THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

ANDREW STREITWIESER

Transcript of an Interview
Conducted by

Leon Gortler

at

Latimer Hall at the University of California

on

22 January 1981

~~Ed~~ Andrew
Streitweiser, Jr.

JH
3/15/96

ER FOR HISTORY OF CHEMISTRY ORAL HISTORY PROJECT

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Andrew Streitwieser, Jr., interview by Leon Gortler at Latimer Hall at the University of California, 22 January 1981(Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0007).

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

ANDREW STREITWIESER, JR.

1927	Born in Buffalo, New York, 23 June

Education

1948	A.B., chemistry, Columbia University
1950	M.A., chemistry, Columbia University
1952	Ph.D., chemistry, Columbia University

Employment

	Department of Chemistry, University of California, Berkeley
1952-1963	Instructor to associate professor
1963-	Professor
1958-1962	Fellow, Sloan Foundation
1959-1960	National Science Foundation Faculty Fellow
1964-1965	Fellow, Miller Institute
1969	Guggenheim Fellow
1979-1980	Fellow, Miller Institute

Honors

1967	ACS Award in Petroleum Chemistry, American Chemical Society
1976	Senior Scientist Award, Humboldt Foundation
1982	Physical Organic Chemistry Award, American Chemical Society

ABSTRACT: Andrew Streitwieser begins this interview by mentioning his family, early education, and undergraduate days at Columbia University. He then elucidates his graduate education at Columbia, stressing the influence of William Doering upon his work, before describing his research on molecular orbital theory as a fellow at the Massachusetts Institute of Technology. Streitwieser then discusses the emergence of organic chemistry at the University of California, Berkeley, and his own ambitious and productive research program there.

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.

NOTE: The following table correlates the tapes of the Streitwieser interview with the pages of the transcript.

Tape 1, side 1..............................pp. 1-9
side 2..............................pp. 9-18
Tape 2, side 1..............................pp. 18-26
side 2..............................pp. 26-33

TABLE OF CONTENTS

1 Family Background and Early Education

Parents and siblings. Scientific interests in grade school. An early publication on fluorene. Chemical experiments during high school. Organic Specialties.

7 Undergraduate Work at Columbia University before the war.

Coursework in chemistry. Doering, Woodward, and early organic chemistry. The influence of Doering and Weaver. Army service.

13 Education at Columbia University after the war.

Completing undergraduate work. Courses in physical and quantitative chemistry. Beginning graduate work. Friends and colleagues at Doering's lab.

17 William Doering and his Research Lab

The seminars and their importance. Organic chemistry in transition. Research in solvolyses. A lab in Katonah.

22 A Research Fellowship at the Massachusetts Institute of Technology

Working with Jack Roberts. Molecular orbital theory and calculations. Saunders and other colleagues.

26 The Beginnings of Organic Chemistry at the University of California, Berkeley

The dominance of physical chemistry. Jensen, Heathcock, Stewart, Olsen, and the rise of organic chemistry. Don Noyce and Bill Dauben.

29 Research Projects at Berkeley

Reaction mechanisms with stereochemistry. Testing Huckel's MO theory. Deuterium isotopes. The relation of organic and physical chemistry today.

INTERVIEW: Andrew Streitwieser

INTERVIEWED BY: Leon Gortler

PLACE: Latimer Hall at the University of California

DATE: January 22, l98l

Gortler: This is Leon Gortler interviewing Prof. Andrew Streitwieser in his office in Latimer Hall at the University of California on January 22, 1981. I know you were born in Buffalo, New York, on June 23, 1927, but I know very little else about your early life. Could you tell me a little bit about your family and about your schooling?

Streitwieser: My early life was, at best, lower middle class. My parents in the early days were very poor. Both my parents came over from Germany after World War I. They met in Buffalo and married there.

Gortler: Can you give me their names?

Streitwieser: My father's name was Andrew and my mother's was Sophie Morlok. My father was born in Munich, Germany; my mother in a little town called Obertal in the Baden-Wurttemburg area of Germany. My father was barely in his twenties when he immigrated and my mother was nineteen when she came to this country. They met, married, and somewhat more than a year later, I was born. My brother was born in Buffalo three years after that.

My father was a carpenter with the Pierce-Arrow Company, and when that company folded in the early depression, he thought the employment opportunities would be better in New York. So, around 1930 or 193l, we moved to the Bronx, New York. Life in those early depression days was very hard. My father got intermittent work as a carpenter. In the late '30s, our lot improved when he got employment helping to build the New York World's Fair. In 1937, my parents had saved enough to put a one hundred dollar down payment on a house near the World's Fair in Jamaica, Queens, Long Island. I think the total price of that house was two thousand, nine hundred dollars, an incredibly low sum of money nowadays. So we moved.

Life improved considerably after we moved to Queens. We became more solidly middle class. My father changed his business; he became the manager of a local Roto-Rooter agency. In fact, as a teenager and later, I cleaned out many a sewer myself.

I went to public schools in the Bronx and in Jamaica, Queens. I went to Stuyvesant High School in Manhattan, commuting from Jamaica. It took one hour each way and I remember those early mornings walking in Manhattan to high school. That worked out very well, however, because I met some lifelong friends there. Ed Kosower is one fellow whose friendship I've shared since those days. Of course, he's been a very successful chemist in his own right. Ed and I participated in the Westinghouse Science Talent Search the first year that Stuyvesant High School participated. Both of us became "trip winners" to Washington--two of only forty such winners selected nationally. Our high school did very well that year and Ed went on to become the major winner, the top awardee of the year. After high school, I went to Columbia. Because our finances were still very constrained, I stayed at home and was able to save money commuting to college. I did that even though I had worked part-time after school and saved some money for my college expenses.

Gortler: When did you start at Columbia?

Streitwieser: I finished high school in three and a half years by going to summer school. Those were the days of acceleration. I started to take classes at Columbia around March, 1945. I finished two regular semesters and the summer session between them before I was drafted in February, 1946.

I spent a year and a half in the Army, mostly in the U.S. Army Medical Corps in Denver, where I served as a clerk in Fitzsimmons General Hospital. I was discharged in August, 1947, just in time to enroll for the fall semester at Columbia. After two more semesters and a summer session, I graduated. While I was in the Army, I took courses at the extension school of the University of Colorado in Denver and got credit for differential equations and French. With those units and by taking heavy loads at Columbia, I was able to complete college in less than four years. As I said, those were real days of acceleration.

Gortler: Let me go back for just a minute and pick up details. Were there any instructors or books that especially influenced you while you were growing up?

Streitwieser: When I was in grade school, still in the Bronx, I became interested in astronomy in a roundabout way. When I was about nine, I noticed horoscopes in the paper. I became interested in astrology, went to the public library, and took out a book on astronomy. That book taught me the difference between astrology and astronomy. It was a marvelous book. I wish I could remember the title because it was pegged at a level I could follow. It was in the conversational style of an adult talking to children, pointing out the constellations. That was marvelous--to see those constellations, to recognize them, and then to go out and see them myself in the sky. So, very early I became an astronomer. I joined the Junior Astronomy Club at the Hayden Planetarium and, even at that young age, I would go to the club's monthly meetings by using the subways. I doubt that anyone would do that nowadays in New York--but it was a different world then.

When I was in the fifth grade, I was definitely influenced by one particular teacher, a Mrs. Dekker, who taught science in P.S. 85 in the Bronx. She had a very unusual class. She had accumulated lots of various naturalist items in the classroom including a dinosaur bone. She had known Roy Chapman Andrews who had discovered dinosaurs in the American West. I remember her taking me once to a museum, just the two of us, one evening. I remember having a meal at the automat, a totally novel event in my life at that time! She really was an important influence on me. We had a classroom newspaper which I edited during that period. I remember that we also had a little baby alligator in class, a one-foot long alligator who had to be force-fed. And we had a rabbit, and some mice.

Chemistry came later, at the age of eleven, when I was given a Gilbert chemistry set for Christmas. That was followed by a larger chemistry set the following June for my birthday. After that, I was a chemist.

Gortler: Aside from giving you a chemistry set, did your parents have other influences on you?

Streitwieser: Oh, no. They let me take my lead and go without restraint. Their own education was very limited; neither graduated from grammar school. The most negative influence they had on me, however, was in my early youth. They spoke only German at home, and, at just about the time I could have become bilingual, they started to speak only English. I guess that they wanted to be American. It's too bad that they switched, because I would very much appreciate being fluent in German now. However, enough of the early language carried through that my German pronunciation is not quite as bad as that of a typical American.

Gortler: I noticed that you have a number of publications in German. Have these been translated by other people or have you written them?

Streitwieser: They have been translated. My German isn't that good. I've given lectures in Germany in German, but I read them. I wrote the lectures in German, and then had friends improve the German.

Gortler: I see. And your brother? What did he do?

Streitwieser: My brother is not a scientist. He suffered from being the youngest in the family and did not do well in his early schooling. After I went into the Army, however, he was freed from having an older brother around. He blossomed and he eventually went to Columbia College and graduated. He teaches modern history, contemporary events, and economics in a high school on Long Island.

Gortler: What is your brother's name?

Streitwieser: William. He's three years younger than I am.

