Price: I imagine Speed had gotten interested in polymers from his consulting. I didn't start consulting for Hercules until '41.

Gortler: I want to spend some time on the problems, but I also want to spend some time just getting a general feeling about physical organic chemistry. You said just a few minutes ago that it really hadn't crystallized in the late thirties. When do you think it did crystallize? When did you consider yourself to be part of a community of physical organic chemists? You were continually fighting that battle at Illinois, I suspect.

Price: Well, the late thirties were certainly the formative years. Physical organic chemistry crystallized, I guess, after World War II, when people like Bartlett, Winstein and Bill Young seemed to have put enough together.

Nothing could really happen during the war because we were concentrating on the war effort. Wasn't it right after the war that we had the first conference on organic mechanisms?

Gortler: That's right. It occurred in 1946. That's when you left Illinois. Setting up that conference must have been your first act as chairman at Notre Dame.

Price: I had proposed before that, that we ought to have some kind of an organized effort in physical organic chemistry. I had suggested undertaking some kind of publishing effort, like Organic Syntheses or Organic Reactions. I thought that we ought to have a monograph published on physical organic chemistry, but I couldn't get much backing for that endeavor.

Paul and I decided, however, that we could organize the mechanisms conference.

Gortler: Paul said that he just went along for the ride. It was your idea.

Price: I think that it probably was my idea. I also pushed this other idea--that we ought to have a little thing that came out summarizing physical organic. There was no other way that people could put their thoughts together.

Gortler: It took another twenty years, or until the early 1960s that Advances in Physical Organic Chemistry and Progress in Physical Organic Chemistry appeared. Getting back to the conference...how did you decide upon who was going to talk and whom to invite? It's become a very elitist conference now. I don't know if it was in those days or not.

Price: Well, that's an interesting question. I didn't save any of that stuff when I left Notre Dame. I had a whole file on that conference. I don't have it now.

Gortler: I've contacted some people out at Notre Dame. Tony Trozzolo has a lot of interesting information about the conferences of the last fifteen or twenty years. He's given me the names of people to contact.

Price: I suppose that Paul and I decided whom to invite. We invited all of the leading people. I'm sure Saul Winstein was there. Jack Roberts was there. I don't know whether the guy at Harvard...

Gortler: Woodward?

Price: I don't know whether he was there or not. Dick Taft was there.

Gortler: Taft may have been too young.

Price: He might not have been there.

Gortler: That's right, I think that he was a post-doc for Hammett about four or five years later.

Price: He might not have been there.

Gortler: Hammett was there. What about Hammett's book?* What kind of impact do you think that had?

Price: Oh, tremendous. I should have mentioned that. It was published after I completed my graduate studies.

Gortler: That's right. It wasn't published until 1940.

Price: I think that was an extremely influential book.

Gortler: You think that sort of crystallized it?

Price: Yes. That would certainly have helped a great deal to crystallize it. It was extremely influential. It was the basis of my paper in <u>Chem. Reviews</u>, in which I tried to calculate some of these effects.** It was very much a part of the Q/e scheme. Hammett was always very funny about my familiarity with Hammett's sigma-rho, how it developed and its

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*Louis P. Hammett, <u>Physical Organic Chemistry</u> (New York: McGraw-Hill Book Company, Inc., 1940).

**See page 15 of this transcript.

theoretical basis. He was such a purist of a physical chemist that he didn't like to admit that his equation had any theoretical basis. He liked to call his equation empirical.

Gortler: Yes.

Price: There's a good theoretical basis for the electrical interaction. Eyring used those electrical interactions as an additive factor in the activation energy. I don't know why Hammett was so skittish about it. He was very critical of me for thinking that there was a theoretical basis. I just didn't quite understand that view because there was a good theoretical basis for it.

The fact that you could add an electrical factor to the activation energy led us to add a resonance factor to the activation energy. That became the Q factor in the copolymerization equation. It became a way of measuring the resonance effect in reactions. It gave a quantitative number that measured the amount of resonance stabilization factors of substituents attached to carbon- carbon double bonds.

A substantial number of the purists didn't like to interpret things, however. I always thought that that's what good science is about. If you just have the cold numbers, you haven't really got the feel of what it is to do science.

Gortler: Yes. I think that's important; the contrasting approaches help me a good deal.

Price: Incidentally, since we're talking about the Q/e scheme, I'd like to mention that Walling was always a detractor of it. He told me that he was a detractor because he had thought of it first, and because there was no absolute way to determine Q and e. That's right. There is none. You have to assign a value for Q and e for a reference monomer. You just have to be reasonable about it. Hammett's sigma gave me a very good way of picking where I wanted the e to be. If e is going to be quantitatively related to sigma and they are linearly related, that gives you a way of picking your scale for e. That then fixes the Q values. Walling was always very negative about that approach, probably because he said he thought of it first and thought it wasn't any good. (laughter) I don't know. That's what he or somebody else told me. Cheves and I are good friends.

Gortler: I'm going to talk to him this fall. I'll see if I can get his side.

At some time during the period from 1938 to 1942, or perhaps from 1938 to 1946, a shift occurred in organic chemistry. It wasn't a revolution, I don't think, but rather, a shift. Physical organic chemistry began to emerge.

Price: Yes, it became recognized at some time in that decade.

Gortler: Can you give me a general impression of what was happening? Were the classical organic chemists fighting the emergence of physical organic chemistry tooth and nail? Or did they recognize that some kind of change was about to occur and that it was inevitable? Can you think of anything that Speed Marvel or Roger Adams said or did?

Price: Well, I suppose that the wartime research itself affected this issue because everybody had to pitch in. Bartlett did a lot of work on nitrogen mustards and we did a lot of work on nitrogen mustards and sulfur mustards. Saul Winstein was shocked at the end of the war to find out how much we had developed during the war of anchimeric assistance that he didn't know about. It was just the funniest damn thing to see how he reacted at the mechanisms conference held at Notre Dame. You know, he just couldn't believe that we really had come up with all of these ideas during the war. That's an interesting story, how Paul and I developed the three membered ring.

Gortler: Yes. I'd like to hear about that. In fact, there's one little paper, "The Analysis of the Mustard Using Thiosulphate,"* that seems almost insignificant and that you recommended I read. It's a clever little paper, but the important thing about it apparently is the three membered ring. Did you recommmend it to me because that was the first time somebody had put that in print?

Price: Before that everybody wrote of it as a carbonium ion, which is just nonsense. There are still crazy characters, not chemists, but biologists, who write about nitrogen mustard going through carbonium ions. It's just ridiculous. It's also amazing how something that is wrong gets started and how hard it is to get it corrected.

I had intended to say that Paul Bartlett had to work on practical problems. I too had to work on practical problems, for example, I worked on the mechanism of emulsion polymerization during the war.

Gortler: That was a war project too?

Price: No. I just did that with one guy as a piece of side research. I think that Speed may have gotten me a little research money so that I could work on the emulsion polymerization of styrene. We had to work on the behavior of chemical warfare agents in water, and so it was very useful to know what the mechanisms of these reactions were. It helped us to know the mechanism of the reaction forming styrene-butadiene rubber, including the mechanism of the mercaptan modifier in order to control the molecular weight. It fit

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*Charles Price and Lynn Wakefield, "Reactions and Analysis of Beta-Chloroethyl Sulfides in Water", Journal of Organic Chemistry, 12 (1947), 232-37.

hand in glove with what I'd been doing and with what I'd been saying about chain transfer mechanisms. So, mechanisms earned their keep in a lot of practical problems on which people were forced to work.

If it weren't for the war, some people might not have worked on practical problems. I might have, but Paul Bartlett wouldn't have. He might have focused upon esoteric, academic matters instead of working on practical things that everybody knew were important. I think that the more conventional chemists realized that this stuff wasn't purely esoteric, that it could perform a useful function. I think that it helped to educate a lot of chemists like Roger Adams, who was head of the National Defense Research Committee's Chemistry Section, and Speed, who was directing the research on synthetic rubber. They began to see that these things weren't totally useless.

A lot of people did, and still do, a lot of totally useless stuff, what I call crossword puzzle chemistry. Solving the New York Times crossword puzzle is an interesting challenge, but when you've solved it, you haven't pushed the state of humanity very far forward. Too much, I think, still happens that way. It's best to pick something that not only would solve the problem, but also would be useful.

Although, in peace time, we were a bunch of the damnedest independent, contrary characters you could imagine; during the war we all had to do useful stuff and work together. So, guys like Adams and Marvel had to listen to guys like Bartlett and myself and Bill Young, who were working on problems that they knew had to be solved. I think that the war played a very important role, accelerating the recognition of the value of applying physical organic chemistry to practical problems. I never really thought about it in this way until now.

Gortler: I think that it makes a lot of sense. It must also have had an enormous impact on industrial outfits, because they suddenly started to look for physical organic chemists after the war.

Price: I think that wartime cooperation greatly helped to speed up the legitimization of the physical organic approach to problems.

