

CENTER FOR HISTORY OF CHEMISTRY

CHEVES WALLING

Transcript of an Interview  
Conducted by

Leon Gortler

at the

Mayflower Hotel  
Washington, DC

on

12 September 1979

Cheves T.  
Walling

JA  
3/15/86

CENTER FOR HISTORY OF CHEMISTRY ORAL HISTORY PROJECT

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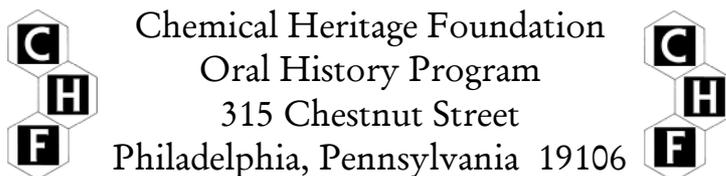
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ABSTRACT: Cheves Walling begins this interview by mentioning his family, early education, and undergraduate days at Harvard. He then discusses his graduate education at the University of Chicago, stressing the major review article on the peroxide effect that he and Frank Mayo wrote in 1940. Walling then describes the research that he undertook at Du Pont, U. S. Rubber, and Lever Brothers. He emphasizes the work that he did before 1950 at U. S. Rubber. Finally, Walling elucidates his academic career at Columbia and the University of Utah. Throughout the interview he reflects upon the emergence and maturation of physical organic chemistry.

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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CHEVES T. WALLING

1916 Born in Evanston, Illinois, 28 February

Education

1937 B.A., chemistry, Harvard University

1939 Ph.D., organic chemistry, University of Chicago

Professional Experience

1939-1942 Research Chemist, Jackson Laboratories, E.I. du Pont de Nemours & Company

1943-1949 Research Chemist, U.S. Rubber Company

1949-1952 Research Associate, Lever Brothers Company  
Department of Chemistry, Columbia University

1952-1970 Professor

1963-1966 Chairman

1970- Distinguished Professor, University of Utah

Honors

1964 Elected to National Academy of Sciences

1965 Elected to American Academy of Arts and Sciences

1971 James Flack Norris Award, American Chemical Society

1975- Editor, Journal of the American Chemical Society

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INTERVIEW: Cheves Walling

INTERVIEWED BY: Leon Gortler

PLACE: Mayflower Hotel  
Washington, D.C.

DATE: 12 September 1979

GORTLER: This is Leon Gortler interviewing Cheves Walling at the Mayflower Hotel in Washington, D.C., on September 12, 1979. Professor Walling is currently Distinguished Professor of Chemistry at the University of Utah and is also Editor of the Journal of the American Chemical Society. I know that you were born in Evanston, Illinois, in 1916 but I know almost nothing about your family. Tell me a little about your family. Do you have brothers and sisters?

WALLING: I had a sister and two brothers. My two brothers are now dead. I was the youngest of four children.

GORTLER: Could you give me their names?

WALLING: Willoughby Haskell Walling and William English Walling II were my brothers. My sister's name is Frederika Christina Ross.

GORTLER: Would you tell me a little bit about your father's education and what he did?

WALLING: He graduated from the University of Chicago and then briefly attended Harvard Law School. After returning to Chicago, he became a banker and an active participant in civic and philanthropic affairs.

GORTLER: I think you'd best give me his name too.

WALLING: Willoughby George Walling. His brother was William English Walling, one of the prominent early Socialists.

GORTLER: That's an interesting fact that won't be found anywhere else.

WALLING: My uncle was quite well known. He wrote a lot.

GORTLER: Are there any particular ways in which your father influenced you?

WALLING: Well certainly not by interesting me in science. There had been a good many doctors in the family and I think either he or my mother would have been pleased if I had gone into medicine. I never seriously contemplated doing that,

however.

My mother came from South Carolina. Her father, who had fought in the Civil War and surrendered the Confederate Cavalry at Appomattox, had become a judge in South Carolina.

GORTLER: A claim to fame. Your mother's name?

WALLING: Frederika Christina Haskell. I think I'm one of the few chemists in the profession who actually came from a fairly well-to-do family.

GORTLER: Actually, that's not all that true. Of the people I've interviewed, a good number have come from families who could readily provide for their children's education. The one's I've talked to have gone to places like Harvard and the better private schools.

WALLING: They tend to come from families who were on the way up and believed in education.

GORTLER: Yes. Your family was relatively well educated and you grew up in Evanston?

WALLING: I grew up in Winnetka, Illinois.

GORTLER: Can you remember what it was like in Winnetka? Tell me about your schooling and about particularly influential teachers.

WALLING: Winnetka had very good public schools and I attended one of them for four years. In the fifth grade, however, I transferred to the North Shore Country Day School, a very good college preparatory school, from which I graduated.

I was interested in science ever since I learned how to read, but I didn't think chemistry was an interesting science until I learned how to make gunpowder in the fifth grade. I've stuck with chemistry ever since.

Although I knew that I wanted to be a chemist very early in life, I didn't get a very good science education before I entered college. I had had a rather good introduction to astronomy and things like that in elementary school, but I never had any chemistry in high school. The only real science course I took in high school was physics. I read a lot and taught myself chemistry. I also talked myself into the chemistry course at Harvard which required the successful completion of a course in high school chemistry. In fact, it only required that you knew how to balance equations.

GORTLER: It's interesting that you managed to learn all of that by yourself. Was there anyone at that level who fostered

your interest in science or in chemistry?

WALLING: No one who really affected me much. The staff at school was sympathetic but didn't help me much more than that.

GORTLER: So because your family had a secure financial position you didn't feel that you missed any experiences as a result of being deprived. I assume that your family expected you to go to college.

WALLING: Yes.

GORTLER: Did you consider any institutions besides Harvard?

WALLING: Not very seriously. One of my older brothers matriculated at Harvard but only lasted until midyear. He wasn't much of a scholar. Most of my friends went East to college. I remember that seven out of about fourteen boys in my senior class went to Harvard. It was easy to do in those days. If you had four hundred dollars tuition and you did satisfactorily on the College Boards, you got in.

GORTLER: I suppose being a midwesterner helped too. Can you tell me something about life as an undergraduate at Harvard? Also, what year did you begin there?

WALLING: 1933. I roomed with one of my high school classmates who also was a chemistry major. My adviser in my freshman year was Henry Bent, Sr., a physical chemist. I remember fascinating him by telling him that I had arranged a Model A Ford so that I could spray stannic chloride into the exhaust pipe. Doing this makes an effective smoke screen. I always had a number of extracurricular chemical projects going on. I persuaded Louis Fieser, who was then teaching the one-semester freshman general chemistry course for people who had taken high school chemistry, that I could take the course. Later, I had him for organic chemistry. He was a very effective teacher because he made chemistry look interesting and because he enjoyed it immensely. I imagine you've heard this from many other people.

GORTLER: Yes, I have. This is a little bit premature, but I notice that you published a couple of times around 1935 with a fellow named C. Harold (Hap) Fisher.\* Somehow I've missed him in looking through the catalogue.

