THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

# RICHARD S. STEIN

Transcript of an Interview
Conducted by

James J. Bohning

at

The University of Massachusetts

on

17 June 1987

Richard S. Stein JH 3/15/96

BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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

# RICHARD S. STEIN

1925 Born in New York, New York on 21 August

## Education

| | |
|---|---|
| 1945 | B.S., chemistry (magna cum laude), Polytechnic Institute of Brooklyn |
| 1948 | M.A., physical chemistry, Princeton University |
| 1949 | Ph.D., physical chemistry, Princeton University |

## Professional Experience

| | |
|---|---|
| 1948-1949 | National Research Council Fellow, Cambridge University |
| 1949-1950 | Research Associate, Princeton University |
| | University of Massachusetts, Amherst |
| 1950-1957 | Assistant Professor of Chemistry |
| 1957-1959 | Associate Professor of Chemistry |
| 1959-1961 | Professor of Chemistry |
| 1961-1980 | Commonwealth Professor |
| 1980- | Charles A Goessmann Professor |
| 1961- | Founder and Director, Polymer Research Institute |

## Honors

| | |
|---|---|
| 1968 | Fulbright Visiting Professor, Kyoto University |
| 1969 | International Award, Society of Plastics Engineers |
| 1970 | Honor Scroll Award, New England Chapter, American Institute of Chemists |
| 1972 | Applied Polymer Chemistry Award, American Chemical Society |
| 1972 | Bingham Medal, Society of Rheology |
| 1976 | Polymer Physics Award, American Physical Society |
| 1978 | Chancellor's Medal, University of Massachusetts |
| 1983 | Polymer Chemistry Award, American Chemical Society |
| 1985 | Whitby Lecturer, University of Akron |
| 1988 | Polymer Science Society of Japan Award |

## ABSTRACT

Richard Stein starts this interview by reflecting on the New York City schools which provided a real stimulus, especially in mathematics and science, to him and his contemporaries. At Brooklyn technical High School he took a more vocational set of courses, thinking that the family resources would not cover college study. In the event, however, Stein was able to go to Brooklyn Polytechnic Institute and, under the wartime circumstances, was able to graduate within three years, including a productive senior project on light scattering with Paul Doty. Stein then accepted a Textile Foundation fellowship at Princeton University. In the three years of his Ph.D. program he worked under a succession of three advisors; Henry Eyring, Robert Rundle and Arthur Tobolsky. During this section of the interview Stein describes the organization of graduate study in chemistry at Princeton and recollects Eyring, Taylor, Rundle and Tobolsky. A NRC fellowship took Richard Stein from Princeton to Cambridge to work on infrared dichroism under Gordon Sutherland and he recalls the austerities of life in postwar England and the primitive facilities in the Cambridge physical chemistry laboratories. Soon after his return to this country Stein was appointed to an assistant professorship in the chemistry department of the University of Massachusetts at Amherst. Stein describes his heavy teaching load, how he started his research program and the growth of polymer interests at UMass. The latter led to the inauguration of the Polymer Research Institute at UMass and Stein reflects on the academic interactions between chemistry and polymer science. The interview concludes with recollections of the visit of a chemistry delegation to China and also with his views on research funding.


## INTERVIEWER

James J. Bohning holds the B.S., M.S., and Ph.D. degrees in chemistry, and has been a member of the chemistry faculty at Wilkes College since 1959. He was chair of the Chemistry Department for sixteen years, and was appointed chair of the Department of Earth and Environmental Sciences in 1988. He has been associated with the development and management of the oral history program at the Beckman Center since 1985, and was elected Chair of the Division of the History of Chemistry of the American Chemical Society for 1987.

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INTERVIEWER: James J. Bohning

PLACE: University of Massachusetts, Amherst

DATE: 17 June 1987

BOHNING: Professor Stein, you were born on the 21st of August, 1925 in New York City. Could you tell me something about your parents, their names and occupations?

STEIN: My father was Isidor Stein. He is deceased now but he was also born in New York City, in Brooklyn I think. He was in the women's clothing business. My late mother was Florence Lewengood, was also born in New York City.

BOHNING: Where did you grow up in New York?

STEIN: In the town of Far Rockaway which is in the southeast corner of New York City, just bordering on Nassau County. It's a kind of interesting town. It turns out to be the same town that Richard Feynman, the Nobel prize winner in physics [1965], grew up in. I got my inspiration in science in elementary school there and I had just visited two days ago on Long Island, my former science teacher who must be distinguished, being one of the few science teachers to have two Nobel prize winners amongst his students. The other Nobel prize winner that was from there is Baruch Blumberg, who got the Nobel prize in medicine [1976]. Baruch and I used to be chums in elementary school. We once had a project of trying to make gold from seawater and we spent most of the summer lugging jars of seawater from the ocean up to our own laboratory.

BOHNING: Where did you first go to elementary school? Was it in Far Rockaway?

STEIN: Yes. It was in P.S. 39 in Far Rockaway, Queens. Then I went to Brooklyn Technical High School. This elementary school teacher, Arthur Charlton recommended that I go there and, at the time, it was a quality high school in New York. That and the Bronx High School of Science were the two places to go for people who were interested in science and engineering. So it meant traveling twenty-five miles back and forth each day to go there, but it was well worth doing.

BOHNING: I'd like to explore a little more this early interest

in science. When did that start? Was it in the elementary level?

STEIN: Yes.

BOHNING: Could you tell me more about that?

STEIN: It probably started when I was in about sixth or seventh grade. Arthur Charlton had a science club and I think this was a very effective device. It was back in the days when the salaries of teachers were competitive with the salaries of doctors and lawyers; they were in a desirable economic state and teachers really devoted much more time to teaching than they do today. They were quality people. The science club met after school. Every week the idea would be that the various members of the club would devise experiments that they would do for the club and then the club would vote on who would get the best experiment that week. It was a competition and was very inspirational.

BOHNING: How many people were involved in that club?

STEIN: There were about ten and at least half of the people have gone on to be university professors.

BOHNING: What kind of science did you cover? Chemistry, biology, physics?

STEIN: The things I did were mostly chemistry oriented but it was the kind of chemistry that appeals to people at that age, explosions, smells and smokes.

BOHNING: Had you had any exposure to chemistry prior to that? Was that science club and teacher your first exposure to chemistry?

STEIN: That was one of the first. A little after that, in 1939, there was the New York World Fair. I was in high school at that time and that was a big inspiration to me. I had a student ticket for going to Brooklyn so the ticket also enabled me to go into Flushing to the World Fair on weekends. I could get an annual pass fairly cheaply that enabled me to travel an unlimited number of times essentially, so I spent most of my weekends that year there. It was my first taste of seeing things like television and automatic dishwashers and artificial lightning. Du Pont's exhibit of better things for better living. That made a very great impression on me.

BOHNING: That was 1939, so you were fourteen then.

STEIN: Yes.

BOHNING: Do you remember any of the other people involved in this science club?

STEIN: Yes, in fact I've kept in contact with a few of them. One of them is a professor at Rutgers University; Lionel Goodman is in the chemistry department and has done some very fine work in molecular spectroscopy. Another person is a physicist, George Salzman who is actually teaching physics at the Boston campus of the University of Massachusetts. He's interested in relativity theory and things of that sort.

BOHNING: It's interesting because you all went in somewhat different ways. You didn't all become chemists. How long were you involved in that science club?

STEIN: It was for my last two or three years of elementary school.

BOHNING: When was this project of making gold from seawater?

STEIN: That was probably toward the end of that period.

BOHNING: What were you doing in trying to carry out that?

STEIN: The first project was evaporating the water down to concentrate the salts but then we tried to make separations using essentially, qualitative analysis type procedures; precipitating out various things in order to concentrate the gold. I think we got just a little bit of it but it was probably the most expensive gold that was ever made.

BOHNING: Where did you get the water from?

STEIN: The ocean. Far Rockaway is right by the ocean so we would go down on our bicycles and run back with gallon jugs of water. This was before one had polyethylene jugs. They were made out of glass and I think we dropped a few of them.

BOHNING: What was it like growing up at that time in Far Rockaway? What was your family situation?

STEIN: It was a really nice place to grow up because it was a much higher class community than it is now, populated mostly by upper middle class people, most of whom worked in Manhattan and commuted. One could spend summers by the seashore, fishing and boating. It was relatively rural for being in New York City.

BOHNING: Did the Depression have any affect on your family?

STEIN: It certainly did. My father had worked his way up in business from being essentially the office boy to being president of the company. The company made real silk undergarments. Obviously these didn't sell very well during the depression so the company folded and he was unemployed for a few years. Then he essentially did door-to-door sales. For a person of his age, this was a great drudge but he did it very well. He worked his way back to starting up a new company and working his way up to an executive position again. I think this was fairly hard on his health and I suspect it led to the disease from which he finally died. It meant that going to college was quite uncertain for me. When I went to Brooklyn Technical School one had the option of taking a college preparatory course, which I didn't do. I took a so-called chemistry course which was job-oriented. It was mostly for people who would go to work right after school which I thought I would have to do. But the family pulled together its resources and then my sister decided not to go to college. She worked and I was able to go to Brooklyn Poly then. That was very fortunate but I did think very carefully about whether I would go to college and actually interviewed for some jobs at that time.

BOHNING: I'd like to back up and talk about Brooklyn Tech for a bit. How many other people in your group from your elementary school went on to Brooklyn Tech?

STEIN: Not too many, only two or three.

BOHNING: Could you tell me something about the school. What was it like?

STEIN: It was very competitive. One had to take an entrance exam to get in. In fact, I was just talking to my ex-science teacher about this last week. I was rather small and puny and young for the age of my graduating class. I had skipped a few grades at school. I was fairly protected, particularly by my mother, so I had never really fared well by myself. This teacher

felt very strongly that I should go to the Brooklyn school but my mother felt very conservative about it. So he actually assigned a big burly Italian person to act as my bodyguard when I went along to take the entrance exam because I had to venture into downtown Brooklyn which I had never done before by myself.

At first traveling to a Brooklyn city school was a relatively traumatic experience for me. But I think it was good for me because I had to do a lot of growing up and I cut the apron strings from home a little bit.

BOHNING: How old were you when you started there?

STEIN: Normally one graduates from eighth grade at about fourteen. I was twelve.

BOHNING: What kind of choices did you have in chemistry courses when you started ?

STEIN: There were a number of options. One of these was chemistry, another electrical, another was mechanical. They were all technically oriented courses of an engineering type. In fact, the chemistry course was strongly engineering oriented. Maybe it was good that I did this rather than take the college course because it meant that I got very good technical training and learned some engineering disciplines like drafting, machine shop work, and foundry work. I got a feeling for how industry worked and this has been very good background. Today, when I go into a factory, I have a good understanding of what happens. Several years ago I was a member of the first delegation of chemists and chemical engineers to go to the People's Republic of China. We visited all sorts of plants in all disciplines of chemistry and I felt pretty much at home. In fact, China a few years ago was back where the U.S. was at the time that I went to high school so I felt very familiar with many of the things that I saw there. In fact, it was a revelation because one doesn't see many of those things in this country anymore.

BOHNING: Did you get practical experience in local industry in Brooklyn?

STEIN: We went on plant tours and drug factories at that time. For example, Charles Pfizer was a backyard operation in Brooklyn. It was a pretty small company. In fact, it was one where I took a job interview when I graduated from high school. We were visited them when they were just starting to manufacture drugs.

BOHNING: Do you remember any of your teachers who had any specific influence on you in high school?

STEIN: One of them was a very good chemistry teacher, named Dushman. He was related to a person, maybe a brother, named Saul Dushman who worked at General Electric Research Lab in Schenactady and was a leading worker in the field of high vacuum science. Dushman was an excellent chemistry teacher. There was another person with an English background named Charmin who taught pattern making, how to make molds for casting metals; he was a very good teacher. Another teacher taught a course where we would study industrial processes. I remember another one who taught a course called power laboratory which was really a lot of fun; it was a laboratory filled with things like diesel engines and gasoline engines. We would actually operate these and do scientific experiments on them. We would measure their thermodynamic efficiencies and weigh the fuel that went in and measure the power output. One learned a lot of good practical science doing that sort of thing.

BOHNING: Where did they get their support for all of this?

STEIN: The New York City school system. One thing that we did there, I think, has a direct implication in what I'm doing now. I'm producing a video course for teaching industry. At the time, Brooklyn Technical High School had a radio station there which was the radio station for the New York City school system. It would broadcast programs produced at Brooklyn Tech and these would be received at various schools. As an alternative to taking an ordinary English class, we could take what was called radio English. We worked in the radio station; we wrote scripts and operated the station, did acting and so on. I did this and learned a lot about these kinds of techniques which I think has paid off in my teaching.

BOHNING: Were the programs technical in nature?