Gortler: Yes. Well, let's talk a little bit more about some of the problems on which you worked. Consider, for example, your article in Chemical Reviews that dealt with aromatic substitution.* What possessed you to write it at that particular time? Did you think that it would be a good thing for you to do or did someone ask you to write it.

Price: No, no one asked me to write it. It was just

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*See page 15 of this transcript.

something that I thought was interesting, to try to assess the effects on electrophilic substitution from electrostatics.

Gortler: OK. You tended to ignore resonance work; you ignored the English school almost entirely. You made no reference to Ingold or Robertson. Was that intentional?

Price: No. I don't think so. I was probably just ignorant of their work at the time. I wasn't as aware of their research as I should have been.

Gortler: I see.

Price: I've never been as much of a scholar as I should have been. You know, I don't like to read a lot of other people's research. I like to do what I like to do. I do only the reading that seems to be absolutely essential. I guess this has gotten me into trouble at times. I simply knew very little of what those people were doing, just as they knew very little of what I was doing. When I went to talk to Ingold and Heilbron, I was flabbergasted to learn how little they knew about free radical reactions. Well, when you get immersed in your own thing, you just don't do as much reading in other areas.

I did have this idea, however, about the importance of simple electrical interactions, and I thought that an article in Chem Reviews was a good vehicle with which to express my views. I guess I was a little brash, giving less emphasis to resonance than I now would do. I think I have a more balanced view now. It just struck me, when I did those calculations, that I could show that the electrical interaction accounted for almost everything. I'm still convinced that it is by far the most important quantitative contributor in ionic reactions. If a dipole interacts with a charge, electrical interaction will still be more important than resonance interaction.

I don't deny that there are resonance effects. Certainly, free radical reactions and the stabilization of carbon free radicals are not entirely electrostatic phenomena. I realize that resonance is a small factor in ionic reactions. In free radical reactions, however, it's a fairly significant one, although not the only one. The e values do affect the copolymerization very significantly. I think, therefore, that I wrote that article in Chem Reviews primarily to express these views.

Gortler: OK. Let's talk now about your work during the war.

Price: I supervised two major programs during the war. One involved antimalarial drugs; the other concerned the treatment of water contaminated with chemical warfare agents. I also collaborated with Speed a little bit on the rubber program and did some work with a few others. I collaborated with Bob Fuson on some of his work on chemical warfare agents.

I'll talk a bit more now about my research with chemical warfare agents. I was requested to ascertain the chemistry of the behavior of sulphur mustard in water, in order to find out how to analyze for it, so as to devise satisfactory methods of decontaminating the water. In order to find out how to analyse for it, however, I needed to know exactly what the mechanism of the hydrolysis was. So, I read a lot. There was a modest amount of relevant material in the literature because, of course, sulphur mustard had been used in World War I. Although it was clear that the reaction was simple first order reaction, the products of the reaction were determined by the nature of nucleophiles in the solution. When you hydrolyzed mustard in pure water you got a half-life of eight minutes. If you put thiosulphate in, you changed the product completely to the thiosulphate replacement product with exactly the same half-life. People had written about it in the literature as though it went through a carbonium ion and called it an S 1 reaction. That just didn't wash with me.

I could not believe that a carbonium ion could be that selective about what it reacted with. There was no evidence that the t-butyl carbonium ion was anywhere near that selective. It just seemed totally wrong to think that it was a carbonium ion. The answer finally dawned on me after thinking about it for a few weeks and worrying about what the proper mechanism was. One day it dawned on me at about five in the morning. I woke up early and there was the answer sitting right in front of me.

I want to acknowledge the subliminal effect upon me of my knowledge of the work of Young and Winstein. They had just explained the stereochemistry of some bromohydrin reactions by bromonium ion intermediates. Certainly, if bromine can participate in such a thing, sulphur ought to be a lot better. Yet, it still took me a couple of weeks to find the explanation. When I did, everything fit together beautifully.

When we went to the next meeting of the NDRC subcommittee, I was prepared to announce that I could explain the mechanism of this reaction. Paul Bartlett reported before me, however, and said that he had just come to the conclusion that nitrogen mustard went through cyclic imonium ion. Within a span of a few weeks therefore, each of us had come to the conclusion that the key intermediate was the intermediate three member ring; the intermediate imonium ion for the nitrogen mustards and the sulfonium for the sulphur mustards. Our hypotheses explained everything. Later, evidence was presented to show that you could predict rearrangements. For example, the sulfonium intermediate was formed when you unsymmetrically substituted Beta-chloro alkylsulfides. I think everybody agrees that that's the case.

In the sulphur mustard cases I don't think anybody has ever been able to isolate the intermediate. It's much more reactive. In later work, in cancer chemotherapy work with diethyl chlorethylamine, the cyclic imonium ion is stable enough to use as a reagent. It cyclizes in solution and hydrolyzes only one percent a week. We just used the cyclic intermediate there. There were great differences in the stability of those cyclic intermediates. In sulphur mustard I don't think anybody has ever been able to prove the intermediate by isolating it, as they have in the case of some of the nitrogen mustards.

That was an exciting time and Saul Winstein was always kind of miffed that we'd come to these conclusions during the war before he came up with his idea of anchimeric assistance. He reacted in this way because these are ideal examples of anchimeric assistance. Of course, he invented the term anchimeric assistance to explain other kinds of reactions as well. Paul and I both talked about this at that Notre Dame mechanism symposium.

Gortler: Is that the first time he had heard about this?

Price: Yes. He was not involved in that sort of research during the war. I don't know what kind of war work he did although he must have done something.

Gortler: I see.

Price: I think that it was during the symposium that he first learned that Paul and I had both come up with a mechanism for those reactions.

Gortler: You mentioned that. Do you remember any more details of his finding that out? Or had you just talked about it at the time, and you just remember his surprise?

Price: Well, surprise and slight annoyance at the fact that he suspected that we might want to claim more credit than that due to us. We weren't trying to steal anybody's credit for anything. We had done it independently of him...well, not entirely independently of him, because Paul Bartlett and I were well aware of that very classic work that Saul did for his Ph.D. topic with Bill Young on the cyclic bromonium intermediates. That was a classic, a great leap of the imagination. Bromonium ions were not nearly as common a thing about which to think. I guess that only iodonium ions were actually known at that time.

Gortler: Well, that covers a lot of that material. It was a revelation to me--for both of us in a way--that the war had drawn attention to physical organic chemistry and its practitioners. This had a great deal to do with the crystallization of the physical organic community and its later success.

How were those war problems assigned? Do you remember how you happened to be working on the antimalarial project, and the water treatment project?

Price: I'm not sure that I recollect how that occurred. I believe, however, that Roger Adams was very high up in the NDRC operation, and I'm sure that Speed Marvel was also very much involved in the upper levels of policy and decision making.

I should remember how the antimalarial work got started. I did get involved with it, and I had two other profs working with me. Initially, it was my project. Harold Snyder then joined it and then Nelson Leonard did likewise. So, we had a good team working on it. I just don't remember though how the program actually got started and became my responsibility. It's rather ridiculous; I should remember.

Gortler: How about the water treatment work?

Price: The Illinois chemistry department was also the Illinois Water Treatment Center. Arthur Buswell, one of the professors there, was its director. The department had gotten involved, probably through Roger Adams, in the water problem and needed some extra help doing the chemical analysis. Since I was the mechanisms man at Illinois at that time, I was asked to work with them. Originally I set up a subproject under Buswell. I worked on the mechanisms of reaction of chemical warfare agents in water. We had a very interesting group. Orville Bullitt, Jr., for example, came complete with wife and maid and not quite a chauffeur.

Gortler: Who?

Price: Orville Bullitt was from a very wealthy Philadelphia family.

Gortler: I see.

Price: Al Pohland was the other fellow who worked with us. Some years later he developed Darvon, which has been in the news a bit recently. We had some other interesting people working on this problem, like Bernie Velzen.

When Buswell went off to some other assignment, probably to serve as a reserve officer, I inherited the whole project along with the pilot plant water treatment outfit. We visited all kinds of carbon black manufacturers for the right kind of carbon black, and we designed a little kit that would be sent out in the field for analysis of all kinds of chemical warfare agents in water.

I had those two major programs that were under my aegis, one on antimalarial drugs and one on water treatment. It's the one on water treatment that got me involved in mechanisms. The one on drugs was straight synthetic organic.

Gortler: When you did the physical organic chemistry, did you think of it as a sort of side research? Did you think that it was necessary for your understanding of the problem?

Price: It was essential. If we were going to cope with these reagents in water, we had to know what was going to happen to them in water and what happened to them when they were treated with chlorine. We got involved in oxidation and degradation and learned some very interesting things. We found out that nitrogen mustard, which is a tertiary amine, is rapidly degraded by chlorine water. At the time, I thought that this was pretty original. Eventually, however, when we looked deeply into the literature we found that somebody had discovered this a long time ago. When you chlorinate trimethylamine, a chlorine goes onto the nitrogen first, apparently to give a quaternary chloramine. It then leaves as HCl, to give a quaternary Schiff base. That then hydrolyzes to give formaldehyde and dimethylamine. The dimethylamine can become a chloramine, split out HCl, and give the Schiff base of methylamine and formaldehyde. You can therefore eventually take all of the alkyl groups off nitrogen and destroy the chlorine in the process.