Gortler: I suppose that as a result of these early influences you decided to go to college rather than to go straight into the work force.

Streitwieser: Well, I remember early in high school doubting that I would go to college. Going to college was a major undertaking then. Of course, it was something I wanted to do but I wasn't at all sure of being able to do so in those days. By the time I got into the higher levels of high school, I knew definitely that I was going to college. By the time I entered the Westinghouse Science Talent Search, I was going to go one way or another.

Gortler: And you were supported primarily through your own labors?

Streitwieser: We had the G.I. Bill. That, of course, was a godsend. I had saved about one thousand dollars on my own, which was a major amount of money in those days. At that time, it cost two hundred dollars a semester to go to Columbia. Those funds helped, and living at home was a major benefit that my parents provided at that time.

Gortler: You had a publication in 1944 while you were still in high school.

Streitwieser: Oh, yes.

Gortler: The chlorination of fluorene.

Streitwieser: Well, that's part of the infamous story of Organic Specialties. It's a marvelous story that starts with Lester Friedman, another high school friend, and with Edward Kosower. We used to get C & E News regularly and an article on chemicals wanted by the Armour Research Foundation appeared weekly. Lester and Ed had noted that many were rather ordinary common organic chemicals that anyone could make. In those days we didn't have Aldrich and the other specialty chemical companies. The only place where one could get chemicals was Kodak and their list was rather limited. So we thought we'd go into the--I should say that Ed and Lester first thought of going into this business. They tried to get me to join them. I was reluctant to do this until they showed me an order in hand from the General Electric Company for one hundred grams of l,3,5,-trichlorobenzene. We knew that this was an easy thing to make. All we had to do was to chlorinate aniline and deaminate. Thus, with this order in hand, I invested my eleven dollars.

Gortler: You were still in high school?

Streitwieser: Yes.

Gortler: But you knew about organic preparations?

Streitwieser: Oh, yes. We were quite active in organic chemistry in those days. We, in effect, taught ourselves, and each other. I remember going through exercises in organic nomenclature. We used to draw complex structures for each other and ask, just as a problem, to name them.

Gortler: What kind of textbooks were you using?

Streitwieser: Just books from the library. I remember at one stage, early in my teens, I must have been about thirteen or fourteen, thinking that I was clearly going to be an inorganic chemist. I remember a quiz program we were listening to at home one day on the radio. One contestant was a chemist from Du Pont who gave his occupation as an organic chemist and I remember remarking to my parents, "I'm going to be an inorganic chemist, not an organic chemist."

At that time I really didn't understand any organic chemistry until I got a book from the library that started with typical inorganic chemistry and merged into organic chemistry so gradually that I could understand it. So for the first time I was able to see what organic chemistry is like and what a fascinating field it is.

Rather early, I became interested in theoretical aspects and when Remick's book on modern electronic theories of organic chemistry came out in 1941, I bought a copy that I still have.* It's a wartime edition with narrow margins and thin paper. I read it and that was my early education in theoretical organic chemistry. I really didn't understand it completely. Remick's resonance structures were a fascination but I really didn't understand what they meant. Nevertheless, it showed the earliest beginnings of where my interests were going. Remick had an influence on the young beyond what he may have planned! At that time I remember taking that book with me to Washington on the Westinghouse trip and remember reading it there--but that gets away from the story of Organic Specialties.

But first, another important book. We each had copies of the Chemical Rubber Company's Handbook of Chemistry and Physics. It was a real bargain, costing something like three dollars at that time. It was very thick even then and to have so much data for three dollars gave us a sense of power.

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*A. E. Remick, Electronic Interpretations of Organic Chemistry (New York: John Wiley & Sons, 1943).

With the G.E. order in hand I invested my eleven dollars and the three of us then took the plunge with Organic Specialties. Some of this money was used to get letterhead stationery that said "Organic Specialties." We used the address of Lester Friedman's parent's lingerie shop on 2nd Avenue and 14th St. Lester had a well equipped laboratory in the basement of this store. We did the first experiment--the chlorination of aniline--at my house because across the street from where I lived was a large empty lot with a couple of old foundations. Since we didn't have hoods, the experiment was carried out in the open air. We bought a lecture bottle of chlorine and a yoke and we learned how to use them. We ran the chlorination of the aniline one afternoon. I took the product and worked it up, isolated the trichloroaniline and then passed it on to one of the others for the deamination.

One of the things we learned is that organic chemistry in the laboratory isn't the same as organic chemistry in books. It takes practice and experience to run reactions and that was a valuable lesson because we ended up with only fifty grams of yellow, but crystalline material. That was the best we could do, but we sent it off and we got part payment anyway. However, that experience soured me on industrial chemistry.

I went back to fundamental research in my own home laboratory in the basement of our house in Jamaica. We used to get free samples of compounds from the Koppers Chemical Company. I was able to get free samples of phenanthrene, anthracene and fluorene. It's amusing that much of my recent research has been with the hydrocarbon fluorene because my earliest work was also with fluorene. Of course, I had no real appreciation for what the novel features of fluorene are--its rather stable anion and the relationship to 4n + 2. Aromatic chemistry was interesting even then because it was relatively simple and one could understand many of the reactions. Aromatic substitution, therefore, was an interest, even then, with polycyclic hydrocarbons. That was an era when polycyclic hydrocarbon chemistry was an especially important part of organic chemistry because it was much simpler in many respects than aliphatic chemistry. I could sense this even then. One of the sample reagents I had was sulfuryl chloride, probably from Hooker. I noticed an article by H. C. Brown on chlorinations with sulfuryl chloride; one of the compounds that he chlorinated was fluorene. But the product was not identified--he simply indicated that the experiment had been done. So, I repeated the experiment in my home laboratory. I isolated a chlorofluorene which I thought was probably 2-chlorofluorene because I knew that the 2-position was the prevalent position of substitution in the fluorene molecule. I made authentic 2-chlorofluorene from the amine. I knew about <u>Organic Syntheses</u>. <u>Organic Syntheses</u> had the nitration and reduction of fluorene and I carried out those reactions according to the <u>Org-Syn</u> specifications--made the amine and ran the Sandmeyer reaction to get the chloride so I could do a mixture melting point determination and showed that I did have the two-isomer. Then I wrote the research up as a little note. While doing the experiment, writing the paper, and getting it accepted for publication, I didn't tell my friends about it. I wanted it to be a surprise. So when it was published and they found out about it, I was a local hero for a little while.

They reciprocated because as part of the Organic Specialties that they continued, they had to make beta-keto-glutaric acid. They worked out a procedure for it, developed that preparation, and submitted it to Organic Syntheses and it was accepted. Whereas I put an acknowledgment to their assistance in my paper, they put an acknowledgment to my assistance in theirs. In truth, I didn't help in that work and they didn't contribute to my paper.

Gortler: I see.

Streitwieser: So Organic Specialties lasted until we finished high school and we all went our separate ways. There is one other story. We needed to have use of a library for original references. The only place where we could really get all of these references was the New York Public Library--the 42nd Street Branch at 5th Avenue.

Gortler: What about the Chemists' Club? Was there one?

Streitwieser: There probably was one but I've never been a member of it. High school students were not permitted in the 42nd Street Library because we would have taken it over. So, on our Organic Specialties letterhead stationery we typed out a letter to the library requesting that the bearer, Andrew Streitwieser, Jr., in the employ of Organic Specialties, be allowed to use the library. I think Lester signed it. I used this letter and got a little card giving me permission to use the library and go into the stacks. I remember having to read some Russian literature at that time and going through it letter by letter.

Gortler: Did you find someone who translated?

Streitwieser: No. I used a Russian dictionary that was there and just went through it letter by letter.

Gortler: I see. Once you started at Columbia, I take it you started taking chemistry courses right away.

Streitwieser: Yes.

Gortler: Who was there? Who was doing the teaching?

Streitwieser: Let's see. When I started at Columbia, I started in midyear and I had kept careful notes of the experiments that I had done at home. I kept cards and a notebook. In fact, I still have some notebooks of the experiments I did in those days. I had occasion to go through them a few months ago and I was pleased and rather surprised at the care and detail of the observations that were made. I had since forgotten all of that. At any rate, I had these records and I went to a chemistry professor to see if I should be exempted from freshman chemistry. He asked me some questions to test the depth of my knowledge. He learned quickly that I really didn't understand pH but he exempted me from the first semester of freshman chemistry and I started right in with the second semester.

Gortler: Who was the professor who tested your knowledge?

Streitwieser: I don't recall who it was.

Gortler: Do you remember who was teaching the course in the second semester?

Streitwieser: It would have been one of the inorganic chemists but I didn't have much experience with him afterwards.

Gortler: It's not terribly important.

Streitwieser: I just don't remember.

Gortler: So you started the second semester.

Streitwieser: I started the second semester and it was work. It was solid chemistry and as I recall I only got a B in the course. I think one of the things that it taught me is that that kind of advancement isn't worthwhile. It doesn't pay. In those days there was a lot of pressure to finish up and accelerate and so on, but in general it's a bad idea. Nevertheless, I got through that. During the summer of 1945, I started organic chemistry, taking only one semester of it. I forget whether I started with the second semester or the first. Nonetheless, I knew the organic chemistry. I had already done enough experimentation that I was sufficiently adept in the laboratory. I easily got an A in that course.

