THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

BRUNO H. ZIMM

Transcript of an Interview
Conducted by

James J. Bohning

at

Anaheim, California

on

9 September 1986

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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(Revised 24 February 1988)

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

BRUNO H. ZIMM

1920	Born in Woodstock, New York on 31 October

Education

1941	A.B., chemistry, Columbia University
1943	M.S., chemistry, Columbia University
1944	Ph.D., chemistry, Columbia Universuty

Professional Experience

1941-1944	Teaching Assistant, Columbia University
1944-1946	Research assistant and Instructor, Polytechnic Institute of Brooklyn
	University of California, Berkeley
1946-1950	Assistant Professor of Chemistry
1950-1952	Associate Professor of Chemistry
1951-1960	Research Asociate, General Electric Co., Schnectady
1960-	Professor of Chemistry, University of California, San Diego

Honors

1957	Baekland Award, North Jersey Section, American Chemical Society
1958	National Academy of Sciences
1960	Bingham Medal, Society of Rheology
1963	High Polymer Physics Award, American Physical Society
1981	Chemical Sciences Award, National Academy of Sciences
1982	Kirkwood Medal, New Haven Section, American Chemical Society

ABSTRACT

Bruno Zimm recalls growing up in Wooodstock, New York and the influence of his father's interests in natural science. After briefly reviewing his schooldays and his developing fascination with science, Zimm describes his undergraduate and graduate studies at Columbia. During this section of the interview, he recalls faculty and curricula and tells of the effect of World War II on the research activities at Columbia. In 1944 Zimm transferred to Brooklyn Polytechnic Institute to work on a wartime project on the degradation of polyvinyl chloride; here he also first started his study of the theory and practice of the light scattering of polymer solutions, which he continued at the University of California, Berkeley. From there, and after a one year sabbatical at Harvard, Zimm moved to the General Electric laboratories at Schenectady, where he developed further his studies of dynamic methods for the investigation of polymer solutions. A short time as a visiting professor at Yale rekindled his interests in biological polymers, especially DNA. At the new University of California, San Diego campus at La Jolla, Zimm continued instrumental research as well as his theoretical interests, which he briefly reviews. The interview closes with Zimm reflecting on the changes in polymer science over the duration of his career, and he comments on educational opportunities in this discipline.

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: Dr. James J. Bohning

PLACE: Anaheim, California

DATE: 9 September 1986

BOHNING: Dr. Zimm, you were born on the 31st of October 1920 in Woodstock, New York. Could you tell me something about your father and mother, their names and occupations?

ZIMM: Yes, if you want. First about Woodstock. That's where they lived, but I was actually born in a hospital in the next town, Kingston, which is about ten miles away. It says Kingston on my birth certificate.

BOHNING: Oh, I see.

ZIMM: We lived in Woodstock. Woodstock has gotten some notoriety since then as sort of an artists' colony, which it also was at that time, but not so well known. My father was a sculptor and my mother was a writer, a novelist.

BOHNING: And their names?

ZIMM: My father's name was Bruno Louis Zimm, and my mother's maiden name was Louise Seymour Hasbrouck.

BOHNING: Did you have any brothers and sisters?

ZIMM: No, I was the only child.

BOHNING: What was it like growing up in Woodstock as a young boy?

ZIMM: As a young boy, of course, I wasn't in a position to appreciate much of the culture going on. The main thing I remember as a young boy is that I grew up more or less in the woods. My father had bought an abandoned farm some eight years or so before I was born and he found that a good place to carry on his sculpture. It was two miles out of the town of Woodstock and actually on the side of a mountain. A very nice rural environment. Until I went to high school I went to the local one-room school where there was a total of about fifteen pupils in all the grades.

BOHNING: Because of the activities of your mother and father, did you have any visitors at home that you remember?

ZIMM: Yes, they came from time to time. Not a great many. There were of course a number of artists and writers and musicians in Woodstock and we knew a lot of them socially.

BOHNING: I understand you play the clarinet, is that correct?

ZIMM: Yes. But that didn't date from then. I picked it up after I went away to boarding school. My roommate started playing the clarinet and I thought this looked liked fun, so I picked it up too, at the same time.

BOHNING: Were there any teachers in the grade school days, or anyone else in the community that had any influence.

ZIMM: Well, of course, there was one teacher in this one-room school the whole time I was there, except for the first year when I think there was a second teacher. But afterwards, there was just this one teacher, a woman who ran the whole place very efficiently and was a nice, sympathetic person. I can't say that she influenced me specifically in any particular way, but she certainly didn't make any problems either. The nice thing about being in a one-room school is you hear everything that goes on. So when I was in the lower grades, I heard what the upper grades were doing and that sort of thing helped me get through a lot faster then I might have in a more structured environment.