So, when you're going to treat water that might contain chemical warfare agents, you have to know what's happening to them when they are just sitting in the water, and you have to know what they are going to do to typical water treatment chemicals. So, we had to do a lot of chemistry.

Gortler: In a sense, though, you weren't just worrying about the products and the starting materials, as I think one might have done in an earlier era. You were understanding the reactions.

Price: Yes. I suppose that because my interests were oriented towards understanding reactions, I was more conscious of doing it that way than I would have been if I'd been a pure organic chemist.

Gortler: I see, so it wasn't hard to justify what you were doing. They were interested in results, and as long as you turned out results, no one questioned your approach.

Price: Yes.

Gortler: The antimalarial program was essentially a synthetic program.

Price: Yes. We attempted mainly to make quinoline analogues of chloroquine. We did a few other things, but that was the main thrust of it.

Gortler: You did later patent some of these syntheses.

Price: Yes. One, for example, was the ethoxymethylene malonic-ester synthesis on which Roy Roberts worked. We patented it through the government. It was a government-owned patent, and it was used commercially. I went out to National Aniline in Buffalo at least once a month while they were doing the pilot plant work on that synthesis. It had a big advantage over the German synthesis. I've forgotten the exact details of the German synthesis, but it started with metachloraniline. They got a mixture of five and seven chloro nucleus because it can close in either of two ways. On the other hand, during our synthesis, the ethoxymethylene closed almost exclusively seven, almost never five. It was therefore a much neater, cleaner snythesis and economically competitive. So, the government patented it and it was used for some time after the war as a way to make 4,7-dichloroquinoline. Roberts, of course, is now a professor at Texas.

Gortler: He went to Merck first?

Price: That's right. He was at Merck for a while. He had a rather remarkable academic record. He earned only A's throughout college and graduate school.

Gortler: He did all of his studies at Illinois?

Price: No. He did his undergraduate work at Austin College in Sherman, Texas.

Gortler: For some reason I didn't write down that work on the alkylation using optically active...

Price: Secondary butyl alcohol?

Gortler: That's right. I thought that was fascinating, as was the fact that you came up with the optically active product using one catalyst. Does that still hold true for optically active synthesis?

Price: Yes. That was confirmed, I think, by Burwell. I think that with the alcohol and boron trifluoride, we got an optically active secondary butyl benzene with inversion. There was, however, something like ninety-nine percent racemization. So, it fit with the carbonium ion, although with a slight preference for inversion.

Gortler: That seemed to have been a very up to date analysis that you made at that time. You really must have been very aware of the kinds of mechanisms you could get into with Lewis catalysts.

Price: You know, people's minds have different ways of working, and mine has always been very geometrically and sterically oriented. Geometry was always just a toy for me in school. I had no trouble with it at all. Of course I did a lot of geometric analysis in order to calculate those electrical interactions. Using geometry helped me to gauge the distances between charges.

When Roger Adams went off to Washington, which he did quite often, I taught his course in stereochemistry. It was a very easy thing for me to do because I've always been handy with geometry and steric factors. I've always felt very much at home with steric effects and stereochemistry.

Gortler: That reminds me, I wanted to ask you to tell me what courses you taught at Illinois and the years that you were there. Do you remember what you first taught?

Price: I started teaching the elementary organic course for pre-meds and home ecs.

Gortler: That's right.

Price: I taught that for quite a few years. The other two courses that I taught there were the advanced course that Adams taught to graduate students on stereochemistry and the qualitative organic course from Shriner and Fuson. Teaching the latter was fun. Adams' course included a lot more than stereochemistry; it included chemistry as well. It involved all of the stereochemistry and chemistry of sugars. So, it was a fairly broad gauge course.

Gortler: During your stay there, didn't you introduce a theoretical organic or a physical organic course? Do you know if a course like that was taught?

Price: I don't think we got around to doing that while I was there. We did as soon as I got to Notre Dame. During the war, there was a lot of pressure to keep the war research going.

Gortler: That's true.

Price: There wasn't too much opportunity to revise the curriculum. We just went through what we had. I left at the end of '45. My new job at Notre Dame began in January of '46. The war had hardly ended before I left.

Gortler: How did you happen to pick up and go? Was the job offered to you?

Price: Oh yes. I had jobs offered to me by Pittsburgh and Missouri. I didn't consider either offer very seriously because Illinois is a great place to work. Father Moore, the dean of the graduate school, and two professors came from Notre Dame to talk to me about joining their faculty, with a possibility of becoming chairman of the chemistry department. Having turned down Pittsburgh and Missouri, I thought of this as a joke. I had the typical opinion of Notre Dame, that it was a football school. I really didn't take it seriously. Yet, because I'd never been there, I thought, hell, I might as well get a look at the place. I was tremendously impressed with the beautiful campus, and Father Moore was just an absolutely superb human being with very high academic standards. Of course, chemistry was the best department that Notre Dame had. Father Nieuwland had pioneered there with some work that had brought in lots of royalties. Because of that, they'd been able to invest a fair amount in the department.

I learned also that, contrary to Illinois where all of the football receipts went only to the football program, Notre Dame used its football receipts to help pay the bill for other athletic programs. Notre Dame actually had a very sensible policy in this regard. They wanted good athletes but they certainly didn't have a bunch of basket weaving courses and things like that for their athletes. Leon Hart was an engineer and a good one. He had a B average. One year, the captain of the football team was a chem engineer. A first string tackle took my organic course as a junior. He wanted to be an M.D. and so he quit playing football to make sure that he got high enough grades to get into med school. Nobody put any pressure on him. It was his choice.

So, I was impressed with the place, and they did offer me the job as head of the chemistry department. I was ambitious enough to think that being chairman would be interesting, especially since I would be the youngest member of the department.

I enjoyed it there. It was, by and large, a very interesting place to work. We got a new building and some pretty good people to work in it. Ernest Eliel was the first faculty member that I hired, and I think a darned good one.

Gortler: Yes. He was not a student of yours?

Price: No, he was Harold Snyder's at Illinois. I hadn't known Ernest. I guess I didn't hire him until three or four years after I got there. He must have joined our faculty about '47 or '48. He got through Illinois in two years and wrote about five papers. Harold said that he was the advisor to most of the other students in the department. A very smart guy. No, he wasn't the first faculty member that I hired. Paul Doty was one of the first ones and a very interesting character.

Gortler: Paul Doty...the biochemist who ended up at Harvard?

Price: Yes. Actually he spent almost a month debating whether to leave Notre Dame or to go to Harvard. I was very pleased that he didn't just snap at it. He liked Notre Dame. Milton Burton, who is pretty well known for his work on radiation chemistry, started the radiation lab at Notre Dame that is now well known.

Being chairman at Notre Dame was an interesting job, and I enjoyed it. I'm sure that it distracted me from my scientific endeavors, but I still managed to publish quite a few papers.

Gortler: I think that I counted twenty-one papers by '46. Of course, there was a lot of war work.

Price: Some of that was from the backlog of war work.

Gortler: Yes, it was war work, but there were quite a few papers for the next several years.

During your years at Illinois you not only did war work but also pursued your own research interests. To whom were you talking at Illinois? Who were your closest colleagues? When you wanted to talk chemistry to whom did you talk, both at Illinois and outside of Illinois?

Price: I suppose that I talked more to chemists outside of Illinois than to those at Illinois, because there weren't too many at Illinois who were interested in my work. I know that I had many discussions with Jack Roberts and numerous arguments with Saul Winstein. I can't pinpoint the time when these occurred, whether before the end of the war or after it, but they certainly occurred sometime during the forties. I'm sure that I had lots of discussions with people like Herb Brown. He's colorful. I'm a great admirer of Herb Brown.

Gortler: That's interesting because you recommended that I look at a paper written in 1951 that reevaluated your views on electrostatic effects and that contained some new calculations.* I found it interesting because you made a comment in the last paragraph of the paper that clearly attributes to Herb Brown a statement that later would be called the Hammond Polstulate.

Price: I've been a little annoyed about the many things that I originated that others christened and claimed as their own. I mean, for example, no-mechanism reactions; that's all over my carbon-carbon double bond book. Nobody has bothered to mention that. I was one of the very first ones to suggest a cyclic reaction mechanism. Pi complexes were also suggested in that book, before Michael Dewar ever mentioned them. He has given me credit for being one of the first ones to postulate these complexes. Somehow or other I didn't have the flair to invent a name or something like that, that caught people's fancies. By the way, I am glad that you too noticed my comment about Herb Brown.

Gortler: It was apparent that you were talking to Herb Brown at that time.

Did you feel at any time that you were competing with other chemists on particular problems? Can you recall a specific instance where you felt that you were actively competing with someone else in order to come up with an answer before he did?

Price: I guess I've never really felt that I had to beat somebody. I do remember very consciously on many occasions feeling a little discouraged about chemistry. Why wasn't I getting more done? I then consoled myself by thinking about what all of the other guys were doing. That left me feeling a little better.