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\*C. Harold Fisher and Cheves T. Walling, "Reaction of Alpha-Dihalo Acetophenones with Alkali," Journal of the American Chemical Society, 57 (1935): 1562-64. Also, C. Harold Fisher and Cheves T. Walling, "Xylic Acids Obtained in the Oxidation of 5-Bromo-and 5-Nitropseudocumene," Journal of the American Chemical Society, 57 (1935): 1700-02.

WALLING: At that time he was an instructor at Harvard and in charge of the undergraduate lab. He had a Ph.D. degree from Illinois. He would let some of the better students in the organic course do some independent research rather than the regular laboratory. He took me under his wing and let me do some work with him.

GORTLER: I see. Obviously, he didn't stay on very long at Harvard.

WALLING: No, he didn't. He's at the ACS meeting here in Washington, D.C. He went to the Department of Agriculture where he had quite a successful career. He was in charge of one of their laboratories for a long time.

GORTLER: What about other courses at Harvard that might have influenced you, either in or out of science?

WALLING: Well, I was certainly taken by organic chemistry very early. I did a lot of reading and thought E. E. Slosson's Creative Chemistry\* to be a marvelous book.

GORTLER: That's interesting.

WALLING: It came out right after World War I.

GORTLER: In the '20s, that's right. Paul Bartlett mentioned the same book.

WALLING: I read it as a kid and was very impressed. By reading Slosson's book I learned how to draw structural formulas and therefore discovered that isomers existed because there were more than two ways of drawing some of these things. I also read Eddington and Jeans rather extensively when I was in high school. At Harvard, E. P. Kohler discussed reaction mechanisms while teaching the advanced organic course. I found it a rather inspiring course.

GORTLER: How do you characterize Kohler?

WALLING: He was a little crusty. I also did some individual research in that course, although I never published any of my results. I worked with Max Tishler, the head of the laboratory in the course. Tishler was Hap Fisher's friend, so I got to know Tishler quite well when I was a sophomore. All of these people had some effect on me. I guess Bartlett was a graduate student when I was an undergraduate student.

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\*E. E. Slosson, Creative Chemistry (New York: The Century Co., 1919).

GORTLER: He'd finished about 1930 and did not return until about 1934. He was at Minnesota when you started at Harvard.

WALLING: I guess I met him when he came back to the staff. I never got to know him while I was an undergraduate.

GORTLER: He was just fresh to the staff.

WALLING: I found physical chemistry to be less rewarding, although it was something of a challenge to figure out. I regarded quantitative analysis as a good test of whether I was serious about chemistry. If I was willing to put up with that...

GORTLER: Do you remember who taught that class?

WALLING: G. P. Baxter.

GORTLER: Was Kistiakowsky teaching physical chemistry?

WALLING: Bent was teaching that. I took it as a junior.

GORTLER: You weren't treated to Forbes then.

WALLING: No. Those were the major courses one took. I also took a course in industrial chemistry but I don't remember the name of the old gentleman who taught that course.

GORTLER: It was Grinnell Jones, I think.

WALLING: It was rather interesting, especially the trips to the plants.

GORTLER: Louis Hammett mentioned the same course. He took it about twenty years before you had.

WALLING: I took a biochemistry course. L. J. Henderson was in charge of it, but there were several lecturers.

GORTLER: Do you remember any of the textbooks that you used during that period?

WALLING: Yes. I used Conant's organic text\* and Hammett's Solutions of Electrolytes\*\* in qualitative analysis. Those are the only ones that I can remember.

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\*James Bryant Conant, Organic Chemistry; a Brief Introductory Course (New York: Macmillan Company, 1928).

\*\*Louis Hammett, Solutions of Electrolytes (New York: McGraw-Hill Book Company, Inc., 1929).

GORTLER: You mentioned that Kohler's course was oriented a little toward mechanisms.

WALLING: At least to the point of figuring out what the intermediates were in chemical reactions.

GORTLER: I'm trying to develop that because he apparently had an influence on a number of people.

WALLING: There was a good deal of stereochemistry in it; a lot of discussion about questions like 1,2 versus 1,4 addition. Why do you observe one result in one case and the other in another?

GORTLER: You had decided to become a chemist rather early in life. What were your perceptions about being a chemist when you were in college?

WALLING: I hoped that I would be able to get a teaching position but my perceptions were rather fuzzy. A good many people tried to discourage me and told me that chemists did analyses in the back of large factories. I think that was probably true about a good many of them. I didn't really have a very clear idea of what they did but I thought that if I could find a way to do it, I would do it.

GORTLER: You shouldn't feel embarrassed about that. I haven't talked to a person yet who really had a clear idea of what it was like to be a chemist. Do you remember any friends from undergraduate days who became chemists or went on to graduate school?

WALLING: I think Saul Cohen was the only classmate of mine who has gone on to a very notable career in chemistry. There are other people I've seen occasionally. Ned Riddle and another fellow went to Rohm and Haas. I think they both went to Illinois and got their Ph.D.'s. A fellow named Morris Zief, who is now with one of the inorganic companies, published an interesting paper in CHEMTECH recently about producing the extraordinarily pure reagents needed to make optical glasses.\* Not many of my classmates went into chemistry. There were a good many chemistry majors, like there are now, who went into medicine.

GORTLER: When did you decide to go to graduate school?

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\*Morris Zief and A. J. Barnard, Jr., "Re: High-Purity Reagents and Their Uses," Chemical Technology, 3 (July, 1973): 440-44.

WALLING: I always thought that if I did well enough in college, I would go to graduate school. Obviously, if I was going to teach, I would need to go.

GORTLER: How did you choose your graduate school?

WALLING: Well, my father was a great promoter of Chicago, so I inquired around and found that Chicago had a perfectly good graduate school. I also thought that it might be nice to be near home. After Harvard offered me a fellowship, I called my father and asked, "Do you want to support me at Chicago or should I take the fellowship?" He said he'd be glad to support me at Chicago.

GORTLER: That was very generous of your father.

WALLING: It only cost three hundred dollars.

GORTLER: Chicago must have been a pretty exciting place for a young chemist at that time. Can you tell me something about your experiences there?

WALLING: Initially, I talked to Max Tishler who said that he thought that Kharasch was doing the most interesting work there. In the spring of my senior year while home on vacation, I visited the chemistry department at Chicago and talked to Kharasch. The procedure was pretty simple in those days. All I did was to show up at registration, say that I wanted to go to graduate school, bring my diploma with me, and then pay my tuition. I think at that point they wrote to Harvard to find out more about me.

GORTLER: You didn't have to apply for admission in advance?

WALLING: No, because I wasn't applying for a teaching fellowship. So, Kharasch was sort of preselected for me as the most likely person to work with. At that point he was certainly the prominent organic chemist there.

GORTLER: That seems to be a fairly common occurrence for students who go to graduate school. People advise them in advance.

WALLING: Well, Frank Westheimer was there but he was just starting. George Wheland was there but he was much more theoretical. Besides, he might have been there for only a year; he started about the same time that I did.

GORTLER: He'd just finished working with Pauling, I guess, and so he was very theoretical.

WALLING: If I remember correctly, I took almost no courses for credit because Chicago had a very flexible system. I audited Kharasch's courses, however, took thermodynamics, took Wheland's course, and sat in on a good many others.

GORTLER: Who was teaching the thermo course at that time?

