THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

FRANK R. MAYO

Transcript of an Interview
Conducted by

Leon B. Gortler

at

SRI International

on

21 January 1981

ELEANOR P. MAYO FOR THE ESTATE OF THE LATE FRANK R. MAYO

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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(Revised 20 February 1989)

Frank R. Mayo, interview by Leon B. Gortler at SRI International, Stanford, California, 21 January 1981 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0031).

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

FRANK REA MAYO

1908	Born in Chicago, Illinois on 23 June
1987	Died at Menlo Park, California on 30 October

Education

1929	B.S., chemistry, University of Chicago
1931	Ph.D., chemistry, University of Chicago (Mentor: Morris Kharasch)

Professional Experience

1931-1932	Lilly Fellow, University of Chicago (with Morris Kharasch)
1933-1935	Research Chemist, E. I. du Pont de Nemours & Co.
1936-1942	Instructor, University of Chicago
1942-1950	Group Leader, Research Chemist, U.S. Rubber Co.
1950-1956	Research Associate, General Electric Research Laboratory
1956-	Scientific Fellow, SRI International

Awards

1967	Award in Polymer Chemistry, American Chemical Society

ABSTRACT

In this interview Dr. Frank Mayo first discusses his educational career as an undergraduate and graduate student at the University of Chicago. He then traces his professional career as a research chemist with Du Pont, as an instructor at the University of Chicago where his primary role was the supervision of Morris Kharasch's research group, as a group leader at U.S. Rubber during and after World War II, as a research associate at General Electric, and finally as a fellow at SRI International. He discusses his closest associates, his scientific work, and comments on the rise of free radical chemistry and the value of applying basic research to practical problems.

INTERVIEWER

Leon Gortler holds an A.B. and M.S. (chemistry) from the University of Chicago and a Ph.D. in chemistry from Harvard University. He has been on the faculty of Brooklyn College since 1962, where he is currently Professor of Chemistry. This interview was conducted as part of Professor Gortler's long range study of the development of physical organic chemistry in the United States.

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PLACE: SRI International, Stanford, California

DATE: 21 January 1981

GORTLER: Is there any teaching that goes on here at SRI?

MAYO: No. One or two people have part time teaching appointments at Stanford, but we do no teaching formally and award no degrees.

GORTLER: Do postdocs come in and work?

MAYO: Yes. Much of the work in chemistry is done by postdocs. The permanent staff members write the proposals and manage the programs and at least check the reports, but they're too expensive to use for something for which we can find somebody less expensive.

GORTLER: I told you that I wanted to get a little information about your background. I know you were born in Chicago on June 23, 1908, but I know absolutely nothing else about it.

MAYO: Both my parents were school teachers. My mother was in an elementary school and my father was eventually a principal.

GORTLER: Can you give me their names?

MAYO: My father was Frank and my mother was Clara Rea, which is my middle name. That part of Chicago where I was born and grew up was, at that time, an area of vacant lots and scattered buildings.

GORTLER: What part of Chicago was that?

MAYO: The South side, on seventy-fourth street, near the lake.

GORTLER: I see. Actually, not too far from the University of Chicago?

MAYO: That's right, three or four miles.

GORTLER: What kind of influence did your father have on you?

MAYO: Well, my parents assumed from the beginning that I was going to college. They didn't try to influence me in what direction I should go. I guess my father was good at figures. My mother was quite a scholar, in a large number of areas, although she never got beyond what was then called Normal School.

GORTLER: Do you have any brothers and sisters?

MAYO: One brother who is now retired. He was a professor of English at Northwestern until quite recently. He's two years younger.

GORTLER: So the family financial position was such that you were not wanting?

MAYO: That's right. We were secure but careful.

GORTLER: In the school system were there any influential teachers that you had at any given time, or were there any books that you were reading that you felt were particularly influential, before you went to college?

MAYO: Well, I had some good teachers and some that were not so good. I guess perhaps the most influential thing was an old book of my father's that probably dated to the nineties. It was Newth's Inorganic_Chemistry (1). It was about 8 cm thick. In odd moments, I just used to look through it. I got used to the lingo, and to what was going on, before I got very far into high school.

I took a course in chemistry in high school, and of course it was very easy for me. Then, as I was leaving high school, the University of Chicago gave a competitive examination for high school seniors in the area. One of these was in chemistry and I got the prize, which was a year's tuition at the University of Chicago. Before that I had been undecided as to what aspect of science or engineering I would go into, but when I got this scholarship, I said, "Well, I might as well start with chemistry," and I stuck with it.

GORTLER: So you didn't consider any other institutions at that time?

MAYO: Well, I was considering what was then Armour Institute of Technology and the University of Illinois, but the scholarship at Chicago settled it. I commuted all the way through undergraduate and graduate school.

GORTLER: Beyond the first year, how were you supported in college?

MAYO: Well, I got more scholarships. In fact, I think I was awarded scholarships to continue for most, if not all, of the time I was there.

GORTLER: Now, can you tell me something about life as an undergraduate at the University of Chicago? What was the department like? What was the college like at that time? I was a student there some twenty-five years later. I'm sure things had changed.

MAYO: Well, [Hermann] Schlesinger gave the general chemistry lectures, assisted by Professor [Adeline DeSale] Link, and I think they were both quite good. [Julius] Stieglitz was the chairman of the department, and he gave the advanced organic chemistry course. It was a very well arranged course, but in retrospect, it was ten to fifteen years behind the times. In other words, I was taking it in the late twenties, and I think he stopped activity in chemistry considerably before that. That was good sound material, but I think out of date.

GORTLER: That's interesting. Stieglitz was always considered one of the forward looking people.

MAYO: Well, he was a pioneer in physical organic chemistry. I think he did the first of it in the nineties. But I would guess that after the First World War his administrative and perhaps other duties kept him from keeping up very well. He was not obsolete. He was still better than many other physical organic chemists, but I think that by the time I finished in 1929 the course was out of date. The other major professor was [William Draper] Harkins, who was, I think, an able research man, but a terrible lecturer.

GORTLER: He was the physical chemist?

MAYO: He was the chief physical chemist. (There were about three junior professors.) I remember asking him, in a course I was taking from him, "What is entropy?" He'd been talking about it. He said, "Well, you just stick around and you'll understand it eventually." I have a working knowledge of it now.

GORTLER: Yes, I think that's the way people come to entropy eventually, by working at it.

MAYO: I found chemistry in general to be quite easy. I had quantitative analysis with W. Albert Noyes, Jr., who just died. I found the courses with him very easy.

GORTLER: He was on the staff at Chicago?

MAYO: Yes. He left in the late twenties for Brown University.

GORTLER: Eventually, he went to Rochester.

MAYO: Brown, Rochester, and Texas.

GORTLER: What did Noyes teach?

MAYO: Well, he taught me quantitative analysis and physical chemistry.

GORTLER: Who taught the undergraduate course in organic chemistry?

MAYO: At first it was Ben Nicolet, who was at least middle aged, and J. W. E. Glattfeld. Before I graduated Nicolet left the department for a job in government. Glattfeld, a sugar chemist of [John Ulric] Nef's, stayed to retirement.

GORTLER: Do you remember any of the textbooks that you used at that time? Who might have written the organic text?

MAYO: I guess I tossed it out. I don't remember the book. Stieglitz didn't have any book for organic chemistry. His own book was used for inorganic analysis (2). I don't think that Harkins used a book in advanced physical chemistry. He just showed up and talked about what was on his mind, often his own specialties. [Morris] Kharasch came about 1928 and I took some courses with him in the last year there, which was 1928-29. Let me see, the other organic chemist, Glattfeld, did a lot of the undergraduate teaching. I'm not sure that I had much, if any, with him.

GORTLER: Did you do any research at that time?

MAYO: Not at the undergraduate level.

GORTLER: Did any of the other undergraduates that you remember go into chemistry?

MAYO: Well, several went into chemistry. I remember some of their names. I don't know that any of them reached prominent positions. At least, I haven't heard from any of them since.

GORTLER: I see. Who were a couple of the people?

MAYO: Well, I remember some of the people I saw the most. There was Solomon, Lowenstein, and [Arthur E.] Siehrs. I guess Siehrs eventually got a Ph.D. at Northwestern. A man I remember better and have kept up with, who was a year ahead of me, was Albert W. Meyer. He was an inorganic student, but we ended up working together at U.S. Rubber. I still keep up with him. He was one of the good students.

GORTLER: Do you remember what the concept of the atom was at that time?

MAYO: Yes. We had an introductory dose of quantum mechanics, and the Bohr atom, which I didn't absorb very well and haven't used. I had a course related to this in the physics department. I think it was probably a course in spectroscopy; a lecture course taught by Henry Gordon Gale, who was also Dean of Sciences. He was a terrible lecturer too. I think the spectroscopy inevitably led into quantum mechanics, but it didn't register very well.

GORTLER: How about math?

MAYO: I got through two quarters of calculus creditably. That's all.

GORTLER: Considering your facility with math later on...

MAYO: ...well, if it's facility, it's with the simplest part of it, differentiation and integration in rate equations.

GORTLER: Do you remember any courses outside of science that were particularly influential or appealed to you in any way?

MAYO: Well, I had some courses in education. I was headed toward high school teaching. However, a month or two before graduation, Stieglitz called me in and asked me what my plans were, and wouldn't I like to apply for a fellowship to do graduate work at Chicago? I hadn't considered it, but I decided I would. Kharasch had some money and would be glad to have me, and so we made a deal, and the high school teaching disappeared.

I thought that I had lived off my parents long enough, and I ought to get out and get to work. I had enjoyed my high school career, and thought I'd go back to it. But I was fortunate to have had the offer, and things have turned out very well.