Gortler: Yes, inevitably, when somebody else says, "Gee, it's going very slowly," you feel relieved.

Had Elliot Alexander come to Illinois before you left?

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*See page 16 of this transcript.

Price: No, he was my replacement at Illinois. I did know him, however. He was a graduate of Swarthmore and I remember playing squash with him a few times. I think he worked at Du Pont for a year or two after he got his degree.

Gortler: OK. I haven't followed his career, but somehow I think that he got his degree at Columbia with Cope and that Cope was there only during the war years. I guess he was officially listed on the Columbia faculty, but Alexander must have been one of the few students he had at Columbia.

Price: Yes. I think that's right. After he got his degree, he worked for a year or two or maybe three at Du Pont.

Gortler: I see.

Price: I remember playing squash with him down at the Y in Wilmington. We got to be very good friends. He was wonderful. He and his wife were just absolutely super human beings. It was a terrible tragedy that he got killed so young. He replaced me at Illinois, but I saw a lot of him. We met at meetings. I remember playing tennis with him at the organic symposium at Northwestern, shortly after the war.

Gortler: I'd like to ask you about two other people. First, did Virgil Boekelheide work with you at one time?

Price: He worked with me as a post-doc on the antimalarial program at Illinois.

Gortler: OK. Did you know Frank Westheimer when you were at Harvard?

Price: Sure. We were and still are very good friends. I know Frank and his wife very well. We still exchange Christmas cards and we've had many discussions about chemistry.

I enjoyed discussing chemistry with Linus Pauling as much as with anybody. He always amused me immensely. Linus and I never had any serious disagreements although his disciples, people like Wheland and Winstein, considered me to be a heretic. As for Linus, I would tell him what I was thinking about and he would agree completely. He had no problems with the way I thought about things. He knew the limitations of resonance theory--that it wasn't the answer for everything. He also understood electrical effects.

Linus and I are still good friends. Of course we've shared not only an interest in trying to understand the structure and reactivity of chemicals, but also a commitment to bring about peace.

Gortler: Yes. How did you justify the war work that you did?

Price: I didn't have any problems justifying to myself either trying to cure malaria or trying to take toxic stuff out of water. Also, although I'm a Quaker and have a strong interest in promoting peace, I'm not an absolute pacifist. I recognize the need for a police force to support community interests. I oppose the use of force in self-interest, which is what I think war is. So, I never had any serious difficulties doing what I did for the war effort.

I have wondered how I would have reacted if I had been pushed to do work more directly related to destructive ends. The closest that I got to that, however, was when I cooperated with Bob Fuson to try to develop water-denial agents. These agents are not toxic but do produce a horrid stench when mixed with water so that people cannot bear to drink the water. It was thought that these agents might be needed in North Africa where wells are a crucial natural resource. Rather than poisoning wells, retreating troops might use a water-denial agent to make the water taste so unpleasantly that nobody would drink it. That's about as close as I came to having to work on things that were destructive. Of course, I was working in chemical warfare on extremely toxic things, but I was working on how to detoxify them.

Gortler: Yes. I noticed that and I thought that that fit in very nicely with your Quaker upbringing and your later work.

We talked a bit previously about the Notre Dame conference. Can you recall anything else about its organization? You knew that you were going to Notre Dame at the time that you got the idea. It was a great way to start your career there.

Price: I guess that I thought that it would be a very good way to put Notre Dame on the map. I must say, however, that even though there probably was a planning committee, I haven't the vaguest idea who was on that committee.

Gortler: You have made a number of contributions to theoretical organic chemistry. You've also spent some time in synthesis and structure determination. Did you ever feel that these two aspects of your career were somehow scientifically separate? Or did you think that you were an organic chemist who looked at a variety of problems?

Price: I certainly felt that they were separate. I didn't think that there was any intimate connection between them. I don't think that I did quite as much thinking about mechanisms for synthesis as, say, Bob Woodward did. According to Bob, those two things were very close together. He liked to use mechanisms as a way to devise synthetic approaches. I don't think I was ever quite that conscious about it, although I did think about the mechanisms involved in reactions.

I might mention a little anecdote about that. Right after the war, in '46 or '47, I read a paper by Herman Bruson while I traveled on a train to the Gordon Conferences, still held then on Gibson Island. Bruson, of Rohm & Haas, wrote about the addition of HX compounds to dicyclopentadiene. That norbornene-type double bond is very reactive. All kinds of HX compounds added easily to the double bond. I guess Bruson had a dozen patents on this area, thousands and thousands of compounds that he'd made by addition of HX to dicyclopentadiene. He proved that there was a carbon skeleton rearrangement. He deduced the structure of the rearranged product.

Using a pencil I tried to see if the structures that he proposed fit with the normal carbonium ion mechanism. They didn't. There was no way that he could have gotten the products that he claimed he got. So, I sketched out what I thought the products ought to be.

When I got to the conference, I learned that Paul Bartlett was my roommate. I showed Paul what I'd done and he said, "Gee, that looks very reasonable. I'm working on dicyclopentadiene; let me try it." Within a few weeks Bartlett proved that all of Bruson's compounds were incorrectly assigned. I arrived at that conclusion by considering the mechanisms involved.

Every time I look at a chemical reaction, I like to see if I can imagine what the mechanism is for the reaction. It's almost an automatic reflex action. I don't think it's as consciously planned as it appears to be from a consideration of Bob Woodward's work. Bob certainly gives the impression that it's consciously planned.

Gortler: Yes. I think most people think about his work in that way. Let's see, he hadn't come to Harvard when you were there. Was he at Illinois when you got out there?

Price: Yes. He was there the first summer I was there, the summer of '37. He had been invited to be an instructor in the summer school. Bob completed his undergraduate and graduate education, through the doctoral degree, at MIT in only five years.

Gortler: He fit in well. (laughter)

Price: He did not win friends and influence people at Illinois, I'll tell you. Later, Bob and I became very good friends, but her was a very brash and egotistical young man at that point. He thought that he'd really come out to the boondocks at Illinois, and he was very condescending. He had been assigned an eight o'clock class. Well, in Cambridge, of course, there aren't any eight o'clock classes. He just never got there on time. He was also supposed to be supervising a lab, but he was very seldom there. He played poker with Speed and a couple of the guys and then never paid his debts. He antagonized people right and left. Harold Snyder and I were still labmates and post-docs for Roger Adams that summer. (I didn't start teaching until that fall). Bob had the office next to us, and he spent a lot of time in our place bumming cigarettes. Harold finally figured out the answer to that. He kept his cigarettes hidden in a drawer, and kept a pack on the desk with just one cigarette in it. Bob would come in, shake the pack, and look at us. He didn't quite have the nerve to take it.

Well, you know, that kind of behavior did influence Bob's career. He never got the ACS award in pure chemistry, a gross miscarriage of justice. Illinois profs had a big influence in the ACS, however, and they just could not stomach this guy. Once Bob won the Nobel prize and a few things began to fall into place for him, he mellowed greatly. He's a fine guy now. I've always enjoyed talking with him. We've had a lot of fun together. But that first summer he taught at Illinois...

Gortler: He just stayed out there for that one short period...

Price: Just one summer.

Gortler: Someone else had told me that he didn't win friends and influence people out there. He said that the worst thing that Bob did was to borrow glassware and then not wash it after he used it.

Price: Well, that's true too. I think Speed got so incensed when Bob didn't pay his poker debts that he wrote to the prof at MIT and asked, "What kind of guy did you send to us?" I think that the prof got after him and made him pay his debts. He just was a very brash, egocentric young man.

Gortler: I'm hoping that he's sufficiently mild enough now that he'll be willing to talk to me one of these days.

Price: Hasn't he been willing?

Gortler: I haven't asked him yet.

Price: Oh, it won't be any problem. Bob is a fine guy now.

Gortler: When I was a graduate student he spent two hours one afternoon describing possible research problems to me.

Price: You won't have any problems with Bob now.

Gortler: At first I thought, gee, he's off in synthesis. Now, however, he's become a really important figure. He was one of those transitional figures who applied physical organic chemistry to synthesis.

In recent years, you've edged into biochemistry. You've worked on alkylations, proteins, and nucleic acids. Can you tell me a little about it? How did you get into that work?

Price: It occurred as a direct consequence of both of my wartime projects. One of the men in the medical school here at Penn, Buck Jones, had been responsible for testing our candidate antimalarial drugs at the Illinois State Prison. I had worked closely with him. After I arrived here, he immediately visited me and said that he had a bright idea about a drug for cancer.

He knew that the attack on a nucleic acid was a key facet in cancer chemotherapy. He also knew that antimalarial drugs intercalated into the DNA chain. That was how they exerted their antimalarial activity. He thought, therefore, why don't we take an antimalarial drug, which has a preference for going to these chains, and put an alkylating agent on the end of it? Most of these antimalarial drugs have a diethylamine group at the end of the chain. All you have to do is to put the two chlorines out there and you've got a drug which looks exactly like an antimalarial drug, except that there is a nitrogen mustard at the end. We therefore made a lot of antimalarial mustards, quinacrine, 7-chloroquinoline, and a whole host of others. These were extremely interesting anticancer drugs.