Gortler: Do you remember who was teaching that?

Streitwieser: Yes. The first course that I took--not the organic chemistry but the inorganic chemistry--was taught by an assistant professor named Barnes who didn't make tenure. During my second semester, my first organic chemistry in summer session was taught by J. M. (Pop) Nelson, who retired shortly thereafter.

Gortler: Do you remember what textbooks they used?

Streitwieser: Yes. It was Caldwell's <u>Organic Chemistry</u>.*

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*William T. Caldwell, <u>Organic Chemistry</u> (Boston: Houghton Mifflin, 1943).

Gortler: That's one I haven't run across.

Streitwieser: I don't think it's distinguished. I don't remember anything special about it. It was a classical organic textbook.

Gortler: And then you went back again that fall?

Streitwieser: Yes. I can't remember exactly what I took that fall. Oh! I remember. There was one other thing about that summer. William Von E. Doering gave his advanced course, S-147. It was an advanced organic chemistry course with reaction mechanisms and theory. I asked permission to audit the course. I didn't tell him I was taking sophomore organic simultaneously but he found out independently. Doering allowed me to sit in on the course and take all of the exams. He actually gave me a grade, a B or a B+.

Gortler: Had he just started at Columbia?

Streitwieser: Doering was then relatively new at Columbia. The year before he and Woodward had published their synthesis of quinine. That was one of the first major natural product syntheses.

Gortler: I thought that he was a student of Woodward's or they somehow worked together?

Streitwieser: No. I don't think that he was a student of Woodward's; rather, they were collaborators. Woodward had started at Harvard. Doering was a graduate student who worked first, I believe, with C. R. Noller.* I think he got his master's degree with him. He did his Ph.D. work with R. P. Linstead who shortly thereafter left Harvard and returned to England. His dissertation research was in the stereochemistry of catalytic hydrogenation. At any rate, I think that's where the association began. Doering was never a formal student of Woodward's but the two worked together. The quinine synthesis may very well have been Woodward's idea with Doering's hands. Doering was an excellent experimenter, although of course, he knew what was going on. He is a first rate chemist. He and Woodward were, as far as I knew, lifelong friends and it started at that time. That's probably something you can do, I hope. I don't know if you've interviewed Doering.

Gortler: No. I think I probably should.

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*It may well have been Kohler. Doering's first paper is with C. R. Noller who may be the Noller from Stanford.

Streitwieser: But certainly that is something that you should do because it gets back to the early days of Woodward, to that era that starts modern synthetic organic chemistry. Woodward and Doering started a new era of organic chemistry by applying modern theory and reaction mechanisms to synthesis. That's certainly something I hope you can follow up on. Doering had an important influence on me because I really loved that stuff. I enjoyed his course immensely. It was just what I wanted and it set the direction of my whole subsequent career.

Gortler: Do you remember if he used a textbook or did he lecture from his notes?

Streitwieser: He lectured entirely from his notes. There was no textbook in those days. There was Hammett's book*, but it was substantially different, primarily physical organic chemistry. In addition to Hammett's book, there was Branch and Calvin,** and there was Remick. All of them published around 1941. Doering, however, went entirely by his lecture notes and his explanations. And it was in a more organic chemistry kind of approach and structure approach to theory. Although I never had any personal association with Doering's background, I judged that this approach derived from Paul Bartlett's. It had been my belief even in the late '50s, that Paul Bartlett of Harvard had a really important effect on organic chemistry. You ought to make sure that you interview him.

Gortler: I have.

Streitwieser: Oh, good. Then you can tell me if...well, before you tell me anything, I'll give you what my impression has been just from gathering evidence without ever having talked about this to anyone. Harvard, in those days, was where much of this modern organic chemistry really began. A number of young researchers and students at Harvard during the war years took Bartlett's course in organic reaction mechanisms and physical organic chemistry and then, right after the war, many of these organic chemists took academic positions all over the country and gave variations of Bartlett's course. I suspect that Doering's course had its origins with Bartlett. I think that many of these modern organic chemistry courses had Bartlett's course as their fundamental inspiration. Is that your impression?

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

**Gerald E. Branch & Melvin Calvin, The Theory of Organic Chemistry (Englewood Cliffs, N. J.: Prentice-Hall, 1941).

Gortler: A lot of people went through there. Yes, I think there is a lot of truth in what you say. It's interesting because he and a good many other physical organic specialists who were of an earlier era were influenced by a fellow most people have not heard of, Elmer Kohler. Bartlett's course was patterned after Kohler's course.

Streitwieser: Ah ha.

Gortler: Conant was a student of Kohler's.

Streitwieser: Kohler and Conant, of course. I'm familiar with some of their early important papers.

Gortler: That's right.

Streitwieser: Kohler was one of the first physical organic chemists.

Gortler: Hammett took Kohler's course. You can't believe the number of people who ended up taking that course. So Bartlett's course was just the continuation of a tradition.

Streitwieser: Branch, of course, had his book and he gave a similar course but I don't think he had the same kind of effect that Bartlett had. In those days Berkeley was all physical chemistry. There were few organic students at Berkeley; so no organic chemists spread out from here the way they did from Harvard. I, therefore, by this kind of progression, was influenced by that school of thought and it was an important influence.

I remember that I joined the American Chemical Society about 1945. I started getting the journals and reading them at that time. I don't remember much of that fall, 1945, quarter. I may not even have taken much chemistry that quarter. I know I took physics and math.

The only other important influence--and it was an important one in those days at Columbia--was Columbia's contemporary civilization and humanities courses. I think the concept of those courses is super. They just don't exist in many schools. One sequence was an integrated course in contemporary civilization and the history of western civilization based on original readings. The other, humanities, consisted of two semesters of humanities, liteature and then subsequent semesters on the music and art of western civilization. Those were marvelous influences. In those days I was all chemistry and science, and I needed some humanities leavening. One limitation of that type of course is that so much depends on who is teaching it. If one has a good teacher such a course can be fabulous. I had a mixed experience. Some of my teachers were young, inexperienced, and not very good. But I had one good piece of fortune. During the first semester of humanities I had an excellent teacher whose name may have been Weaver. He died only a few years thereafter. He was a marvelous lecturer, a dynamic speaker, and he loved his subject. I remember sitting in on the first class while he was talking about the personality of Helen of Troy. I remember wondering that if this woman had never existed, if she was merely a mythical creature, then how could one bother about her personality? That's how young and naive I was then. But I learned that semester. Weaver stimulated my interest in the humanities and I've been interested ever since. He was definitely one of my major influences in college. He and Doering probably influenced me the most during my collegiate career and both did so during that first year before I was drafted. As a result, I remember that when I was drafted I went to the induction center and took with me a copy of Dostoyevsky's Brothers Karamazov which was a thick book that lasted me for a long time during my basic training. During my army career I was also able to continue my chemical reading.

An annoying thing happened while I was in the Army. It was that I wanted to keep up my membership in the ACS but I couldn't. The American Chemical Society would not accept my renewal because I wasn't in school at the time; I was in the Army. I've never forgotten this. I hope that policy no longer exists.

Gortler: They certainly weren't very patriotic.

Streitwieser: I wanted to continue getting the journals and to keep reading them, but I couldn't during that period. Nonetheless, I was able to keep up with my studies and when I was discharged, I returned to Columbia and finished my college career.

Gortler: Were you taking chemistry courses in Denver or wherever you were staying?

Streitwieser: No, it wasn't convenient to take chemistry, but I was able to take French and differential equations. The differential equations course was very worthwhile. It was taught by an engineer and was a good, practically oriented course. I did learn something. I also took advanced calculus by correspondence but that didn't work out as well. Some people can learn mathematics by reading books, but I find that I need to have a teacher instructing me. Unfortunately, that was the extent of my advanced calculus. I got credit for the course but even now I'm weak when it comes to multiple integrations. Fortunately, I haven't had to do much of that.

Gortler: You seem to have strengths otherwise in mathematics. We'll get back to that. So then you came back to Columbia. What courses did you take then?

Streitwieser: Let's see. I know that I took Hammett's Physical Chemistry. That was an important course. We used

Eastman and Rollefson which was not a good textbook.* Hammett is not a dynamic speaker.

Gortler: I've heard that before.

Streitwieser: But it was a good course and I learned a lot. That's where I learned physical chemistry. I took a course by George Kimball on aspects of quantum chemistry that was disappointing. The course should have been a good course, but he emphasized some simplified models and calculations he was doing at the time that never led anywhere. Ironically, Kimball usually taught simple MO theory in this course, but not in our year and I didn't learn it then. I'm sure that if he had taught it then, I would have eaten it up. Despite the disappointment of the course, I did learn some aspects of chemical physics. The molecular orbital theory of organic chemistry had to come later.

Gortler: Should we talk about when you started that or do you want to get to that in chronological order?

Streitwieser: I could talk more about it but that actually involves graduate school after my undergraduate years.

Gortler: Well, then let's finish up the undergraduate years first.