STEIN: Yes. In English, instead of writing a theme on some abstract subject, one would often learn how to write a technical report. I think it's a pity that the quality of education at schools like those in New York City has declined. Those kinds of things aren't happening today. I think they have a great influence in one's life. I got a letter yesterday from a professor in Japan who is the president of a technical junior college in Japan which sounds to me very much like this sort of a place. Probably the success of Japan today in producing outstanding engineers and technologists is based on a very fine education at this level. I feel it's a pity that we're really not getting this. For the most part students don't learn good science and math in elementary school.

BOHNING: Could you tell me a little more about the chemistry? What kind chemistry did you do?

STEIN: Very good actually. The chemistry courses we got were at a level comparable to elementary college level courses. We had a course in analytical chemistry, where I think the qualitative analysis that we had to do was harder than the qualitative analysis I had to do at Brooklyn Poly when I got there. As a result, when I got to Brooklyn Poly, the first few years were pretty easy because it was mostly a repeat of material that I had learned in high school.

BOHNING: What kind of lab facilities did they have?

STEIN: They were quite good. There was good equipment that would rival what one would find in elementary laboratories at most universities.

BOHNING: Did you take chemistry throughout the four years?

STEIN: Yes. We had elementary chemistry the first year, then analytical chemistry, then industrial chemistry. We had chemical engineering oriented courses.

BOHNING: It sounds like an incredible amount of chemistry for a high school atmosphere in addition to all of the other things that you described.

STEIN: I would say if I were working in a company today and wanted to hire someone, I think a person graduating from that chemistry curriculum would make a better employee than a graduate of many of the less outstanding colleges today.

BOHNING: What was the entrance exam like?

STEIN: Similar to the college board exams today but at a lower level. There was a general exam and there was one specifically oriented to math and science. One really had to exhibit skills in mathematics at the elementary school level, particularly at problem solving and applications of mathematical concepts as well as understanding the scientific principles as applied to everyday life.

BOHNING: What kind of math background did you get at the high school level?

STEIN: Again, it was strongly engineering oriented math. For example, we had to take a course that involved surveying and this meant a real strong application of trigonometry. It was before calculators were available and I remember that I was using five place logarithms to do my calculations. I had a duplex slide ruler which I still have in my desk drawer. I had won this in a math contest in high school and I still have it. Now, when students come in and see the slide ruler they ask what it is.

BOHNING: I recently bought each of my sons one. Our bookstore had found an old box that they were selling for fifty cents each and I bought them so that my sons would know what they were like. As you were finishing high school, you weren't sure that you were going to be able to go onto college and that you did some job interviews, Charles Pfizer being one. Did you interview at any other places as you were finishing up high school?

STEIN: Yes. I've forgotten what the other companies were but they were mostly companies in the New York City metropolitan, or New Jersey area.

BOHNING: What was the turning point in your being able to go to college? And why did you choose Brooklyn Polytechnic?

STEIN: One, the reasons I considered Brooklyn Poly is that they had a very extensive evening school program. I think their evening school was better known and larger than their day school. One advantage of going there is that it would have been possible to supplement one's income by working during the day. I actually didn't do that, my family support was such that I didn't need to, but I still would take a lot of my courses in the evening which set my days more free to do research types of things. It was actually during the war and as a result that the population of the day school was fairly low. The professors were involved in military projects and there was a lot of flexibility in the education which I think was a big advantage. One could work on project oriented things and spend a lot of time during the day doing that. For example, I worked on a project for the Army that involved developing polyvinylchloride coated fabrics that were used for Army raincoats and gun covers in the Pacific landings. One of the concerns was the degradation of these by sunlight. Actually, I remember taking some of these into the laboratory of the New York department store, R. H. Macy, because they had a fadeometer. That was a carbon arc device for testing exposure of samples to strong sunlight. I would go there during the day to see how well they would stand up to irradiation. There were many projects of that sort going on.

One of my acquaintances there was Sam [Samuel] Krimm, who is now a professor at the University of Michigan. He's been head of

the biophysics department there and he's the professor who recommended to us Shaw Ling Hsu, who is now a professor here in spectroscopy, and who was Krimm's graduate student. I got to know Sam at Brooklyn and we were quite strong buddies. Our careers have paralleled each other quite a bit since then. Sam was also working on a military project with Roland Ward, who was a professor of inorganic chemistry and the person that taught me freshman chemistry at Brooklyn Poly. Ward subsequently became a professor at the University of Connecticut. Sam was working on a project at developing phosphors for the Navy. These were used for signaling back and forth between ships during the war so that radio signals wouldn't be intercepted by submarines. The phosphors that would glow when hit by infrared radiation so they would be able to convert infrared into visible light. A project similar to the sniperscope which enabled soldiers to see in the dark. Sam was involved with Roland Ward in the development of these phosphors for this program.

BOHNING: I'm not clear what year you started at Brooklyn.

STEIN: I graduated from Brooklyn Technical High School in 1942. Then I graduated from Brooklyn Poly in 1945. So I managed to finish four years of college in about two and three-quarter years. The reason for that was that, in the day school, most of the students were in the Army Specialized Training Program, the ASTP, and civilian students just got tagged onto this program and took the same course curriculum. Of course, the soldiers didn't get the summer vacations off, so we just went round the clock in our training, and as a result we got done in a fairly short time. The program was sort of unconventional. I remember taking physics I and physics II simultaneously. I complained to the registrar that physics I was a prerequisite for physics II. He looked at my program and said, "Well, you're taking physics I in the morning and physics II in the afternoon, so that's okay." [laughter] That was typical of the types of things that went on there.

At Brooklyn Poly, I proofread a manuscript of Turner's [Alfrey] book on mechanical properties in polymers which was a very significant book in polymer science.

BOHNING: Before we get to Turner: when you went to Brooklyn Poly, did you sign up as a chemistry major? I'm not clear as to what your choices were at that time.

STEIN: Actually, I started out in electrical engineering. Then I changed to chemical engineering and then to chemistry. I suspect if I had stayed in school long enough, I might have become a physicist. [laughter] I think that was good too because it meant that I got some training in a variety of fields and I think the name of the game in science today is interdisciplinary activities. I think I got introduced into

interdisciplinary thinking fairly early in my career.

BOHNING: What reasons were there for your making these changes?

STEIN: I think maybe seeing the artificial lightning and all of those things at the World Fair inspired me quite a bit. I was looking at my elementary school yearbook recently and I noticed at that time my choice of a career was to be a television engineer. I think this might have been influenced by my fascination with television at the World Fair. Of course that was before solid state electronics got started. I did very well in electrical engineering. I probably had the best grades of anyone but I got a little bit bored. I think it was mostly conventional electric engineering at the time and there wasn't anything too exciting going on. I found chemistry more innovative. There were more things to do but yet I had the engineering background from Brooklyn Technical High School. So my entrance into chemistry was through chemical engineering.

If the U.S. had been on the metric system I might still have been a chemical engineer, but I got disgusted with pressures in pounds per square foot and BTUs. I thought that chemistry was much more logical with the metric system. Consequently, I abandoned engineering.

BOHNING: Can you tell me something about the facilities at Brooklyn Poly at the time?

STEIN: It was back when they were on their Livingstone Street campus. The buildings were pretty ancient and falling apart. My research was done in what was called the South building, a converted morgue which still smelled of formaldehyde. One of the discussions at the time was whether the building held an electrical conduit up or whether the electrical conduit held the building up. [laughter]

BOHNING: What kind of chemistry were you doing? Even as an electrical engineer, I would assume that you were taking general chemistry, and that was a repeat of the qualitative analysis that you had done in high school.

STEIN: Yes.

BOHNING: When did you start getting into some new chemistry?

STEIN: The general chemistry course was one that was given by Roland Ward who was a very good teacher. I think that livened up my interest. I took a physical chemistry course taught by an

older professor by the name of Benjamin Caldwell. It was a very conventional physical chemistry course, but I had some very good lab instructors. In fact, it turned out that one of my lab instructors in organic chemistry at Brooklyn Poly was Sidney Siggia. He has since become an analytical chemistry professor here. It was interesting in that I was on the committee which hired him as a professor when he had been my instructor.

As I mentioned the courses were very flexible. In physical chemistry lab, we could essentially do research rather than doing experiments from the lab manuals. So instead of doing lots of little experiments, we did about two things during the semester that were sort of mini research projects.

BOHNING: Do you recall what you were doing?

STEIN: Yes, I do because it was a kind of a marriage between with my interests in chemistry and electricity. I had done an electrochemistry type research project in which we were trying to develop electrodes for analysis. We were looking at things like the silver electrode for halogen analysis. I made up solutions and mixtures of various halide ions and tried to see how I could analyze these. It was kind of fun and as a result of that, I almost became an electrochemist.

BOHNING: That leads to my next question. When did you first become interested in polymers?

STEIN: One of the good things that they had at Brooklyn Poly was that you were required to write a senior thesis. I elected to do this under an analytical chemist possibly as a result of the physical chemistry course by Gilbert B. L. Smith. One of Smith's interests was the use of electrochemistry for analysis. I was just working for him for a short while when he became ill and consequently, he gave up teaching for that year and I had to find a new thesis adviser. Raymond Kirk, who was head of the department, suggested that I might try working for Paul Doty who had just come out of graduate school at Columbia, the one that Bruno Zimm was in. I suspect I was one of the first students that worked with Doty. Paul had gotten his Ph.D. at Columbia as had Bruno where they worked with guys like Joe [Joseph E.] Mayer and Victor LaMer.

It was in the early days of doing good physical chemistry on polymers, measuring molecular weights, and things of that sort. Paul Flory had just published his work on thermodynamics of polymer solutions. One of the things they used to have at Brooklyn at that time was the Saturday morning seminars. Just about that time Peter Debye gave a seminar on his new light scattering method for measuring molecular weights, which he had developed as a result of the Rubber Reserve program during the war. In making synthetic rubber there was a need for new methods of measuring molecular weights of polymers. Debye had recalled

Einstein's work on the fluctuation theory of light scattering and applied this to studying polymers in solution. We heard this seminar and I had this encounter with Doty and Zimm shortly after that. They suggested that perhaps I could use this method for my senior thesis, which I did. It was one of the first experimental papers in studying materials using that technique.

BOHNING: That was the cellulose acetate?

STEIN: The paper published in the Journal of the American Chemical Society was the cellulose acetate work (1). At about the same time I had a paper on the classical theory of light scattering that was coauthored with Zimm and Doty (2). It was published in volume one of the Polymer Bulletin, which lasted for one year and was the predecessor for the Journal of Polymer Science which got started the next year. Essentially that got written because in preparing a background chapter for my thesis, I had to find out what the theory was all about and I worked pretty closely with Bruno Zimm in trying to understand it. That was a great experience for me because I really knew nothing about statistical mechanics when I started. It was a wonderful way to learn something about it. Writing that article was a very good educational experience for me. I still look back at that article. I'm still pleased by it. I remember Debye was very intuitive in the way that he presented things. He often gave results of his work without showing how they were derived. In fact, I remember that, having written to find out how he got some of his equations, I don't think he remembered any more but referred me to one of his students to provide the details. At the time Zimm, Doty and I wrote this article, we wanted to find out the derivation of the Debye equation for the scattering from a random polymer coil which is still used today. I'm using it this very day for neutron scattering of polymer coils. Debye gave the result but at that time, had never published the derivation of it. I wanted to find out where it came from and Zimm and I re-derived it essentially starting from scratch. That was good training because I really learned how to do things that way.

BOHNING: Had you taken any math courses in your early days at Poly so that your math background was adequate.

STEIN: Yes. I feel that learning math and science together is a very valuable thing to do. It makes math much more realistic. I essentially wanted to learn math because I needed it. I had problems that I needed math for. Consequently, I was driven to learn the math. I remember that I bought a book by Farrington Daniels, Mathematical Preparation for Physical Chemistry (3), while I was in high school because I realized that I needed calculus and I didn't know any calculus. My first calculus was what I learned by myself from that book.

BOHNING: I have a copy of that book too. Can you tell me more about Doty and Zimm? You were an undergraduate and were finishing up your senior thesis.

STEIN: Yes. Well they were young, enthusiastic, and they were both involved in these military projects too. Brooklyn was really a very exciting place because Tobolsky and Stockmayer and people like that were coming in and out. There were really important people doing work in polymers who were going through there. It was very crowded and everyone was working evenings. Lab space was very tight so you didn't get assigned a lab bench but you were assigned a lab bench between such and such a time. It was good training for me when I came to UMass because UMass was at about that state when I first got here. Many people when they came here at that time were aghast and more or less gave up, but I didn't feel so very discouraged because I was used to working in that sort of environment. I knew that I could do good work even if the conditions weren't ideal.

BOHNING: When did you meet Herman Mark?