Buck Jones actually tested one of them clinically because it had been extremely effective in mice and rats. Actually, in his clinical tests, it was extremely effective in humans. It was probably one of the most potent of all the mustards for treating cancer. However, a number of people who were treated got the most weird psychotic experiences. It was therefore struck from the program.

By the time we were done here at Penn we had made five or six hundred compounds of modified mustards of all kinds. We also made a variety of heterocyclic antimetabolites. After spending five years making compounds, testing them, and finding that most of them were not much better than nitrogen mustard, we began to get discouraged about the synthetic approach.

We decided that it would be best to take a mechanistic approach, that is, to try to determine quantitatively what happens when the alkylating agents react with nucleic acids. So, we started measuring the rates of reaction of adenine, thymine, cytosine, and guanine with nitrogen mustard. We then put on the sugar, put on the phosphate, and strung them together into homopolymers. While doing this work, incidentally, we used the diethyl chlorethylamine instead of nitrogen mustard because doing so assisted our analysis. It is far simpler to look at a monofunctional alkylating agent than a difunctional agent. When you have a sequence of two reactions that have different rates you have a much more complicated situation.

After we decided to shift to diethyl chlorethylamine, I told an Indian student who started this work to put the mustard in as the hydrochloride, neutralize it to pH 7, let it cyclize, and then use it as an alkylating agent. He tried to do all of this, but then returned to me and said, "It didn't cyclize, Doc."

I said, "Go back and do it again."

He did. He then returned and repeated, "It didn't cyclize, Doc."

All of a sudden it dawned on me that in the dichlorethyl mustard, both of the chlorines lower the pH of the base to about 7 or 6, so that at pH 7 it's mostly deprotonated. I checked and the pH of diethyl chlorethylamine is 10. At pH 7 only one-tenth of one percent is deprotonated. All we had to do therefore was to move to pH 10. It cyclized almost instantly. That gave us the stable reagent. We then backed it down to pH 7 and we used it for our alkylations.

Most of our alkylation studies actually used diethyl ethyleneimonium ions as the alkylating agent. It reacted readily with nucleophiles. We titrated it with thiosulphate. It reacted with nucleophilic centers and we proved where it went on the different nucleophiles, guanines, adenines and so on.

I guess that we really only discovered one thing, although it was an interesting and important discovery. It had been reported in the literature that when DNA was in the double helix, adenine didn't alkylate because the nucleophilic center from adenosine is the one that's hydrogen bonded. The guanine nucleophilic center was open, and it was known qualitatively that you get 7-alkylguanine. What wasn't known was that it was about fifty times as reactive as ordinary guanine. There was enhanced reactivity. This explains, of course, why DNA is a selective target for nitrogen mustard. You have such an extremely highly enhanced nucleophilicity. Enhanced reactivity holds true only in the double helix DNA. In random coil, the reactivity drops way down. The double helix structure definitely affects the reactivity.

Gortler: I see. This probably just exposes the nucleophilic center in certain ways, or...

Price: I don't think that's the main reason. We hypothesized that the double helix structure made a very effective structure for distributing the charge that you're going to put on the alkylated center. From my old electrostatic days I know that whenever you place a charge on a small body, it possesses a much higher potential energy than if you put the same charge on a big body. Anything that we can do to distribute charge automatically lowers the energy. And, of course, the fact that it's hydrogen bonded means that you can shift the proton more, from nose to nose, to distribute more charge to the other chain.

Gortler: Right.

Price: The fact that they're also pi-bonded means that the electrons in the base above and below could shift into that ring. That leaves these two bases with more plus charge. When you put some plus charge on the adjacent rings you distribute the charge over a whole segment of the chain, by both nose to nose proton sharing and cheek to cheek electron sharing.

Gortler: Right.

Price: That was our explanation. You've lowered the potential energy of the product very greatly and that lowers the potential energy of the transition state. Of course, the nucleophilic center is exposed.

Gortler: I see, you lower the transition state considerably by stabilizing it.

Price: We showed by alkylating a mixture of poly G and poly C that the guanine-cytosine pair was greatly activated by the pairing. That was true for synthetic as well as for natural pairing. We finally published quite an extensive paper because we had to do all of this groundwork. It was a hell of a lot of work to prove that one fact. That was the only surprise and the only interesting feature of the work that really came out and that was quantitatively measured.

We then started to do the same for proteins; that is, to take proteins of known three-dimensional structure in order to know what was bonded to what, and to see if we could come out with any similar observations. Well, we just barely got started on it. We published one paper when the grant supporting my antimalarial work was terminated. My thiobenzene grant was also terminated the same year. These occurrences figured prominently in my decision to retire from chemistry. I also thought that I might be able to do something more interesting and useful for society in some other field.

Gortler: How did you begin your writing about evolution and the synthesis of life? Was it an outgrowth of some prior research?

Price: Well, my work on DNA and protein helped me very much because it gave me background, but my writing about evolution and the synthesis of life started for an entirely different reason.

Perhaps you don't recall that I was president of the ACS in 1965, and that one of the few functions that the president has to discharge is to give a presidential address. I wanted to do something that I hoped would be significant. Such an opportunity doesn't come along very often. In fact, I did two things as president of the ACS that were of some significance. One was to give the presidential address. The other was to start the Chemistry and Public Affairs activities of the society.

While trying to decide what to say during my presidential address, I remembered having heard chemists often say, "Gee, everything that's interesting and exciting has already been done; there's nothing left to do in chemistry." I thought that I'd deflate that sentiment by talking about the synthesis of life. This kind of talk hadn't been given. There were all kinds of reasons why it would be a very exciting, interesting, and significant enterprise. I said during my address, that the synthesis of life would probably be even more significant in the long run than the discovery of atomic energy. The address attracted a lot of attention, some favorable and some very unfavorable.

Gortler: As you might expect.

Price: As a matter of fact, six weeks after my talk, Sol Spiegelman reported on the synthesis of RNA viruses in a test tube--a really remarkable work--and referred to my talk in his paper. I had predicted that it might take twenty years. He said that it took only six weeks.

Sol had done a beautiful job. He wondered about how an RNA virus works in a cell. Why does the RNA virus replicate at the expense of all the other RNA's that are synthesized? I mean, there are all kinds of RNA replicases available in the cell and ordinarily, RNA replicases will replicate any RNA. When an RNA virus invades the cell, however, most of the RNA that's synthesized is new virus RNA. It turns that on and turns off a lot of other reactions.

Sol hypothesized that an RNA virus might have "messenger" RNA in it, that is, that part of the RNA virus that went to the ribosome instructed the ribosome to synthesize a protein that was a very specific enzyme for replicating only that RNA virus. So, he looked at virus-infected single celled organisms and found that they did have an extra enzyme in them. He isolated that enzyme and put it in a test tube, along with a few virus molecules and a lot of triphosphates, ATP, GTP, CTP and UTP for RNA. The enzyme then just started replicating, that is, synthesizing virus in the test tube. Whether Spiegelman synthesized life or not depends upon whether you call a virus a form of life.

Gortler: Right.

Price: Having begun seriously to consider the synthesis of life, I got more and more interested in the topic and have followed the literature about it very carefully. It, and the related topic of evolution, worked its way into my concern about society in a very interesting way. I guess that very shortly after I gave the talk to the ACS, I began talking about and writing articles about the relation of order to evolution. I have prepared a manuscript for my latest book which is on the societal and philosophical consequences of this discovery.

The book on the synthesis of life* was actually written as a prelude to this later book.** I wrote the former in order to become conversant with the scientific facts, and also to establish my credentials to write about this sort of topic.

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*The Synthesis of Life (Stroudsburg, Pennsylvania: Dowden, Hutchinson, and Ross, 1974.)

**Energy and Order: Some Reflections on Evolution (n.p., 1983).

Gortler: You are talking about the book published in the series, Benchmark Papers in Organic Chemistry.

Price: Yes, that's a scientific book. It doesn't deal with societal or philosophical consequences.

Anyhow, it all started for me with the ACS talk. It was a really remarkable happenstance that that topic would become so closely interwoven with my concern for world order. The principles just flow from one to the other. The applicability is remarkable.

The work that we did on the alkylation of DNA and protein helped to get me immersed in biochemistry. It also got me acquainted with a lot of biochemists. All of that helped me to get organized for this kind of work.

Gortler: Before we move on to other aspects of your career, I want to ask you a few more questions about physical organic chemistry. What are the major characteristics of physical organic chemistry? Additionally, what was it and what is it? In just a few minutes I'll ask you where you think it's headed. For the moment, however, tell me its major characteristics.

Price: Well, I guess that I can respond in a very simple-minded way by saying that an organic chemist is interested in what happens, whereas a physical organic chemist is interested in why and how it happens. I think that's the distinguishing characteristic between the traditional approach to organic chemistry and the approach of physical organic chemistry.