Streitwieser: I remember quantitative analysis but I don't remember whether I studied it before or after I returned from the Army. It was a good course in which I learned a great deal, for example, weighings with analytical balances, gravimetric and volumetric analysis, and so on. It gave me good experience. The other important course was Beaver's physical chemistry laboratory. Ben Widom and I worked together in that laboratory. Ben says that I got him through the course. He claims to be a completely theoretical chemist and says that I did the experiments and he wrote them up. I don't remember it that way. He's a professor of chemistry now at Cornell and a member of the National Academy of Sciences. He was one of a number of students at Columbia who have become noted chemists.

Gortler: I'd like some of those names. I've got some of them but I'd like to get your recollections too.

Streitwiesser: Well, Ben Widom was one. The main thing I learned in that course was precision of measurements and analysis of errors. That was important. Alright, I graduated and started Columbia graduate school.

Gortler: Had you thought of going anywhere else?

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*Ermon Eastman and Gerhard Rollefson, Physical Chemistry (New York: McGraw-Hill Book Company, Inc., 1943).

Streitwieser: Yes. I had also applied to Wisconsin. Columbia and Wisconsin were two of the major schools in inorganic chemistry at that time. I was involved with a girl in New York at that time and she was one of the reasons I stayed in New York. The relationship didn't last. Doering, of course, was the other major reason for going to Columbia.

Gortler: Did he make a pitch for you to stay?

Streitwieser: None that I can recall.

Gortler: You had been impressed by that earlier course?

Streitwieser: Yes.

Gortler: Had you done any undergraduate research? Or were things going so fast that there wasn't any time?

Streitwieser: There was no undergraduate research in those days. It was a time of acceleration and filled schedules. I remember one quarter having laboratory four afternoons a week; the fifth afternoon, Wednesday, I would relax by playing bridge at the Student Union Building.

Gortler: OK. So then you entered graduate school.

Streitwieser: At Columbia in those days, you didn't do any research the first year. It consisted rather of courses and a series of qualifying examinations. Once those were passed, you were ready for research. At that time I took Doering's course for credit, Chemistry 147. One semester, Chemistry 148, was taught by David Curtin. He was a significant influence in those days too, being another modern organic chemist. I remember having many discussions with other students concerning which of the two to work with. I had thought at one time of working with Curtin and talked with him about his research problems. I guess though that there never was any question that I would work with Doering.

The one other course that was important to me during my first graduate year was advanced organic laboratory with advanced preparations. I learned a great deal in that course. I did large scale preparations, vacuum distillations, and my first hydrogenation. It was a marvelous course. This advanced organic laboratory course has been dropped by many universities. I think that's a bad idea. These universities have substituted undergraduate research and it doesn't really compensate. I'm glad that we still have such a course at Berkeley. I believe it is one of the reasons that Berkeley undergraduates are so well prepared for starting research when they go to graduate school. My teaching assistant in that course was Al Wolf, Alfred Peter Wolf, who was a labmate in my subsequent research laboratory and who has been a lifelong, warm friend. He went to Brookhaven after graduation and he's been at Brookhaven ever since.

Those were some of the important courses. Of course, some of the important influences at Columbia were the student contacts that I made and the friendships that I developed.

Gortler: Who were some of the people with whom you were friendly?

Streitwieser: William Symes, now at Monsanto, worked with Dawson on poison ivy. We became friends and I still regard him as a close friend. This situation exists with a number of such associates. I regard them as warm friends even though we don't see each other for years. When we do get together, there is a different kind of relationship because it has deep roots. Symes is one such friend. Estelle Kalechstein and Herb Meislich are two others. They married each other.

Gortler: I know them. I meet them on occasion.

Streitwieser: Herb was one of Doering's students. Estelle worked with Curtin. She was an original women's libber even then. We used to have marvelous discussions. Lots of fun. I knew Frank Goodrich at that time, just by name and face. He was a student of Kimball's and we became good friends later because we both came to Berkeley at the same time; he to work at Chevron Research and I, at Berkeley. We both knew the Symes' independently. Frank and Bill Symes had music in common. Frank was an ardent musician and played the guitar. The Symes' sang in the chorus. It was through our mutual friendship with the Symes that the Goodriches and the Streitwiesers were really introduced in 1951 when we both independently came to the west coast. That friendship continued until Frank died in November, 1980, under very tragic circumstances. He was a professor of chemistry at Clarkson College.

There are several other people that I knew, but not very well. Other names that come to mind are Peter Geiduschek, now at U.C.-San Diego in biology and Gert Ehrlich. I forget where he is now, but both were in the same physical chemistry laboratory course so they date back to my undergraduate years. I haven't seen either of them in two decades. I'm trying to think of others. I knew some of the people in Beckmann's group. They were all on that top floor of Chandler Hall. Maybe some of the other names will come to me.

Gortler: Sure.

Streitwieser: Doering's group was the major one. There were two groups actually. One group was in a single, large laboratory in Havemeyer that Doering shared with Hammett. Doering had six desks in it. At the time I was there I overlapped with Al Wolf and Ken Wiberg who were in that same lab. Later Wiberg's desk was taken by Charles DePuy who is now in Colorado. Another person who worked directly across from me and who had a very important influence on that phase of my life was Richard W. Young. After getting his Ph.D., Dick Young joined the Polaroid Corporation where I believe he is now a vice president in charge of research. I haven't seen him in a long time and haven't kept up with him, but he was an important influence.

Gortler: But you did interact?

Streitwieser: Yes, because we were right across from each other and I learned a lot from them. Another labmate was Bill "Ugi" Whelan. I don't know whatever happened to him. He became an industrial chemist and I lost all contact with him. The labs were arranged in three aisles and I shared an aisle with Howard Vaughn. We spent years together. He was a very relaxed fellow--went into industry and, again, I haven't heard from him since.

Milton Farber overlapped my first year; Arthur Hoffmann took his place. Art spent a few years at Shell Development Co. in Emeryville, California, close to Berkeley and we extended our association in California. He then left and joined American Cyanamid Co. in Stamford, Connecticut. Unfortunately, I've seen little of him in the last two decades.

Doug Wiley overlapped my last few months. He went with Doering to Yale and also spent a postdoctoral year at Hickrell. He is now in the central research department of Du Pont, Wilmington. Having heard much of the Hickrell story; he might be a good person to interview or at least have a phone conversation with.

Gortler: I wonder if it could be the guy who shares an office with me...Bob Odum. Was Bob Odum around at that time?

Streitwieser: Bob Odum came later, although we did overlap. I don't even remember if Bob Odum was in that laboratory in Havemeyer Hall. We called it Siberia because it was isolated from the others.

Gortler: I see.

Streitwieser: Other people, however, who were in the Hammett side of the laboratory were post-doctorates. Bob Taft was among them so I knew him at that early stage. Another was Warren Watanabe.

Gortler: I know of him.

Streitwieser: He worked on kinetics of ester hydrolysis.

Gortler: He's at Rohm and Haas, I believe.

Streitwieser: Yes. I remember a lot of interaction with Warren.

Gortler: Let's see. I don't remember if he had already been with Roberts before that or...

Streitwieser: Probably before, because he was a post-doctorate.

Gortler: He was with Westheimer and then he was with Roberts for a while and then he may have gone with Hammett after that. Anyway, who were the others?

Streitwieser: Let's see. Another was Joe Levy who was more of a biochemist, or a biophysical chemist. He was a disaster in the laboratory; his experimental technique was a source of frequent discussions. Sidney Bernstein became a well-known chemist, but I haven't seen him in decades.

Gortler: His major influence has been where? The name doesn't mean anything to me.

Streitweiser: That's because he is in biophysical chemistry.

Gortler: Oh, OK.

Streitwieser: I shouldn't forget Ernest Grunwald who worked with Hammett in 1949. I drove with him to the organic symposium in Madison in June, 1949. Aside from knowing the names I don't remember anything special that went on at that time. All of these people did kinetic studies. Sid Bernstein, I believe, was a graduate student with Hammett at that time. The other people were post-docs. Other names in the Doering group--Jerry Berson was part of the Chandler laboratories. Jerry was a very good influence. He was a serious-minded but fair guy, and he had an impressive personality even then.

Gortler: Tell me more about Doering. Was he running this lab out in the country at that time too?

Streitwieser: That started during that period?

Gortler: After this first year did you go to Doering and say, "I'd like to work with you."

Streitwieser: Well, after we were admitted to research all of us would talk with the organic professors about research problems. Doering never really gave me a specific problem; he just talked about some interesting aspects of chemistry. I joined his group because it was Doering's.

Gortler: Who were the other organic chemists who were still there? Curtin was still there.

Streitwieser: Curtin was still there.

Gortler: And Dawson and Doering. Did you run across Cope in that period?

Streitwieser: No. I never saw Cope there. I met Cope later at MIT.

Gortler: His name appears in one Columbia catalogue sometime during the war years.

Streitwieser: Never saw him. I don't think he ever showed up.

Gortler: Who were the others?

Streitwieser: Elderfield. I took a course with Elderfield and I knew him quite well. Elderfield's research was in the synthesis of heterocyclic compounds. I never considered working with him. I had a thermodynamics course with Beckman. I took another course with Ralph Halford. Halford was very low key, and was not a very good teacher. He became dean of the graduate school and eventually part of Columbia's administration. (I think I remember reading that he died recently.) Those are the important teachers I had at that time; Columbia wasn't very large.