STEIN: During the time that I was an undergraduate.

BOHNING: Can you tell me something about him at that time?

STEIN: He was the father figure there. I had never really taken a formal course with him but I would hear him lecture and see him in action. He was the reason why people would come. I remember one of the things that happened while I was there. We had some Russians visit. This was close to, if not during the period of McCarthyism and there was a lot of anti-Russian feeling. At that time Russia very much restricted travel and Americans were prohibited from visiting certain cities in Russia. As a retaliation, the State Department prohibited the Russians from visiting places in the U.S. and one of them was Brooklyn, but they could come to Manhattan, so we had discussions via the subway. We had to travel to Manhattan to see our Russian visitors. [laughter]

BOHNING: Did Herman have any influence on you?

STEIN: Sure. He was legendary. He was a fascinating guy. A much more worldly person that I had known, someone who did so many different things about this time. There was an article about his life in the New Yorker (4).

BOHNING: Did he come through the labs when you were there?

STEIN: Yes. He was extremely energetic and active. He knew everyone.

BOHNING: The war was drawing to a close at the time that you finished your studies. What were you looking at in terms of career choices?

STEIN: To come back to Herman Mark, I think one of the things he did was to start the Polymer Research Institute at Brooklyn. I remember in this New Yorker article they said, "How did you get the Polymer Research Institute started?" He said, "Well, the first thing I needed to do was to create a letterhead and that was about all that Brooklyn Poly could afford." That was something I remembered when I came here. I started a Polymer Institute here and I think I followed his example. I created a letterhead and we sort of pulled ourselves up by our bootstraps. Observing him in action and how he got things done was a good lesson to me. Doty and Zimm and Mark and so on all encouraged me to go onto graduate school. I had considered Columbia and also Cornell. I think I had fellowship offers from those two. Tobolsky was at Princeton at the time. I had traveled from Far Rockaway into Brooklyn through the four years of high school and three years of college and I was tired of the competitive, hectic life of New York City. Going to look at Princeton impressed me; as compared to Brooklyn it seemed to be a little bit of heaven. I think that is what sold me. I actually went to Princeton and elected to work with Henry Eyring at the time. Eyring had been the director of the Textile Institute and offered me a fellowship at the Textile Research Institute, which I accepted.

BOHNING: How did you make that contact with Eyring?

STEIN: Through Tobolsky.

BOHNING: Could you tell me something about that arrangement. There was a Textile Foundation.

STEIN: I think the Textile Foundation got started as a result of the U.S. taking over some of the patents from German companies during the war. This went into a fund and part of that fund set up the Textile Foundation. One of the Textile Foundation's endeavors was to set up the Textile Research Institute Laboratory, which was a laboratory independent from Princeton University. They had a building which was in a former estate on Lake Carnegie, about a mile away from the campus. They would

offer fellowships to about five students a year who could pursue Ph.D. studies at Princeton University, work with a Princeton professor for their thesis work but on a problem related to the Textile Institute's goal, which is what I did.

BOHNING: Eyring was associated with...

STEIN: Eyring was the director of the Textile Institute. In fact, when I came to Princeton, the Textile Institute was set up in the Frick Laboratory before they had the separate building. During my first year, they moved out to their building on Lake Carnegie. We were very intimately involved because Henry Eyring was a very economical soul and the way we moved was that we rented a truck and all of the graduate students moved the laboratory equipment out to the new building. I remember the road was an old dirt road that was filled with ruts and puddles. Eyring was a Mormon and lived a very parsimonious life. He had a coal furnace in his house and every morning when he came into work he would bring in a barrel of ashes from his furnace, stop on his way in to work and dump the ashes in one of the mudholes. [laughter]

I remember when I first arrived at Princeton I fell under the clutches of George Halsey, who is now a professor at the University of Washington. He was also working with Eyring. George was a very peculiar person. He got kicked out of Princeton for a while because George thought the only books in chemistry that were worth reading were the collected works of J. Willard Gibbs and Statistical Thermodynamics by Fowler and Guggenheim (5). The organic chemists at Princeton, particularly Everett Wallis were not too sympathetic with that deal. George didn't do very well on the organic qualifying exams and as a result, he got thrown out of graduate school and only got back after he repented a bit. George was very much of an individual; he was sort of a child prodigy. He took me over to Nassau Tavern for lunch every day. I was pretty naive when I first came and I thought this was the typical behavior of a graduate student. I soon learned differently. Henry Eyring would bring his peanut butter sandwich and apple into the laboratory to have his lunch and George would ask him to watch our apparatus while we went to Nassau Tavern to eat lunch. After a few weeks of this, I learned that this was not normal behavior.

BOHNING: How much of an interaction did you have with Eyring?

STEIN: Quite a bit. My first paper at Princeton was a joint paper with Eyring and Halsey (6). We tried to apply absolute reaction rate theory to the mechanical properties of textile fibers.

BOHNING: What kind of a person was he to work for?

STEIN: Extremely informal and enthusiastic. He was very inspirational. Eyring came from a western background, he was born in Mexico and he actually started out in mining engineering. According to his own story, he changed to chemistry because he was afraid the rocks would fall on his head in a mine. I remember that in his statistical thermodynamics course he started out by showing how you would calculate the partition function of a horse. But that was the approach he would take. He was very enthusiastic about absolute reaction rate theory to the extent that he would try to explain how everything in the world worked its basis. Before he came in to teach a course we would draw an activation energy diagram on the blackboard and then Eyring would just come in and fill in the details of a different problem. I remember one of the things that had a great impression on me. He said, "Give a theory for everything. You should always do an experiment with a theory to guide you. It's much better to do an experiment guided by a wrong theory than guided by none at all because then the experiment does something for you. If the theory is wrong, the experiment proves it's wrong and then you go on to correct the theory, whereas if you don't have any theory, the experiment may not do anything." I think there's a lot of truth to that and I've repeated that story to students on many occasions.

I remember presenting a paper at a scientific meeting, the American Chemical Society meeting in Chicago. I went there by train from Princeton, traveling by overnight train with Henry Eyring. We shared a room and most of the night was spent with Eyring trying to convert me to Mormonism. [laughter]

BOHNING: I thought you were going to say that you were talking about absolute reaction rate theory.

STEIN: I remember going to another meeting with Bob [Robert E.] Rundle who I worked with after Eyring left. He was a crystallographer. I also shared a room with him and spent the night analyzing the space groups of the wallpaper. In those days when you had to share rooms with your professor, you had a good learning experience which the students miss today.

BOHNING: I meant to follow up on Turner Alfrey before. You said you proofread his book. Did you have any other interaction with Turner at Brooklyn?

STEIN: Yes. I discussed research with him. One of the interactions most people at Brooklyn had with Turner was drinking with him. He was a great beer guzzler.

BOHNING: Did he extend that to a wide circle of people?

STEIN: Yes. Turner was an extremely informal guy. I remember that we invited him to Princeton to give a seminar. After the seminar he disappeared and we wondered what had happened to him. Well, it turned out that he went to Trenton to drink beer with the stockroom keeper. [laughter] Turner wasn't too selective. He would be chummy with anyone. He was a very informal guy.

A person who worked with Tobolsky when I was a graduate student was Bob [Robert B.] Mesrobian. Afterwards he went onto Brooklyn Poly, so that sort of completes the cycle. Mesrobian subsequently became vice president of Continental Can. In the memorial book for Turner Alfrey that Ray [Raymond F.] Boyer edited (7), there were many Turner Alfrey stories and one of these was involved with the fact that he and Mesrobian had a bet. They used to play pinball together when they were at Brooklyn Poly. They would bet beer but Turner was much better than Mes and I think at the end of their stay at Brooklyn together Mes ended up owing Turner something like 2000 cans of beer. When Mes became vice president of Continental Can, his company was considering canning beer. They were testing the canning of beer and ended up with a large number of cans of bad beer. Mes thought that this was a wonderful opportunity to pay back his debt. He had a truck unload 2000 cans of beer on Turner's doorstep. [laughter]

BOHNING: Was Turner's comment to that ever published?

STEIN: The story is in this book that Ray Boyer got together.

STEIN: Other stories about my Princeton experience: one involved interacting with Sam Krimm. Sam came to Princeton and he also worked with Arthur Tobolsky. We overlapped for a year and published a paper together (8). We built one of the first x-ray diffractometers together. Then I went on to Cambridge for postdoctoral work with Sir Gordon Sutherland. Gordon came to Michigan and Sam then worked for him too. As a result Sam became a professor at Michigan from which Show Ling Hsu graduated and came here to UMass. Another connection is George Halsey, who became a professor at the University of Washington and Frank Karasz, who is in this department, got his Ph.D. at the University of Washington where he was working with George Halsey. So there have been a lot of interconnections.

BOHNING: What was it like at Princeton at that time, in terms of the courses that you had to take?

STEIN: Princeton was a very selective sort of place, with a very fine group of students. One of the people who had graduated from that department was Bill [William O.] Baker, who subsequently

became vice president in charge of Bell Labs and a presidential science adviser. Jack [John D.] Hoffman, who became head of the Materials Institute of the National Bureau of Standards, was a classmate at that time. Julian Gibbs was there; he became president of Amherst College. So there were many good associations that came from that period.

Something one had to do there which had its effect on my teaching was the system of research proposals which, at the time, was fairly unique. In addition to passing qualifying examinations, a graduate student had to propose some original research. At that time, you had to make ten of these research proposals, and three-quarters of them had to be in fields different from your own specialty. These had to be ten original ideas and the final exam for your Ph.D. was really not an exam on your thesis but on these research proposals; the whole faculty would come and these would include specialists in all areas so the candidate really had a good going over. Actually, about half of my research proposals turned out to be subjects on which I subsequently did research and wrote papers. So it was extremely good training. Princeton subsequently decided that this was too tough a test for students and I think they cut it down to two or three proposals. But as a result of my experience, I recommended that this be a requirement here, which it is now in both the chemistry and polymer science departments.

BOHNING: How many do these students have to do?

STEIN: We originally had two in polymer science, but now we have cut it down to one.

BOHNING: That's quite a change, ten compared to one.

STEIN: Yes, it is. Getting those ten together really was quite a hard test. One of the advantages was that you had to continually be alert for ideas for original research. It changed one's point of view. When you got to a seminar, you would always be hungry for ideas for one of these proposals. Everytime a new journal would come out, a student would rush to look at it to get some ideas. It developed some good habits.

BOHNING: Do you think that only asking people to do one today provides the effect that you want?

STEIN: You always think that you had things tougher when you were younger. We did. I think we're being more lenient and I'm not sure it's entirely good. In a sense students are leading a easier life than we had back then. I remember Hugh Taylor was head of the department then and he was a tyrant. He had a rule that graduate students couldn't get married and if they got

married, he decided that they didn't need a fellowship and he took the fellowship away. After the students passed their exams they would make songs about the faculty members. The one about Taylor was,

"Here's to Taylor, Hughie Stott,
Says his graduates marry not,
So they must lead a life of sin,
Until they get their theses in."

Then when the GI Bill students came along, there was a changed last verse which was

"But now the Army foots the bill,
So Hughie lets them bring their Jill." [laughter]

That was sort of typical. I remember George Halsey and I going to Henry Eyring and telling him that we couldn't live on an $800 per year fellowship. We needed an increase to $1000 per year. That was typical of the life that one had to lead. One couldn't afford a car. If you did get married, the only way you could afford it was if your wife worked.

BOHNING: Just by comparison, what are your fellowships here at UMass these days?

STEIN: We're going up from $9500 to $10,000 in September. And now the average graduate student is married; and they have cars. Many times they have better cars than their professors. The lifestyle is different. I went back to Japan as a Fulbright fellow about fifteen or twenty years ago and I found at that time that life in Japan was a little bit like life was back when I was a graduate student. It was sort of like stepping back in society twenty years, when people were working real hard, were hungry for education and when becoming educated was your means of improving your status in life. In a sense, going to China a few years ago was another repeat of this same experience, seeing people going through these same stages of development. Maybe the fact that the best graduate students in this country today are the Orientals is possibly a consequence of that. Back when I was a student, the Jews were essentially the people who were making it in school. People have commented that today the Orientals are the Jews of our society.

BOHNING: When did you switch to Tobolsky?

STEIN: After my first year, Henry Eyring decided to leave Princeton and go to Utah as dean of the graduate school. He did this because of his Mormon association. He was one of the leaders of the Mormon church and he took Mormonism very seriously. He invited me to go along with him, but I felt that

Utah was more of a step than I wanted to take. Princeton was then a mecca of science; people like Einstein and [J. Robert] Oppenheimer and [Eugene P.] Wigner were all there, whereas Utah was out in the Wild West. Even though it meant changing my thesis, I didn't want to move although I had the problem of what to do next. I thought seriously about changing schools. I actually went up to Cornell and interviewed with Debye. He offered me a fellowship and I thought very seriously about accepting it, but I would have had to start again and take more qualifying exams. I decided that that wasn't worth doing.