As one of the better freshmen, I was invited to take a course at the University on the "Nature of the World of Man," a survey course that ran for three quarters. They had leaders in many fields of science come around and talk for a week or so apiece, and then we would discuss it. The first two quarters covered physical and biological sciences and were pretty good. The third quarter was a course called "Reflective Thinking" in the philosophy department that was pretty much of a total loss. Some of the other students would have animated discussions with the professor. I couldn't figure out either what they were saying or whether it was worth following.

GORTLER: I understand that.

MAYO: I have some blind spots.

GORTLER: Because of this sudden offer, you really didn't consider going to any other graduate school?

MAYO: That's right.

GORTLER: Did Kharasch support you?

MAYO: Yes, he supported me all the way through my graduate work and for one year thereafter.

GORTLER: That was an Eli Lilly fellowship. Did you take any further courses in graduate school?

MAYO: Oh yes. There were several special topics courses and there were required courses in inorganic chemistry.

GORTLER: I suppose Schlesinger taught the inorganic chemistry?

MAYO: I think he may have, although Warren Johnson was there also. Anton Burg was active in inorganic chemistry at Chicago during most of my career there. I guess he was a year or so ahead of me. Does the name register?

GORTLER: Yes. I just came across it the other day. He went to the West Coast.

MAYO: He was at USC and is retired now.

GORTLER: Then you already had the advanced organic course. I asked you before about the atom and about bonding theory.

MAYO: Well, we got a great deal of [G. N.] Lewis and I think the textbook for organic chemistry in the middle thirties was Lucas (3). I figured I had enough of it to get by. In fact, [George] Wheland came to Chicago as an instructor about 1936 and gave a course in quantum mechanics. I asked him if I should take it. He said, "You'll get along all right without it," and so I didn't take it.

GORTLER: And you have.

MAYO: Some of the language is still a little foreign to me, but it doesn't bother me.

GORTLER: At this point you're about to work with Kharasch. How did you decide on the research problem?

MAYO: He decided that.

GORTLER: He told you? Did you begin by working on the HBr addition to allyl bromide?

MAYO: No, we started on 2-pentene. A couple of his previous students had worked on that, with contradictory or uncertain results. They were able to get, they thought, either 2-bromopentane or 3-bromopentane, depending on the conditions of the reaction. My job was to go through this work and see if I could find any difference between the 2-pentenes made from 2-bromopentane and 3-bromopentane. It turned out there was none. Or I couldn't find any, and I went through a lot of fractions and monkey business. Kharasch still wasn't wholly satisfied, because this was critical to his theory of electromers at the time, the idea that these 2-pentenes would be electronic isomers alike in every other respect.

And so [Cheves] Walling was put on this problem later, and that's the work that was published (4). He eventually found the same thing that I did, although he extended it to cis and trans.

GORTLER: At what point was it fairly clear to you that the peroxide was the problem?

MAYO: After I got my degree. In other words, after I'd done the required nine quarters.

GORTLER: That's how one satisfied the requirement for the Ph.D. degree?

MAYO: Well, that was the minimum requirement, and Kharasch told me I could get my degree if I would stay for another year and continue to work. So I did, and the thesis says that the effect was due to moisture, but a few experiments after that showed that it was indeed air and oxygen. He suggested putting in antioxidants and peroxides, and that clinched it.

GORTLER: What kind of a research director was Kharasch? You had a very long relationship with him, but how about at the beginning, and did it change as time went on?

MAYO: Well, he was a bright guy and I got along fine with him after making allowances for the fact that he was something of a prima donna. He was good enough to get away with it. We had a very pleasant relationship and very little conflict.

One of the things that used to irritate me was that I would say I needed to talk to him. He would say, "Come in and see me tomorrow." The next day he had gone to Wilmington or Indianapolis or some place.

GORTLER: He was already doing a lot of consulting at that point?

MAYO: Yes. He was well into consulting before he came to Chicago, mostly with Lilly and Du Pont, but also with a lot of other companies.

GORTLER: He had a very close relationship with industry over a long time.

MAYO: Well, I think the reason is, he was well worth his keep.

GORTLER: I see. We'll get back to Kharasch and your later relationship with him. Who were the other graduate students in the group at the time, or in the department, that you had close relationships with or talked to?

MAYO: One who worked in the same lab with me was Bruce J. Miller, who was at a small college but eventually ended up with Union Carbide and died in retirement in Florida. Another was Adeline Bloodgood, who got into teaching, and died in middle age. These people worked in the same room. Another who worked in the same room was Jules D. Porsche. He ended up, I think, as a director of research for Armour, and then as a consultant, and he's still around.

I have heard little or nothing from the other students since. One I have heard from was Thomas Beck, who worked for the Victor Chemical Company in Chicago Heights until he retired. Now, let's see. One of Kharasch's part-time students was Herman Pines. Is that name familiar?

GORTLER: Yes, Herman Pines did hydrogenation work later on at Northwestern.

MAYO: And catalysis at UOP [Universal Oil Products Company]. I think that Herman Block overlapped me somewhat. Do you know that name?

GORTLER: No.

MAYO: Well, he was a director of the ACS for many years, and I think he got an award in catalysis a few years ago. He got pretty well up in the management of UOP. I didn't see very much of these people. I tended to stick pretty close to business while I was there in the daytime, and then go home and stay there for the evening. Let me see--I think I'll quit there. Kharasch didn't have as large a crew then as he did later.

GORTLER: Whom did you talk to about chemistry?

MAYO: Whom did I talk to about chemistry? Not very much with anybody but Kharasch, except, to some extent, with the people who were working with me. Mostly we tended to our own business and tried to turn out the work.

GORTLER: You people turned out an enormous amount of work.

MAYO: We did have seminars. I remember Linus Pauling was there at one of them, and quite a lively young guy at the time, as you might guess. I have a rather clear mental picture of him at the end of his lecture, laid out like this on one of the tables in the seminar room.

GORTLER: Sounds like something he might do. What kind of a chemist did you consider yourself when you finished? Were you an organic chemist?

MAYO: Physical organic.

GORTLER: Now, I don't think the term "physical organic" even existed at that time.

MAYO: Well, I got my degree in organic chemistry. But you can see what kind it was.

GORTLER: Sure, you were already doing what became very classical physical organic work. When you finished at Chicago and you finished that fellowship year, you went to Du Pont.

MAYO: Well, when I finished the third year with Kharasch, there wasn't a very good selection of jobs.

GORTLER: That's right, it wasn't a very good time to be looking for a job.

MAYO: It was the summer of 1932. I had one offer, which wasn't particularly attractive, to go into the dye business in Milwaukee. Kharasch offered me an opportunity to collaborate with him on a book that he was going to write during that one year, and I did. This is where I spent most of the time, looking through the literature and trying to organize some of it. This made up for a lot of the reading I didn't do as a graduate student.

GORTLER: What kind of a book was it?

MAYO: It was to be a book on his ideas of organic chemistry, perhaps based more on electromers than on resonance. But of course, things changed after that, and the result was that hardly one chapter really got written. But, I developed a very large card file on the things that he was most interested in.

GORTLER: That served you well later on. And then?

MAYO: Then I got an offer from Du Pont and took it. I think I got it on his recommendation. I was in the first group of six or eight organic chemists to come in since the Depression started. The old timers were glad to see us--that was their security. At one time there had been some cutbacks.

GORTLER: What kind of work did you do at Jackson Labs?

MAYO: The Jackson Laboratory is across the river from Wilmington. My job was on rubber vulcanization accelerators exclusively. I got exposed to antioxidants and to the neoprene business, which was going on in the same building, and the lingo and procedures and testing in the rubber business.

GORTLER: You were picking up all the proper background for your later work.

MAYO: So it turned out. This work gave me a head start on a good many other people in the polymer business.

GORTLER: You also did one piece of independent research in the reduction of the pyridine ring.

MAYO: Yes. When I got to Du Pont, it had been determined that some quaternary ammonium salts were quite good vulcanization accelerators. It was suggested that I start looking at a large number of quaternary ammonium salts. I was trying to make methylpyridinium chloride and dimethylpiperidinium chloride, and tried methyl formate with pyridine and so on. This paper is what came out of it (5). That it was published showed that the management didn't have much use for it.

[END OF TAPE 1, SIDE 1]

GORTLER: You were married in 1933.

MAYO: Yes.

GORTLER: Can you tell me just a few things about your wife?

MAYO: Her name was Eleanor Pope. I met her at a summer resort, when she was sixteen and I was twenty, and it stuck. We're still doing pretty well. She finally finished school about the time I started at Du Pont. I didn't have any money and I wasn't ready to get married.

GORTLER: When you say finished school...

MAYO: She finished her B.A. at Illinois, about the time I went to Du Pont. She would have liked to get married right away, but I hadn't any money. Times have changed some--now all you need is a credit card, I guess--but we were married the following Christmas, after six months. I still didn't have very much money in reserve, but...

GORTLER: ...but you managed.

MAYO: We managed well.

GORTLER: Has she had any influence on your career? She's not a chemist, I take it?

MAYO: She is not a chemist and has no interest in it. She's interested in some of the personalities in it.

GORTLER: Maybe I should talk to her.

MAYO: Many of them have visited us at home, both from this country and abroad. She has been cooperative in the entertaining and has done a real good job of raising our two daughters.

GORTLER: Were your daughters influenced at all by your scientific career?

MAYO: No. One of them thought she was pretty good at mathematics, but when she got to Stanford and found she was competing with the engineers, she decided she wasn't so hot after all. She went into speech therapy and teaching.

Neither my wife nor my two daughters showed any interest whatsoever, nor have any of them gone to Chicago. But interestingly enough, we have two granddaughters. One of them is a junior there now, and the other one is going to apply there, as well as at some other places. So, after skipping a generation, we may have one or two back there.