Gortler: Yes.

Price: If I mix A and B, what do I get? The physical organic chemist wants to know the intermediates, how the reaction goes, and why it goes as it does. He also wants to know all of the factors influencing it.

The effect of structure on reactivity is important for both synthetic and physical organic chemistry. Even the synthetic chemists need to know that. Trying to understand the nature of the effect of the structure on reactivity is certainly a characteristic feature of physical organic chemistry. Traditional organic chemists might have the feel for this in their bones, without really knowing why.

Gortler: Yes. Do you think, in fact, that organic chemistry is now, with the advent of physical organic chemistry, undergoing some kind of a change?

Price: I don't think there's any question about it. The organic chemist today is a very, very rare bird if he doesn't think about mechanisms of reactions. Even the most synthetic chemist thinks about them.

Gortler: These are my feelings too. I feel more secure when other people also say that they feel that way.

Price: Oh, no question about it.

Gortler: Where do you think physical organic chemistry is headed? Phil Skell said to me a year or two ago, "Organic chemistry is dead. The only thing physical organic chemists do now is to add another figure after the decimal point on already well known numbers."

I'm not sure that I agree with that, but I wonder if you've given it thought?

Price: No, I don't agree with that. There are still some reactions that are fascinating and that I don't know anything about. As long as they're sitting there unexplained, they offer a challenge to a physical organic chemist.

I shall cite one example, the norbornene business. If you add HSCN to norbornene, you get almost instantaneous addition with no rearranging, whereas every other HX instantly rearranges it. Well, you know, you can say that it just goes by a four-center addition reaction, and wave your arms at it. But I don't think anybody knows or has done work to elucidate that mechanism completely.

I do agree that an awful lot of academic research has no more significance than solving a crossword puzzle. The researcher may merely put another figure after the decimal point on the end of numbers he already knows, or solve a very clever kind of insignificant problem.

Gortler: Yes.

Price: I think that's probably true in every field of chemistry. It's certainly true that the emphasis on practical results led to decreasing financial support for physical organic by the NSF. Consequently, physical organic chemistry came on hard times. Yet, I don't think that that necessarily means that there isn't anything interesting left to do in physical organic chemistry.

Gortler: What about the orientation that you took, that is, working more on biological problems? I assume that you felt that there was a role for you there.

Price: Yes. There probably still is a fairly substantial role for me to play. I also think that there is a lot of stuff in that area that we don't understand yet. Somebody with a good background in physical organic chemistry is bound to be able to make important contributions to biochemistry.

I certainly don't think that everything that can be done in physical organic chemistry has been done, or that it's a dead subject. I think that even if a lot of the fundamentals have been worked out, people will be using these principles of physical organic chemistry in the way that they approach chemical problems in biochemistry, solar energy, or whatever.

The interaction of molecules in the solid state is an area that we don't understand too well yet. There's a lot of exciting stuff that can be done there. We know dilute solutions because that's where it's easy to study and understand the behavior of molecules. How the molecules behave en masse, however, is another thing. A lot of very important things are yet to be found in that area. Consider, for example, heterogeneous catalysis. Think about how important Ziegler catalysis is and about how little people really know about the catalytic center.

Incidentally, this is another of my suggestions that has been totally ignored. I was the first one to propose the mechanism of Ziegler catalysis that is now accepted as the basic mechanism. One of the few who has acknowledged my priority in this matter is Natta. Everyone else ascribes that mechanism to somebody else. I'm not angry or anything but...

When we were doing our work on the stereochemistry of epoxide polymerization with base and with coordination catalysts, it seemed to me that the mechanism was related closely to the Ziegler system. At the time, we obtained much evidence for epoxide polymerization because we had optically active monomers with which to work. We postulated a two metal center, with coordination of the olefin, and then rearrangement. Everybody accepts that as the mechanism, but I don't think anybody mentions that I was the first one to propose it.

Gortler: That's interesting. I was looking at your papers, as well as at some others, and I realized that you were at the forefront of a number of important problems. Did you consciously sit down and say, "Gee, I wonder what's important this year? Maybe I should turn my attention to that." Did you think, "This is going to be a significant problem?" Or did you just fall into these situations?

Price: Well, it was probably not highly conscious, but certainly not entirely unconscious either. Consider, for example, that I got interested in epoxide polymerization because I sensed that polypropylene oxide might make a good rubber. That idea came to me when I was chairing a special conference of the National Research Council. We had decided to gather about twenty research scientists, half from universities and half from industry, and to ask them why a rubber behaves the way it does and how we could get better rubberlike compounds for low temperature use.

As a result of these discussions, it became clear to me that we needed a chain with a high degree of flexibility and a low Van der Waals interaction between chains. To me, that all meant an ether because an ether has a much lower barrier of rotation than a carbon-carbon bond. It has a very low Van der Waals interaction. I thought that one very good rubber for low temperature would be a chain of amorphous polymeric ether. The chain had to be amorphous or it would crystallize. I therefore put the methyl group in there and decided to make polypropylene oxide.

There was a problem associated with my approach, however. Nobody knew how to make a high molecular weight network structure out of polypropylene oxide. That oxide didn't yield a polymeric ether of high molecular weight.

We started to work on that problem at Notre Dame. My idea was to make branched, low molecular weight material and to string it together as a polyester, or polyurethane, or something like that. We succeeded in doing that. I had a student who had finished his Ph.D. in June and wasn't going to teach until September. General Tire agreed to support him. We made a bouncing rubber ball within a month or so.

Everybody thought we were crazy. They didn't believe that we were going to use polypropylene oxide, known as a lubricating fluid, to make rubber. Of course, we did. A few years later, the executives of Union Carbide found out that we could make rubber out of their lubricating oil, and were they flabbergasted. Of course, we had gotten a patent on the process. I bought my first big racing sailboat with the first royalty check.

This work got me thinking about the problems associated with polymerizing propylene oxide. Why, for example, does ethylene oxide grow as a living polymer to any molecular weight, but propylene oxide will only grow to a molecular weight of about 2000. I learned that the latter occurs because of a chain transfer process. The hydrogens on the methyls can be plucked off by a base in an E2 elimination giving an alcoholate ion which starts a new chain. One chain terminates and a new one starts.

About that time, Herman Mark gave a talk at Notre Dame. He had learned about the Ziegler business and could talk about it long before it had been published. He also, I think, happened to know about some patented work at Dow.

Dow had discovered a catalyst for propylene oxide that was uniquely different from any others and that gave a high molecular crystalline polymer. It was for the discovery of propylene oxide what Ziegler's was for propylene. Nobody knew the crystal structure for either crystalline polypropylene oxide or crystalline polypropylene. Because of my background in stereochemistry, it was immediately obvious to me that we would gain a big advantage by working on the epoxide because we could make the monomer optically active. This was known. We made it optically active, polymerized it, and got a polymer with the same characteristics as the material that Dow had produced. While ours was of low molecular weight, it had the same crystal pattern. This proved unequivocally that the Dow catalyst had been stereoselective in making an isotactic polypropylene oxide. We made an optically active polymer with the same melting point and the same crystalline structure.

This occurred within six months after Natta had reported on polypropylene. Furthermore, Natta's evidence that propylene was a crystalline isotactic polymer was simply his analysis of the X-ray spacing repeat distance. His interpretation of isotacticity was, of course, purely hypothesis. He has given us credit for being the first to <u>prove</u> that a catalyst can be stereoselective and give an isotactic polymer. Our proof was based on an oriented fiber that gave an X-ray spacing of 5.85. This fit nicely with the helical isotactic polymer.

We also asked "What's the mechanism of this isotactic polymerization?" We immediately discovered that we could make a lot of catalysts that Dow didn't. They used a ferric chloride-propylene oxide reaction product. We showed that we could use almost any Lewis acid. These gave the crystalline polymer. When it became clear that the important feature was a Lewis acid alkoxide, we postulated that the Lewis acid was coordinating with the epoxy oxygen. An alkoxide on the metal then rearranged in the complex by an internal nucleophilic replacement. We had an epoxide coordinated with alkoxide--it just migrated over--and that made a new alkoxide into which another epoxide molecule was inserted. We therefore proposed that another epoxide coordinated where the alkoxide group was, and then that alkoxide, with the monomer unit in it, could rearrange, and we'd have two units; a coordination, rearrangement mechanism.

In almost our very first paper, when we proposed that mechanism for epoxide, we also proposed it for the Ziegler. Everybody had the most weird ideas on what Ziegler was all about--surfaces, and all kinds of crazy things. We said it's just a catalyst site and everybody now accepts that. As I say, nobody ever credits Charles Price for having been the first one to propose something. I haven't seen anybody ever give me credit. Yet, there's no question that my report was the first in the literature about that mechanism. We had no evidence for olefins at that point, except the analogy. It's there, however, and it gives exactly the picture that you see written in all of the articles today.

Gortler: One never understands the why of that kind of thing. Why is credit given in certain instances, and not in others?