WALLING: T. F. Young. He used good old Thermodynamics by Lewis and Randall.\*

GORTLER: Twenty years later I took a course with T. F. Young--the laboratory part, anyway. Do you remember who was chairman at Chicago at that time?

WALLING: Schlessinger, I guess.

GORTLER: What was George Wheland teaching?

WALLING: He was teaching a course in theoretical organic chemistry and the application of resonance theory to organic. He got a somewhat mixed reaction from the students, I think, because he seemed to offer an awful lot of ad hoc explanations. It was a very qualitative type of treatment. Essentially, he combined Pauling and the British electronic theory of chemistry--moving electrons around.

GORTLER: Did you sit in on Westheimer's course? Did he teach a course at that particular time?

WALLING: He taught a course in chemical kinetics that I sat in on. It was very good.

GORTLER: I remember his mentioning an early course that he taught where the students were very sharp. The group included H. C. Brown and you and a group of other people who made up a really formidable audience.

WALLING: H. C. Brown was there at the same time that I was. Westheimer accused me of sleeping in his class. Actually, that happened because we had class at three in the afternoon and not because I didn't think it was very interesting.  
(laughter)

GORTLER: You took only two years to complete your Ph.D. How did you manage that?

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\*Gilbert N. Lewis and Merle Randall, Thermodynamics and the Free Energy of Chemical Substances (New York: McGraw-Hill Book Company, Inc., 1923).

WALLING: I was able to start doing research right away. I also worked two summers. I passed the comprehensive examinations at the end of the first quarter. Since I wasn't taking many courses for credit, I spent my time doing research.

GORTLER: I see. Can you describe the tenor of the Kharasch group? How was it run? Did you see Kharasch very often?

WALLING: Kharasch always had a subordinate who looked after his research group. Frank Mayo filled the position at that time. Most of our day-to-day dealings were with Frank Mayo who became a very good friend of mine. Kharasch did have seminars in the evening every other week or so. Students would then make reports and discuss things. Kharasch usually attended the seminars; otherwise, you only saw him when you had done something important or were in serious trouble. He didn't have remarkably good students. There were a few good ones, but most of them were more or less filling in pieces of projects that he laid out. Almost all of the research at that point was on the peroxide effect--mostly with hydrogen bromide, but also, in part, looking for other reactions that did the same thing. Each student looked at a different piece.

GORTLER: Did you have any feelings about Kharasch as a chemist at that particular time? How did you feel about his comprehension of the problems on which he was working?

WALLING: Well, at that time I felt he knew much more than I did. Students tended to be afraid of him, although I don't think I was. Later on, I got to know him quite well.

He knew what he was trying to establish. The explanation of the peroxide effect in terms of the bromine atom chain reactions was worked out about the time that I got there. At least I know it was published while I was there. He and Frank and I were going to write a review of the work. Kharasch decided though that he didn't have the time to work on it, so Frank and I wrote it.\* I'm sure that Kharasch looked it over to make sure that he agreed with it.

GORTLER: Somewhere I heard a rumor that his name was on the original review and that a referee objected to that. I don't know why a referee would have objected.

WALLING: His name was going to be on the original review but Frank said that Kharasch decided that he wanted to take it off and just leave our names on it. That is the way the manuscript was submitted.

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\*F. R. Mayo and C. Walling, "The Peroxide Effect in the Addition of Reagents to Unsaturated Compounds and in Rearrangement Reactions," Chemical Reviews, 27 (1940): 351-412.

GORTLER: I see.

WALLING: It didn't contradict his views, I don't think. We may have put in some things about which he may not have thought.

GORTLER: You started the review before you left Chicago.

WALLING: I wrote the first draft and Frank and I went over it quite extensively before I left.

GORTLER: That was quite an undertaking for a student who had been a graduate student for only two years.

WALLING: The literature wasn't as extensive in those days.

GORTLER: James Senior was acknowledged in that paper.

WALLING: Yes, probably for his discussions with Frank. Wheland was acknowledged in it too.

GORTLER: He looked it over as well?

WALLING: Yes. He, Frank, and I used to eat lunch together occasionally. I think the most important new idea in that review was the attempt to show why you observed these reactions with hydrogen bromide but not with the other halogen acids on account of the energetics. I think that was my contribution, and a rather major one at that, which was undoubtedly stimulated by my discussions with Wheland. It might have been his idea too, I don't know.

GORTLER: Well, that was obviously an important idea because it has now entered the undergraduate textbooks.

WALLING: The numbers have changed a little bit but it still holds together.

GORTLER: It only took about twenty-five years to get into the undergraduate texts. Was Julius Stieglitz around at that time?

WALLING: No, he died before I got there.

GORTLER: Did you have any feeling at that time that there was a conscious effort to build a physical organic group at Chicago? Someone was collecting a group of essentially physical organic chemists. I know that Kharasch had recruited Frank Westheimer. I don't know who brought in Wheland. Mayo was an instructor even though he was Kharasch's lieutenant.

WALLING: I wouldn't have known about it if there had been. Either Frank Westheimer or Frank Mayo might know more about that. Actually, Kharasch was always something of a lone wolf. In subsequent years he was one of these large trees in the shade of which other things don't grow very well.

GORTLER: Yes. I think I heard that before. I was an undergraduate at Chicago and he died shortly after I got there, but I had that same impression from a number of people.

WALLING: I'm sure that he had strong ideas about how the department should be run, but I don't know what they were.

GORTLER: Had H. C. Brown also worked with Kharasch?

WALLING: He worked as a post-doc for Kharasch for a year or so. He took his degree with Schlessinger.

GORTLER: Then during the war he stayed on for a while with Schlessinger.

WALLING: Yes. He has written extensively on this.

GORTLER: I know. I haven't gotten to that but I will. You mentioned the Kharasch seminars and I wondered what the climate was like as far as chemical communication went. What other seminars did you attend? To whom did you talk about chemistry?

WALLING: There were no other formal ones. I only remember hearing Pauling when he came and gave some lectures. That's about the only event outside of Kharasch's group that I can remember.

GORTLER: To whom were you talking? Frank Mayo, obviously. Were there other people with whom you discussed chemistry?

WALLING: Not a great deal. One of the more able people in the group was a fellow named Ernie May. He went back to Newark to work for the family concern, a dye company. I saw him once about twenty years ago. I have no idea what happened to him subsequently. There weren't many others who overlapped with me.

GORTLER: You were obviously reading papers on free radical chemistry at that time. Do you remember whose papers you were reading aside from those that you quoted in the Reviews article? Who did you think were the most influential chemists?

WALLING: I probably cited everything I read in the article. I went abroad after my senior year in college and remember

being advised that I should get a copy of Hückel's two-volume book on theoretical organic chemistry.\* I bought it in Heidelberg, took it home with me, and tried to read it in German for a long time. There was a book by Hickinbottom on organic synthesis that would tell one how to make everything out of anything and that one was advised to read and almost commit to memory.\*\* Gilman's two-volume work was another important book.\*\*\* I didn't read much theoretical. I was mostly reading organic. There was a very sensible feeling that one really needed to know an enormous number of facts about organic chemistry before he could begin thinking about theory. This view contrasts with the present approach.