The granddaughter that's at Chicago now is interested in economics and politics. The other is interested in science or mathematics or medicine. Both are quite good students.

GORTLER: These are daughters of your oldest daughter?

MAYO: Yes. My younger daughter has two sons who haven't picked a career yet.

GORTLER: What is your older daughter's name?

MAYO: Carolyn, with a "y".

GORTLER: And the younger one?

MAYO: Her name is Jean, with four letters.

GORTLER: When did you return to Chicago?

MAYO: January, 1936.

GORTLER: What was the occasion of your leaving Du Pont and coming back to Chicago?

MAYO: Well, Kharasch made me an offer to be his research assistant. He had asked me a year sooner, I think, but I thought I ought to give Du Pont a better try. Then the man he got apparently left, and he tried me again, and I decided that I was ready to go. While Du Pont wasn't a bad place, it wasn't anything that appealed to me on a long term basis.

So I went back with a title of Instructor, and my chief job was to service his crew of students and postdocs. I guess there were twenty or more of them. Some of them got a fair amount of attention from him, but many of them did not. He was away a good deal of the time on consulting and committee work and other sorts of things.

GORTLER: When you were a student there, did he have that kind of arrangement, or weren't there enough students and postdocs yet?

MAYO: No, there weren't enough. I guess he didn't have enough money or influence. He'd just arrived from Maryland.

GORTLER: I know what I forgot to ask you. Do you remember what were you making when you first went to Du Pont?

MAYO: Two thousand four hundred dollars.

GORTLER: Everybody seems to remember their first salary. How much did Kharasch offer you to go back?

MAYO: Two thousand four hundred dollars.

GORTLER: I see.

MAYO: Well, I had had a raise at Du Pont in the meantime, but not very much.

GORTLER: Who was the chairman at Chicago at the time you came back?

MAYO: I think it was Schlesinger. Stieglitz was retired, but he had been drafted to continue his organic chemistry course. I don't know whether Schlesinger had the title of head or chairman of the executive committee or what, but for practical purposes, he was running things.

GORTLER: Jim Parsons eventually became executive secretary. Was he around at that time, or wasn't that until much later?

MAYO: That was later. He may have been a student some time while I was at Chicago, but I think he was at one of the city colleges. Is that correct?

GORTLER: I think you are right. I was going to go down and talk to him some time if I could still find him. He was at Brooklyn College after he retired from Chicago.

I guess you've already told me pretty much how the Kharasch group was run by that time. I think something has crossed your mind and I'd like to hear what it was?

MAYO: Well, I used to go around and encourage them [the students] to clean up. Walling resented this, but he told me much later that he does that to his students.

GORTLER: In general, how was the group run? Kharasch would talk to some people but you were sort of in charge of the group?

MAYO: I kept things moving between the times that he talked to them. I had a good deal to suggest about how they worked on the goals that he had set.

GORTLER: So, he set the problems?

MAYO: He set the problems. I did much of the execution and much of the writing up, at least in the first draft.

GORTLER: I've heard some rumors about the relationships between the people at the University of Illinois and the Kharasch people or the Chicago people. Do you remember much about that?

MAYO: Yes. My wife lived in and worked around the house for George L. Clark in her last year at the University of Illinois. Does the name register with you? He was the X-ray expert down there. She met some of his staff, and on one occasion Roger Adams and his wife drove her up to Chicago for a holiday or something, and the question of Kharasch came up. Roger told her in no uncertain terms that I would never get anywhere as long as I was associated with Kharasch.

GORTLER: I suppose we're all allowed some mistakes.

MAYO: Well, the situation, I think, was this. Kharasch was a loner. He didn't have good relations with most of his contemporaries, partly because he didn't care, I think, and partly because he wasn't inclined to play ball and politics with them. So, he got very few students from the leading chemistry teachers in the country. They came in from the smaller places, or people who had a special connection, or something. Walling came in because he had already been through Harvard. His father was a Chicago alumnus. Another one came in because he was well off financially and his father was in the chemical business. Those were probably among the two smartest people.

GORTLER: Who was the other fellow? I know Walling.

MAYO: Ernest May, and he was with the May Chemical in New Jersey for many years. He was a real bright guy too.

Now, these are the students I supervised for Kharasch. Another very bright one was James A. Norton, who was sort of an oddball, and didn't get along very well, either in industry or in academic work. He finally ended up working for the Champion Spark Plug division of General Motors. He was something of a boy wonder. I think he started at about sixteen, something like that.

There were some other pretty good ones, but quite a number were pretty ordinary. I think that in general the average of Kharasch's students wasn't as good as it was in many other universities at the time.

GORTLER: I see. So he just had people working through simple problems?

MAYO: And they had my attention. There's another one who did fairly well--Ralph L. Dannley. Does that name register?

GORTLER: No, I'm afraid not.

MAYO: He was one of Kharasch's students. He went to Western Reserve, and worked on some aspects of free radicals until he retired. Herb Brown was a postdoc for Kharasch. I guess I don't need to say anything about him. Brown was a postdoc while I was an instructor there. He took a course or two from me as a graduate student. William B. Reynolds became research vice president for General Foods; he was good.

GORTLER: What was your relationship to James Senior?

MAYO: Well, he was an interesting character, and we were on good terms. I liked to listen to him. He perhaps did more for me in learning how to write a chemical paper than the whole English department did at Chicago.

GORTLER: Westheimer said exactly the same thing to me.

MAYO: When Walling and I wrote this review on the peroxide effect...

GORTLER: ...he [Senior] is acknowledged at the end.

MAYO: Senior went over it with me paragraph by paragraph, and this was the best instruction I guess I ever had. Maybe it isn't enough, but it's the best I had (6).

GORTLER: What was his role at Chicago?

MAYO: He was something like a research associate. He was privately well off, and just preferred to amuse himself at Chicago, rather than taking a regular job somewhere. He got into the workings of the department and ran the library. He gave a course occasionally and he liked to talk to the other faculty, usually in a very loud voice, with great confidence.

GORTLER: That's good. He certainly served a great purpose if he taught both you and Frank Westheimer how to write.

MAYO: Well, whatever it is.

GORTLER: One of the next papers you checked off was a very short paper that ended up in <u>Chemistry and Industry</u> on the addition of HBr to butadiene. You felt that this was one of your more significant papers (7).

MAYO: Well, this was a preview of another paper, and of course Kharasch originated the problem. At that time [William G.] Young and Saul Winstein had been studying allylic rearrangements, starting out with allylic bromides. This came to our attention rather forcefully when we started looking at the addition of HBr to butadiene. What I think is important was that this showed that the whole business of addition of reagents to dienes was a real mess, and had never been done right.

GORTLER: I was a little bothered by the ionic product. For some reason, it turned out to be the 2-bromo-2-butene, and I was wondering whether that was a mistake in the article.

MAYO: Well, I can check the reprint.

GORTLER: So that was an error, and as it appears in all the undergraduate textbooks today is the right way. Oh, Frank Westheimer showed up.

MAYO: When I went back as an instructor in early 1936, both Westheimer and Wheland showed up quite soon. Westheimer had an office across the hall, and Wheland one floor below, and we used to meet in my office for lunch, more often than not. I happened to have the biggest office, having gotten there first. I think I learned a lot from these two people under these conditions.

GORTLER: I know that Kharasch had recruited Westheimer. Do you know who brought Wheland in?

MAYO: Kharasch.

GORTLER: He must have had some kind of vision in mind, about building a strong physical organic group.

MAYO: He did. Although Kharasch had conflicts with Pauling, we should give him credit for getting a recommendation from Pauling. He almost certainly went through Pauling, although it might have been through somebody at Harvard. Perhaps it was the same person who got him Westheimer.

GORTLER: I see. You say that Wheland got a recommendation from Pauling?

MAYO: Well, Wheland came from Pauling, and now the question is, how did Kharasch find out that he wanted Wheland? Either he must have asked Pauling or he got a lead from Harvard.

GORTLER: Yes, that's right, because Wheland had worked with James B. Conant.

MAYO: Yes, and Westheimer worked partly with Conant.

GORTLER: Who did you think the other important organic or physical organic chemists were at that particular time?

MAYO: Well, I was at Chicago as an instructor from 1936 to 1942. Do you want to talk about the whole time?

GORTLER: Well, we can talk about that period, and if you want to expand into a later period, that's fine too.

MAYO: Well, there's no question that as far as influence and reputation were concerned, there was [Roger] Adams, [Homer] Adkins, L. I. Smith at Minnesota, and Conant; [Paul] Bartlett was coming along fast; [Arthur] Cope was soon to come along fast if he hadn't already. These were the people with the influence.

GORTLER: You mentioned the West Coast school?

MAYO: All right, there was Lewis and Pauling. Young was doing pretty well. [Howard] Lucas had turned out quite a number of prominent physical organic chemists. There were quite a number of people at Berkeley whose names I don't remember now, and whom I seldom saw, but there was no doubt that they had a potent organization going out in Berkeley.

GORTLER: Was it mainly physical chemists?

MAYO: It was mostly physical and some inorganic chemists. Now, as far as I was concerned, the important people were in physical organic chemistry. And the important people were Westheimer and Wheland and Young and Winstein and Bartlett and [C. Gardner] Swain. In general, it was the people who showed up at the first Conference on Mechanisms, whoever they might be. Of course, Ingold and some of his comtemporaries were leading the field abroad. Also, [William E.] Vaughan and [Frederick F.] Rust were at the Shell Oil Company.

GORTLER: Were they already active at that time?

MAYO: By 1940, they were, and they were starting to publish. Eventually they carried on much of the work that Kharasch had originated in free radicals and added a good deal of their own.