I remember an amusing incident quite vividly during my very first semester as a graduate student when I was a teaching assistant in freshman chemistry in a laboratory run by Julian Miller who was still finishing his Ph.D. He completed the final defense of his dissertation and he gave a party to celebrate. It was customary in those days to have a party when you finished your orals. His party was quite a party. Liquor just flowed and everyone got drunk. It was quite a revelation to me as a new, young graduate student to see all of these hallowed professors. I won't mention any names, but I remember one of them sitting against the wall by the door stroking women's legs as they walked through. Well, I can mention Hammett who talked and told a lot of stories about his boyhood days. Julian Miller went on to work at Brookhaven where he developed a respectable reputation. My association with him was primarily during that first semester at Columbia.

Gortler: So then you proceeded to do research?

Streitwieser: Yes. One of the important influences in Doering's group was the group meetings--the seminars. They were a wonderful experience. They were held in the evening and a group of us would generally go out to dinner beforehand to a local Chinese restaurant. Doering often went with us. Those were marvelous occasions. Then we'd have our group meetings, sometimes talking about research and sometimes talking about chemical problems that came up. It was quite common to present some interesting problem in organic chemistry. I remember what a whiz Doering was. He seemed to know all of the answers, all of the people involved, and all of the literature.

Working out those problems certainly had an important influence upon the development of organic chemistry. That era of organic chemistry marked a transition from classical to modern. The transition worked by applying modern reaction mechanisms to the solution of novel rearrangements and unusual reaction products, and then rationalizing results in a way that wouldn't have been possible several decades earlier. I didn't realize at the time that we were going through a period of transition in organic chemistry.

The Doering seminars were not unique. Similar discussions also went on in Woodward's group and I'm sure, although I didn't have any direct contact with it, that the same thing went on in Winstein's group.

Gortler: Yes, it did.

Streitwieser: But it marked a kind of organic chemistry which was new and novel and perhaps unique to those places in the United States. I certainly didn't see it happen in Germany then and probably not in England.

Gortler: There is some question in my mind as to why no school of physical organic chemistry was built in England. That is, there weren't these offshoots the way there were here.

Streitwieser: Yes, Ingold didn't have that same effect.

Gortler: I can't even think of an Ingold student who went on to become particularly influential. I don't know many of his students, but I can't think of one that became "famous."

Streitwieser: De la Mare was one. There were lots of "little" names but no one who has been associated with any marked change in organic chemistry. Just more of the same. What would you like to do at this point?

Gortler: What kind of research did you do with Doering?

Streitwieser: I followed up the problem that was started by Harold Zeiss. He was a graduate student with Doering who finished just before I started research. Zeiss' problem involved the first optical resolution of a tertiary alcohol. He resolved methylethylisobutylcarbinol and ran one solvolysis of an ester.

My problem was to start with Zeiss' results and to look at the stereochemistry of solvolyses involving tertiary systems using this particular tertiary system. So in my research I had to repeat Zeiss' resolutions and I did that. It was a laborious resolution requiring an elaborate fractional crystallization. That's when I learned fractional crystallization. It involves a pyramid that Zeiss, I believe, took down to something like the twenty-second level to get optically pure material. I repeated this resolution with a large quantity of the brucine salt of the hydrogen phthalate ester. I remember first having to go through the synthesis of the alcohol with a Grignard reaction on a large scale. I dropped the freshly distilled previous material and the bottle broke on the floor. What a crushing event that was. I soaked up as much as I could with paper toweling to recover the material and as soon as I finished with that, took off and spent the rest of the afternoon in a rowboat on a lake in Central Park to recuperate. I survived that, however, and went ahead with the resolution.

I then got lucky. Midway in the course of the normal resolution, when I got the tight little hemispherical igloos that Zeiss reported, all of a sudden I got a new crystalline form in some of the fractions. These were transparent prisms instead, and when I checked, I found that they represented the other diastereomer. In fact, when I used the new crystals as seeds, the resolution, which is normally a very long and laborious one, became a much quicker one. I obtained optically pure material with just a few crystallizations. The different course of crystallizaton may have been stimulated by different dust in different buildings. Zeiss worked in Chandler Hall which is an old building, whereas I worked in Havemeyer which is a different building. One hears numerous stories of unusual cases of initiation of crystallization, such as carrying a syrup from one building to another and then all of a sudden it crystallizes.

I took my newly resolved material, and studied the stereochemistry of solvolysis of the hydrogen phthalate ester under a number of different conditions. We learned primarily that these tertiary systems do not go with complete racemization; rather, they react with substantial amounts of inversion of configuration.

These results supported Doering's view of the solvolysis reaction as involving a modification of the S 2 displacement reaction, in which an intermediate is involved with a hierarchy of gradations from the S 2 transition state to stable solvolysis intermediates. That view, that assigned specific structural roles to a solvent at the rear of the substrate, never really took hold. Doering's mechanism occurred at the same time as Winstein's formulation of ion-pairs in solvolysis and it has been the ion-pair aspect that has taken hold. There is little doubt at this time that in many of these solvolytic reactions, some covalency is involved with specific solvent molecules at the rear of the reactant; but the Doering picture has never really caught on. In that era of organic chemistry during the late '40s and early '50s--in fact during the whole period of the '50s--solvolysis reactions received a great deal of experimental study.

Gortler: And that was your first contact with them. Later on they became very important for you.

Streitwieser: Yes.

Gortler: That work was never published?

Streitwieser: No. It was only published in my review.* In effect, I published it myself but the experimental details were never published.

Gortler: How about the molecular orbital (MO) theory?

Streitwieser: That started when Doering gave a seminar at one of our group meetings. He talked about Huckel and the 4n+2 rule that he in turn had gotten from Kimball. That's where I first encountered the bond integral (beta). But I really didn't understand it. I heard Doering's presentation and learned of the significance of 4n+2 and some of the consequences, but without knowing the background. Doering, of course, went on to do pioneering experimental studies of the tropylium cation and related systems; hence, the 4n+2 rule had an important influence on his work, especially at Katonah. That's another story that we can develop. I really first started to learn MO theory when I did postdoctoral work with Roberts at MIT. Do you want to follow that story?

Gortler: Perhaps you should first tell me a little bit about Katonah.

Streitwieser: I understand that Doering had a graduate student, Alice Wilds, who was a wealthy woman. She wanted a Ph.D. and got it with Doering before my time. Then she set him up in this laboratory at Katonah where he had postdoctoral students; that was where the tropylium cation work went on. Doering had a black student working with him.

Gortler: Larry Knox.

Streitwieser: Larry Knox, of course. I didn't know Larry very well because he was up in Katonah. He came into the city rarely. Larry was an able chemist at a time when there were few black Ph.D.s in chemistry. I remember visiting Katonah once or twice. They had a disastrous fire. The tropylium system was made at that time by photolysis of diazomethane in benzene. That was done on a large scale with a benzene solution of diazomethane circulating past a photolysis lamp. Somehow the benzene caught fire and involved a substantial amount of burning. I heard recently that the fire was serious but not disastrous. It destroyed part of the laboratory.

Gortler: And that was the end of it?

Streitwieser: Well, at the least it destroyed part of it. That wasn't the end at the time. It still went on when I left Columbia. I believe it went on until Alice died. the family broke it up as soon as they got hold of the estate.

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*Andrew Streitwieser, "Solvolytic Displacement Reactions at Saturated Carbon Atoms," <u>Chemical Reviews</u>, 56 (1956): 571-752.

Gortler: Do you remember who was chairman of the Columbia chemistry department at that time?

Streitwieser: I believe it was Larkin H. Fahrenholt who taught freshman chemistry. He was always pleasant and, in general, a nice guy. He was a lightweight chemist who did no research. I don't know what kind of chairman he was.

Gortler: Aside from the Doering seminars, were there other seminars that you remember or other kinds of communication that went on between students? I assume you were talking to all the people that you mentioned before.

Streitwieser: Oh, yes. There were frequent bull sessions and laboratory discussions about laboratory problems. It was at those times that Al Wolf, Dick Young, and Art Hoffmann had their major effect on me because we discussed chemical problems at the blackboard and worked out ideas.

Gortler: After you finished your Ph.D., you decided to go to MIT.

Streitwieser: Yes.

Gortler: Did you get the fellowship on your own or did you apply through Jack Roberts.

Streitwieser: I got it on my own. I wrote a research proposal for an AEC fellowship based on deuterium exchange of esters with base and it was a perfectly good proposal. I was going to look at the effect of ring size in cycloalkyl esters and it would be a reasonable proposal even today. I remember that at the time I had gotten the award I was going up to MIT to look for a place to live. While doing so, I thought of hydrogen-deuterium asymmetry as a way of studying the stereochemistry of primary systems. Dick Young's research problem with Doering on asymmetric induction in Meerwein-Pondorff reductions of ketones influenced me. Young used an optically active secondary alcohol to transfer a hydride to a ketone in order to see how much optical activity was induced in the product secondary alcohol. He found that indeed there is asymmetric induction in such reductions that can be explained quite well in terms of cyclic transition states. I conceived of the idea of making optically active primary alcohols by applying that method to aldehydes with the transferral of deuterium. I suggested this to Roberts and he let me go ahead with it.

Gortler: You were going to work in Roberts' lab.