At that time, Bob Rundle came to the Textile Research Institute from Iowa. He was an x-ray diffraction specialist. I didn't know anything about x-ray diffraction but thought it was a good chance to learn something new, so I elected to work with him, so I changed from Eyring to Rundle. We got in an x-ray diffractometer as there was no x-ray diffraction at Princeton up to that time. We set it up and started doing some elementary x-ray diffraction work. I learned some crystallography. One of the first things that we did was a x-ray diffraction study on starch, because coming from Iowa, Bob Rundle was interested in starch chemistry. He had come from an agriculturally oriented department. We published a paper together on the starch-iron complex (9).

BOHNING: That was an interesting paper.

STEIN: After a year, Bob Rundle decided that he really liked Iowa and went back there. So I had to go through all this again. I started on my third thesis with Arthur Tobolsky. I managed to get out of Princeton in three years despite having to work with three different thesis advisers. It was a good experience but one that could have been discouraging. I began to wonder whether it was me that made all of these people leave Princeton. [laughter]

BOHNING: You started Princeton in 1946?

STEIN: Actually, in the fall of 1945.

BOHNING: Then you got your Ph.D. in 1949.

STEIN: I actually left in 1948. My degree was awarded after I left.

BOHNING: So you only had three years there?

STEIN: Yes.

BOHNING: Tell me something about Tobolsky.

STEIN: He was one of this Columbia group along with Doty and Zimm.

BOHNING: So you already knew him?

STEIN: Yes. Tobolsky also had worked with Henry Eyring and he had gotten his Ph.D. with him. Tobolsky was an undergraduate at Columbia, so he was an undergraduate at the time that Doty and Zimm were graduate students. Then he came to Princeton and worked with Henry Eyring. He was schooled in the Eyring tradition so changing to him was a natural. He was building up his group in polymer science and it was at the time before he got very busy so it meant that I could easily interact with him. I think one of the reasons I decided not to go to Cornell and work with Debye was that Debye was obviously a very famous person; I got the sense that the students of Debye didn't work very closely with him. I enjoy a close association with a faculty member and I had that with all of the people that I worked with at Princeton. In the latter part of Tobolsky's career, when he became busy and famous, he probably wasn't as good a person to work with as when I worked with him.

BOHNING: I noticed in one of the papers that there was a Plastics Laboratory. Did the Textile Institute continue at Princeton and what is their relationship with the Plastics Lab?

STEIN: That was a different endeavor but the Textile Research Institute is still there. It's changed in its nature. Eyring was the first director of the Textile Institute and after he left, Jack [John M.] Dillon became its director. He was a very different sort of person. Eyring was mostly a scientist but a horrible businessman so that the finances and organization of the Institute were pretty chaotic during Eyring's reign. But I think the science was the best that ever went on there. I know that I spent most of my time trying to convince the textile people that before we looked at textile fibers, one ought to really understand what I thought was the simplest polymer, polyethylene. I managed to get the Textile Institute to support most of my work on polyethylene during the course of my career.

Dillon was a very industrially oriented person and ran a pretty tight ship from an organizational point of view, but probably with less inspiration in science, although he was one of the people who got the Division of Polymer Physics of the American Physical Society going. He was a pretty reputable polymer scientist and there is now the Dillon award which is an

award to a young polymer physicist. Now Luddie [Ludwig] Rebenfeld is the director of the Institute and I think the orientation of their research is much more industrial and applied.

The Plastics Laboratory was something that got its start with Louie [Louis F.] Rahm in the mechanical engineering department. One of the people who worked with Louie Rahm then as a student was Bryce Maxwell, who later developed a polymer engineering program at Princeton in the chemical engineering department. Again this is an interesting evolution because one of my students, Garth Wilkes, who's now a professor at Virgina Polytechnic, went to Princeton as a faculty member and worked in the polymer program. Then he left Princeton to take up a good position at VPI and has built up a strong polymer program there. Then he was followed by another one of my students, Jeff [Jeffrey T.] Koberstein who had come to me from Wisconsin where he had worked with Stu [Stuart L.] Cooper. From here, Jeff went to Princeton and spent a few years there, but last year he left Princeton and went to the University of Connecticut which, I think, has a department that's on its way up in polymer science.

I and several others who have been alumni of Princeton are concerned about the fact that the polymer effort at Princeton may be dying because of the various people who have left. Tobolsky never got replaced in the chemistry department. Maxwell retired and Wilkes and Koberstein both left because I think both of them felt that they weren't receiving adequate support and encouragement in the chemical engineering department at Princeton. So a group of us, Bill [William J.] MacKnight from our department here, who is a Princeton alumnus and who also worked with Tobolsky, and Shiro Matsuoka, who was a Maxwell student and is now at Bell Labs, and Jack Hoffman, who was Charlie [Charles P.] Smyth's student in Princeton and was at NBS and is now at MMI and building up what looks like a growing polymer program there. We all went and visited with the new dean of engineering, Hisashi Kobayashi, at Princeton to express our concern. We also talked with Bill [William R.] Showalter who had been head of the chemical engineering department at Princeton. We hope it had some impact but we don't know.

BOHNING: It would be a shame to see that happen.

STEIN: It's so easy for a department to decline and so difficult to build one up. It's a pity to have something had been so good disappear.

BOHNING: Does the Plastics Laboratory still exist?

STEIN: Yes.

BOHNING: That's in mechanical engineering?

STEIN: Yes, it's been absorbed in that department. It became a plastics program in mechanical engineering and the plastics program kind of got integrated into the chemical engineering program so it has lost some of its identity. I think seeing what has happened there and also seeing the decline of Brooklyn Poly as a leading place after Herman Mark retired and Charlie [Charles G.] Overberger left, has taught me something about what ought to be done here to prevent a similar thing from happening. I hope I'm doing things in a way to preserve the polymer group here so that it will not decline with my and other people's leaving, that will be a self-perpetuating organization.

BOHNING: What kind of steps did you take to try to assure that?

STEIN: I have less of a cult of personality, so it is less centered about a single person. I think that's very important. Brooklyn was so centered about Herman Mark and Princeton was centered about Eyring and then Tobolsky, that when they went, a gaping hole was left. Also, building up at the young level, having a strong base so that there were people prepared to fill in when others left. I think I'm very encouraged here where we have an excellent group of very fine young people. I have no concern about their maintaining the integrity of the place as long as UMass is smart enough to keep them happy by providing them with the wherewithal so that they don't leave us.

We've had three presidential young investigators in the group and I suspect there are very few groups in any university in the country that have had three presidential young investigators among their young scientists.

BOHNING: How do you find the attitude of chemistry departments both here and elsewhere?

STEIN: I think the attitudes both here and elsewhere have been very conservative. I'm still a member of the chemistry department here even though I got the polymer science department started. I felt that polymer science has a place in chemistry departments and consequently, I elected to stay in chemistry for this reason. Yet chemistry departments have not been very encouraging toward the growth of polymer science or, for that matter, any other highly applied area of chemistry. When I first came to UMass, chemical engineering was part of chemistry. There was no biochemistry, but when it developed later, it did so within chemistry. Ernie Lindsey, who became head of the chemical engineering department was a member of the chemistry department. Then the head of biochemistry came in and he was a member of the chemistry department. I got polymer science started within chemistry. But all of these were parts of chemistry and I think all of them felt frustrated in their attempts to develop within chemistry. They all split off eventually to become separate

departments, each of which is as large and as illustrious, if not more so, than the chemistry department. So I feel that chemistry has really made a mistake in not encouraging its children to develop within its own walls. Maybe it's been a mistake for chemistry, but healthy for some of these subfields that chemistry departments have served to nurture many of these fields which have since gone out and grown up and become very good in themselves. That has happened here as well as at many other places. It's still a running battle. How much is polymer science going to develop within chemistry departments? It hasn't in very many departments.

BOHNING: It seems as if polymer science has found homes in departments other than chemistry on most campuses. Recently more in engineering than anywhere else.

STEIN: That's right. In a sense, this has been a difficult problem here because when the polymer group got going here, I was the only polymer person in chemistry. I suspect that if they understood what polymers were, they never would have hired me here in chemistry. [laughter] In the beginning part of my career I built up a polymer group here and we hired Bill MacKnight to be the second polymer person in chemistry. It became evident that polymers could not develop just within chemistry. I saw the need for input from other disciplines, like chemical engineering, and the need for interdisciplinary activities. I proposed that we have a polymer institute, learning a lesson from Herman Mark. This got approved by the board of trustees of the university through the help of John Lederle, who was president of UMass at the time. It was a device that got readily approved because it didn't take any significant budgetary support. We got an agreement from the board of trustees that we could get a portion of the overhead kicked back. At that time no one worried about it because the overhead rate was only five percent; today, they're worried about it and they're changing the rules because the overhead rate is now fifty percent. You have a factor of ten coming in. [laughter]

But we were able to have this interdisciplinary program. Then Bob [Robert W.] Lenz got hired in chemical engineering, he was really a chemist but the chemistry department wouldn't hire an organic polymer chemist so chemical engineering did. Then Roger Porter came in. He was the first head of the polymer science program and was hired not in any department but as a member of the program. It was a time during university expansion when UMass grew from the 3000 or so students that were here when I started to the 25,000 students that are here today. Again, that's almost a factor of ten. Consequently, we were adding 1000 students per year during that period of time so there was a great need for new faculty. Adding people like Roger and Frank Karasz was not too difficult and they got hired as members of the polymer program. The polymer program was very successful, it grew very well and eventually became a department. Now, it's probably one of the largest departments in the country of its

kind. Roger Porter, who was hired at that time, became the head of polymer science program and when it became a department he was the first department head. Bill MacKnight, who I helped bring into chemistry, subsequently transferred into the polymer science department and later became head of polymer science. Jimmy [James C.W.] Chen, who was hired in chemistry just last year, changed departments and moved over to the polymer science department. The polymer science department has been trying to get me to move and only last year I decided that I wouldn't. I would stay in chemistry and try to preserve a foot in each camp just as a matter of philosophical principles. I feel that I don't want to see polymers die in chemistry.

BOHNING: I have some more questions I'd like to ask you about that but let me return for a moment to Princeton. You said you did your postdoc with Sutherland. What were you looking at beyond Princeton as you were finishing up your work there? Were you looking at industrial positions?

STEIN: I don't think so at that time. We had a seminar at Princeton by Gordon Sutherland who was a professor at Cambridge University in England. It was about the time that infrared spectroscopy was just getting started and he talked about infrared dichroism, which I had never heard of before but I realized what some of its potentialities were in polymer science. I thought it was something that I would like to learn something about. So I did some thinking about how I could get to Cambridge. I heard that there were National Research Council scholarships and I talked to Arthur Tobolsky about this. He said, "Why not try for one." I went to H. S. Taylor and asked him whether he would recommend me for one and he was very discouraging. I remembering him saying, "Oh, a polymer scientist would never get one of these. There's no point in trying." I did anyway. I remember one morning a telegram was delivered to me saying that I had won this scholarship. It was a complete shock and surprise. I remember telling my mother who was very conservative that I was going to England and she was pretty shocked at this. Again, it was a good thing for me to do. I remember telling H.S. Taylor with great glee that I had gotten one. He didn't believe it at first. [laughter]

BOHNING: Had you had any other plans? You didn't seem too optimistic about getting the NRC.

STEIN: I think I had pretty much determined that I wanted to do postdoctoral work. I don't think I had applied for anything else up until that stage.

BOHNING: Do you recall what time of the year it was when you got this notification?

STEIN: I think it was in the spring of the year in which I had finished. I finished up my thesis by the end of the summer and I went to Cambridge at the end of that year.

BOHNING: That would have been in 1949.

STEIN: Yes. I got my masters degree in June. A masters degree was sort of automatically awarded at Princeton. Then I didn't get my doctorate until the following year.

BOHNING: Let's talk about England. You were there only a few years after the war was over. What was Cambridge like?