I still see Fred Rust frequently. He lives fifty miles or so away. He's still active. He had a remarkably good record at Shell, although he never went beyond the B.S. academically. Our work has overlapped and complemented from time to time, since the 1940s.

There is of course Herb Brown and [Phillip] Skell, both of whom were post-docs at Chicago with Kharasch. Kharasch really gave the impetus to the free radical portion of physical organic chemistry in this country. Most of it grew out of his labs and this was my main interest. These are the people I know the best. I never did any business with Adams, but he knew who I was. He would speak to me, but we never had any particular friendship. I have heard from Speed [Carl S.] Marvel since, that they were considering me for a job at Illinois during the Adams regime but it never came across. I guess Marvel has promoted me there and in some other places.

This is surmise, but I suspect that if Kharasch had made Roger Adams the editor of the Journal of Organic Chemistry, things might have gone a good deal better. Adams and Marvel and many others were on the editorial board, and I think that Marvel appreciated Kharasch and was on reasonably good terms with him.

GORTLER: Who was the first editor of the _Journal of Organic Chemistry_?

MAYO: Otto Reinmuth.

GORTLER: He had a close relationship with Kharasch?

MAYO: Yes. They had met at Maryland.

GORTLER: You felt that Kharasch really had the power to appoint the editors at that point? I don't know how that journal started.

MAYO: He started up the journal. He got a grant from some place to start it, and so that gave him a good deal of influence. He was a young and feisty independent when he started off. He softened a good deal by the time he died. He was more relaxed, and on better terms with a good many more people. But, I'm not aware that any of them funneled any good students to him, nor did he send any to them.

GORTLER: There was a good deal of interest in the abnormal addition problem. Lucas and Young and Winstein all were at least interested. So were a Japanese fellow by the name of Urushibara, as well as Sherman, Quimby and Sutherland. Did you feel any sense of competition with these other groups?

MAYO: Not I, because most of what I thought were the most important things had been done by the time I quit activity in the field.

GORTLER: In the late 1930s did the Kharasch group feel that they were in competition?

MAYO: Kharasch had a very strong feeling of competition. When I was a student, I remember talking to another instructor in the department, Irving Muskat, who was working on additions to butadiene. I talked to him about it at lunch time, and Kharasch didn't like this. Afterwards, he said that he wished I wouldn't talk about this because he wanted to investigate that himself.

GORTLER: Oh, Muskat was working for someone else?

MAYO: He was an independent instructor in the department. He had been one of Stieglitz's students, and a good man. I don't know that his work was worth very much because he didn't appreciate allyic rearrangements; but, I didn't know that at the time.

GORTLER: You also checked the 1937 paper on the bromination of toluene in the presence of a free radical initiator, ascaridole (8). What did you feel was the most significant aspect of this?

MAYO: This, for the first time, put the side-chain substituiton in the radical category, and the nuclear substitution in the non-radical category, and perhaps also put the addition to the nucleus in the radical category. For the first time, I think, these things were sorted out.

GORTLER: That was a very significant accomplishment then. Can you tell me how the 1940 peroxide review with Cheves Walling came about (9)? What are your recollections of how you decided to do that?

MAYO: Well, I was Kharasch's research assistant at the time. He got the invitation to write it, and passed the job on to me and Walling. We did essentially all the job, with some help from Senior. Then Kharasch looked over the final draft and said it was a sound review but it didn't give any lift. He wasn't enthusiastic about it, but nevertheless, he let it be known that his name should appear on it first. I said I didn't think that was the case. He said, "Well, if it isn't first, it doesn't go on at all."

I said, "OK, if that's the way you want it." And that's the way it was.

GORTLER: Aha. I had heard another rumor, that in fact his name had appeared on an early draft and that some reviewer said that it shouldn't be on there. But you surely know the story.

MAYO: The reviewer said that Kharasch shouldn't be on there? Well, the reviewer never saw it in that form.

GORTLER: Fine. I think that may straighten out that story.

MAYO: I don't know that he had any permanent resentment about it, but I had some rather strong feelings. In general, with very few exceptions, Kharasch's name appears first on the papers he's on.

GORTLER: Whether he wrote them or not.

MAYO: On his book, with the Grignard reactions, Reinmuth did essentially all of it (10).

GORTLER: That's what I'd heard when I was a student there. But, it's still Kharasch and Reinmuth.

MAYO: Yes, that's right.

GORTLER: You were also permitted to have your own students?

MAYO: Yes. After a while, I was.

GORTLER: You also taught some courses?

MAYO: Yes.

GORTLER: What kinds of courses did you teach?

MAYO: I taught one quarter of elementary organic once, but mostly I taught qualitative organic analysis, organic preparations, and a special topics course on polynuclear compounds.

[END OF TAPE 1, SIDE 2]

MAYO: Kharasch specified the title and I decided what the course would be. It was the reactions of the compounds with more that one ring in them. I specifically avoided getting to the sterols, because I just thought that was a can of worms in which I had no interest. So, I talked about biphenyl and naphthalene, indane, indene, phenanthrene, and anthracene. This had been quite useful to me in the coal business since. [Editor's Note: Dr. Mayo has done work on the chemistry of coal for a number of years.]

GORTLER: You did have a couple of students of your own. You said you were permitted to have students a little later?

MAYO: Yes. One of these was William B. Hardy who is a group leader at American Cyanamid in New Jersey. He's been a lower level manager there. He was one of the best undergraduates and he did a good job with me. He worked mostly on the bromination of naphthalene, where we separated it into radical and non-radical mechanisms (11).

GORTLER: He also had a paper on the cleavage of ethers with HBr (12)?

MAYO: Yes.

GORTLER: That was a fairly extensive paper that you did with him.

MAYO: Yes. He had two different jobs, and I think the ether...

GORTLER: ...he did a Ph.D. with that one, I think.

MAYO: He did both of them for his thesis. The ether paper was fairly important, because it showed the variety of mechanisms that is possible in essentially the same reaction. I don't know that anybody else has paid much attention to it.

Another student was Joseph J. Katz, who has been at Argonne Labs most of the time. He went from his research problem directly into the atomic energy project at Chicago. He then stayed on at Argonne and had quite a distinguished career. He was elected into the National Academy of Sciences. He's still working after he retired. He worked on the nonradical addition of hydrogen chloride to isobutylene. In other words, I was trying to find out the mechanism of the normal addition, and what we found is that in nonpolar solvents, it's polymolecular. This has been elaborated on by others since.

GORTLER: Certainly these two papers were sort of classic kinetic studies, and you did end up with fairly complex kinetics (13). Did you just pick up the kinetics, the ability to deal with complex kinetic situations as you went along, or were you taught this?

MAYO: No, I just learned it as I went along. All I had to know was how to differentiate and integrate. I didn't have any problem. In other words, I could use simple calculus.

GORTLER: I don't remember who it was. I wouldn't tell you who it was if I did remember, but someone suggested that you managed to take very simple problems and make them into very complicated ones.

MAYO: That wasn't the objective.

GORTLER: Were these complex kinetic situations frustrating, or did you sort of delight in handling them?

MAYO: I was just interested. It was something new. I had always been curious about the mechansim of the normal addition, in contrast to the abnormal addition, and this is what we found.

GORTLER: The reason this was so complicated was because the reactions were run in nonionic solutions.

MAYO: It was in heptane or something like that.

Finally I should say that there was a third graduate student. This was Arthur Dolnick who died early, a few years ago. He was not in the class with the others, but he did quite a respectable job on the rearrangement and the equilibration of alkyl halides. It turns out that neither the normal nor the abnormal addition gives the equilibrium mixture of halides, but if you let it sit long enough with the catalyst, they both give you the equilibrium mixture (14).

GORTLER: What catalysts were you working with at that time?

MAYO: Well, we were using peroxides for the radical rearrangement of the halides, and probably aluminum or iron chloride as the catalyst for the nonradical rearrangement. Both of them come to the same place.

GORTLER: That's funny, because I was just talking to Jack Roberts about a paper he had with Hine in the early fifties where they had done equilibrations. These were not addition reactions, but were just equilibrations of alkyl halides with aluminum chloride. We can talk about that later. Did you start any war work at Chicago?

MAYO: No. When the war came along, things started to rearrange rapidly and I started war work. I had had no promotion. I was still an instructor after six years there, the chances of moving up were slim, and academic research was over for the duration.

So, I decided I should look around. I got a very interesting offer from U.S. Rubber, and took it before I ever did much war work at Chicago.

GORTLER: How did you happen to find out about the U.S. Rubber job?

MAYO: I think Harkins arranged it for me. Harkins was consulting for U.S. Rubber, and he knew me. He knew that I had a pretty good reputation as a student, and I think he arranged the contact.

GORTLER: Did you know what kind of work they were going to do there, or the people who were there at the time?

MAYO: They told me they wanted research on the mechanism of polymerization. It hadn't been spelled out specifically, but I found out I was supposed to decide what I was going to do.

GORTLER: So you were essentially the group leader of one of the groups.

MAYO: I started it.

GORTLER: Were there other groups already formed at that time?

MAYO: No. There were some veterans there, who, under collaboration with Harkins, were working on emulsion polymerization.

GORTLER: Could you describe the organization of the research units and a few of the people?

MAYO: This was the top floor of an old fabric mill.

GORTLER: Where was this, by the way?

MAYO: Passaic, New Jersey. It had windows around the sides and a skylight and six foot partitions to separate the rooms. The physical chemists working on emulsion polymerization were quite an able group. At the same time that I started, a group started on the applied aspects of polymerization. This was headed by Pliny Tawney, who was one of Kharasch's students. I was to have charge of the basic work.