Streitwieser: Yes. He let me work on my own problem, which is rather rare. I did and that worked out successfully.

Gortler: Key insight, then.

Streitwieser: Bill Saunders was an important influence there. William H. Saunders, now at Rochester, was a post-doc with Roberts. He stayed with Roberts for a year after I left and then went on to Rochester.

Gortler: How did you decide to go up to MIT?

Streitwieser: Well, I discussed with Doering who I ought to work with. There were a couple of possibilities. Winstein and Roberts were two of the important ones and I don't know why I didn't go to Winstein. Working with him would have been a perfectly reasonable alternative. I don't recall what went into that decision. It may have been a reluctance to go all the way to the west coast. I was married then. In fact, that may have been a significant consideration. I got married in 1950 after I got a fellowship while in my third year of graduate school. With the support that I received from my fellowship and the salary that Mary Ann received from her job, we were able to get married. My wife's mother lived in New Jersey and I think that our reluctance to leave Mary Ann's family and go all the way to the west coast was probably an important aspect.

Gortler: The first couple of years in graduate school you supported yourself through a T.A.

Streitwieser: And the G.I. Bill.

Gortler: Oh. That still supported you in graduate school, as well?

Streitwieser: Yes. After all, I got something like two years of support from the G.I. Bill. That was a marvelous thing for me. I wish we had something similar now--something as good as a fellowship or a scholarship. Certainly, my career would have been much different without it. I remember that in graduate school at that time we paid for everything. The kind of modern era where graduate students don't pay for a thing didn't exist then. Each of us had his or her own account with the storeroom; we were even charged for ice, six cents a bag. I had an account that added up to over one thousand dollars worth of equipment that I had checked out. Doering supplied us with solvents and chemicals but we had to check out all of the equipment ourselves.

Gortler: And it was returnable?

Streitwieser: It was returnable but some of the items were consumable.

Gortler: I see.

Streitwieser: Glass blowing, for instance. I had to pay for all the glass blowing that was done for me. As a result, that equipment was mine and I brought it with me to MIT and then to Berkeley.

Gortler: I see.

Streitwieser: I used that equipment for many years.

Gortler: How did the MO calculations start?

Streitwieser: That started at MIT. Roberts had learned the MO calculations from Bill McMillan at UCLA. In turn, Roberts gave a series of seminars--sort of a mini-class--to several of his students of whom I was one. I quickly did a lot of calculations the hard way with an electric calculator. I solved all of the determinants by hand, getting the roots and the energies.
I became very adept at this and those calculations became a collaborative research study with Roberts. We published several papers together on those calculations.

Gortler: To hear him tell it, he wasn't very confident about his ability to use these things. I guess he learned it through your work more than anything else.

Streitwieser: Well, I remember that he imparted what he knew, and my knowledge, of course, wasn't that strong at the time, so we learned a lot by doing the actual calculations which were straightforward. The theoretical basis or really solid understanding of the theory didn't come until years later for me. Again, this involved learning by doing and seeing other relationships.

Gortler: That must have been very time-consuming. I know that you did well over twenty structures in one paper.

Streitwieser: It was very time-consuming. My independent paper on the ionization of triarylmethyl halides resulted from my doing this sort of thing. Furthermore, this work is a good example of the sort of influence we were talking about. Roberts used to attend Bartlett's group seminars and Saunders and I went with him regularly to those meetings. One of these seminars was given by Norman Lichtin who was, I think, a graduate student of Bartlett's at that time.

Gortler: Yes.

Streitwieser: Lichtin talked about the ionization equilibria of these triarylmethyl halides in liquid sulfur dioxides--equilibria between these chlorides and the corresponding carbonium ions. I thought that this provided a good opportunity for getting a quantitative correlation with MO theory. So I did those calculations, and that became one of my first independent papers. That was published after I got my offer from Berkeley. That worked out reasonably well and that work is still one of the first quantitative correlations of experimental chemistry with MO calculations.

Gortler: Besides Saunders and Roberts with whom else were you interacting at MIT?

Streitwieser: Let's see. Who was at MIT? Howard Steinberg, Crum's first graduate student and a post-doc with Cope, influenced me greatly during my first months at MIT. We related on a professional and social basis until he and his wife and child returned to California in January, 1952. He now works for U. S. Borax. I also used to go to the Woodward seminars in the evening and met several of the people there. I knew Mike Rosenblum from those days but I think that our acquaintance may actually have gone back to earlier school days as well. I'm not sure if we went to the same high school together. If we did, he was a year behind me. Also, Jerry Meinwald, who is now at Cornell, was there in those days and we also had an earlier association that I think goes back to high school.

Gortler: That's interesting.

Streitwieser: There were several other people whose names aren't firm.

Gortler: I'm just interested in the ones to whom you talked about chemistry at the time.

Streitwieser: Well, Woodward's seminars, of course, were an important influence. Again, they were an extension of solving problems by modern mechanisms. One of the things that impressed me then was how chemists of natural products memorized long lists of tables of UV and infra-red frequencies for carbonyl groups. That was one of the major tools for the analysis of carbonyl groups in those days...whether it was an ester or a cyclic ketone, etc. I never could do that. I just wasn't a chemist of natural products. But I was impressed that this was the sort of thing such chemists did and that, of course, was a major change in natural products organic chemistry. This was the first important aspect of the introduction of modern machines. When I was a graduate student, even taking infra-red spectra was a rare thing. Columbia had one of the first double-beam instruments, designed and built in Halford's group. As a result, I have some spectra in my dissertation taken on this instrument. Harvard got one of the first commercial instruments and it was much used in Woodward's group. Very rapidly, of course, such commercial spectrometers changed organic chemistry.

I don't know if there is much more to say about MO at this point. Howard Simmons was a senior at that time in my MIT laboratory. He continued his graduate work at MIT and did his graduate research with Roberts. He is now head of central research at Du Pont.

Gortler: You didn't run into Ed Kosower at that point?

Streitwieser: No. We saw each other occasionally in New York. He was an undergraduate at MIT. He then did graduate work at UCLA with Winstein.

Gortler: But he must have been at MIT after the war because he did the first part of the benzene problem with Roberts.

Streitwieser: Ed did his undergraduate studies at MIT starting in '45. Ed is a year and a half younger than I am. He had a blood problem in those days, so he didn't have to worry about the Army. He didn't accelerate and had a more normal career.

Gortler: How did you hear of the job at Berkeley?

Streitwieser: Well, I was interested in academic work and the year I was at MIT was a bad year for academic jobs. About the only position I had heard of was one at Duquesne. I heard a rumor that there was a position at California. The next thing I knew, I got a letter in January offering it to me.

Gortler: And you didn't even apply.

Streitwieser: No. Berkeley had contacted the major universities in the usual way; Columbia and Hammett had recommended me and apparently so did Roberts at MIT. The offer was quite a welcome and unexpected surprise. Naturally I accepted right away and it was delightful then to have the last six months in Boston with a position assured. I could relax. That's why in my first independent paper at MIT on these MO correlations, I included a footnote giving Berkeley as my future address.

I understand that some of the physical chemists at Berkeley wondered, "Who is this guy? Had anyone even heard of him?" They hadn't because I had been hired as an organic chemist.

Gortler: What was going on at Berkeley at that time?

Streitwieser: Well, Calvin was here in full swing in the old radiation laboratory doing the C-14 work. At that point he had just begun using the radioautography approach of looking at the paper chromatograms of the C-14 labelled compounds. Jerry Branch was still alive then. He died early in my career at Berkeley.

Gortler: Do you remember anything about Branch? His name keeps coming up.

Streitwieser: I didn't know him very well. He was already close to retirement and was pretty inactive at that stage. Branch was a physical chemist who was active in the area of physical organic chemistry. I think his principal contribution was his book with Calvin.*

Gortler: Calvin was the younger one.** Was he a student of Branch's or...

Streitwieser: No. He was Lewis's student. Berkeley did not play a great role in organic chemistry.

Gortler: Well, that's become very apparent. Presumably, it was because the physical chemists were just so strong here.

Streitwieser: Yes. They dominated.

Gortler: The same thing seems to be true at Caltech even though Lucas was there. He was sort of alone.

Streitwieser: Lucas never got a Ph.D.

Gortler: We speculate that he had worked with Stieglitz for the short time that he was at Chicago.

Streitwieser: I understand that after the war, Latimer made the deliberate decision to build organic chemistry (as well as chemical engineering) at Berkeley. He did so by bringing in Jim Cason from Vanderbilt and Bill Dauben from Harvard. Bill started as an instructor or assistant professor.

Gortler: He must have come in 1946 or so or about the time that Roberts went to MIT.

Streitwieser: '46 or '47.

Gortler: Roberts went to MIT. I know that Roberts wanted the job here very badly. I was trying to figure out who came in his stead.

Streitwieser: Well, it would have been Cason. Dauben was the young guy and Henry Rapaport came around '49. Maybe it was even '48 because Don Noyce came in '48, four years before me.

Gortler: So there were at least these four organic chemists here before you came.

Streitwieser: That was the start of organic chemistry at Berkeley. They had already gotten things started when I arrived in '52, so Fritz Jensen and I were really the next wave. Jensen came in '53, and then the next tenured man didn't come until Clayton Heathcock, about '64.