Having listened to George Wheland I was briefly taken with the idea that maybe quantum mechanics was the solution to organic chemistry. So, I sat in on courses on atomic and molecular spectra. I also took a couple of math courses but decided at that point that doing that was too difficult. It was quicker to do experiments. Periodically, the mathematics sentiment surfaces and becomes fashionable.

GORTLER: I think we're back into that cycle right now.

WALLING: Very much so. Computers are a great help. In those days calculations were impossibly tedious.

GORTLER: At that time, Westheimer told me about the lengthy calculations he did for the Kirkwood paper.

WALLING: Yes, I was going to mention that. Here's something that is just a trivial exercise right now but at that time it took a long time to do.

GORTLER: From your Reviews article I got the impression that there was a great deal of interest in that abnormal addition problem, and that people like Lucas, Young, Winstein, Urusibara, Takebayasi, Sherman, Quimby, and Sutherland had also worked on it. Was there competition within the Kharasch group?

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\*Walter Hückel, Theoretische Grundlagen der Organischen Chemie (Leipzig: Akademische Verlagsgesellschaft, 1931).

\*\*W.J. Hickinbottom, Reactions of Organic Compounds (New York: Longmans, Green & Co., 1936).

\*\*\*Henry Gilman, ed., Organic Chemistry, an Advanced Treatise (New York: John Wiley & Sons, 1943).

WALLING: Well, there certainly was on the part of Kharasch.

GORTLER: I had the feeling that he was combative.

WALLING: Yes, he was. I read the papers, of course, and I was aware of the whole history. I wrote a summary of the papers. It was published in my thesis. I remember Frank telling me that Kharasch might want to change my introductions to the papers a little bit to make sure that they were consistent with his viewpoint. I figured that was his privilege.

GORTLER: Do you remember his doing so?

WALLING: Well, it was just a matter of emphasis. Some of his early ideas turned out not to be very realistic and he didn't want these reviewed in too much detail.

GORTLER: I see. When you graduated, what kind of chemist did you feel you were? Could you have categorized yourself?

WALLING: Well, I'm sure I thought of myself as an organic chemist. Had I been free to do whatever I wanted to do in chemistry, I'm not quite sure what I would have done. I didn't need to make up my mind.

GORTLER: Somehow you've implied that you were directed.

WALLING: Well, there weren't very many jobs. The only good academic job for which I applied and for which I was recommended was a position up in Rochester. I was interviewed there. It turned out that the two prime contenders for the job were myself and Ted Cairns. They decided to take Cairns. I was offered a job with the Du Pont Company. That was the only other job offer I got. I went to Du Pont and he went to Rochester. A little bit later, we switched between industry and teaching. I don't remember how long he stayed at Rochester or what led him to go back to Du Pont.

GORTLER: So you went to Du Pont because there was a job there?

WALLING: Yes.

GORTLER: Where are the Jackson Labs?

WALLING: In Deepwater, New Jersey. I went to the Chamber Works, the big dye works across from Wilmington. I guess that was one of the biggest Du Pont plants. That's where they located all of their dye industry and most of their synthetic organic chemistry work.

GORTLER: Do you remember your starting salary?

WALLING: Yes. Two thousand and seven hundred dollars.

GORTLER: Everyone remembers his starting salary.

WALLING: That was quite a lot of money at that time.

GORTLER: I'm sure it was. What kinds of things did you do there?

WALLING: Well, I started working on azo dyes. I synthesized new ones and tested them although I remember being a little conscious that I wasn't quite sure of their composition. I was making metallized azo dyes which are chelates, mostly chromium complexes, but also other transition metals. Nobody ever really bothered to analyze them; they just looked at the colors. I knew that one of the most important properties of a dye is its light fastness and that no one had made any effort to find out why some dyes were fast and others weren't, except by looking for a structural correlation. It was absolutely empirical. I did that for about a year and then got sensitized to the azo dye intermediates. I was therefore switched to something else. I think I synthesized some of the polychlorodioxins accidentally. It was well known in those days that some people got sensitized to these things. If they did, they were assigned to work on other things.

GORTLER: I noticed that there was a gap of four years in your publication record. I assume it has to do with the time you spent at Du Pont.

WALLING: Yes. I got a few patents at that time for dyes, oil additives, and gasoline additives.

GORTLER: You were married in 1940. Can you tell me your wife's name, how you met her, and a little bit about her influence on your career?

WALLING: Her name is Jane Ann Wilson. I met her in Winnetka. She had gone to Vassar and graduated the same year that I did. Her parents died and she took care of her younger brother and sister in Winnetka. We were tentatively engaged, I guess, when I went to work. In those days you made sure that you had a job before you got married. After working for a year I demonstrated that I could hold a job. Marrying Jane then became socially acceptable. She doesn't have any chemical background at all, not being inclined in that direction.

GORTLER: Later on, you may want to say something about any influence that she's had on your career. I'll ask you about

your children later, if that is all right with you.

Before I get to your switching jobs, let me ask you about your impressions about the state of organic chemistry and of physical organic chemistry at that particular point.

WALLING: Well, organic chemistry was largely an empirical art at that time. I rather enjoyed working in the laboratory and having reactions occur and getting a product. I found this to be very interesting because I wondered why things behaved the way that they did. There was a little discussion about this in Fieser's courses. I think orientation in aromatic substitution was probably one of the first things people began to try to interpret. I was still pretty empirical at that point; I just learned the rules.

GORTLER: Even though Ingold and Robinson had had some pretty thorough discussions about that in the 1920s and early 1930s.

WALLING: If I remember correctly Kohler had required that we read two relevant articles and a review article by a fellow at Du Pont who had discovered nylon.

GORTLER: So, from your perspective physical organic chemistry wasn't making inroads in 1940 even though you had been in a department in Chicago that was oriented in that direction.

WALLING: It was beginning to do so. During my Ph.D. oral defense Frank Westheimer asked me to discuss something that I had read and found interesting and that was outside of my thesis research. I talked about Ingold's work on  $S_N1$  and  $S_N2$  reactions. We were encouraged to read these things and they were discussed a little bit. It was a lucky choice.

GORTLER: If you were going to run an international symposium in physical organic chemistry in 1940, whom would you have invited?

WALLING: Certainly Ingold and Hammett. Hammett's book had been published by then or was about to be published.\* I'll have another comment on that in a minute. I guess that Calvin was considered active as were Lucas and Kharasch. I suppose that Hückel was a plausible person, as was Wheland. Those were the major ones. Incidentally, about 1937 when I joined ACS and subscribed to the JACS, the first article I remember

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

reading with interest and enthusiasm was Louis Hammett's paper on the sigma-rho relationship.\* Even at that point I recognized it as being interesting.

GORTLER: You said that you had a comment about Hammett.

WALLING: That's what I was going to say. I later heard Frank Westheimer talk about it. He had just worked with Hammett.

GORTLER: What brought about the switch to U.S. Rubber?

WALLING: I didn't think that I was advancing quickly enough at Du Pont. Frank Mayo had gone to U. S. Rubber where they were starting a fundamental research program in polymer chemistry underlying their work on synthetic rubber. Essentially, he recruited me, although he had to keep it slightly under wraps. In those days there were rather firm non-hiring agreements between major companies. I had to resign from Du Pont before I could have any open negotiations with U. S. Rubber.