You talked about your being chairman at Notre Dame. You were also chairman here at Penn. I suspect that you approached this appointment in much the same way that you did the one at Notre Dame. When you came here, did you also feel that you had to build or revive a department?

Price: Oh, no question about that. This department was not highly regarded as a center for chemical research. I was very much a missionary.

Gortler: You came here because it was a new challenge?

Price: No question about it. You know, it was very amusing. My friends would say, "Hey, I hear you're going to the University of Pennsylvania." They would then pause and say, "Let's see, who's at Penn now?" That was a typical comment.

The chemistry department at Penn had been very inbred. Most of the faculty had gotten Ph.D.s at Penn. Nice gents, but they had no reputation. It was, therefore, very much of a building job. The physical facilities were abysmal. I came here only because Ralph Connor, who was president of

Rohm and Haas at that time, strongly encouraged me to do so. He had been coauthor of a book with me. Reynold Fuson and Ralph Connor had written an elementary book for pre-med and ag students at Illinois. Snyder and I joined as coauthors for the second edition.*

Gortler: Connor had also been here at Penn, up until the war?

Price: Yes. He then got involved in some NDRC work in Washington.

Gortler: Right.

Price: After the war he became director of research at Rohm and Haas. He then became president. He's a wonderful man and a good friend. He strongly encouraged me to come here, with the promise that Rohm and Haas would help. It did.

Gortler: I see.

Price: An awful lot of what happened here was due to the assistance of Rohm and Haas. It helped us, for example, to build a new building. This was essential to get us started. Yet, Rohm and Haas never allowed us to use its name, so it didn't receive credit due to it.

Gortler: When you came here, were you housed next door, in E. F. Smith Hall, or somewhere else?

Price: Well, there was a much older building right where this one is.

Gortler: I see.

Price: A <u>much</u> older building, an old wreck of a building with a little wing at its back. It seemed to me that we needed to have a decent lab built as quickly as possible. We needed meanwhile to move into something. Tearing the old building down and building a new one took a little longer than I had anticipated it would take, but it was done. First, we built a new wing that was designed for undergraduate labs, but that was used for research until we could build the new building designed specifically for research. It was exactly what I planned, but doing it took about fifteen years longer than I thought it was going to take.

Gortler: It always does. Let's talk about your presidency and your ACS activities a little bit. That you ended up as president of the ACS demonstrates that you have always been fairly active in ACS affairs.

Price: Yes, I have been reasonably active in ACS affairs

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*See page 20 of this transcript.

and have done quite a few things at ACS. My two main accomplishments as president were the things I mentioned previously. My presidential address interested me very much. Whether it did likewise for anybody else, I don't know. I think that starting the Department of Chemistry and Public Affairs in the ACS office and the Committee on Chemistry and Public Affairs were very significant events. Scientists weren't supposed to become involved in politics. I had been active in politics, however, and therefore felt that my relatively easy election indicated that a lot of chemists thought that maybe it was time to get involved in politics. Everything just came together. The time was right.

Gortler: Did the ACS's political involvement create problems with the IRS? Did its involvement in public affairs mean that the ACS was no longer a tax-exempt organization. Did that have anything to do with its tax problems?

Price: No. I don't think that was a significant part of the problem. I think that the main problem was that _C & E News_ had become an income producing operation.

Gortler: I see.

Price: They sold advertisements and received income. They got into trouble because of that.

Gortler: Oh, I see. It was not as a result of lobbying or political activities.

Price: Not really. In any event, you're allowed to use five percent of your income for these activities. We expended a much smaller fraction.

Gortler: What about the United World Federalists? You've been very active with them.

Price: Well, that's an interesting and amusing little sidelight. I was a totally apolitical animal who hardly even registered to vote, I guess. My political conversion occurred just after the war. I went to Washington in '47 for several meetings about war operations: with the Chemical Corps, with the Quartermaster Corps, and with the Navy. We were just beginning to rearm. I was feeling very blue about the prospect of our country's heading for another World War when I visited my family's farm in Sellersville, Pennsylvania, before going back to Indiana. My brother played me a tape recording of a talk by Congressman Judd from Minnesota. Judd had been at a local Quaker meeting, talking about world government. His talk hit me right between the eyes. My brother then gave me a copy of Emery Reves', _The Anatomy of Peace_.* Reves had reported about the League of Nations for many years as a

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*(New York: Harper and Brothers, 1945).

correspondent, had realized its shortcommings, and had written that the only way to have an effective world organization is to have a world government, rather than a debating society.

I read Reves' book on the way back to Indiana and discovered that his view was that of the United World Federalists. By the time I got home, I was a convinced World Federalist. I found the answer to the question of how to avoid war. Establish a peace system based on law and order. This was the only alternative to the war system.

Gortler: Your brother and someone else in your family, your mother perhaps, were also involved with the United World Federalists?

Price: They were also members, although not as active as I became. I became active immediately. I organized a chapter in South Bend and a state branch in Indiana. I even lobbied in the state legislature.

Gortler: It must have been a little hard to do that out in the conservative Midwest.

Price: Yes. Well, in the beginning there was no opposition, and so being an advocate for a world government wasn't that bad. Eventually, however, opposition to my views swelled. I became a candidate for the U.S. Senate, and the House, in order to become an advocate in our political system for world federalism. These were interesting experiences.

I am still absolutely convinced that there's only one alternative to war and that it is to have a peace system. Furthermore, the only peace system of which I now that history has demonstrated works, is some kind of a system of law and order under government. Certainly the United Nations is not that.

We and the Russians proposed world government in 1961 in a revolutionary document, the "Joint Statement of Agreed Principles for Disarmament Negotiations". The Russians, through Valerian Zorin, and the United States, through John McCloy, and with the approval of John Kennedy proposed it. Endorsed unanimously by the UN General Assembly, it contained two basic principles. The first was to dismantle <u>all</u> national military establishments, <u>all</u>, leaving only enough lightly armed forces for internal policing. The second authorized the United Nations to settle disputes and to assure compliance. We proposed a draft treaty, as did the Russians, to implement the agreed principles. All of this was just totally sabotaged by the military in this country.

Gortler: What an incredible idea.

Price: According to the military propaganda circulating down in Washington, this country never really meant to abide by the "Joint Statement". It was just propaganda.

I talked to Bill Foster, who was the first chairman of the Arms Control and Disarmament Agency and asked him if the United States' position was just propaganda. He said, "I want you to know that John Kennedy and I were absolutely sincere about general and complete disarmament." He continued, "I will add, that up until the time of the Zorin-McCloy agreement, we had no problems with the military. After that, my negotiating problems were not with the Russians; rather, they were with the military."

The military simply sabotaged Foster's position. That was the essence of an article that I just published, "Re-establishing Disarmament on the U.S. Agenda."* The World Affairs Council published it for me and Norman Cousins wrote a lovely editorial about it in the Saturday Review a couple of weeks ago.**

Gortler: If you have an extra copy of that article, I'd like to take it with me. Was the Federation of American Scientists an offshoot of this, or was it something else?

Price: No. It was entirely independent. It grew out of the Federation of Atomic Scientists that started just before the end of World War II. It tried to arouse people to the problems of nuclear weapons. I was the chairman of that group at one time, and somehow I never really clicked with it. Many of its members were in the arms business and were much more interested in what I'd call arms control than in solving the war problem by general and complete disarmament.

One of the points I've tried very vigorously to make in all of the things that I've written, is that arms control is totally different from disarmament. Arms control is part of the war game. You try to get an advantage by limiting arms instead of by building arms. It has some advantages because it's better than not having arms control. It will not, however, get rid of the war system. It's part of it. It has a totally different objective. The general and complete disarmament that Zorin-McCloy envisioned entailed getting rid of our national military establishments and setting up the U.N. as an international instrument of law and order. That's not easy to do, believe me. Yet, it's an awful lot easier than putting the world back together again after a nuclear war.

Gortler: We've been talking about your work with the United World Federalists and the Federation of American Scientists. You haven't explained yet how your involvement with the latter group started. What exactly is the Federation of American Scientists? I've read about that group in your biographical

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*Charles Price, "Reestablishing Disarmament on the American Agenda," Occasional Paper 2 (Philadelphia: Global Interdependence Center, 1979).

**"History Lesson" (May 12, 1979): 12.

sketch in the '57 edition of Current Biography.* I think that you were written about in that volume because you were the spokesman for that group.

Price: I was national chairman that year.

Gortler: Yes, that's right.

Price: Yes, I had been active with that group for a few years before I became chairman. Somehow, we didn't quite see eye to eye on the issues of disarmament versus arms control. They were much more pragmatic about arms control measures and nuclear test bans, which was all fine. I have nothing against them, but their agenda didn't solve the problem.

Gortler: Yes. That's right.

Price: As with my chairmanship of the board of the Council for a Liveable World for three or four years, I had an awful hard time convincing the Federation of American Scientists of the importance of GCD.

Gortler: GCD being?

Price: General Complete Disarmament as envisaged in the principles of Zorin and McCloy. Anyway, arms control has done some useful and important things. CLW leads the fight against the antiballistic missile. My preference, however, is to cure the disease rather than to administe some aspirin tablets.