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*See page 10 of this transcript.

**See Arthur Norberg's six part interview with Melvin Calvin, 1973-1978. (Bancroft Library, University of California, 1984).

Gortler: So that was a period of a little over ten years.

Streitwieser: Organic chemistry at Berkeley started to have an impact during the '50s and not really before then.

Gortler: The new wave was well under way by that time.

Streitwieser: Consider T. Dale Stewart. He was a physical organic chemist who worked in kinetics and reaction mechanisms but never really did a great deal probably because he never had many students. He worked in mechanisms of protonation of isobutylene and mechanisms of acid-catalyzed dimerization of isobutylene. He retired in the 1950s.

Gortler: He was still here when you arrived.

Streitwieser: Yes. Now the one other person who did have an impact on organic chemistry was A.R. Olson. He did work in displacement reactions in the '40s and he was one of the people involved in showing that every displacement is accompanied by an inversion of configuration. The important experiments were those of Hughes and Juliusburger in England and those of Olson's.* Olson was a physical chemist, however. He taught the quantitative analysis course here and had a textbook on the subject, but at least some of his research was involved in this kind of work--reaction mechanisms, by kinetics and stereochemistry.

Gortler: So these were the people with whom you were interacting. With whom did you talk chemistry when you got here?

Streitwieser: Mostly Don Noyce. He had started a research group. I began my research group in 1953 with one student. It slowly grew after that, so that Don's group and mine held joint meetings. Although my principal interaction was with Don, Bill Dauben also had an important influence. Bill suggested that I give a seminar course on advanced reaction mechanisms. I did. I talked about molecular orbital approaches in organic chemistry. Bill also suggested that I write a review on solvolytic reactions. That came at just the right time. A year earlier and I wouldn't have known enough; I wouldn't have been prepared to do such a long review. A year later and my research group would be too large and I wouldn't have the time.

Gortler: It must have consumed an enormous amount of time.

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*One early paper on this subject is A.R. Olson, "The Mechanism of Substitution Reactions," Journal of Chemical Physics, 1, (1933): 418-23.

Streitwieser: It did. It was very time-consuming because it took a lot of work in the library. I made quite extensive use of the large amount of literature work I had done preparing my dissertation; but even so it took a lot of work after that.

Gortler: When you arrived what kind of problems did you think were the important ones to work on at that time?

Streitwieser: Well, there were two. One was the continuing study of reaction mechanisms. The use of stereochemistry as a tool in reaction mechanisms had been very popular. It was used in the Hughes and Ingold group and in Doering's group. Doering made extensive use of stereochemistry in reaction mechanisms as did Winstein. The stereochemical approach was in vogue at the time. Since I had developed the new tool of the asymmetric primary carbon, I was able to explore the stereochemistry of various reactions at primary centers as a major part of the research of my first decade.

The first graduate student who filed a dissertation under me, Bill Schaeffer, helped me a great deal. Bill had a master's degree from the University of Redlands on the peninsula. He went to work at Union Oil in Brea but after only a year or two he saw the handwriting on the wall. He thought that as a non-Ph.D. his progress would be limited. So he came back to take a Ph.D. He worked with me, despite his being a year older that I. His choice of a research problem was a considered decision because it involved a new kind of chemistry for him and one that he wanted to learn. He also knew that with the problem I presented he could get results rapidly and therefore get a Ph.D. quickly. He did. He made a large batch of optically active primary butyl alcohol with deuterium stereochemistry. He then used it in a number of reactions. He ended up with seven papers. Our collaboration benefitted me as well as him because he was very good in the lab.

Gortler: Yes.

Streitwieser: He went back to Union and about a year later he was promoted to group leader. He has done very well since then.

Gortler: So it worked out very well.

Streitwieser: Yes. His taking a leave of absence apparently benefitted him.

Gortler: I just remembered that your first paper--the one from MIT--on the optically active alcohol followed the Westheimer paper. They were back to back. Did you know about his work at the time?

Streitwieser: Yes. I had submitted a communication of these results. The communication was turned down because although this was interesting chemistry, optically active deuterium compounds had been made before. I was advised to write a full paper. In the interim, I became aware of the Westheimer work and Frank proposed that we publish together.

Gortler: I guess that was a generous gesture on his part.

Streitwieser: I don't know which referee turned down my communication.

Gortler: Maybe Westheimer knew...

Streitwieser: The other part of the beginning of my research was in MO theory. Up to that point I had done a fair amount of work on molecular orbital theory and its development and application to organic chemistry, but I had done little quantitative correlation. Dewar had started his work in the late '40s and early '50s. Coulson and Longuet-Higgins had done a lot of work in the late '40s. The application of simple MO theory to organic chemistry forms a part of your history because the MO theory was developed virtually simultaneously with resonance or valence bond theory.

Both theories occur in Erich Huckel's first paper on the subject. His method 1 and method 2 correspond to molecular orbital theory and the valence bond method that was done independently by Pauling. This part of chemical history involves the personality and limitations of Erich Huckel. He was not a salesman. He couldn't give an effective seminar. Moreover, as a physicist doing chemistry, neither the physicists nor the chemists paid any attention to him. So he played a minor role in the development of theoretical organic chemistry in the 1930s--not at all like Pauling. Huckel realized the fundamental importance of the 4n+2 rule. He wrote a paper in the middle thirties that shows his understanding of the acidity of cyclopentadiene. He recognized the relationship of the 6-pi electron ring, of cyclopentadienyl anion and its relationship to benzene. Nothing was done, however, with his work. The only one of his countrymen who understood its significance was his brother, Walter. He gave quite a bit of attention to this approach in his books. It had no influence, however, in Germany.

It wasn't until the British took simple MO theory over in the '40s that the theory became recognized. This was the work primarily of Coulson and Longuet-Higgins right in the middle of the 1940s and then especially of Dewar shortly thereafter. Dewar had these famous papers in 1952--a series in JACS--on perturbation approximations with the simple Huckel theory. One of my innovations when I first started at Berkeley was to test experimentally the Huckel MO theory by trying to do correlations with actual reactions--quantitative reactivities of compounds to which the MO theory could be applied, namely aromatic compounds. Dewar had the same idea independently and at the same time. We both worked on this and I expect for that period we were the only ones doing that kind of work. He was in a more advanced position than I was;

for one thing he understood the basis of MO theory much better than I did, although he was self-taught. He had to teach this to himself in the '40s.

Gortler: In a sense you were almost self-taught, if all your background came from Roberts.

Streitwieser: That's true. The interesting thing is that even though Berkeley had all these physical chemists I couldn't learn much from them. It was very difficult for me to talk to them and understand them. So I was self taught. When I wrote my book,* the section on applying the simple theory to spectroscopy, was almost entirely self-taught. I had to learn how to use symmetry, etc.

Gortler: Was there anyone here who was interacting with you at that time?

Streitwieser: I was interacting with a number of people but not at a teacher-student level. I could ask people questions and there were some areas where they were very helpful but there were others where they weren't.

My use of hydrogen-deuterium asymmetry in stereochemistry represents one aspect of deuterium isotope effects. Primary isotope effects (involved in the breaking of bonds) constituted a known and important tool in the study of reaction mechanisms but secondary deuterium isotope effects (isotope effects not directly involved in bond breaking) were much rarer. My first paper on this subject in 1957 was therefore a significant contribution. My thinking about secondary deuterium isotope effects derived from my postdoctoral work. I kept a notebook of research ideas. One of these ideas was the use of deuterium isotope effects as a test of hyperconjugation in carbonium ions. It turned out that Jack Shiner, a post-doctorate working with Bartlett, conceived of this same idea independently, and actually started working on it earlier. He beat me on that with an excellent and pioneering study.

I continued on another approach. I looked not only at the stereochemistry of the effect, but then also at the alpha-deuterium isotope effect. We did the first study of that and got a result opposite to what I had expected. I knew that in going from a tetrahedral to a trigonal carbon, the bond to an attached hydrogen shortens and its stretching frequency goes up. I therefore expected to see the first inverse isotope effect. Instead, we observed a normal secondary isotope effect. The rate was down by ten percent. That came as a complete surprise. I had therefore to make a more detailed analysis. That's when I came up with the idea that it was the

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*A. Streitwieser, Molecular Orbital Theory for Organic Chemists (New York: John Wiley & Sons, 1961).

out-of-plane bending that was primarily responsible. I discussed this idea with Pitzer and he agreed with me. I gave a seminar on the subject before physical chemists and they were convinced. So that work was published as a communication and was probably one of my most important contributions of that early period. It was the first discovery of a tool that's been used quite a bit in organic chemistry, and now also in biochemistry.

Gortler: Let me ask you one more question. We talked a little bit about your perceptions of the transition from classical organic chemistry to a more modern organic chemistry to what we have today. We went through this stage of physical organic chemistry which may not really be a separate entity anymore. Any organic chemist today has really been well trained in what is considered classical physical organic chemistry.