STEIN: Life was very austere. I went to England by ship on the Queen Mary. Rod [Rodney D.] Andrews, who was also a student of Tobolsky also went to Europe. He worked for Werner Kuhn at Basel, so we went over on the Queen Mary together. We arrived in England and then Rod went on to Switzerland. I went to Cambridge just to leave off my bags and find out where I was living. I remember stopping in at the college and telling the porter who I was. I was informal and told the porter I was Dick Stein and for the rest of my term at Cambridge I was Mr. Dickstein. [laughter]

At the time, housing was short at Cambridge so I wasn't able to live in college but I had my meals there. I lived in a private house in town where I rented a room. The landlady, Mrs. Hayes, was a working class person, who would rent out her living room and dining room, the best rooms in the house, to a Cambridge student. I think I paid nine dollars a week including breakfast and getting my laundry done. It was a good experience meeting some English people. I remember at the time that there wasn't any electricity in the bedroom, only in the downstairs part of the house. I think Mrs. Hayes was a little concerned about my burning the house down because I had to light gas lamps. There wasn't any hot water in the house. There was running water in the kitchen but there wasn't any running water in the bathroom of the house. The lavatory was outdoors. The bathtub was in the kitchen under the kitchen table. One had to heat the water on the stove for this. Consequently, it was a lot of trouble to take a bath. You had to take it after everyone went to sleep at night and you had to heat up the water. I could understand why people in England only took weekly baths. The weather was cold enough so that you didn't smell too badly. There wasn't any heat in the bedroom and my living room got heated by a coal fireplace. The coal ration was one hundred weight, which is a little over 100 pounds, of coal for a month which isn't very much. You couldn't stay up very late because it got pretty cold. Consequently, I would spend a lot of time at the laboratory which did have central heating and was warmer, so that made me work a little bit harder. Food was rationed and life was pretty austere

at that time.

When I initially arrived at Cambridge, I just stayed for a day to find out where I was living and leave off my luggage. Then I immediately took the channel ship across from Harwich to the Netherlands when I got very seasick. I went to the Rheology Conference at the Hague where I met up with Rod Andrews. It was the first international scientific meeting that I had ever gone to. We stayed with some Dutch students at the University at Delft which was interesting. I visited Rod a couple of times during the year, mostly to eat because the food in England was pretty bad, whereas Switzerland had good food. I remember that the ration of milk in England was about a glass per week which most people used for their tea and I didn't care for tea. There wasn't any hot water in England at the time. To wash I had to get a basin of water that was supplied by my landlady. She would bring me a cup of tea in the morning when I got up but I didn't want to drink it so I got to shaving with this tea. [laughter] I don't think my landlady ever realized what I did with her tea. I found it a benefit as the tea sort of tanned my face and made it tough. It was so cold that I would be shivering and would cut myself but my face eventually got tanned from the tea and I cut myself less.

The food was pretty bad so getting over to Switzerland for some good food was a pleasant experience. A few times during the year I would go over and visit Rod. The first time I visited I got invited to give a seminar by Werner Kuhn. This was a good opportunity to meet that famous man but discovered to my horror that they expected me to give it in German, which I was very bad at. After about half a minute of trying to talk German without any grammar at all, he decided that it would be better for me to continue in English. [laughter]

BOHNING: Had Rod warned you about that before?

STEIN: No, it was quite a shock.

BOHNING: What did you talk about?

STEIN: It was pretty much about the work I had done with Arthur Tobolsky in studying birefringence. It was a good thing to do because Kuhn, working with Grün, had developed the first theory of the birefringence of polymers. We did some of the early experimental applications of this theory.

BOHNING: Can you tell me something about Sutherland. Did you interact with him much while you were there?

STEIN: Not really closely. Sutherland was kind of a mystery person. He was very far up in British science and did a lot of

traveling as well as a lot of industrial consulting. He was on a lot of government committees; eventually he was knighted and became director of the National Physical Laboratory. He sort of had a secret office where he would hide out. You did not see him very often and consequently, most of one's learning was from one's fellow students rather than from him. But the group at Cambridge was excellent. It was a very good year for me because it was a postdoctoral year where you could take courses that you want to without having to pass them, just enjoying them was great fun.

BOHNING: What did you take?

STEIN: I took a course in physical chemistry with Lennard-Jones and another one with Moelwyn-Hughes. I took a course in diffraction with Lawrence Bragg and I took one with Max Perutz. I took a course in statistical mechanics with Fred Hoyle, one in quantum mechanics with Dirac, and a course in philosophy with Bertrand Russell. It was a wonderful year.

BOHNING: That's quite a line-up of names. What about the laboratory facilities?

STEIN: They were pretty primitive. The story of my life has been going to places too early, when techniques were just getting going and laboratories were just getting developed. Infrared spectroscopy and Cambridge University was the place to do science in its very early stages. We were doing infrared spectroscopy and our spectrometer was made by essentially fastening prisms to the top of a stone tabletop using beeswax. We had to polish our own rocksalt. I remember one laboratory was in the basement of the colloid science building; that was in a department that has now disappeared; it had been set up by [Sir Eric K.] Rideal. Infrared spectroscopy was not done in the chemistry department but done in the colloid science department which was kind of interesting. This was in the basement of the old Cavendish laboratory on Old School Lane. It was a basement lab that got flooded. I remember one very anxious day when the laboratory was getting flooded and we were rescuing all of the rocksalt optics from our spectrometers. We also built one of the first double beam x-ray spectrometers and this was before DC amplifiers were developed, so we had to use galvanometer amplifiers which were extremely sensitive. I remember trying to take a spectrum on the first of May and getting some anomalous absorption bands that were periodic, at the frequency of the Maypole dance which students at a local school were doing. [laughter]

I think that having to essentially build instruments and be in at the early stages of science was good training because it made one not fear getting into new fields. I think that when one gets into a new field the instruments are crude but you have a whole new area of science to work in. Consequently, you can do

some pretty important things with instruments that might not be too good but very often the order of magnitude experiments are much more meaningful than the very accurate experiments later on.

BOHNING: Who were some of your coworkers?

STEIN: Norman Sheppard who has become a very important person in British science. Fred [Frederick S.] Dainton is another famous British scientist who was there at the time. Alister Vallance Jones was there. He is now in Saskattoon at the National Research Council of Canada, doing some very good atmospheric spectroscopy. Another one of my friends was Maurice D'Hont who became director of the chemistry division of the Belgium Atomic Energy Commission, and then, to my delight, became president of a chocolate company in Belgium. I'm a chocoholic so visiting him was one of the happiest experiences of my life. [laughter] Also, he became a millionaire, owning an estate in Spain which he lent us for a week. Our family had a delightful vacation at his estate. Sutherland became master of Emmanuel College at Cambridge. [A.E.] Alexander who was a colloid scientist and who went back to Australia and did some outstanding colloid chemistry. They were a very good group of people.

BOHNING: Had you thought about what you were going to do after you returned?

STEIN: No. In fact, I really hadn't done any looking into jobs. When I finished up the year and came back I didn't have anything and Arthur Tobolsky offered me a postdoc at Princeton with the idea that it would be something that I could just stay in as long as I would want to. We had a lot of stuff to write up that was a carryover from my time there. I spent a few months at Princeton, mostly writing and keeping my eye open for job opportunities. I went on several interviews both industrial and academic. I remember one day there was a notice on the bulletin board saying that this job was available at the University of Massachusetts, which I had never heard of before. I took a train up to Northampton and looked around UMass. I was impressed by all of the mountains and the nice country town which was really quiet and just the antithesis of New York. There was nothing much going on there but it looked like an awfully nice place to work. I think one of the reasons I might have gotten the job was that at the time UMass was quite poor and they couldn't afford to put me up at a hotel so I stayed at Walter Ritchie's house. He was the head of the department. I had supper at his house and I helped his wife, Maggie Bell dry the dishes. I think this made a good impression. They couldn't afford to take me out to dinner so I took them out to dinner in Northampton.

So I got a job offer and I had to start about two weeks after I accepted the job because Dick [Richard W.] Fessenden, who had taught physical chemistry here at UMass, became ill and they

badly needed someone to teach physical chemistry and freshmen chemistry. I took the job in mid-year in January and the course started within two weeks. I started out teaching a freshmen chemistry course of eighty people and I had never taught a course before in my life. It was sort of like diving into a pool of cold water. [laughter] I really had to pull my act together fast. But I survived it.

I remember one of the things that shook me up was that this course was given in the Goessmann Auditorium which is still here. I had been giving this freshmen lecture and halfway into my lecture a dog wandered into the lecture hall and proceeded to get sick in front of the class, which the class loved, but it really shook me up. [laughter] At that time, there was no modern physical chemistry taught here, no quantum mechanics or statistical mechanics. It turned out that I every semester I was teaching a new course. I gave the first quantum chemistry course, the first statistical mechanics course, the first kinetics course and the first polymer course. Each of these courses I had to get going so I had to learn an awful lot. I think at that time teaching loads were about twenty to twenty-five hours per week. I was not yet married. I had gone to all-boys schools for all of my career; high school, college, postdoc. and I began to sense that something was missing in my life. I remember taking the scientific point of view and asking Dr. Walter Ritchie about the relative population of men and women at the various colleges in the area. I think one of the things that encouraged me to take the job was the proximity of Smith and Mt. Holyoke. Finding a wife was one of my missions in life at the time and I did marry Judy who had been a graduate student at Smith College.

BOHNING: What other positions had you interviewed for?

STEIN: I remember the Naval Research Lab but I've forgotten the rest. I interviewed at two or three other places. My parents were very much for my taking an industrial job; I should say, my mother was, my father had died by then. In fact, one of the reasons why it was precarious whether I could go to college at Brooklyn Poly or not was that my father had died at about that time. Consequently, this was a burden on the family finances. My mother was very much for my taking an industrial job because at that time university salaries were very low compared with industrial salaries. I remember that my National Research Council fellowship in England was very generous. I think I had $4000 per year taxfree which was a fabulous salary at that time. Living in England was very cheap. The food ration was by money rather than amount of food. You couldn't spend more than a dollar a meal so you could either eat a lot of bad food or very little good food or go from restaurant to restaurant eating a dollar at each. As a result I couldn't spend more than half of what I was earning which gave me a little bit of a nestegg. Then when I came back and took the job at UMass I think my salary was also $4000 but I had to pay taxes on that so I was really earning

less than I got as a postdoc.

BOHNING: What was the department like when you arrived? Was there a graduate program?

STEIN: Yes, there was a graduate program. The graduate school at UMass was rather good but very agriculturally oriented. I think there was a study at the time that indicated that UMass was number four in the country for the number of its undergraduates who went on to get Ph.D.s. Some of the departments at that time, like food technology, were very good. They were world famous, things like cranberry juice and so on were developed by that department. Food preserving and food freezing processes were also developed by them. The president of the University was Ralph van Meter who was a pomologist. The fame of the University was its Agriculture School. Hokkaido University in Japan got developed in the last century by William Clarke who was the president of UMass. William Clarke is buried in Amherst but most people here don't know about him anymore except that there's a building named after him. He was a very famous person in Japan and every Japanese schoolboy learns about him because he went up to Hokkaido and introduced American agricultural methods into Japan. There's a statue of him in Sapporo with the words which he uttered when he left, "Boys, be ambitious." This is a story that Japanese schoolboys learn like we learn about George Washington's and Abraham Lincoln's childhood experiences. Whenever any Japanese come to visit Amherst they want to visit his grave but most Amherst people don't even know where it is. That was the kind of person that we had back then. We still have beautiful Japanese cherry blossoms right around our house because of Clarke and the people who followed him bringing these back to Amherst.

BOHNING: How many people were in the chemistry department?

STEIN: There was a faculty of about seven or eight.

BOHNING: Were there a lot of students? A lot of chemistry majors?

STEIN: There were about six or seven graduate students in chemistry but the undergraduate education was pretty strong. There were maybe about thirty chemistry majors as undergraduates. In fact, the size of the undergraduate body has not changed too greatly for chemistry majors as the University has grown. There's probably less of an emphasis on technical subjects and the great growth has been in peripheral areas. Part of the reason why chemistry hasn't grown has been that many of the people who would have gone into chemistry are going into areas

like polymer science and biochemistry and so on.

BOHNING: What about the facilities?

STEIN: Very primitive. The outstanding piece of equipment that I remember was a Beckman DU spectrophotometer. My first laboratory was a basement room in old Goessmann that had been a mill room which was a place where the agricultural people used to grind up grain to make flour. The laboratory had no electric lights and it was saturated with particles of grain. I was trying to do work on light scattering [laughter] with very great difficulty. The first thing I had to do was to get some electric lights installed. There was the Beckman DU spectrophotometer and I had to figure out what could I do for a small research project. I knew something about light scattering and realized that polymers like polyethylene were turbid because they scatter light. I figured that I could put some of my ideas together this way and I could measure light scattering by measuring the transmission with the Beckman spectrophotometer. To do this, I had to have better collimation. So I made a chamber for the Beckman spectrophotometer so that you could measure scattering by the light transmission. This device got fabricated in a local automotive garage in the center of town. I think it cost about eighty dollars which I suspect exhausted the department's research budget that year. Then I got a research grant for a couple of thousand of dollars from the Research Corporation and I thought what could I do with this that would be for the best. So I bought a Sears Roebuck lathe so I could build other equipment. I built my first light scattering apparatus to the extent of actually engraving the angular scale for measuring the scattering angle. There was a famous correction that my students had to make for the next ten years because for every angle over forty-five degrees, a degree had to be added because I left a degree out [laughter] between forty and forty-five degrees. When I scribed the scale I got tired of putting the degree marks on.