When I got there, I found that Fred Lewis, who I think had an M.S. in chemistry from Illinois, had started some work on copolymerization with the assistance of Fred Wall, who was a consultant from Illinois. They were testing out Wall's alpha parameter for following copolymerizations, and they had a few scattered data which didn't look very encouraging.

In very short order, (I don't know whether it was a week or a month) I saw that the copolymer composition depended on the reactivities of both radicals concerned, and that you couldn't use one number for both of them. Development of the equation turned out to be very simple.

So, with my direction and collaboration, Fred Lewis set out on the testing of the copolymerization equation that I developed there, and did remarkably well (15). He was an excellent experimentalist who eventually got a Ph.D. and went on to General Electric.

GORTLER: Did he go to General Electric before or after you did?

MAYO: He went there before me. I guess he suggested that I might be available.

GORTLER: I see.

MAYO: One of [Melvin] Calvin's students was another employee brought in to work with me. We agreed that he would work on the chain transfer aspects of polymerization, where I had also developed the equation soon after I got there. I decided that copolymerization and transfer were things that had the most potential. I developed the equations for them and we were off. This man's name was Donald Sherwood, and before long he left, I think, for an academic job. I'm told that he thought he should either be independent, or at least equal, in directing the work with me, but apparently he wasn't successful. I didn't know this until he had gone.

Well, on his departure Robert A. Gregg took over the work on chain transfer, and did most of it. As you'll see from the literature, his name was on most of the papers. He did a real good job on it (16).

GORTLER: Where was he from?

MAYO: The University of Michigan.

GORTLER: Did he take a Ph.D. with someone there?

MAYO: Yes, but I have forgotten whom.

GORTLER: That's all right. Whatever happened to Gregg?

MAYO: He is still there, unless he has been retired or laid off. He did a real good job on that and got to be the expert on it.

Well, of course, Walling participated in some of the copolymerization work. He played some of his own angles, like the Hammett version.

GORTLER: Yes, I was going to ask you about that, because that wasn't one that you mentioned. But, I've talked to him about it. Your name was on that paper (17)

MAYO: Well, he originated the work and did essentially all of it. I think I had an oar in on the conclusions.

Kenneth W. Doak, who came from Hopkins, was also at U.S. Rubber working on copolymerization and some related problems. He was also quite an able man. He eventually left U.S. Rubber to go into industry, where he did quite well in several places. With some new patents he helped to put Dart Industries into the polyethylene business, against all the competition of the time. He even sold some licenses to Japan. I guess he was director of research of Koppers and its successive companies in Pittsburgh. He has moved around some, is now partly retired, and a consultant for Koppers.

Now, I should say clearly that much of my success at U.S. Rubber is due to the able help that the management provided for me. I really had a remarkably good crew there, and except for Walling, whom I picked out, they were picked by the management and passed on to me. This crew also had some competent B.S. chemists and some high school girls to do the most routine work. I think that this was one of the most efficient operations I've ever worked in, because we had a range of skills. We were all in one lab, and the unskilled help could be passed around and distributed as it was needed.

GORTLER: It was obviously one of the most exciting and productive industrial research groups that had ever been assembled.

MAYO: It shows that if you have the management support, industry is a more efficient place than universities to do research, because you can keep the people after you've trained them.

GORTLER: Right.

MAYO: I pointed this out at a meeting once. [Turner] Alfrey came up and said he agreed with me. He was competing with me from a handicap position.

GORTLER: I know who he is, but where was he?

MAYO: He was at Brooklyn Poly at the time. He and apparently others developed the copolymerization equation after I did, but they got into print with it first.

GORTLER: I see. [Charles] Price was in on one of those things.

MAYO: No, Price came in later.

GORTLER: With something else?

MAYO: Yes. In fact it was the Alfrey and [G.] Goldfinger publication of the copolymerization equation that persuaded my management to let out what we had done (18).
GORTLER: I see.

MAYO: Otherwise it would have been held up for a long time.

GORTLER: I see. Was management using this in some of their applied problems as well?

MAYO: It was being used by the people working in emulsion polymerization. You see, the copolymerization fitted in with the styrene-butadiene problem, and the transfer fitted in with the mercaptan regulators that were being used. It also probably had some application in the applied work that was going on, but Tawney was a rather unusual character, and I think he had more interest in proving that I was wrong than in putting what I did to work. He died early, and unhappily. He was a real bright guy who was just missing a few cogs. What he would say about me, I don't know. It would be more interesting.

GORTLER: I remember, when I heard about the copolymerization work, I heard it from Wheland. I was taking his advanced organic course at Chicago. Later on, I realized that it came from U.S. Rubber, and I was absolutely amazed that this kind of work could go on in industry.

MAYO: Well, there were some special circumstances. U.S. Rubber was then under a 90 percent excess profits tax, so it cost them very little to support what I was doing. Besides supporting the synthetic rubber program, they were trying to lay out a program or foundation that they could use after the war. That proved to be an excellent opportunity for me, but when the 90 percent excess profits tax was off, they began asking quite properly, "Now, what can you do with this?" We told them a few things, but there were management problems there which I won't go into unless you insist.

GORTLER: No, I won't. As time went on, it was not as good an atmosphere as it had been before?

MAYO: That's right, and the problem was partly with the Director of Research, Sidney Cadwell, also a Chicago Ph.D. He had problems. He wanted to know what we could do with this work, and I told him I thought the best opportunities were trying to see if we could get a 1,4 polymerization of butadiene by use of metals or quaternary ammonium salt combinations. He agreed that was a good idea, but they had a contract with the government that said that anything we did on butadiene had to be shared with all the other companies in the agreement. He couldn't figure out what to do. One month he'd tell me to go ahead and start anyway. The next month he'd say, "No, wait a while." This went on for several months, and then the offer from GE came along, and I decided I would get out.

But I would like to say that it turned out that Goodyear and Firestone and others made quite a practical thing of the 1,4 polymerization of butadiene, somewhat along the lines I had in mind. I never had a process because I never did any experiments. It wasn't a bad idea for U.S. Rubber, and the management recognized it, but they had problems.

Now, the other rubber companies admitted--well, they did the work and got themselves positions and I don't say they violated the agreement any worse than they had to.

GORTLER: Were there outside consultants for U.S. Rubber at that time?

MAYO: Yes. The management wanted consultants and asked me whom I'd recommend. I recommended Bill Young of UCLA. After he'd been there once or twice, he said he could do much better if he had somebody to talk to. He wanted to have Winstein travel with him and consult with us, and so we talked to them. They were both good people and they did some good, but mostly I think they learned from us rather than telling us very much new. Now, this is not a criticism of them. We were way ahead of them. It was nice to talk to them, but I don't think they contributed what it cost the company. I don't know that I would like to be quoted on that, because I valued both of them as friends. They're both dead now.

Byron Riegel, from Northwestern, was also a consultant in his capacity as organic chemist. He went on to be Director of Research at Searle. [Peter] Debye was on the consulting staff, talking mostly to the people working on polymer properties and emulsion polymerization. I got to know him, and sat in on discussions with him. He was a remarkable guy, a real walking encyclopedia.

I remember one day I was in a conference with Debye and the people working on emulsion, and the Director of Research said, "Maybe what we ought to do is to send somebody to Urbana to sit at the feet of Speed Marvel and learn about emulsion polymerization."

I said, "I don't think you need to do that. You've got better people here." The director turned to Debye and said, "Is that right?" and Debye said, "Yes, that's right."

I think the guys appreciated it.

GORTLER: Just the other day somebody told me a little story about Saul Winstein consulting at U.S. Rubber. I guess he continued to consult after you left. I don't know whether this happened a few years after or ten years after. Winstein said, "The people who were there then couldn't even read the notebooks that you people had left behind." He said the decline had been enormous.

MAYO: Because they were illegible or because they couldn't understand them?

GORTLER: I think it was just that they weren't the quality of chemists that you were.

MAYO: Well, it may be that the notebooks weren't very good but I think mine was intelligible.

GORTLER: I think he was merely commenting that they weren't in the same class. I think that was it.

MAYO: Well, of course the best ones got drained away.

GORTLER: Yes. In fact you said Fred Lewis and Walling left before you did.

MAYO: Yes.

GORTLER: And finally you left. Do you want to say anything more about the work that you did at U.S. Rubber?

MAYO: Not if you don't.

GORTLER: OK. How about the copolymerization review (19)? How did you decide to do that?

MAYO: Well, I think Walling was the promoter. He, and to some extent I, were anxious to put ourselves out in front in copolymerization; obviously the review for Chemical Reviews would do that. We had much more experimental data than the competitors, such as Alfrey at Brooklyn. He [Walling] said that he would do most of the work on this. On that basis, I agreed to proceed. We wrote in to the editor of Chemical Reviews--I think it was [Arthur A., Jr.] Noyes--and found that Alfrey and [Herman] Mark had also approached them on the subject. This put Noyes on the spot. I remember him saying something about a certain amount of bitterness in the situation, although I wasn't aware of it. But, Noyes gave us the job.

Shortly after that the lab was closed down for vacation, although a few people stayed. Walling was among those who stayed. When I came back from vacation, I asked Walling how he was doing with the review. It turned out he had done nothing at all. Instead he had spent his time working on a plan for reorganizing the laboratory.

Well, it turned out that I did about as much on the review as he did, by working nights and weekends and holidays. As a result, I decided I would put my name on it first. If he had done what he had agreed to do, there would be no question that his would come first.

I have never been able to understand how he misused that vacation period. It still irks me.

GORTLER: Yes, I can hear a little bit of that in your voice.

MAYO: However, we get along well. My wife and I recently visited them in Utah and made a tour of Capital Reef National Park. We get along fine.