Streitwieser: One could characterize the change by saying that organic chemistry of a European type was practiced during the first half of the century and that a transition occurred in the '40s and '50s towards an American approach and now more recently to a more international one. Now, although the American contribution is still large, I perceive that it's diminishing. One can see a growth of a modern organic chemistry particularly on the Continent and in Japan. In part, the diminution of the American role has resulted because of changes in the level of funding. Modern organic chemistry has much more of a structural synthesis orientation that involves stereochemistry, reaction mechanisms, and specialized methods. The growth of physical organic chemistry and the following of reactions by modern instruments have been key characteristics of this orientation. Organic chemistry today is so different from what it was in either of the earlier periods. Physical organic chemistry clearly does not play the same role now that it did in the '50s.

Gortler: Do you think there is something we can call physical organic chemistry today? Do you still consider yourself a physical organic chemist?

Streitwieser: In a sense I suppose I do, although I'm more of a generalist. After all, I do organo-metallic chemistry and lots of synthesis. I still do lots of theory and I still do some classical physical organic chemistry--my carbon acidity work, for example. So, I'd have to call myself a chemist.

Gortler: Yes.

Streitwieser: Really, labels aren't as useful.

Gortler: Maybe I've come full circle. I started all of this with Hammett by asking what kind of chemist did you think you are and he said "I considered myself a chemist." He wrote a text in analytical and organic and so now we're back around again and people are more generally oriented.

Streitwieser: Yes. Even synthetic chemists have to know a lot of reaction mechanisms but they don't need to know a lot of theory. They still have to have a good memory. They have to memorize lots of conditions.

Gortler: I think that completes this part of the interview. Thank you very much for your help.

INDEX

Aldrich Chemical Co., Inc. 4
American Chemical Society (ACS) 11, 12
American Cyanamid Co. 16
Andrews, Roy C. 3
Armour Research Foundation (Armour & Co.) 4
aromatic chemistry 6
Army (United States) 2, 3, 12, 13, 26
astrology 2
astronomy 2
asymmetric induction 22

Barnes, Roderick A. 8
Bartlett, Paul 10, 11, 24, 31
Beaver, Jacob J. 13
Beckmann, Charles O. 15, 18
Berkeley, California 16
Bernstein, Sidney 17
Berson, Jerome A. 17
beta-keto-glutaric acid 7
biochemistry 32
biophysical chemistry 17
Branch, Gerald E. 10, 11, 26, 27
Bronx, New York 1, 2, 3
Brookhaven Instruments Corp. 14, 18
Brothers Karamazov 12
Brown, Herbert C. 6
Buffalo, New York 1

Caldwell, Benjamin P. 8
California Institute of Technology (Caltech) 27
California, University of, Berkeley 11, 14, 15 24-26, 27, 28, 30, 31
California, University of, Los Angeles 24, 26
California, University of, San Diego 15
Calvin, Melvin 10, 26, 27
carbon acidity research 32
Cason, James 27
Chandler Hall 15, 17, 20
Chemical and Engineering News *(C&E News)* 4
Chemical Reviews (*Chem. Rev.)* 21
Chemists' Club 7
Chevron Chemical Company 15
Chicago, University of 27
chlorination 4, 6
Clarkson College of Technology 15
Colorado, University of, at Denver 2, 12

Columbia College 3
Columbia University 2, 4, 7, 9, 11-14, 18, 21, 25, 26
Conant, James B. 11
Cope, Arthur C. 17, 18, 25
Cornell University 13, 25
Coulson, Charles A. 30
Crum, Ferris B. 25
Curtin, David 14, 15, 17

Dauben, William G. 27, 28
Dawson, Charles R. 15, 17
Dekker, O.M. 2-3
de la Mare, P. B. 19
DePuy, Charles 15
Dewar, William J. 30
Doering, William Von E. 9, 10, 12, 14-23, 29
Dostoyevsky, Fyodor 12
Du Pont de Nemours & Co., E.I. 5, 16, 25
Duquesne University 26

Eastman, Ermon 13
Elderfield, Robert C. 18
Electronic Interpretations of Organic Chemistry 5, 10
Emeryville, California 16
England 9, 19, 28, 30
Erlich, Gert 15
error analysis 13

Fahrenholt, Larkin H. 22
Farber, Milton 16
Fitzsimmons General Hospital 2
fluorene 4, 6
4n+2 rule 6, 21, 30
fractional crystallization 19
Friedman, Lester 4, 6, 7

Geiduschek, Peter 15
General Electric Co. (GE) 4, 6
Germany 1, 3, 19, 30
Gilbert chemistry set 3
Goodrich, Frank 15
Grignard reactions 19, 20
Grunwald, Ernest 17

Halford, Ralph S. 18, 25
Hammett, Louis P. 10, 11, 13, 15, 16, 17, 26, 32
Handbook of Chemistry and Physics 5
Harvard University 9-11, 27

Havemeyer Hall 16
Hayden Planetarium 2
Heathcock, Clayton 27
Hoffmann, Arthur 16, 22
Hooker Chemical Corp. 6
Huckel, Erich 21, 30
Huckel, Walter 30
Hughes, Edward W. 28, 29
hydrogenation 14

Ingold, Sir Christopher 19, 29
inorganic chemistry 5, 8, 14

Jamaica, New York 1, 6
Jensen, Fritz 27
Journal of Chemical Physics (J. Chem. Phys.) 28
Journal of the American Chemical Society (*JACS*) 30
Juliusburger, F. 28
Junior Astronomy Club 2

Kalechstein, Estelle 15
Katonah 21
Kimball, George 13, 15, 21
kinetics 16, 17, 28
Knox, Larry 21
Kodak (Eastman Kodak Company) 4
Kohler, Elmer 9, 11
Koppers, Inc. 6
Kosower, Edward M. 2, 4, 26

Latimer Hall 1
Latimer, Wendell M. 27
Levy, Joseph B. 17
Lewis, Gilbert N. 27
Lichtin, Norman 24
Linstead, R. P. 9
Longuet-Higgins, Hugh C. 30
Lucas, Howard J. 27

Madison, Wisconsin 17
Massachusetts Institute of Technology (MIT) 22-27, 29
McMillan, William 24
Meerwein-Pondorff reductions 22
Meinwald, Jerrold 25
Meislich, Herb 15
Miller, Julian 18
molecular orbital theory (MO theory) 13, 21, 24-26, 28, 30-31
Molecular Orbital Theory for Organic Chemists 31
Monsanto Company 15
Morlok, Sophie 1

National Academy of Sciences 13
natural products chemistry 9, 25
Nelson, John M. ("Pop") 8
New York Public Library 7
New York World's Fair 1
Noller, Carl R. 9
Norberg, Arthur L. 27
Noyce, Donald 27, 28

Obertal, Germany 1
Odum, Robert 16
Olsen, A.R. 28
optically active compounds 29, 30
organic chemistry 5, 6, 8, 9, 10, 11, 13, 14, 17, 18, 19, 20, 27, 30, 32
Organic Chemistry 8, 9
Organic Specialties 4-7
Organic Syntheses 6
organo-metallic chemistry 32

party at Columbia 18
Pauling, Linus 30
physical chemistry 11, 13, 15, 26, 27, 28, 30, 31, 32
Physical Chemistry 13
physical organic chemistry 10, 11, 19, 26 28, 32
Physical Organic Chemistry 10
Pierce-Arrow Company 1
Pitzer, Kenneth S. 32
Polaroid Corporation 16
polycylic hydrocarbons 6
Public School 85 (New York City) 3

quantitative analysis 13, 28
quantum chemistry 13
quinine, synthesis of 9

Rapaport, Henry 27
reaction mechanisms 28, 29, 31 32, 33
Redlands, University of 29
Remick, Arthur E. 5, 10
Roberts, Jack 16, 17, 21, 22-27, 31
Rohm & Haas Co. 16
Rollefson, Gerhard 13
Rosenblum, Mike 25

Sandmeyer reaction 6
Saunders, William H. 23, 25

Schaeffer, William	29
Shell Development Co.	16
Shiner, Jack	31
Simmons, Howard	25
solvolytic reactions	19-20, 28
spectroscopic instruments	25
Stamford, Connecticut	16
Steinberg, Howard	25
stereochemistry	20, 22, 28, 29, 31, 32
Stewart, T. Dale	28
Stieglitz, Julius O.	27
Streitwieser, Andrew, Sr.	1
Streitwieser, Mary Ann	23
Streitwieser, William	3
Stuyvesant High School	1, 2
sulfuryl chloride	6
Symes, William	15
synthesis, chemical	9, 10, 18, 32, 33
Taft, Robert	16
tertiary systems	19-20
Theory of Organic Chemistry, The	10
triarylmethyl halides, ionization of	24
Union Oil Co. of California	29
United States Borax & Chemical Corp.	25
University of Rochester	23
Vanderbilt University	27
Vaughn, Howard	16
Watanabe, Warren	16
Weaver, A.H.	11-12
Westheimer, Frank	17, 29, 30
Westinghouse Science Talent Search	2, 4
Whelan, William (Ugi)	16
Wiberg, Kenneth B.	15
Widom, Benjamin	13
Wilds, Alice	21
Wiley, Douglas W.	16
Winstein, Saul	19, 20, 23, 26, 29
Wisconsin, University of	14
Wolf, Alfred P.	14, 15, 22
Woodward, Robert	9, 10, 19, 25
Yale University	16
Young, Richard W.	15, 16, 22
Zeiss, Harold	19, 20