GORTLER: Do you know anything about Mayo's leaving Chicago?

WALLING: I think that he left, in part, because the salary scale at Chicago was not very high. By contrast, U. S. Rubber looked like a really good opportunity. Also he could not expect to get very far at Chicago because he was always in the shadow of Kharasch.

GORTLER: Particularly if he was Kharasch's lieutenant. You moved during wartime, around 1942.

WALLING: January, 1943.

GORTLER: Tell me something about U. S. Rubber and how the groups were set up.

WALLING: They had essentially decided to set up groups to do fundamental research both to support their ongoing program and to prepare themselves to be wherever they wanted to be when the war ended.

GORTLER: Was R. T. Armstrong involved? You mentioned him.

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\*Louis P. Hammett, "Effect of Structure upon the Reaction of Organic Compounds. Benzene Derivatives," Journal of the American Chemical Society, 59 (1937): 96-103.

WALLING: Gibbons was director of research and a strong supporter of this policy. Hubert Jordan and Armstrong also believed in it. They set up a physical chemical group and an organic group. Frank headed the organic group and Ros Ewart headed the physical group. It turned out to be a marvelous time because the work was just at the point where the principles were around if anyone could see how to apply them. By the time I got there Frank had already worked out the mathematical theory of copolymerization and the mathematical theory of chain transfer. He had also accumulated data showing that they worked. This turned out to be a very exciting and productive period for a few years. The company wasn't very good at doing anything with its research, so after a while they lost interest and this program folded.

GORTLER: But you kept working there for six or seven years.

WALLING: I went there in the beginning of '43 and I left in '49. I took a leave of absence in order to work for the government for six months during the war.

GORTLER: You published with a number of people during that period. They include Frank Mayo, Emorene Briggs, and Fred Lewis. Tell me a little bit about them.

WALLING: Fred Lewis was a very competent fellow who had a master's degree from Illinois. He was a very good experimentalist and was full of ideas. He did the first good experimental work on the copolymerization problem. Lewis contributed a great deal to the program. He got himself a Ph.D., although he didn't need one, by working nights. He ended up in the silicone division of G.E. I think that he's retired now.

GORTLER: Did he move to G.E. when Frank Mayo did?

WALLING: I left first and then he left and then Frank left. Two other very competent people left. They were Ed Hart and Max Matheson who were successively the directors of part of the radiation lab at Argonne. Ros Ewart and a fellow named Smith who worked with him stayed with the company until they retired. They did very important work on the theory of emulsion polymerization. There was a very good group there for a while.

GORTLER: Yes, it sounds like it was a strong group. Who was K. W. Doak?

WALLING: He was another new Ph.D. who arrived about the time that I was there. He left and worked in a couple of industrial jobs. I'm not sure where he is now.

GORTLER: The only reason I ask is because I didn't see him as a coauthor on any of your papers. Yet, in the copolymerization review, you mentioned him along with Fred Lewis and I assume that he must have contributed.

WALLING: He didn't work with me. He worked for Frank and published some things with him.

GORTLER: Who was Emorene Briggs? Her name appeared on several papers.

WALLING: Yes. She just had a bachelor's degree. She married and eventually went to Alaska with her husband.

GORTLER: There was at least one other woman who published, Katherine Wolfstirn.

WALLING: I think that she was the one woman in the group with a Ph.D. degree. I think that she became the head of the chemical library. I don't know what she finally did.

GORTLER: All of the other women were mainly technicians?

WALLING: Yes.

GORTLER: You told me that you did a paper with Emorene Briggs that the JACS published. It appeared after the one on peroxide and was a mathematical analysis of copolymerization in a system containing more than two monomers.\*

WALLING: She did the experimental work. I did the analyses and the mathematics.

GORTLER: Did that paper have some practical value or did you feel that it was significant only because it was an extension of the mathematical analysis that was already being used?

WALLING: I think that it has been used in technology. It was a further confirmation that the scheme worked, that you were able to extend it to the multi-component systems. It was as much a tour de force as it was important. It was my first venture into matrix algebra.

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\*C. Walling and E. R. Briggs, "Copolymerization. III. Systems Containing More Than Two Monomers," Journal of the American Chemical Society, 67 (1945): 1774-78.

GORTLER: You were a technical aide in the Office of Scientific Research and Development from '45 to '46. How did that happen and what did it involve?

WALLING: I guess that my wife had an effect on my working there. She went down to Washington to visit some friends and ran into an old friend of hers whose husband was the head of the legal department of OSRD. I guess he told her that they were looking for people. That's how I got that job.

GORTLER: What did you do down there?

WALLING: I worked for the group that was developing new anti-malarial drugs. I coordinated the office, keeping track of activities and visiting the contracts that they had. It was a valuable experience because I met a lot of people around the country and visited many laboratories.

GORTLER: This occurred either close to the end of the war or after it had ended.

WALLING: The war was still going on when I started at the OSRD and I stayed to the end of that calendar year. I would say that the most important thing I got out of my stay at OSRD was the opportunity to travel. Speed Marvel was a major figure in this enterprise and he advised me to visit all of the contracts. I remember that it seemed as if the war were just about to end and that my boss had gone to Peru because he wanted to see something. I wrote myself travel orders so that I could visit all of the contracts before the war ended. It did end as I was going through Hagerstown, Maryland. My boss was away and I didn't see any reason to cancel my trip. So, I finished my tour.

GORTLER: Were there any significant changes in the organic community or the physical organic community after the war?

WALLING: There certainly was much more interest in reaction mechanisms. A certain amount of work had been done on this topic during the war. People hadn't had much opportunity to get together, however, so after the war everybody was congregating and exchanging ideas about everything about which they had thought during the war. There was really quite a backlog of stuff to think about and talk about and do. There was an atmosphere of excitement. Additionally, there were a lot more people who had been trained as physical organic chemists. Some had been trained during the war and a new bunch were coming out of the graduate schools after the war. Many of these chemists had been working with Winstein and Bartlett and people who were getting the show on the road. To me, one of the most interesting things occurred in

'47 when I went to a meeting of the Faraday Society. There, I heard that the British had been doing a lot of interesting work in free radical chemistry, auto-oxidation, and things of this sort. I learned all about these endeavors at that time.

GORTLER: That's a totally new view of how physical organic chemistry spread so fast after the war. What about the industrial attitude toward physical organic chemists before and after the war? I think that you indicated its status before the war.

WALLING: Before the war there were very few places in industry that had any interest in physical organic chemistry. The work being done in most of Du Pont was certainly more classical. Roger Adams and Speed Marvel were the big suppliers of chemists to Du Pont--and they had a more nearly classical organic outlook. We've always had the situation that a few laboratories around the country did important fundamental work in industry. One company did it for a while and then another did. Du Pont was rather late in getting into the physical organic approach to organic chemistry. Frank Mayo went to General Electric where there was favorable sentiment for that sort of thing. Physical organic chemistry spread rather slowly in industry as companies began hiring people who had been trained to do physical organic chemistry.