I chose light scattering because it was something that you could do pretty cheaply and it turned out to be something that few other people did. It wasn't a competitive field. Again, my training and having to make do paid off because it forced me to get into a new area of research that was innovative and I think an exciting area that turned out to be pretty good.

BOHNING: Was there much other research activity going on in the department?

STEIN: Not really a great deal. There were three or four graduate students working with some of the faculty. Harold Smith in physical chemistry, who is now retired, was doing some work on coordination complexes largely using the Beckman spectrophotometer. John Roberts, who's also retired, was doing some work in analytical chemistry. George Cannon, who was an organic

chemist, was doing some work on organic synthesis, mostly classical organic chemistry. That was about it. During the course of my postdoc career, I had a fellowship. During my graduate career, I was supported by the Textile Fellowship and during that six months that I was back with Arthur Tobolsky. This was slightly after the Vannevar Bush report got written. The first government organization to support research to any extent was the Office of Naval Research. Before then, university research was pretty low key, there wasn't much government money. I got supported by an ONR contract at Princeton. When I came to UMass, and I got off the ground after my first year, I applied for ONR support. I got an ONR grant and I suspect this was one of the first government research grants at UMass. I got steady support on this for about twenty years at UMass and then that petered out with other support coming. The last couple of years I've gotten some ONR support again.

I found that it was an interesting experience in bureaucracy to get started in sponsored research at UMass because I discovered to my horror that the laws of Massachusetts were such that any income coming to the University went directly into the State Treasury in Boston. This meant that the funds from the ONR contract just went to the State Treasury and there wasn't any mechanism by which the money could come to me. This was a pretty bad situation for awhile because I had to do the research but got no money to do it. Then the dean of science at that time, Charles Alexander, who was a carryover from the agricultural school, he was a famous entomologist, came to my rescue. He pulled some strings and got some special act passed by the State Legislature to make an appropriation to me that enabled me to carry out my research. At that time we figured out how you could do business and get around this by use of trust funds. We had this money go into a trust fund account rather than to the State Treasury. This proved to be a mechanism that is still in existence and that enabled many things to be done at the University that was not possible at that time.

BOHNING: Was that Alexander's doing or were you involved?

STEIN: I helped with it. It was a mutual effort.

BOHNING: He was certainly sympathetic to your problem.

STEIN: Yes. UMass at that time was very much of an undergraduate oriented institution. Any research that was done was gotten by pirating funds from undergraduate teaching. When we got visited by anyone from Boston, we kept secret the fact that we were making any research progress because if they learned about this, they probably would prohibit us from doing it. They would think we were squandering the taxpayer's money. We had to keep it quiet that we were doing some research in our spare time.

I think the real breakthrough came with one of the

presidents of the University, Jean Paul Mather, who got through the State Legislature something called the Freedom Bill. It used to be that all expenditures at UMass had to be processed through Boston. Boston was a place that had very little interaction with UMass. I think Bostonians viewed Amherst as something out in the country and barely knew we existed. The University was sort of run by Boston but there was no appreciation for what happened here. All of our ordering and processing of appointments and so on had to go through Boston. It was just impossible to carry on a first-rate research operation by this kind of a process. If you wanted fifty dollars worth of something you had to get this order through Boston. Purchasing in Boston was very graft-ridden at the time. That's probably why they wanted to keep it there. Often it would take two months often to get an order filled and you just really couldn't do first-rate research. Jean Paul Mather was able to change that. He was able to get purchasing, hiring, and so on done in Amherst. I think few people today in Amherst really know who he is anymore but I think he was the president at the University who really devoted his career to getting this through the Legislature. Without that, UMass never could have become a research university.

BOHNING: What was happening at the same time in other departments? You were trying to build up your research efforts in chemistry. There wasn't much else happening. What about other departments on campus? Were young faculty coming in, trying to get research done too?

STEIN: There were some good people here and there at the University. Physics had a few shining lights. I remember I would go over to physics when I built my light scattering apparatus, it was an apparatus with a vacuum tube amplifier. I think I still have a letter from Bruno Zimm in my files where I asked him how to put this amplifier together and he was giving me very explicit directions about how to go around to a local radio shop and get a number 30 tube or something like that. [laughter] I remember in building this I needed an oscilloscope. There wasn't one in chemistry but physics had an oscilloscope. I went over there to use it and I remember seeing some interference in the oscilloscope. I traced this down to the fact that the department head in physics used to shave in his office with an electric razor and the interference was due to that.

There was not a great deal of research but some very good teaching professors in physics and also in engineering. George Marston who is still living, was a Dean in engineering at the time. He really made a good effort to get a quality engineering school going. There was a lot of comradeship at the time: I think that one of the virtues of being poor or in having to make do is that people feel very close to each other. There was a very good spirit, everybody knew everyone else and it was a very enjoyable time of life. Certainly, very austere and life was hard but it was an experience I treasured.

BOHNING: Did you continue to teach freshmen chemistry or did you start to develop the graduate physical chemistry curriculum?

STEIN: I taught chemistry for maybe ten or fifteen years until freshmen chemistry got so big and organized that there wasn't too much room for individualism anymore. I didn't enjoy it as much as I used to.

One of the jokes in the family is that I married my wife for her money because she had $500 and I didn't. [laughter] We bought our land, which was a half a block away from the university, and it cost less than $1000 at the time. That's gone up by much more than a factor of ten today. At the time, we had to do the landscaping ourselves. I noticed that they were repairing one of the streets and you could buy fill cheaply. So we arranged for these trucks to dump their fill in front of our house. I spent my weekends shoveling the fill to landscape our property. It's the property that we're still living in. Unknown to me, the president, Jean Paul Mather, had observed us in operation. He was a great political stumper and I discovered in his speeches around the state trying to get autonomy for the University he was talking about this University professor who came to us from Princeton and had had to spend his time shoveling gravel. [laughter]

BOHNING: When did the growth start within the department?

STEIN: For my first eight years here there was no salary increase, no promotion, no growth. Then public education began to take off. There was a big influx of students due to the bulge in the birthrate. UMass was developing at this very great rate when we were growing ten percent a year and the department began to grow along with that.

BOHNING: Had you ever got discouraged during those eight years and thought about looking elsewhere?

STEIN: Yes. I actually went on some interviews. I took an interview at the University of Mississippi. This was a time when Mississippi was very conservative; there was segregation. I remember people actually spitting at me because I went into the black men's room. That turned me off so I thought New England was still pretty good for me.

BOHNING: You published a number of papers on light scattering in the early years. Was this work done mostly with undergraduate students or with graduate students?

STEIN: With both. In fact, there was an interesting story. I mentioned Walter Ritchie was the first department head and the person who hired me. One of my early light scattering projects was trying to look at the starch-iodine complex by light scattering. This came from my association with Bob Rundle at Princeton. I thought here's a technique I have and here's something I know about. I tried to put the two of these together and it turned out to be not a very good thing to do. It was a very difficult problem for the primitive state of the art at that time. People have since gone back and done much better with modern techniques. We wanted some amylose and at the time you couldn't go and order amylose. I went down to a local shop, Louie's, which had just expanded and became a big supermarket and bought a bushel of potatoes. I had this undergraduate, Carolyn Kendrew, who was working for me. Carolyn was trying to make starch from these potatoes. She was in my laboratory peeling potatoes when Walter Ritchie walked in. He was kind of skeptical about people doing research and he shook his head and said, "It's bad enough you buy your potatoes on your research grant, but you've got your students peeling them for you." [laughter]

But making do with research was something that I learned about. When I was in Cambridge we had to do some work in spectroscopy on deuterated polyethylene, which was hard to come by. I mentioned that Sutherland was someone who had good government connections. He had known about the fact that British commandos had made a raid during the war on the Norwegian heavy water plant where Germany was trying to build an atomic bomb. They had a reactor there and deuterium was the moderator of the reactor and deuterated polyethylene was one of the materials. He knew there were some samples of deuterated polyethylene in a safe somewhere in the British Secret Service. He got some of this and we did our first spectroscopy on deuterated polyethylene with it.

BOHNING: Does that show up in the literature as your source?

STEIN: I don't mention it in the literature. [laughter]

BOHNING: You also started some industrial connections very early.

STEIN: Yes. I became a consultant of Monsanto at the time. Monsanto had a plant in Springfield which was a principal plastics manufacturing operation. When I gave my course in polymer science, there were a dearth of good students so I decided that I would give this class late afternoons or evenings because there were some people in Monsanto who wanted to attend. Actually, I had more Monsanto students taking my course than UMass students. In that way I developed an association with Monsanto and I have been consulting with them for about thirty-five years. On my last visit to Princeton I commented that I've been with Monsanto longer than with anyone I've ever consulted

with. Last year I had dinner at the chancellor's house that was attended by Richard Mahoney, who is now president or chief executive officer of Monsanto. He's a UMass graduate. I had completely forgotten about this but he commented at dinner that he was in my freshmen chemistry class in 1952 and I gave him a D. [laughter]

BOHNING: You had a paper in the Journal of Chemical Education on inflating balloons (10). It was a very intriguing paper but I was curious as to how that fit into everything else that you were doing?

STEIN: There are some interesting stories on that one. There was a field day custom. The chimes in the old chapel would be rung to cancel classes. All the students would go off and drink beer. I decided that since I had nothing to do this day, I would have some fun playing around in the laboratory. I got curious. My kids were young at the time and I remember trying to blow up Mickey Mouse balloons and wondering why it was always difficult to get the ears inflated. You had to squeeze the balloon to get the air to go into the ears. I tried to figure out why this was and thought that this must mean that it's hard to get the inflation of the balloon started. So I rigged up an experiment where I hooked up a balloon to a simple water manometer, a U-tube that was filled with water, or it might have been ethylene glycol to give a little bit more density. I just blew up the balloon and watched the pressure in the balloon and discovered to my great surprise that the pressure would go through a maximum. It would first go up but then when the balloon began to inflate, it would drop back down again. So it was immediately obvious why it was difficult to get the ears inflated because the body of the balloon got over the maximum before the ears did. Then the body of the balloon would be of a lower pressure than that necessary to get the ears inflated and they never would inflate.

In this paper in the Journal of Chemical Education, extended rubber elasticity theory showed that this is predictable and I had a curve in there comparing prediction with experiment. That worked out pretty well. This suggested some simple experiments that have been fun to do, like hooking two balloons together with a valve; a big balloon and a little balloon. You'd open the valve and to everyone's surprise, the small balloon would get smaller and the big balloon would get bigger. I remember taking this apparatus into the local bar one night and paying for drinks for the night by having people bet on what was going to happen when I opened up the valve. [laughter] Ninety percent of the people would make the wrong guess. That was kind of fun to do.

There were some good after-results of that paper. I remember getting one letter from a person at a medical school who wanted reprints. It turns out that the development of an aneurysm in the brain is the result of an elastic instability that is very similar to this one. When you blow up a bubble in an artery, it continues to grow because the pressure begins to drop as it gets bigger. The inflation of the cells in a foam

plastic obey the same principles. It's a good illustration of how an everyday phenomenon can have applications.

I also got a paper to referee from the Journal of Chemical Education from someone who wanted to write a sequel to my paper. I got a big chuckle out of a remark in the paper and I had great appreciation for what this guy said. It was from a university that was even more in dire straits than the University of Massachusetts. He had a footnote in the paper saying that he was forced to give up this research because his department could no longer afford balloons. [laughter] I don't know whether it was tongue in cheek but that was kind of funny.

BOHNING: Were you able to continue attracting students from the local industry to help build the program?

STEIN: Things have changed. Now, very few people come from Monsanto because the growth of that laboratory has dropped. In fact, Monsanto just sold its polystyrene effort to Polysar. Most of the people of Monsanto have either gotten a polymer education somewhere or else they've taken our courses. They're not doing very much hiring, so the number of such people from industry has declined. I'm now engaging in an experiment to do it differently. I've just obtained a grant from an organization called Bay State Skills for developing a video-cassette course. I'm preparing this now on the electro-optical properties of polymers. UMass has what's called the Video Instruction Program where people can get University credits by videocassette courses which the University mails out. We're going to try giving this course on electro-optical properties of polymers and this is supported jointly by Bay State Skills and a few other companies. We're hoping to be able to have a nationwide clientele to take this course. The University now has a TV satellite transmitting antenna so we could transmit worldwide and could give it in real time. I think this is probably the way in which teaching is going to go. I find some of the thoughts about it very exciting; next week I'm going to visit Xerox in Rochester with one of my students and a few technicians. We're going to do some filming of the laboratory and industrial scenes at Xerox. We have some guest lecturers and we're thinking of putting these kinds of sequences into such a course. It means you can do things with teaching that weren't technically possible before. Also, when you think of the possibility that you could have, in real time, Japanese and Chinese students in your course, the clientele would be extremely large.