GORTLER: He must have been just about ready to leave then.

MAYO: Yes, within a year or so.

GORTLER: I see.

MAYO: It appeared in 1950. We probably started it a year or more earlier. Maybe he left just about as it appeared.

GORTLER: We've communicated about the Organic Mechanisms Conference in 1946. Do you remember the conference?

MAYO: I remember some of it. I remember it was an unusually interesting conference but I couldn't tell you what went on. I will say, the reason that it was so interesting was that most of the people there could understand everything that was going on. Now, physical organic chemistry has become so complex and varied that only the experienced people can follow the current discussions. I haven't gone to a mechanisms conference for maybe twenty years. I'm off the mailing list. The reason is I would look over the program and decide that I wasn't interested in what they were talking about. It was too detailed and too far afield from my interests to spend the time on. I would be better off at home.

GORTLER: I think that at least what some people term physical organic chemistry today has gone fairly far afield. There is in fact some question as to whether there truly is a field of physical organic chemistry now.

[END OF TAPE 2, SIDE 3]

GORTLER: I had asked you about the conference at Notre Dame. Do you remember some of the people who were there who you were particularly impressed with, or just people you talked with?

MAYO: I assume that Bartlett and Swain and Young and Winstein and probably Herb Brown and Skell and I think, Fred Rust were there.

GORTLER: Could Kharasch have been there?

MAYO: He might have. I'm not sure. He didn't go to very many meetings. I think the odds may be a little better than 50 percent that he was there, but I don't remember. Probably Walter Nudenberg was there.

GORTLER: Did he take over after you left the Kharasch group?

MAYO: I was at Chicago as instructor for six years. Toward the end of that period, I was spending less time on his students and more on my own work and teaching.

GORTLER: Since you were no longer working for him he probably wasn't all that unhappy about your leaving.

MAYO: Well, I think that Bill Reynolds followed me immediately, and then Bill Urry after that, and then I think it was Nudenberg. I think I have the order right. Nudenberg, of course, stayed for many years.

GORTLER: Yes. He was still there when I got there in 1953. You were obviously a member of the new wave or part of the new wave in organic chemistry, but you were always involved in that aspect of organic chemistry. Did you realize that a transition was taking place between classical and modern organic chemistry? Or were you just so much into that new physical organic chemistry that you were not conscious of the transition?

MAYO: When I went back to Chicago from Du Pont, Kharasch was starting up several projects related to free radical chemistry: additions to double bonds, peroxide decompositions, and halogenations of aliphatic and aromatic compounds. I was in there watching all those things grow. In retrospect, this was interesting and important, and other things didn't count very much. This was the world to me, and we were out in front. The other people who were working in physical organic chemistry weren't working in just the same field. For instance, solvolysis was still a hot subject.

GORTLER: Right.

MAYO: But it was clear that free radical mechanisms were going to grow for quite a while. I guess I didn't look too far ahead.

GORTLER: Had much work been done on free radical polymerization at that point in the mid 1930s?

MAYO: It wasn't recognized very well as free radical polymerization. The polymerization mechanism work with unsaturated compounds was dominated by the German physical chemists, who apparently didn't know or care much about free radical chemistry. They would put down the kinetics, but they had no free radical interpretations in the papers. I looked at some of these papers on chain transfer, and I could tell that they didn't have the answer.

In 1936, [Paul] Flory came out with a paper on free radical polymerization, where he introduced the concepts of chain transfer with the monomer. Not with the solvent, but with the monomer. When I got to U.S. Rubber, I saw that this was immediately applicable to the solvent work. I cooked up the equations, and they worked.

There were lots of kinetic studies, but nothing much related to mechanisms except for Flory's work until about 1940, when it was just starting.

Just before I left Chicago, I was giving a course on free radical chain reactions, and this set me up for the work on polymerization.

GORTLER: There was no polymerization work going on in the Kharasch group at that time?

MAYO: No. Harkins may have been dabbling some in emulsion polymerization, but I think it was mostly through his consulting operations.

GORTLER: You went to GE in about 1950?

MAYO: Yes, May, I think.

GORTLER: What was the nature of the group there that made you feel you wanted to go? Could you say a few words about the work that you did there?

MAYO: First, I was sure that things weren't going very well at U.S. Rubber, either then or in the near future. GE made me a very good offer. They brought me in alone, without any group, and said the custom in the laboratory was for every Ph.D. to do his own research. However, they put in another Ph.D. that I had to collaborate with, although he was not my subordinate. In a few months they brought in another man in the same category to work in my laboratory, but not as a subordinate. That was Glen Russell. He had written to GE to ask if he could get a position there, because his home had been in the area. So, they gave him a job and put him in there.

GORTLER: He was just leaving Purdue at that time?

MAYO: Yes. I eventually got a B.S. assistant to help, and we all started off on various aspects of the oxidation of olefins, most of which were polymerizable. I thought that the study of cooxidation might be as rewarding in opening up the oxidation field as copolymerization was in the polymer field. So, I decided we would do the cooxidation of styrene and methyl methacrylate.

I think Al Miller started this. He said, "Should we look at the effect of pressure on this cooxidation?" I said, "Well, I don't think there's going to be any there, but we had better check with styrene." We did, and it opened up the whole field of making carbonyl compounds and epoxides and polyethers and so on. We stayed with that for several years. Glen Russell took a variation by looking into oxidations of indene, which was remarkably reactive in copolymerization with oxygen. On his own he stirred up work on the oxidation of 2-nitropropane, which apparently is an ion radical chain. I guess he's still working on some aspects of ion radicals and oxidation.

After a while, I told the management that I was spending more than half of my time doing routine oxidations that a good BS could do for me. I didn't think I was being used very efficiently, and I would like another assistant. They said, "No." I decided that they weren't getting my money's worth and I wasn't being as productive as I should have been. So, when SRI put out an offer to me...

GORTLER: ...had SRI been in business for a long time?

MAYO: Let's see, this was 1956. They were founded about 1947 or 1948. They had taken on Dr. Robert M. Burns, who had been the chemical director at the Bell Laboratories, as an advisor to the administration here. He went out head hunting, and apparently got me through Bartlett. I decided that since I wanted to try the West sometime, and that I wouldn't be any worse off here in terms of assistants than I was at GE, I would have a try at it, and I did.

At the request of the management I had a period at GE when I worked on silicones. This was while I was trying to figure out what to do do with the oxidation work, which wasn't finished, but which was going to take a good deal of effort. I think I had a good start on silicones. I got two short papers out of it (20). I think they are significant, although I don't know whether anybody else does. But, my work on silicones ended there. However, the exposure to the silicones was interesting and useful.

GORTLER: Here at SRI you've done more oxidation work?

MAYO: Yes. They came and told me I could do anything I wanted to here. So I decided that I would finish up the oxidation work that I had started at GE, and it took about a year. The result was about 50 pages in the Journal of the American Chemical Society, which I think contributed considerably to the understanding of the oxidation of unsaturated compounds (21).

Thereafter, I decided that I ought to continue this work on the oxidation of unsaturated compounds, extending it beyond the polymerizable monomers which would copolymerize to the ordinary olefins. With the assistance of Chester Himmel, the department head here, I got $50,000 a year from the Air Force for three years to look at the oxidation of unsaturated hydrocarbons in general.

This work showed that formation of only polyperoxides or formation of only allylic hydroperoxides was a rather unusual situation. Usually, you get a combination of both in various proportions.

This work with the Air Force money also inspired me to look for industrial support. I've forgotten whether we started out at five or seven thousand a year, but we worked on the basis that the first customer in for say seven thousand, would have quarterly reports on $57,000 worth of research, and the next one would get $64,000, and so on. They were getting a good deal for their money.

This program took off and eventually got up to twenty sponsors, including several in Europe and at least one in Japan. The objective of this was to look over some fundamental aspects of oxidation and answer some basic questions without trying to get any patents. The idea was that we would try to produce ideas on which these companies could get patents or start their own programs. It sold. It lasted for a total of about twelve years, but after eight or ten years, sponsors began to drop off. When they got down to five, the program was killed. I think that we settled many of the problems that we set out to answer. One was, what are the kinetics of these oxidations? What is the real difference between liquid phase and vapor phase oxidation? The answer is, not much but concentration. What is the mechanism of decompositions of primary, secondary, and tertiary peroxides? Some work was on the effects of metals on oxidations.

Some of the work was done by full time Ph.D. employees of SRI. Much of the rest was done by postdocs. I decided pretty much what was done on the program, raised the money, and checked the reports. But, I gave the Ph.D.s a good deal of latitude in what they did. I decided that to keep them happy they'd have to be the first name on the publications. On the publications, where they were mostly responsible for the work, that is how it was done. I think there may have been twenty publications, more or less, that came out of this work. All of them were original and some of them were quite important.

My interest then turned toward the oxidation of polymers, especially polyolefins. I had two very able postdocs on that, Dr. [Etsuo] Niki from Japan and Dr. [Christian] Decker from Strasbourg. Together with the help of a B.S. here, we got out a series of five papers on the oxidation of polypropylenes, polyethylenes, their copolymers, and monomers (22). The work was done with both peroxide initiators of oxidation and gamma initiation of oxidation. The oxidations were done mostly in a temperature range of twenty to forty-five degrees, where all the products were stable.

For the first time, and perhaps better than anybody else has done to date, we got a material balance on the oxygen of better than ninety percent in terms of products. We could get a balance for the fates of the radicals generated. Now, this is the basis for interpreting all the work of others at higher temperatures and in the light, where the products are not stable, and where, in most cases, they do not know the rates of initiation.