GORTLER: I was shocked to learn that the work that you had done on copolymerization had come out of U. S. Rubber. I didn't think anything like that could ever come out of industry. Do you think that industrial outfits began to look for physical organic chemists after the war?

WALLING: Yes. One could persuade his employer that he might be able to make the process run better if he understood why it worked. There's always been a mixture of empirical and theoretical.

GORTLER: Do you think that what they had observed during the war had any influence on their attitudes?

WALLING: It certainly did in the polymer based industries. I'm sure that the oil companies did a good deal of this too because catalytic cracking was their major technical development during the war years and the carbonium ion basis of this was recognized very early.

GORTLER: When I spoke to you on the phone about the Organic Reactions Mechanisms Conference of 1946, you told me that you were there. Can you remember much about the conference, especially about its origins? You've already told me about some of the people who attended it.

WALLING: Charlie Price and Paul Bartlett had a hand in setting it up. Wheland was there. I don't know who else was. Winstein was there, I think.

GORTLER: Yes. He gave a talk. Hammett gave a talk. In fact, H. C. Brown gave a talk.

WALLING: On steric effects.

GORTLER: That's right. He called it "Non-Classical Steric Effects." Do you remember anything else about the conference? I realize that it was a long time ago.

WALLING: Not really, except that I felt that it was a good meeting.

GORTLER: The next paper that you recommended that I read is the one that you wrote with Briggs, Wolfstirn and Mayo, "The Effect of meta and para Substitution on the Reactivity of the Styrene Double Bond."\* Could you give me its background and what you felt was its significance?

WALLING: Well, it was the first time anyone tried to apply the Hammett Equation to a radical reaction. What looked like a polar effect on free radical reactions had come out of the very first paper done by Mayo and Lewis on copolymerization. We'd been gathering data on how this worked. I wanted to see if I could isolate the phenomenon better by going to the substituted styrenes in the same way that one does in polar reactions. The latter is done by moving the groups away from the reaction site.

GORTLER: The topic of polar effects on free radical reactions is something that's always fascinated me. I don't know if anyone's ever totally explained it yet, aside from their being a partial polar influence. One can draw structures with partial carbonium ion character.

WALLING: Yes. Attempts to treat it more quantitatively have not been awfully successful. But the principle is there. There are several ways of looking at it that lead to much the same conclusion.

GORTLER: Yes. I know Bartlett did the same thing with peresters and found exactly the same thing. The question still remains, what is it?

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\*Cheves Walling, Emorene R. Briggs, Katherine B. Wolfstirn, and Frank R. Mayo, "Copolymerization. X. The Effect of meta- and para- Substitution on the Reactivity of the Styrene Double Bond," Journal of the American Chemical Society, 70 (1948): 1537-42.

WALLING: Paul Bartlett and I were in close communication on that subject ever since we both started working on it.

GORTLER: The next paper was, "Copolymerization by Non-Radical Mechanisms."\* That paper contained a discussion of the different substances produced from radical and carbonium ion polymerization. You suggested that it might be a tool for determining mechanisms of polymerization using different catalysts.

WALLING: It works very nicely.

GORTLER: It does. Has it been used?

WALLING: Yes. There's almost nothing you can do to alter the reactivity in the radical reactions. You may be able to alter them somewhat in the ionic reactions because these things are at best, ion pairs, and sometimes they're rather covalent. Still, it has been a very useful tool, helping to elucidate what mechanism is involved.

GORTLER: The next paper, "The Acid Strength of Surfaces," was from Lever Brothers.\*\*

WALLING: Well, actually I think I probably did the work with U. S. Rubber although I was then with Lever Bros. I think it was a little hobby that I did in my spare time.

GORTLER: It was a nice little paper. It was another attempt to quantify or semi-quantify some kind of chemical fact.

WALLING: This approach has been used very extensively since then by petroleum people to characterize catalysts.

GORTLER: I take it that their measurements are a bit more quantitative now.

WALLING: Yes. It's hard to make them really quantitative. Acid catalysts basically have two properties. One is stoichiometric, that is, how many acid sites are there. The second tells how strong they are. You have a little trouble deciding whether these things are acting as Brønsted acids or as Lewis acids. I never got involved with that.

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\*Cheves Walling, "The Acid Strength of Surfaces," Journal of the American Chemical Society, 72 (1950): 48-51.

\*\*Cheves Walling, Emorene R. Briggs, William Cummings, and Frank R. Mayo, "Copolymerization. XIV. Copolymerization by Non-radical Mechanisms," Journal of the American Chemical Society, 72 (1950): 1164-68.

GORTLER: Finally, the Chem Reviews article\* on copolymerization that you and Frank Mayo wrote was another monumental effort. What prompted the two of you to write that one?

WALLING: Well, we were asked to write it and I guess we both thought it was a good idea. The important thing is that besides putting it all together in one place, it was the first real attempt to show how these things applied to other radical reactions.

GORTLER: Did the editors of Chem Reviews suggest that you write it? You said that you were asked to write it.

WALLING: I think that they asked us to write it, although I don't really remember. Maybe Frank and I thought it up; I'm not sure.

GORTLER: Did you again prepare the first draft?

WALLING: Well, we worked on different pieces of it. Frank's a very careful writer, very meticulous. He liked to make sure that we wrote everything just right, so we spent much of the time arguing.

GORTLER: A good deal of it came out of the work that you had been doing. Did this give you any incentive to do the 1957 book?\*\* Did it give you a running start on it or have any effect on it? Had you begun to think about a book?

WALLING: Well, I guess that I had a lot more practice doing writing by then. Writing got easier.

GORTLER: When did you start on the '57 book?

WALLING: It took about three years to write. I think I started writing in the summer of 1954.

GORTLER: So you were already at Columbia at that time.

WALLING: Yes.

GORTLER: I talked to Charlie Price in the Spring of 1979 and he, at one point, mentioned his Q/e scheme. He said that you were never a big fan of that scheme. While reading the papers that you wrote, and your book, I noticed that you always gave

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\*Frank R. Mayo and Cheves Walling, "Copolymerization," Chemical Reviews, 46 (1950): 191-287.

\*\*Cheves Walling, Free Radicals in Solution, (New York: John Wiley & Sons, 1957).

it some praise. Nonetheless, he said that you were a detractor. He mentioned that this had been so because you had thought of the Q/e scheme at some earlier time but had abandoned the idea. You had thought that there was no feasible way to determine an initial Q/e.

WALLING: It took me some time to convince him that he had to make two arbitrary choices before he could start building the scale.

GORTLER: I guess that he knew that, but somehow he managed to make the arbitrary choices.

WALLING: And he made plausible arbitrary choices.

GORTLER: I guess that one of your criticisms was that it wasn't an absolute; it was a relative thing.

WALLING: There are some deviations from it. I just thought it was rather qualitative. It's an awfully simple model from my viewpoint.

GORTLER: You left U.S. Rubber in 1949. You already told me that a couple of other people were leaving about the same time. Were there any particular reasons for that?

WALLING: U.S. Rubber was diminishing its support of its basic research. Additionally, I didn't very much like the new research director. I thought, therefore, that I would like to change positions.