BOHNING: How do you find the attitudes of your colleagues towards the new teaching techniques? Are they receptive to that?

STEIN: Not as much as I would like to see. Some are but most are conservative. In fact, you can almost see the attitudes of people by those who adopt personal computers. The older members

of the faculty don't use personal computers, they still dictate letters to secretaries. Young people don't bother with secretaries anymore; they have PCs in their office and they type their own letters and manuscripts. I do this myself and it's a way of keeping young. I think it's kind of fun to learn something new. It's just a difference in attitude between the young people and old.

BOHNING: I suppose that that difference has probably always been there. The phenomenon is not necessarily new but it is more pronounced because the technology is so different. In reading your biographical statement, I have a quote here that I would like to follow up on. You said it became evident that to serve the needs of the developing polymer community, a larger university effort was needed. I'm not sure when you realized that and what steps you took that then led to the 1961 formation of the Polymer Research Institute.

STEIN: I think it was evident that there was some sort of a peak in industrial research efforts. You look back to the great industrial laboratories, places like Bell Labs and Du Pont; when Flory and Carothers were at Du Pont and Langmuir was at General Electric. Two or three of our faculty are veterans of those days at General Electric. Those kind of industrial laboratories don't exist anymore, so the problem was where to get good fundamental research done. I think the universities have to do it but it's difficult to do it in the context of the usual university structure; we need new mechanisms to encourage this. I'm still not sure what the answer is but part of my effort in trying to get a Polymer Center going here was to proceed in that direction. I just got a letter from one of my Japanese colleagues, in a technical university in Japan. He feels that we're probably the outstanding place for polymer education now. Coming from the Japanese, I take that as a very good compliment.

BOHNING: What was it like in those early days of 1961? You said your first job was a letterhead following Herman Mark's advice. What was it like to get the real thing off the ground?

STEIN: It was very slow and took a lot of patience. More things went wrong than went right but you just had to hang in. One of the things one needs in life is patience and persistence to stay with it.

BOHNING: Did you have administrative support?

STEIN: Never enough.

BOHNING: What about the department? What was the attitude of the chemistry department?

STEIN: Liberal. I think the chemistry department has always been tight on money. They still are, but I think that they have been relatively generous with me. I have essentially been given the freedom to do whatever I want with regard to teaching. I suspect I am a stronger personal task-master than any university administrator might have been, so I think the University hasn't done badly in this way. I think that the chemistry department had no complaints about it, indeed they have been very tolerant of what I have done. I think there have been some jealousies, of course; as people have seen what has been accomplished. That was inevitable and I have a relatively thick skin about that. But they gave me the Goessmann Professorship which is the most distinguished professorship in chemistry, named after Charles Goessmann, who was the first professor of chemistry at UMass. I think that was nice and one of the reasons why I decided not to move into polymer science from chemistry. I feel warm toward chemistry. Lou [Louis D.] Quin, who is the new chemistry department head is a breath of fresh air. He's trying very hard to build up the department and I didn't want to abandon him when he's just in and is trying to do something good. I want to be supportive.

BOHNING: From the time that you had established your research institute, did that help you to bring in more research funds and external funds?

STEIN: Yes. It developed some coherence and an image that would not have been possible had it been just a departmental effort alone.

BOHNING: How long was it before you developed your program and you actually started giving out degrees?

STEIN: The way it worked is that when we established the polymer science and engineering program, even though there was no department, we could give an interdisciplinary degree in polymer science and engineering where the thesis committee would involve people in the participating departments. It was successful and it gradually got more and more popular and grew to the stage where the administration finally said that this is really a department and it shouldn't be a program.

When we first got started we reported to the dean of the graduate school. When we became a department it was felt that as the dean of the graduate school doesn't administer any other departments other than the graduate programs we should report to a normal academic dean. The problem was should we be reporting to the dean of faculty of arts and science or the dean of

engineering. We were courted by both deans but we essentially had to choose which dean we wanted to belong to. We went the arts and science route probably largely on the basis of the personality of the deans at the time. I think it could have gone either way.

BOHNING: You commented that there was a critical mass of faculty that almost helped attain self-perpetuation. Can you put a number on that critical mass?

STEIN: Around four or so. It's probably why the Princeton effort isn't going because they don't have the critical mass. One of the bits of advice that we gave to Dean Kobayashi is that in order to keep someone, you have got to get a critical mass. We recommended that what they really needed to do is to make a major appointment and have a few minor appointments. You bring one polymer scientist into a department with no others and he just doesn't survive. He has to interact. That's why I think that Garth Wilkes and Jeff Koberstein didn't stay at Princeton. Garth went to VPI and teamed up with Jim [James E.] McGrath. The two of them are sort of replicating the kind of thing that we've done here, maybe learning something from me the way I learned from Herman Mark.

BOHNING: Do you think that's probably a function of the fact that it's so interdisciplinary?

STEIN: Yes. When I first got started in polymer science and I needed a polymer, I took some industrial sample but you can't do good research that way. The field has gotten sufficiently sophisticated that you have to be working on a sample that is well characterized and you need the help of others to provide this. One faculty member can't be an expert in synthesizing materials and characterizing them and doing new physics on them. The joke used to be that physical chemists did good measurements on bad things and organic chemists did bad measurements on good things. Today, to really be successful, you have to do good measurements on good things and that takes a joint effort.

BOHNING: You were active in developing a computing facility. When did that take place? How did that develop?

STEIN: I'm not sure exactly when. It was in the early to mid 1960s. It was done just out of need. One of the early papers which I published in Journal of Chemical Physics was a calculation of essentially the polarizability of normal paraffin vapors (11). Actually Kuhn did a theory to calculate the birefringence of a molecule by adding together the bond polarizability tensors. This could be used for calculating the

polarizability of a solid polymer that was oriented. A lot of the work I did in my doctor's thesis on birefringence was based on this.

This theory was subsequently improved by Flory. As it turned out, this theory predicted that there should be zero birefringence for polyisobutylene. I published in my Ph.D. thesis the fact that this was not so and that polyisobutylene had a pretty large birefringence. Flory didn't believe this because it violated his calculation. He essentially repeated my thesis with a postdoc and confirmed our results and that interaction resulted in a very good friendship with Paul.

One of the things that we tried to do was to step backwards. We realized that if we wanted to understand the birefringence of polymers, we ought to be able to understand the anisotropy of polarizability of things like ethane and propane and butane. I began to look into the literature and found most of the work on that was by people in India, essentially students of Raman, using fairly primitive techniques. Sunlight was their principal light source and most of the data was published in a book by Bhagavantam on the scattering of light and the Raman effect (12). If they had gone on to test some of these theories quantitatively, they would have found that the data just wasn't good enough.

It turned out that we had to measure some of these things ourselves. The way to learn about the polarizability of small molecules is to measure the depolarization of scattered light of their vapors. So I started some experiments doing this and collaborated with Bob [Robert L.] Rowell, who was in the chemistry department. Bob was interested in light scattering because he did his doctor's thesis at Indiana with [Frank T.] Gucker on the scattering of light from colloidal particles. We had an NSF grant where we built an apparatus to look at the scattering and we also did some calculations. We looked at how the polarizability of the normal paraffins went up the entire homologous series, from methane to ethane and so on. We compared this with good experimental measurements. We did this before the laser (13), and then we did it over again when lasers got developed (14). It turned out that the correlation of theory and experiment were pretty bad. It didn't work with the simple cases and consequently we weren't very surprised that there were problems with polymers. Then Bob and I tried to extend this. It became very apparent that what I published in the theory in the Journal of Chemical Physics was well worked out. The calculations were worked out using a Marchant mechanical calculator. I got up to $C_9$ and I wanted to go to $C_{10}$ so I hired a student to do this. It took him a whole summer on a mechanical calculator to do the calculations, which I didn't altogether trust and realized that this was the limit. We had to do something different. I went down and used the vacuum tube computer at IBM world headquarters and realized then that computing was something wonderful. When I was at Princeton I heard von Neuman give a seminar. That kind of planted a seed in my mind. Bob Rowell and I decided that the University needed a computer, and we tried to figure out how we could go about it. There wasn't any source of money but we found that we could rent

a computer for something like $30,000 per month. If we got enough people together to work with us, maybe we could raise $30,000.

We finally got approval to do this and we managed to pay, nervously, the bill each month. We got an IBM 1620 punchtape computer which we installed as the first University Computing Center in the library of the chemistry department. I was chairman of the University Computing Committee and Bob was director of the Computing Center; we essentially operated this ourselves. As it grew the computing science department got started and a permanent head of the Computing Center was brought in. We got better and better computers. At that time, Ed [Edward] Moore became dean of the graduate school and was very supportive [in this venture]. We were going to get a real professional quality computer and the big choice was whether we should get an IBM or Control Data. We finally got a Control Data 3600 but the two companies were courting us. We were having a good time. They were treating us to Broadway shows and things like that. Bob Rowell, Dean Moore and I were visiting these various companies to try to make a decision. As I said, we went the Control Data route. We also had to decide whether we were going to have academic and business office computing separate or together. We decided to do it separately which proved to be a wise decision. The computer science department got started. At that time, people who knew what they were doing took over and I became a consumer rather than a producer. History took over from there. Now we have a very good computer group here.

BOHNING: In terms of your international activities, I wanted to ask you about your early connections with the Japanese and how that originated and also about your first trip to China.

STEIN: The interaction with the Japanese got started with a person that I had met when I was at Princeton; Herbert Leadermann. He worked for the National Bureau of Standards where he did some of the very early work on mechanical relaxation of polymers, superposition of relaxation spectra and so on. Tobolsky knew him and he came to visit. Leaderman was one of the first Americans to ever go to Japan and interact with Japanese polymer scientists. The Japanese polymer science was done by people like Sakurada and Horio and mostly in textile schools. The Japanese were in the silk business and that was where the good work was being done. For example, Sakurada established polyvinyl alcohol as a commercial fiber which was one of the first synthetic fibers to be used in Japan.

BOHNING: How long after the war was this?

STEIN: This was very soon after the war. Horio got to know Hiromichi Kawai, who was co-winner of the American Physical Society Prize in polymer physics last year. Kawai was a

professor in what was then the department of textile chemistry and then became the department of polymer chemistry in Kyoto University. He was one of the first polymer scientists to come over to the U.S. to study. Leadermann suggested that he ought to work with me, which he did. That was in the early 1960s, and when he went back Professor [Shigeharu] Onogi came and worked with me. He was the second Japanese in the group and Onogi was the guy that I got a letter from yesterday. Both of these people have retired now. Then their students started coming. One of the early students was [Takeji] Hashimoto who was a co-winner of the Polymer Physics prize. He's now a professor at Kyoto University and now Hashamoto's students are working with us. When I was over in Japan in January I had dinner in Tokyo with a group of about 25 people who had worked with me but there must be 50 to 100. I've lost track.

BOHNING: There were at least three scientific generations.

STEIN: With China, it was before we recognized China. It was just after the era of ping-pong diplomacy and Nixon and all of that. There were missions to China in various fields. China was just being opened to the west. We didn't have much idea what the state of science was in China. The first mission in chemistry was a group of twelve chemists and chemical engineers, representing various disciplines in chemistry, and Glenn Seaborg was the chairman. Paul Flory was supposed to be the polymer scientist to go but for some reason he had to drop out. I had read in Chemical Engineering News that this delegation was going. They were soliciting members but really this was just a formal solicitation. The official members had already been selected but I thought this was an interesting thing to do, so I had written in saying that I would like to be considered if there was an opening. Nothing happened because Flory was already chosen but when Flory dropped out, they had to get someone quickly. There was my letter sitting there; I guessed they checked with Paul who knew me and said that I would be a good person. So I got a call one morning asking me if I wanted to go to China, which was a big shock because going there was pretty unheard of at the time. I called home to my wife to ask her if I should go to China, but she was uncertain. They wanted an answer that afternoon so I had a couple of martinis with my lunch and was feeling good so I called back and said, "Sure, I'll go.", and I left the next week for China.

BOHNING: What year was that?

STEIN: I don't remember but I think it was about two years before China was recognized.

BOHNING: Can you tell me something about the trip?

STEIN: We visited all kinds of places in China, steel mills and coal mines, fiber factories. Things were very primitive. The factories were built by either the Japanese or the Russians during their periods of occupation. China was sort of making do, just trying to keep them going. Mao was still in power then but the Gang of Four was just about to discredit him. People would refer to the Gang of Four and hold up five fingers, the fifth finger being Mao. I remember visiting a nursery school in a commune and the children were playing games stamping out the Gang of Four. They had pictures of these four on the floor and the children were going around, stamping on these pictures.