Having put that plug in for this, giving my collaborators full credit, I'll go on to the next thing. I must say that Decker had done a somewhat similar job with the radiation of polyethylene glycols at Strasbourg. It turned out that he was just the guy to do the job here, but I think we had a simpler system and got a cleaner result than they ever did at Strasbourg. Getting him at that time was a big break that I didn't appreciate until later.

GORTLER: How did they characterize the products? These are high molecular weight compounds that are being oxidized.

MAYO: In terms of alcohol groups, carbonyl groups, peroxide content, and molecular weight change.

GORTLER: I see.

MAYO: The peroxides are of two kinds, dialkyl peroxides and hydroperoxides. They were titrated. The other things were determined mostly by IR. They balanced out pretty well.

GORTLER: Before you go on to some of your other work, at any time during this period, when you moved to U.S. Rubber and then to GE and SRI, were you ever inclined to go back into academia?

MAYO: Yes. Six months to a year after I got to GE, I got more than one phone call asking me if I'd take a job at Indiana

University. I thought I just couldn't decently leave GE after such a short period, till I found out what it was all about. I think they may have come back to me later. I'm pretty sure I could have had the job if I'd wanted it. I'm not sure that I would have been better off. I don't think I'm a particularly good administrator or politician or fur smoother. I don't know how I would have done. They were considering me for chairman of the department. I have often wondered what would have happened if I had gone there, but I've never had any serious regrets. I don't mind some teaching. I would hate to be swamped in it. I can get along on a diet of research alone.

While I was at U.S. Rubber I had several inquiries. I think one was Colorado. One was another school. I think it was at GE that I had an inquiry from the University of Pennsylvania in Philadelphia. There may have been another one or two. But none of them came at just the right time. When the offer came from Pennsylvania, I told them, "Well, I'm relatively happy here and I'm getting $15,000 a year. If you think you can compete with that I'll be glad to listen to you." That turned them right off. It was quite good money at that time.

GORTLER: Yes. Well, those are the things you have to bargain about.

MAYO: Colorado was quite interested, and I think it was Stan Cristol who was behind that. This was attractive, but things were going too well at U.S. Rubber at the time.

GORTLER: You talked about the oxidation of polymers. What was next?

MAYO: I decided that the most useful thing I could do in the polymer oxidation field was to try to devise a reliable accelerated aging test for stabilized polyolefins. When a stabilized polyolefin is exposed to the weather, or heated at above 100 ^{o}C, there is a long period where, by conventional methods, nothing happens. This is the period during which the stabilizers are being depleted, and maybe some peroxides are being formed. Finally, at least if you have a single stablilizer in the polymer, there is an abrupt change. The rate, instead of going along very slowly, turns up quite rapidly and then becomes very high because the peroxides accumulate. The reaction is autocatalytic, and soon after the reaction starts to go fast the polymer fails. That is, it becomes brittle. At this point, only about one mole percent of oxygen has been absorbed, and ninety to ninety-nine percent of the elapsed time has been induction period, when you couldn't see anything by conventional methods.

What we have done is to develop some methods for measuring extents of reaction during the induction period. These have to be extremely sensitive. We are concerned with hundredths or thousandths of a mole percent of reaction. But after a good deal of trial and error we have found some things that will do this, and this is where we are now.

The plan was that from the steady or very slowly increasing rate during the induction period, we could predict the time to failure. The early experiments indicated that we could. However, a few months ago, the roof fell in. We found that with mixtures of stabilizers, we don't get anything that we can extrapolate from. In other words, one stabilizer goes, and the rate changes, and then it may increase gradually over quite a period. So, we have no decent basis for prediction at this time.

However, there are a good many ways in which we can get out of this. One is to follow the depletion of the stabilizers. This can be done by HPLC or by UV. Another is to measure oxygen consumption during this period. That's one of our ways of following the reaction. By using a cell that isn't much larger than the film, we can get fairly large changes in oxygen concentration during the induction period by changes in pressure of the oxygen-nitrogen mixture. The mixture is determined by gas chromatography. In other words, we can see slight changes in the synthetic air mixture when the amount of air mixture is small compared with the film. It may be that when X percent of oxygen has been absorbed, say, one-tenth to five-tenths of that for normal failure, we can extrapolate from there.

[END OF TAPE 2, SIDE 4]

GORTLER: Was the change in your interest and involvement in oxidation, going from the more abstract or the more theoretical to the more concrete or practical uses of the study of oxidation, was a result of your being here at SRI?

MAYO: I don't consider that my approach has changed much. Starting at U.S. Rubber, I have used basic research to attack practical problems. I think the operation at U.S. Rubber was the most efficient one I was ever in, because of the variety and amount of help. Among the most interesting and challenging of my programs was the oxidation program that I ran here, where I had government money for bait, and I had up to twenty companies putting money into it.

This meant that first, I was doing some interesting basic research. Secondly, I knew it was useful or they wouldn't pay for it. And third, the contract called for me or somebody in the group to visit each company each year. This required a trip to Europe almost every year. One year, it turned out, it was really more economical to go around the world via Japan. Some of the other people in the group were offered the opportunity. One turned it down and one took it.

I would like to get something started like this again, because it's a fascinating combination of really basic research and something that can be useful now. This is what I would like to do with the polymer aging program, but I never got off on the same scale. With the studies in the dark I got maybe five sponsors. Presently I have only two: Exxon Chemical and Shell Development. These are places with some savvy, and they're putting this to use and getting some ideas. It pays my way in the organization here. The Institute is making money off of me. That's my security. Now, does that answer your question?

GORTLER: When you were visiting the companies, were they...?

MAYO: They were using me as a consultant, and they were pumping me for all they could get. Sometimes they would give me some information, and sometimes they wouldn't. But, I was always treated as a VIP.

GORTLER: Obviously the information that you were yielding as a result of your program must have been of some value to them?

MAYO: That's right. Some companies were in it for more than ten years.

GORTLER: Your current work is very applied. The kinds of polymers you're looking at now are the kinds of things that people use every day.

MAYO: These are important real world problems. But, I'm using a basic approach on it, and doing something that nobody has ever done before. We'll understand what we've got when we get it.

I think that most of the people in academic work would probably turn up their noses at this. That's all right with me. I think I'm more useful than they are. Some of the postdocs that I'm turning out are going out into industry with useful training. They couldn't get anything but a postdoc if they came directly out of the university.

A lot of people, particularly abroad, address me as Professor. I never made it past instructor.

GORTLER: Considering the kind of work you've done over the last forty or fifty years I think the title is very deserved.

MAYO: I think they mean well.

When I was at U.S. Rubber we were turning out lots of papers on copolymerization and transfer. I had lots of academic contacts because those people were interested in it, and I got a lot of invitations to go around and talk about it. But, when I went to GE and published much less, these contacts sort of faded away. Everybody's direction of research changed. Now, my generation is disappearing from the research scene and the academic institutions.

GORTLER: Can you give me your perceptions of how the transition took place from classical organic chemistry of the Roger Adams type to the rise of physical organic chemistry, and then, what I perceive, as the integration of the two? How did you see all that taking place? You did say earlier that Stieglitz was doing very early work in physical organic chemistry, and yet there was never any concerted effort until the 1930s.

MAYO: Ingold was obviously an early instigator in England. As far as I know, Conant was the instigator in this country, and he turned out some very good people who went on with it. I know Bartlett and Wheland and Westheimer, and Conant was the leader in this country. Of course Kharasch started out in physical organic chemistry in the middle 1920s but I don't think he made very much of an impression. He had a rather weird theory of organic chemistry, or mechanisms for reactivity in organic chemistry.

GORTLER: This was that electromer...

MAYO: Yes. He had a general idea about electronegativity which he understood, and perhaps very few other people ever did. It was based on heats of combustion of organic compounds. After he received his Ph.D. at Chicago, he went to the University of Maryland.

GORTLER: Whom had he worked with at Chicago?

MAYO: Jean Piccard, who eventually became the balloonist in Wilmington. Kharasch worked on a dye problem at Chicago, and eventually he got a job on the staff at the University of Maryland. According to him, they didn't have any equipment but a ring stand and a bunsen burner, and these were in poor order. Wanting to start research, he started out on library work. Through the Bureau of Standards in Washington he correlated heats of combustion and tried to make a reactivity theory out of it. I understand he did a good job of picking the good ones and compiling heats of combustion.

Now, he knew the literature well enough to know where the problems were. For fifty to one hundred years previously, different people had been reporting different addition products of HBr to allyl bromides. They got quite different results, and there wasn't any system at all. He decided that the results were due to electromers of allyl bromides, and he was going to look for them. While the theory may have been weird, he knew where to look.

I think somewhat the same thing happened with the 2-pentene. He started some of his early students in that field. Either the results were inconclusive, or the public didn't believe them. When the work at Chicago on allyl bromide and propylene came out, then people started to pay attention, and he was off.

GORTLER: At any time did you feel that you were further ahead in your understanding of these problems than Kharasch was? I have the impression that he didn't always know what was going on.

MAYO: No. In one case, I think it was the addition of HBr and HCl to some unsaturated chloride, I was largely responsible for resolving the radical and nonradical products clearly, without much help from him or even his knowing the details of just what was going on. That's the most extreme case I can think of.

He took the war and Hitler more seriously than the average American, and worked very hard on war research. After the war, he had some kind of a breakdown. I wasn't there. I've had conflicting reports, ranging from the fact that he was really in a sanitarium for a month or two, to the fact that it didn't amount to anything serious. I don't know how much he accomplished after that. One view is that he never fully recovered his old powers, and that Nudenberg ran the show. Another one is that without Nudenberg he wouldn't have gotten much of anything after 1950. Another one is that he was still calling the main problems, and Nudenberg was executing them. He was very dependent on Nudenberg. It has been indicated to me that he didn't do any reading of the literature, and that he depended on Nudenberg to keep him informed and even to read some of the literature to him.