I inquired about teaching jobs. I had been teaching a night course for a couple of years at Brooklyn Poly. Lever Brothers approached me, however, and offered me a very large salary. They probably got my name from Morris Kharasch.

GORTLER: They made an offer that you couldn't refuse.

WALLING: Since I was restive where I was, I figured that if I were going to be an industrial chemist I might as well be an industrial chemist with a little more power and a little more money. Although I didn't do much science when I was at Lever Brothers, I found it quite an interesting experience and I learned quite a lot about chemical technology. I don't regret having spent some time there at all.

GORTLER: This was in Cambridge. What was Lever Brothers interested in?

WALLING: I was essentially in charge of their organic research. We were working mostly on new synthetic detergents and formulations. Lever Brothers was also in the hair wave business, so we developed an odorless hair waving agent.

Reduced hair smells a little bit anyhow, so our product didn't entirely work. I essentially worked on the development of new products and processes. I published three papers while there.

GORTLER: Can you tell me a little bit about your move to Columbia? I think that you were recruited by Hammett?

WALLING: Yes.

GORTLER: How did that come about?

WALLING: I was fortunate. I think that I tried to apply his equations to radical reactions and that convinced him that I was a profound thinker. I guess that I first met him in 1946 at the Mechanisms Conference. I may have met him before, but I'm not sure. I talked to him about an academic job before I went to Lever Brothers, so he knew that I was interested. After some vacancies developed, I think, in all honesty, that he offered a job to Jack Roberts who declined the offer. Hammett then offered the job to me. In fact, I know that. I always felt that Jack's wife, who wanted to go back to California, did me a really good turn.

GORTLER: Roberts was at MIT at that time?

WALLING: Yes.

GORTLER: You remained sufficiently interested about an academic position even though two or three years had elapsed after your discussion with Hammett. You therefore made the change. Louis Hammett was chairman at that time?

WALLING: Right.

GORTLER: Tell me a little bit about the rest of the staff at Columbia.

WALLING: A gap had just developed in organic chemistry because Bill Doering, Dave Curtin, and [Robert] Elderfield had left. Charlie Dawson and a young instructor named [Harold] Conroy were the only organic chemists on the staff at that time. Very shortly afterwards Columbia recruited another young man, Leighton McCoy. At the same time that they offered me a position, they also offered one to Gil Stork. He didn't actually join the faculty until the middle of the next year. The next organic chemist they hired was Ron Breslow. At that point everything was fine.

GORTLER: I don't know if Hammett was still chairman when Breslow was hired. Hammett told me something about the extra money that they used to pay Breslow during his first year at Columbia as a research instructor.

WALLING: Yes. I guess his title was instructor. He wasn't on the regular university budget. They got ten thousand dollars a year from Du Pont. So, by paying him half of that we were able to get him to come. Five thousand dollars was about the starting salary at that time.

GORTLER: Best bargain they got in a long time.

WALLING: Five thousand dollars was a darn good salary at that time. I went to Columbia as a full professor at nine thousand dollars.

GORTLER: It must have been a comedown from your salary at Lever Brothers.

WALLING: I was getting twelve thousand and five hundred dollars at Lever Brothers.

GORTLER: Oh. I figured that you were in the fifty thousand dollar bracket.

WALLING: No. That twelve thousand and five hundred dollars was a lot of money. Very few people in industry were getting double the starting salaries in academia.

GORTLER: Right.

WALLING: At that time, the industrial starting salary was about six thousand dollars.

GORTLER: You did some work with George Fraenkel.

WALLING: He came to Columbia about the same time that I did. It may have been the previous year. He was working on ESR. We did a couple of short cooperative projects together.\*

GORTLER: That was really a very early entrance of an organic chemist into that area.

WALLING: Yes. He had one of the first really sensitive machines. You had to build your own machine in those days. I wanted to see the radicals in a gelled polymer system, and it turned out that I could see them pretty well.

GORTLER: How did you start building a research group? How did you go about choosing the graduate students?

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\*Cheves Walling, Jack M. Hirshon, and George K. Fraenkel, "Detection of Polymerization Radicals by Paramagnetic Resonance," Journal of the American Chemical Society, 76 (1954): 3606.

WALLING: I took the students I could get, of course.

GORTLER: Did they come around and see each of the instructors?

WALLING: Yes. I got a little money to hire a post-doc to get myself started. I got a Chinese student named [Yu-wei] Chang who was a very careful experimentalist. He eventually took a job with Du Pont. I got the first student, Shelly Buckler, rather promptly because he was ready to start research. He is now well up in the Polaroid Corporation. After that, I just slowly accumulated them.

GORTLER: Would you care to compare graduate students from that day with graduate students of today?

WALLING: Well, as many other people have said, I think that students probably expected to work harder in the late '50s than in more recent times. Students recruited at Columbia tended to be very hard working. They came mostly from the northeast, from families on the way up. Many came from the New York Jewish community. I don't know if they were any smarter than they are now.

GORTLER: You were chairman at Columbia from...

WALLING: '63 to '66 I guess. Columbia's chemistry department has a rotating chairmanship.

GORTLER: Was there anything that you felt characterized your chairmanship? Any high points?

WALLING: We won some and we lost some. I lost Harry Gray and Martin Karplus but I hired Nick Turro and George Flynn. They did pretty well.

GORTLER: Did you have any particular philosophy about leading the chemistry department at that time?

WALLING: That was the period during which we were approaching the peak of the boom of government support. I used to tell my colleagues that it was a good idea to continue to be nice to industrial visitors because at some time in the future they might not be so eager to recruit students. Government support wasn't going to expand forever.

GORTLER: You did some consulting after you entered academia.

WALLING: I've done quite a lot.

GORTLER: For whom did you consult?

WALLING: I consulted for Lever Brothers and for Celanese for a long time. At one time or another I also consulted for Union Carbide, Chevron, Sun Oil Company, and two or three other companies.

GORTLER: Did you think that that had any influence on your academic research? Did it support any of it?

WALLING: Sometimes I got very small pieces of support out of it, but never very much. I don't know that I ever did much of any research at the university that was really closely connected with my consulting. I'm sorry that industry isn't making as much use of consultants as previously. They always worry that there isn't enough mutual understanding between the academic world and industry. Employing consultants, however, is one of the cheapest and easiest ways to give people some ideas of what the problems are and of doing something about it.

GORTLER: So you think they're cutting back on it?

WALLING: Well, they certainly did cut back on it enormously in the early '70s. I'm not quite sure whether they've come back part of the way or not.

GORTLER: Let's consider now your move to Utah. You comment a little bit about it in the various journals that asked you about it.

WALLING: Well, I wanted a change. I got tired of thinking about spending the rest of my life commuting into New York from Montclair, New Jersey. My wife didn't want to move nearer to New York. Doing that didn't appeal very much to me either. The troubles at Columbia didn't improve my morale although I felt the chemistry department behaved quite sensibly.

GORTLER: These were the days of student protests in the late '60s.

WALLING: Yes. I became convinced that Columbia really didn't know very well what it was doing when it got itself involved with the business of the cigarette filter. You might remember that affair.

GORTLER: Vaguely.