China was just recovering from the cultural revolution. In fact, it wasn't completely over and people were unsure about whether they should talk or not. You would hear all kinds of sad tales. One of my students wanted me to try to find his uncle. He somehow got word to him and this guy called at my hotel; he wanted to visit me. The student hadn't seen any of his family in China for thirty years. This guy told me the sad tale of his wife being under such pressure that she committed suicide. It was kind of surprising that people talked as frankly as they did although the state of electronics in China was such that they couldn't really do any eavesdropping so people didn't worry too much. I heard many tales of that sort. There were a few Chinese-Americans in the group. One of them was the head of chemical engineering at MIT. Another one was the Chinese-American from Calfornia [Yuan T. Lee] who just got the Nobel Prize in chemistry a year ago.

At any rate, this was kind of a heart-rending experience because these people were seeing family, brothers and sisters that they hadn't seen for twenty years. It was really such a wonderful time to be there because it was completely unspoiled then. Today, it's just another tourist place.

BOHNING: What kind of chemistry did you find?

STEIN: Very good and very bad. There were very sophisticated projects in pharmaceutical chemistry. One of the people in our group was Ron [Ronald] Breslow from Columbia, who was interested in pharmaceutical chemistry. He was curious as to whether these Chinese folk medicines had any basis. He looked pretty deeply into the kinds of things that people were doing in studying these herbal medicines and he thought it was pretty good stuff. In another area, I think there was some good work being done in rare earth chemistry. China has very abundant supplies of rare earths, and they were trying to find some applications for these by investigating their catalytic properties. One of the things that I've been thinking about recently is to do with some of these rare earths and minerals being high temperature superconductors. This might affect science in China.

Glenn Seaborg was interested in what the Chinese were doing in nuclear energy. No one knew at that time whether China had an

atom bomb or not. I think one of Glenn's secret missions was to try and learn what he could about this so, as a result, we visited places where they were doing a lot of nuclear work. The Chinese were very guarded in what they would tell us. Glenn was just trying to put two and two together, seeing the vacuum techniques and power sources, separation techniques they would need. I think Glenn thought that it could be possible. The Chinese situation struck a note of familiarity; they were making do with very primitive apparatus doing some very good things in selected areas by being very ingenious and working very hard. I think they were able to put an atom bomb together on this basis.

BOHNING: Was this work being done in an academic environment?

STEIN: What was academic and what wasn't was very confused at this time because universities as such did not operate during the cultural revolution, and they hadn't gotten started again when we were there. All of the universities were very practically oriented and each university was essentially being operated as kind of a little factory. The students were manufacturing things. They really had to build their own apparatus. For instance, people were trying to do gel permeation chromatography. The head of the Institute of Chemistry in Beijing was Renyan Qian and one of the things that he was doing was trying to make GPCs for separating polymers. Essentially he had to start from scratch and make his own columns and his own crosslinked polystyrenes. I've since had some Chinese students work with me on light scattering. One of the things that they showed me on my second visit to China, which was before they came over, was a light scattering apparatus which they had built following my papers. I subsequently learned that they had worked day and night for two months to get this apparatus ready to show to me when I visited. They did some very fine work with me when they came here.

BOHNING: Did they have access to the scientific literature?

STEIN: Not much. They got what they could by hook or crook. At that time, they didn't respect copyright so they got photocopies of American journals legally or illegally, by whatever devious routes they could, but it was very spotty.

BOHNING: Is the relationship with the Chinese and this Institute pretty strong now?

STEIN: We went through a period when we were getting inundated by Chinese. I think we had twenty mainland Chinese in the department at once, which is a significant fraction of our graduate population, something like one-fifth. We were really

concerned about this. These were mostly people who were sent over who had their education prior to the cultural revolution so it was well founded. China had a very large effort to get these people retrained because they would then be the teachers. Most of these people have now been educated in the west and they're busy trying to educate those native Chinese who couldn't really get any education at all during the cultural revolution. So, after that first wave of people, there was no one to come over because of this missing gap in education. Now, they're beginning to trickle over in reasonable numbers. Perhaps five people a year in the department from Mainland China. I have one girl in my research group and I'm really not sure what her connections are because she just applied like an ordinary graduate student and I'm supporting her by a fellowship. She's being treated no different than any other student; she is a very good student. I think the relationship is more normal now.

BOHNING: How is the preparation the new ones who are coming in?

STEIN: This girl is excellent. They are awfully well selected. Of course, there are a lot of Chinese and very few get here so those that do are usually very good.

BOHNING: Could you tell me something about the Polymer Center of Excellence?

STEIN: This is a state organization. The Dukakis administration has been very high on trying to stimulate technology in the state. Massachusetts is in a good economic situation because of its emphasis on high-tech. I think that Dukakis and particularly the person who was his secretary of economic affairs and is now lieutenant governor and probably will someday be governor, Evelyn Murphy [are responsible]. The Center of Excellence was her inspiration. She told me that she saw the effort we had here in polymer science and this was one of her inspirations to get this organization going in the state, which essentially has five groups in various disciplines. Polymer science is one. There's one in biotechnology and one in marine science among the others. These are not physical centers but actually an organization, which provides supportive funding, and is a combination of a nonprofit group, usually a university, and an industry, each of which is supposed to make a contribution to the research project. The state then provides matching money. I'm a member of the technology board of the Polymer Center of Excellence. The chairman of this board is Joe [Joseph G.] Wirth, who is vice president and manager of the General Electric group at Pittsfield. I'm vice chairman of the board. We just met last week and we approved about a half million dollars worth of funding that will probably be supplemented by half a million dollars in matching money. So we have about a million dollars worth of funds distributed among eight projects, selected from

maybe about four times that number. Each of the other centers is doing something similar. It's sponsoring something like $5 million worth per year. We got a doubling of funding from last year's effort and I think it's a very innovative, good thing. The quality of the research being done is quite high, in a variety of fields.

BOHNING: The Commonwealth of Pennsylvania operates the Ben Franklin trust which sounds very similar to what you're talking about. Could you tell me something about the new polymer center that's coming here?

STEIN: The Polymer Center is essentially a polymer building. It was given the name to help in gathering money. The psychological aspects of all these things was something we dwelled on before. Right now we're space limited; we're very crowded. The chancellor, Joe [Joseph D.] Duffey, has been very supportive and this year managed to persuade a wealthy alumnus of the University, who is not a chemist actually, to give an endowed chair to the University of over a million dollars, which is being given to polymer science. We will be able to hire a distinguished professor but we've decided not to do so until we get our new building just because we don't have the space. Frank Karasz just got one of these university research initiatives, a Department of Defense grant. We've essentially had to put a trailer outside to have any space for this, so we're very tight on space. The new building is really what we need. The department has gotten a reputation such that research money has been coming in reasonably well, but getting money to build the building is hard.

So, the Polymer Center is a building. Through Dukakis and our interaction with him, there was initially a state appropriation of $6 million towards this. We estimated at the time, the building we want would cost something of the order of $20-25 million. I made an effort to try to publicize our need for this with fund raising and so on. I think this is something that took our chancellor's fancy and we made the rounds in Washington to see what we could do. Paul Flory and I jointly went to visit George Keyworth a few years ago, when he was Reagan's science advisor, to look at funding. It was kind of discouraging. I think all of these people realized that it was a good cause but there just wasn't any good source of putting money together for bricks and mortar. The scientific structure of the Reagan administration is such that there is no good way to fund buildings. Then we proceeded by the infamous route of the pork barrel. Actually, one of my friends who had been dealing with this Washington organizational lobbyist that has gotten infamous from this activity, put them on our tail and they came to visit me. I met with them and was not very enthusiastic about it. At the time they looked to me like a bunch of operators. They wanted us to join forces with another university that I won't name but I didn't think was very distinguished. However, it had good political connections so that they thought we might be able

to make more of a clout by having two Congressman with power working together. Our chancellor was very politically oriented man, he was an Undersecretary of State in the Carter administration and his wife is a Washington lobbyist, and he was very enthusiastic about this approach. At the time, I wanted to wash my hands of it. I had a big fight with most of the department because they developed enthusiasm and I didn't. I essentially said that if they want to do it, go ahead, but I wasn't going to cooperate. The chancellor realized that if I didn't go along, it probably wouldn't happen. He said that he would talk to the lobbyists again and the lobbyists agreed to do it the honest way, in a way that would have peer review, and I agreed with that.

Although they tried, the problem was that there still wasn't anything that they could do. I was having some surgery in the hospital a couple of years ago and I got a call in the hospital from the chancellor. He was calling from a radio telephone from a car in Washington saying that he was with our Congressman who could get us $10 million but by the pork barrel route. If we proceeded by the legitimate route, there was no way within the next few years and maybe never. He wanted to know what he should do because he wasn't going to use by the pork barrel route unless I agreed. I was just completely frustrated and I said, "Well, okay." So, it worked, we got our $10 million and then we got $5 million more and it looks like more is coming. I was interacting with Paul Flory during all of this time. Paul was very much against pork barreling and I kept him abreast of what we were doing. I think we both thought similarly about this. Paul didn't have any solution either and I think he was pretty frustrated. But last year, I got invited by a Congressman to give a talk on polymers to the Appropriations Committee of the House of Representatives. That got us $5 million more. Now, we not only have assurance for $25 million but now we're talking about putting up a building of $30 to $40 million.

So, it's happening. I never felt happy about the way that it happened but, again, I think the problem is with the system. One just has to get around the system; I tried my darndest but there just wasn't any other way. In a way, I decided the end was worth the means. It's the kind of argument we hear a lot in politics these days. Maybe it is, maybe it isn't; but that's what we did.

BOHNING: This should certainly continue your position in terms of polymer science by having this facility.

STEIN: With the building coming along and with the endowed chair. There was just a testimonial dinner recently for our Congressman, Silvio Conte, where people paid $1000 a plate. Several companies came to this. I gave them money with the condition that the money go to us and as a result we're getting another $100,000 from that. That's going to finance a visiting professorship here, which is actually going to be given to Hashimoto, who I mentioned before. Then we have this University

research initiative. We've got the only material research laboratory in the country that's entirely devoted to polymers. The funding on this has increased by about twenty percent this year. We got one of these IBM grants; we're one of twelve universities in the country who are being supported by IBM for polymer research related to computer science. It seems to me that everything's going very well. The faculty we have our first rate and I'm enthusiastic.

BOHNING: Is there anything else that you want to add that I haven't mentioned?

STEIN: My comment is that I felt that at this time of life I would be slowing down and enjoying my reputation. I suspect that I'm working harder than I ever have in my life but I am really enjoying it. My principal regret is that I'm only one person and not five, and that there are only twenty-four hours in a day. I'm only going to live a finite number of years. I just wish I had more time and energy and everything to do all the things that I would like to be doing.

BOHNING: I think it's obvious that your time and energy have paid off in terms of the development of the polymer work here at UMass.

STEIN: One thing I might say is that tonight, arriving from Turkey, is Burak Erman, who is one of the last important collaborators of Paul Flory. He developed the new theory of rubber elasticity with Paul. He's going to come and spend three months working with me. If you want to get some reminiscences about Paul, he would be a good person to talk to. I think another good person to talk to for that is a student of mine, Do [Yeung] Yoon, who is now a manager of research labs at IBM. He got his Ph.D. with me and then took a post doctorate with Paul and maintained a close association with him. He's the person who assembled Paul's papers for publication after Paul died. He was just over in Japan to help to organize the IBM conference with the Japanese a few weeks ago.

Some other people to talk to about Paul are Leo Mandelkern at Florida State, Guy Berry at Carnegie Mellon, and Jim [James E.] Mark at Cincinnati. I think Paul's loss is something that polymer science feels not only because of his loss as a scientist but because of his loss as a statesman for science. I don't think people realize how much he did.

BOHNING: I first met him in 1974 when he won the Priestley Medal. But I hadn't, until just recently, realized beyond the scientific aspects of what you're saying.

STEIN: I was at the ACS meeting in Chicago when Paul was supposed to have delivered a talk. I had gotten a letter from him a couple of days before that saying let's have lunch together to talk over some of the new things in rubber elasticity theory. More important that we do that than listen to papers. I walked in that morning and everyone had a glum look on their face and I got the news. Jim Mark and Bruce Eichinger had just gotten the news that Paul had died.

BOHNING: All of his scientific papers are now at the Center in Philadelphia. They are being cataloged. He worked very closely with the Center, and as I understand, was instrumental in getting it funded.

STEIN: I guess his wife Emily has been very supportive.

BOHNING: Yes.

STEIN: We had both Paul and Emily staying in our cabin out at a lake here. It's very primitive and some of our guests complain about the bumpy beds but I point out to them that Paul Flory has slept in that bed. [laughter]

BOHNING: I guess I'll close with that. Thank you very much for taking time out of your schedule.

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