GORTLER: I see.

MAYO: On the other hand, if you talked with him, he would seem as perky as ever.

GORTLER: Had you talked with him much about scientific problems after you left?

MAYO: Through the 1940s and into the beginning of the 1950s, I would at least call on him every time I went to Chicago. But then, I began to see him less and less for reasons I'm not sure about. I didn't talk to him so much in his later years. Once I refereed one of his papers, and when I went to see him he said, "Did you do this?"

GORTLER: He recognized your style of criticism, I take it?

MAYO: He might have. Well, I think there was a problem in his kinetics and conclusions. I soothed him and got some changes.

GORTLER: Do you think that World War II had much to do with the development of physical organic chemistry? You were certainly utilizing it on practical kinds of problems and in basic research.

MAYO: I wasn't around enough to know, but I'll give you an opinion. I think there was rather little physical organic chemistry in the universities during the war. I was doing some. The rubber companies were applying it as best they could. Something must have been going on in industry. I suspect that Shell, Du Pont, and others must have been doing something.

GORTLER: You mean, they were doing something along physical organic lines?

MAYO: Yes.

GORTLER: The reason I ask is because, after the war, physical organic chemistry sort of burst on the scene.

MAYO: That's right.

GORTLER: Not only did a great deal of material come out of academia, but industry was willing to accept these people for the first time.

MAYO: The free radicals and polymerization gave it a big boost. Now, I think that a lot of the big growth in physical organic chemistry in the 1940s was in the free radical aspect of it. It did have applications.

GORTLER: That's right. Even Bartlett had turned to free radical chemistry, and he was doing kinetics with a rotating sector.

MAYO: Yes. Rate constants and copolymerization. Brown and Winstein were continuing the hydrolysis work.

GORTLER: Bartlett was doing hydrolysis work, because he and Swain were doing the mustard gases at that time. It was a war related problem, but it was yielding neighboring group participation results. Winstein apparently was very upset when he found they'd done all that work.

MAYO: On the other hand, at the Mechanisms Conference at Northwestern, that would be 1948 or 1950, they had Ingold and several other solvolysis people there. Coming out of the meeting, Westheimer said to me, "It seems to me that this discussion is analogous to the old theological question of how many angels can dance on the head of a pin." So, I think it was over the hill at that time.

GORTLER: You mean, all that work that was done in the 1950s and 1960s was just trimming on the cake?

MAYO: I think it was largely a waste of time. During the 1950s and early 1960s Winstein visited me. I had him to dinner. I said, "Saul, has anything practically useful ever come out of this solvolysis work?" He said, "I don't care whether it did." Then he said, "Well, I think maybe it was of some use in a reaction for Hercules," something like that.

GORTLER: I don't think he was ever concerned with the practical applications of his work.

MAYO: I think he was thereby missing the boat. I think that I have found first rate scientific problems to work on. In addition, I had the practical application to give them a kick. If you can get two for the price of one, why settle for one?

GORTLER: Of course you were being paid by a variety of institutions to at least turn some of your attention to practical applications.

MAYO: Yes. But I found that this copolymerization work attracted great academic attention.

GORTLER: Yes, it certainly did.

MAYO: Besides, it was useful.

GORTLER: Yes. As I told you, I first heard about the work when it was introduced in an advanced organic class. The class was not a class in polymerization. It was just the high regard that George Wheland certainly had for it. I suspect some of that came from the fact that you were an old and good friend of his, but I think when Wheland had respect for something, he talked about it. If he didn't he wouldn't have said a word.

MAYO: Well, I gather that Winstein made a good deal of use of our work and data in teaching at UCLA. For a long time, this was the principal body of available work on relative reactivity of free radicals.

GORTLER: Yes.

MAYO: And substrates.

GORTLER: Yes.

MAYO: There wasn't much else.

GORTLER: You and Cheves Walling were very interested in instabilities of radicals and how that applied to reactivity. You published a number of papers of that sort (23).

[INTERRUPTION]

GORTLER: Do you have anything further to tell about your perception of the development of physical organic chemistry?

MAYO: As far as I can make out, a few years ago industrial or applied chemistry was a dirty word in a good many chemistry departments. I don't know the extent to which it still is.

GORTLER: I think not.

MAYO: Am I correct about a few years ago?

GORTLER: Oh, there's no question about it. Louis Hammett has said to me that we spend all of our time training people to be academic chemists, and seventy-five or eighty percent of them end up in industry. I think today more people are worried about the academic-industrial interface. At least they talk about it but I don't know whether they're doing anything about it or not. The interesting thing is that industry is calling for fewer and fewer consultants from the universities. They seem to be cutting back on that. I don't know whether that's an important part of the interface. Some industrial outfits are talking about either bringing in faculty members on a sabbatical basis, or trying to interact with the universities in another way. I don't know, have you heard much about any of this?

MAYO: No. I see the C&E News and the associated publicity. I know some of the educational groups in the ACS are much concerned about the interface. I think a lot of them are really more concerned about research money.

GORTLER: Well, there is no question about that. You've seen through the guise. But, I think there will be a lot of good that comes out of it.

MAYO: Yes. It can't help but do some good.

GORTLER: You said what success you've had has been largely due to two great pieces of luck. What were they?

MAYO: One was when Stieglitz diverted me from high school teaching to research. The other was when I connected with U.S. Rubber. That's all.

In a sense, I rescued Walling from Du Pont. He has said so.

GORTLER: Yes, he implied as much to me as well.

MAYO: He was the best of the students I had seen at Chicago. When I was asked with whom I would like to increase my group at U.S. Rubber, he was my first choice. It took a little finagling.

GORTLER: Yes, I understand it was a bit sticky at that point, but go ahead, tell me what happened.

MAYO: You probably heard it from him.

GORTLER: No, he didn't tell me what happened. I guess that was wartime already and he wasn't supposed to go to another industrial firm.

MAYO: U.S. Rubber was controlled by Du Pont, and we had to be very careful about hiring anybody away from them. Also the rule was that if at any time he was unemployed, he was subject to the draft at once. It was finally arranged that he would resign from Du Pont and immediately he would have an offer from U.S. Rubber. That's the way it went. He apparently had had a couple of difficulties with Du Pont. I think he had been involved in an accident, and was partly behind the eight ball down there. He was glad to get out and also into a place where he could publish.

GORTLER: He didn't mention the accident. He did say he had difficulty with the dyes. He had become allergic to the dyes.

MAYO: That may well be the case, too. But, he wasn't on the inside track with the management. This is another case where he would liked to have had an academic offer upon leaving Chicago but Kharasch couldn't produce it. I think that Kharasch never placed one of his students in a first rate school.

Herb Brown was a Kharasch postdoc and went in through Wayne University where Kharasch had a connection. Walling got into Columbia on his own. I didn't get any offers at a time when I wanted them.

GORTLER: That is interesting. That is a piece of information that I guess was there but I just hadn't recognized it--the fact that Kharasch had not placed anyone in a major university.

MAYO: He just wouldn't play ball with the organic power structure.

[END OF TAPE 3, SIDE 5]

GORTLER: Walling finished his Ph.D. quickly.

MAYO: He got out in the university minimum time of nine quarters (2 1/4 years).

GORTLER: Is there anything else we may have forgotten?

MAYO: Nothing important.

GORTLER: Thanks a lot. I really appreciate the time we've spent.

[END OF TAPE 3, SIDE 6]

NOTES

A list of the publications of Frank R. Mayo may be found in the CHOC Oral History Program File #0031.

1. George S. Newth, _A Text-Book of Inorganic Chemistry_, 2nd ed., (New York: Longmans, Green, and Company, 1895).

2. Julius Stieglitz, _The Elements of Qualitative Analysis, with Special Consideration of the Application of the Laws of Equilibrium and of Modern Theories of Solution_ (New York: The Century Company, 1922, c1911).

3. Howard J. Lucas, _Organic Chemistry_ (New York: American Book Company, 1935).

4. M. S. Kharasch, C. Walling, and F. R. Mayo, "The Addition of Hydrogen Halides to _cis_- and _trans_-2-Pentene," _Journal of the American Chemical Society_, 61 (1939): 1559-1564, 3605.

5. F. R. Mayo, "The Reduction of the Pyridine Ring by Formic Acid," _Journal of Organic Chemistry_, 1 (1936): 496-503.

6. 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.

7. M. S. Kharasch, E. T. Margolis, and F. R. Mayo, "The Peroxide Effect in the Addition of Reagents to Unsaturated Compounds. XII. The Addition of Hydrogen Bromide to Butadiene," _Chemistry and Industry_, (1936): 663.

8. M. S. Kharasch, E. T. Margolis, P. C. White, and F. R. Mayo, "The Peroxide Effect in the Halogenation of Aromatic Side-Chains," _Journal of the American Chemical Society_, 59 (1937): 1405-1406.

9. See Note 6.

10. Morris S. Kharasch and Otto Reinmuth, _Grignard Reactions of Nonmetallic Substances_ (Englewood Cliffs, NJ: Prentice-Hall, Inc., 1954).

11. F. R. Mayo and W. B. Hardy, "The Bromination of Naphthalene," _Journal of the American Chemical Society_, 74 (1952): 911-917.

12. F. R. Mayo, W. B. Hardy, and C. G. Schults, "The Cleavage of Diethyl Ether by Hydrogen Bromide," _Journal of the American Chemical Society_, 63 (1941): 426-436.

13. (a) See Notes 11 and 12.

13. (b) F. R. Mayo and J. J. Katz, "The Addition of Hydrogen Chloride to Isobutylene," Journal of the American Chemical Society, 69 (1947): 1339-1348.

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(b) See Note 17.

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