WALLING: Well, anyhow, some chump at the medical school allowed himself to get persuaded that a promoter had developed a marvelously effective cigarette filter. You could make a perfectly good argument that, if somebody had done this, it

might be a good idea to have people who can't stop smoking, smoke more effectively filtered cigarettes. I went to a meeting where they announced this supposedly effective filter to the press. I then called up someone in the administration and said, "Do you really know anything about cigarette filters? You may be needing some technical advice." In a few weeks I got a desperate phone call from someone high up in the administration. I then became involved in getting them extricated from the project. There's nothing like a little straight-forward applied physical chemistry.

GORTLER: What year did you take over as Editor of the Journal of The American Chemical Society?

WALLING: In 1975. I've done it for five years now.

GORTLER: What prompted you to do that?

WALLING: I'd just left a couple of committees and Bryce Crawford called me about it at a time when I was feeling somewhat susceptible. I'd just finished being the chairman of the Committee on Professional Training of the ACS and a couple of other things. I'd always been interested in the enterprise.

GORTLER: Has it taken a great deal of your time?

WALLING: Yes. Quite a lot. I don't regret it. I told them I'd do it for two more years. I'll be sixty-five and I'll see what I want to do then.

GORTLER: What have been your most satisfying contributions to chemistry?

WALLING: My book, Free Radicals in Solution, was most satisfying because it had quite an impact. It appeared at the right time. Additionally, during my many years of consulting, I was once involved with one very successful product.

GORTLER: What was that?

WALLING: Celanese makes a polyformaldehyde that is a plastic named Celcon and my name is on their basic patent. What particularly pleased me about that was that the idea came up at a discussion about how to solve the problem, and I went home and carefully wrote Celanese a letter about it. This gave them a nice early date of conception and also established who had done it.

I've had many students who have gone out and done things successfully. That's been a satisfaction too.

GORTLER: Hammett has suggested that there was a gap in the

development of physical organic chemistry. Around the turn of the century, Lapworth was already starting to do kinetics and people like Stieglitz and Nef and Arthur Michael were talking about mechanisms. Yet, there was a sort of gap, at least in Hammett's eyes, between the early 1900s and the mid-1920s. During that period of time, people started getting interested again in organic mechanisms. Can you think of anything that might have stimulated the interest of more people?

WALLING: I never thought of it as being a gap, but rather as a slow progression that in the end somehow eventually culminated in someone's making enough useful observations to finally develop something of a critical mass. When enough little pieces turned up, you could put it together. The idea of a carbonium ion as an intermediate was suggested about 1920 in connection with the Meerwein terpene rearrangement. It took a while, of course. And then there was this rather sharp dichotomy between physical chemists who actually measured kinetics and did things like that, and organic chemists who were reared to think of organic chemistry as an art. Rather late in his career, Roger Adams was quoted as saying, "No useful thing had ever come out of physical organic chemistry; no useful new reaction had ever been discovered this way." I think, of course, that this is no longer true.

GORTLER: One could hardly put a date on things like this, but how would you characterize the change in emphasis from classical organic chemistry to today's more integrated approach, which is a combination of physical organic plus classical organic.

WALLING: Well, it moved down slowly from the graduate schools into undergraduate teaching. Do you want to know when it occurred or do you wish to know why it occurred?

GORTLER: Both of those questions.

WALLING: Well, it really occurred in the late '40s and the '50s. In the early '50s when you taught a graduate course in physical organic chemistry, this was all new to the students. They simply had learned a good deal of descriptive chemistry and because of that it was a marvelous time to teach physical organic chemistry. It's much less satisfying to teach it now because the students have been getting watered down versions of it ever since they started college. Consequently, they've gotten bored with it.

GORTLER: Yes. The graduate course just adds more detail. A few years ago when I was starting this project, I talked with Phil Skell and he said that he felt that physical organic chemistry was dead and that those who were practicing it were merely adding more digits after the decimal point to already

well known numbers. Do you have any thoughts about the state of physical organic chemistry today?

WALLING: Much of it is fairly dead. The techniques are certainly absolutely fundamental to all of the work on enzyme reactions and things of this sort, indeed, to all of that area of biological chemistry. The application of these principles and methods in organometallic chemistry is really just getting started and becoming quite active. Photochemistry is probably a little over the peak but there are still some interesting things to do with it. Many of the classical problems are sort of exhausted but...

GORTLER: I have the impression that they're just beginning to expand their horizons.

WALLING: Like any other field of chemistry, physical organic chemistry has been a fad and the peak of publication is sometime after the peak of interest.

GORTLER: Where do you think physical organic chemistry, in particular, or organic chemistry, in general, are headed?

WALLING: Biological chemistry is becoming an active field of research. I don't know where else physical organic chemistry is headed. I've been to various inspirational meetings. I went to a workshop on physical organic chemistry that the NSF sponsored two years ago. Everybody sat around and talked, telling each other how great everything was. Yet, there's still quite a lot to do. There are a lot of things that aren't understood and there are lots of interesting phenomena to investigate. Much less is known about oxidation-reduction processes than is known about solvolysis and displacement, for instance. When you hear about how complicated the oxidation-reduction processes are in photochemistry--which is what I've been listening to at this meeting\*--there is obviously a lot of very interesting stuff still coming out. Furthermore, as a basic approach to really technical problems-- that is, to try to improve processes--physical organic chemistry is one of the most powerful tools available. There is the whole area of heterogeneous reactions that is still quite mysterious. Those are the major problems.

GORTLER: There are just a few other questions. I had told you that I was going to ask about your children. According to American Men of Science, you have five.

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\*178th National Meeting of the American Chemical Society, Washington, D.C. (September, 1979).

WALLING: I have five children, none of whom have gone into science or have shown any particular interest in it. The nearest approach to science occurred when my second daughter married a chemistry major. Today, he is a technical salesman for IBM.

GORTLER: I see. Maybe you had a negative influence.

WALLING: I didn't have a positive influence on them.

GORTLER: I'm considering doing a Benchmark Papers book. Perhaps this is not the time to list them, but if you have any all time favorites in physical organic chemistry I would appreciate your letting me know.

WALLING: Both of Hammett's papers on the Hammett equations-- both the acidity function and the other one. One or two of the early Winstein papers. Certainly George Hammond's paper, the first one on...

GORTLER: That's the energy paper.

WALLING: Yes. Singlet-triplet reactions of benzophene.

GORTLER: I was thinking of another one.

WALLING: Many people would consider his paper on the Hammond postulate to be more fundamental than I do. I think otherwise.

GORTLER: Yes. It was a statement of something everyone believed.

WALLING: It's certainly been quoted though.

GORTLER: You know most of the people to whom I've talked so far. Are there others to whom you think it might be useful for me to talk about the early development of physical organic chemistry.

WALLING: I don't know.

GORTLER: It sounds like talking to Frank Mayo is probably a very good idea.

WALLING: He could tell you something about the politics at Chicago. There is all of the work of the Winstein group. Ernie Grunwald might be able to tell you something about that because he was one of Winstein's earlier students. Speak with Jack Roberts.

GORTLER: I've been in contact with Jack Roberts. Earlier you had mentioned Saul Cohen. I hope to talk to him.

WALLING: There are very few people who got into it before I did and are still around.

GORTLER: Thank you very much. I appreciate the time that you've spent with me.

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