Gortler: It's a bigger undertaking, but more than worthwhile in the end. What about your current activities? You say that you've retired from active chemical research to take up more important things. Do these mainly have to do with disarmament?

Price: I guess the main thing for which I want to work is to establish a system that will eliminate preparation for nuclear war. I haven't seen anything that indicates anything simpler than having necessary institutions to permit nations to disarm. It's not going to be easy, but I still think it's the only right principle. Other principles are like perpetual motion machines. They operate against the basic correctness of the way the world works. We're not going to eliminate the threat of war by any arms control measure I've seen discussed seriously.

I'm still active in the World Federalist Association and want to continue to be so. I also happen to be chairman of the board of Swarthmore College, which takes some time and

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*"Price, Charles C(oale), Current Biography 1957 (New York: H. W. Wilson, 1958), p. 438-40.

energy. I want to finish the book that I'm writing.* I've written the first draft, and I've been so busy with this crazy law case, I haven't even had a chance to proofread it, let alone think about where I'm going to get it published. I definitely didn't want to get a publisher first, because then I'd be hounded to get the thing done. I wanted to do it in my own time and my own way, and then see who wants to publish it.

Gortler: Yes.

Price: I'm also on the Phi Beta Kappa Science Book Award Committee. I read about twenty-five to thirty boods each year. During the next three months I'll be trying to read this year's crop of books for the Science Book Awards. Three years ago I pushed very vigorously for the book by Gerard O'Neill of Princeton about colonies in space that can be used as manufacturing facilities and that can also radiate solar energy back to earth in order to solve our energy problem.** Oh, there are some very interesting books.

Gortler: Yes. It sounds like it. Well, you're certainly going to be busy. It's hardly a retirement.

Price: I also want to do more sailing and to play more tennis and golf than I have had time to do lately.

Gortler: Well, let's round things up. I have a few odds and ends to resolve. First, however, I'd like to ask the one big question. Looking back over your career, what are the things that have given you the most satisfaction? I'm sure that it's hard to give an answer. You've done so many things.

Price: Well, I've always gotten a big kick out of research, and doing things that were new and different. As you've seen, I'm slightly annoyed and frustrated that others didn't think that my research was pretty original and significant.

I've always enjoyed the political arena, or I wouldn't have spent as much time and energy in organizations like the World Federalists. I have also enjoyed my extracurricular activities. I've always been an ardent sportsman. I don't know whether I get more frustration or enjoyment out of playing golf. I still enjoy tennis and sailing.

I have a very lovely family. It's been a personal satisfaction. My children have been very interesting.

I don't know what great accomplishments I could list.

Gortler: I think that you've pretty much summed up a lot of

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*See page 45 of this transcript.

**Gerard K. O'Neill, The High Frontier, Human Colonies in Space (New York: Morrow, 1977).

what we've talked about. If you'd suddenly thought of something new and different, I might have been shocked.

You've mentioned your wife a couple of times. What kind of role has she played in your career?

Price: She's not at all scientifically oriented or inclined. We've nonetheless always had a very fine relationship. She's always been extremely helpful. She makes it possible for me to devote time and energy to the things I've wanted to do. In that sense, she has been enormously helpful. She's always encouraged me to do what I want to do, even something as crazy as running for political office. She never really thought that I should have done that, but she certainly pitched in and helped. She's also helped me with my writing. She's a good editor and puts a lot of my writing into sharper focus. Additionally, she's been a great hostess. When I was at Notre Dame, a variety of lecturers visited us. Frequently, they stayed with us, rather than at a hotel. On all kinds of occasions, she was very helpful, entertaining all kinds of guests like Japanese people, for example. We've had a lot of contacts with Japan. I spent a semester there and have had about twenty-five or thirty Ph.D.s and post-docs from Japan.

My relationship with my wife has always been very, very pleasant, helpful, and stimulating. I'm sure that because I didn't have any significant marital problems, I was able to devote my time and energy to my jobs.

Gortler: What effect has you career had on your children? Did they become scientists as a result of your work? Did they do other things?

Price: No. I've had a good relationship with all of my children. They were all very ardent sailors and crewed with me. Lots of my racers were big boats, so I needed lots of hands for crew. They were an excellent crew. Having a big sailboat was a great focus for the family. If I ever dared to threaten to sell it, I was just jumped on by all of my kids.

None of the girls chose to become scientists, although my boy could have been an extremely good one. He took advanced chemistry, advanced physics, and advanced math in high school. He was on three or four athletic teams, was the captain of two or three of them, and was elected president of the student council. He did whatever he wanted to do.

He went to Swarthmore and within a few months they turned him off to science. He dropped out twice and barely managed to keep interested enough to finish his bachelor's degree in biology. He's since worked at three marine biology labs: at Woods Hole Oceanographic Institution, at another lab in Maine, and now at Scripps. He's still not interested enough, however, to want to get a graduate degree in marine biology. He's not sure that he wants to continue to work in marine biology. He may or may not. I don't know. He still doesn't know what he wants to do. Once he finds out what he wants to do, however, he will be able to do anything. He has the ability.

Gortler: Yes. It's interesting, how somebody can sometimes get turned off by a bad experience.

Price: Yes. He sure got turned off for one reason or another. I never have really found out what it was. I think that it may have been the way that physics was taught. Anyhow, he just got totally turned off from his studies and the academic venture. He did manage to graduate and earn his bachelor's degree in biology. He spent the next year and a half as a carpenter's apprentice because he didn't seem to have any interest in an academic career.

Gortler: That happens.

Price: I have influenced my middle daughter very significantly, I think. She's not only a national champion sailor, but she's also been in the sailmaking business for several of the major companies. Most recently, she worked for North Sails before leaving them in order to go into business for herself. She's currently North American champion in her class, the 505 class. She's been a Yachtswoman of the Year, at least twice. She won the women's championship and didn't even defend it. She said, "That's too easy. I'd much rather beat the men."

Let me tell you a story about my daughter. Ted Turner won the Yachtsman of the Year award the same year she won the Yachtswoman of the Year award. He made some thoughtless remarks. He's quite an egocentric character, the Mouth of the South.

Gortler: Yes. I know who he is.

Price: Well, he remarked that the only place for a woman on a ship was in the galley. Sally is a women's libber, and that just drive her up the wall. She challenged him to a match race. He accepted and was willing to sail in a 505, her boat. He eventually backed out of the race. I think it would have been very intersting to see that race. I think she might have dusted him off. She's beaten an awful lot of damn good male sailors including the best North American. Although she did not do that well in the world championship--she finished fifth among seventy--she's a very good sailor.

I have a son-in-law who's currently a North American Champion. So we've got two North American Sailing Champions in the family. My child and son-in-law have been more successful ssilors than I've ever been.

Gortler: You've had so many careers that if some of it rubbed off some place, that's really very good. I've pretty much concluded my questioning except for a few odds and ends. Do you have anything else to add to the history of physical organic chemistry? Do you have any comments to make about where you think chemistry might be headed? Has anything else crossed your mind that we haven't talked about?

Price: Well, I guess there's nothing very spectacular to say. We've covered the waterfront pretty well, I think.

Gortler: Well, if anything should come up, we can talk again. I did want to ask you about whom I might interview concerning physical organic chemistry, particularly about those years between '25 and '46 or '50. I've already talked to Hammett, Bartlett, Westheimer, and Tarbell. I'll be talking to Cheves Walling and probably Jack Roberts within a year. Hauser...

Price: Hauser's dead now, isn't he?

Gortler: I don't know. A number of people have said he did have some influence in that period. I have to check into that.

Price: I have a feeling that he may be dead. I think that he was a real pioneer and original character. If he's alive, he might provide you with an interesting interview.

Conant...

Gortler: He died last year.

Price: Oh, he died?

Gortler: I'm very interested in what he did.

Price: Bill Young. Is he still alive?

Gortler: He is. I have not contacted him, but I should.

Price: I think that he would be a good one to interview. He was a real pioneer.

Gortler: Yes.

Price: He got into academic administration, but he certainly had an interesting career and was very influential. He certainly influenced some of my thinking.

Oh, there's another guy for whom I have great admiration, Melvin Calvin. He goes back quite a ways.

Gortler: That's right. I hadn't...

Price: Melvin's a fascinating guy. Pauling and I always talked very nicely. Calvin and I did likewise. We've always been very close, visited each other's homes. I've always felt that we saw things very much eye to eye. We had an entirely different relationship than I had with guys like Winstein and some of the others. I'd even argue with Jack Roberts, even though we were good friends. Calvin and I, however, could talk for hours and hours about chemistry and see things the same way. He went back a long way.

Gortler: Yes. He started working in the late thirties, about the same time you did, I think.

Price: Yes. He knows all there is to know about thermodynamics.

Gortler: Yes. That's true. His orientation wasn't strictly organic chemistry.

Price: Wheland of course would have been an interesting one with whom to talk. He's gone.

Gortler: All right. Fine. That's just about it. Thank you very much. I appreciate it.

Price: Well, it's been fun and interesting.

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