BOHNING: Did you have any interest in science that developed at this time?

ZIMM: Yes. That developed quite early. I should say that my father, although a professional sculptor, was actually an amateur scientist, a natural scientist. He was interested in botany and in geology, and he frequently went collecting fossils from the local rocks. I would go with him on these trips. Even before that I was very much interested in mechanical things. Living on an abandoned farm there were a lot of things of a mechanical nature. The water system was always breaking down and needed plumbing. Initially we were too far away to be on the electric line and we had our own generating plant which also needed quite a lot of attention. My father wired the house for electricity when I was about eight years old and I was very much interested in that. So I sort of developed an interest in engineering things. Then my uncles, my father's brothers, gave me a chemistry set when I was eight years old, and that really determined me to be a chemist.

BOHNING: Did you do any experiments that weren't in the manual?

ZIMM: Oh, I certainly did! There was another fellow in the the same class at high school, Walter Lakusta, who has since become a chemical engineer, and we used to plan and do a lot of little experiments outside of school.

BOHNING: Did your parents encourage you in this?

ZIMM: They certainly didn't discourage me any.

BOHNING: You went to high school in Saugerties?

ZIMM: Yes, I went to Saugerties High School for two years in 1932 to 1934. It was about ten miles away and I had to take a bus so that there was no chance for any after-school activities. My parents got worried about this and decided to send me to boarding school. So they sent me to Kent School in Connecticut.

BOHNING: I'm not familiar with that. Where in Connecticut is it?

ZIMM: That's in Kent, Connecticut, which is near Danbury; on the Housatonic River.

BOHNING: What was the high school like in Saugerties. Did you take any chemistry there?

ZIMM: I took general science. I was there two years, and one year, the second year I think, I took general science, which was well taught. For a country high school it was quite good . Saugerties is a town that has some old money in it. The town was rather better run than you might expect.

BOHNING: And then you took your chemistry at Kent.

ZIMM: I took chemistry and physics at Kent.

BOHNING: Do you remember the names of any of the teachers?

ZIMM: Yes. The chemistry teacher was a priest named Father Woods. Actually I didn't see a great deal of chemistry because I got sick in the middle of the year and had to drop out of school for half the year so that I only got half the chemistry course. But by that time I knew it all anyway.

BOHNING: You had already determined that that was going to be your area.

ZIMM: Yes. And I had been reading books.

BOHNING: What kind of things were you reading?

ZIMM: Oh, we had a big textbook, about three inches thick, called I think, Pharmaceutical Chemistry, which was lying around the house. I believe it was there because my father had previously been married to a woman who was a chemist. I think this was something she left behind.

BOHNING: I see. Did you have anyone that you could talk to when reading something of that nature?

ZIMM: Not really, no. There were no other chemists around. So I just read the book.

BOHNING: What about math? Certainly your work was highly mathematical.

ZIMM: Yes. Well, math was the usual high school mathematics but I did a little bit on my own. I remember at Kent going a little beyond the courses occasionally when something looked really interesting. Mostly I found high school mathematics pretty much a bore. A lot of drill and not very much inspiration.

BOHNING: That's interesting in the sense that when you did get into chemistry you became a physical chemist where mathematics played a very important part.

ZIMM: That's right. I think if I'd really known then what I've learned since, I would not have gone into chemistry but into physics. But high school physics was also a bore. Not that there was anything wrong with our teacher at Kent, a man named Anders, who was a good teacher. But the course was the usual high school course of the time with a lot of mechanics. It got a little more interesting when we got into electricity and magnetism. Most of it was very elementary, used hardly any mathematics. Really not very exciting.

BOHNING: Did you have any friends during your high school days, anybody that you...

ZIMM: Well, there was this fellow Lakusta that I mentioned at Saugerties. We used to do chemistry together. I had a great many friends at Kent, but none of them were particularly scientific.

BOHNING: What kind of laboratory facilities did your school have?

ZIMM: The usual high school laboratory. I don't remember whether there was a chemistry laboratory. I think the laboratory must have been in the second half of the term when I was sick and out of school. We had an experimental session in physics about once a week where we used some of the apparatus which was bought from supply houses. Inclined planes, pulleys, some electricity and magnetism. I think the teacher demonstrated things more than having us actually doing anything. On looking back, there wasn't much laboratory exercise in that physics course, it was mainly demonstrations.

BOHNING: Outside of the mechanical experience that you had back at home, you had very little actual laboratory experience?

ZIMM: Very little. Except what I was doing on my own at home. Walter Lakusta and I used to boil up things and mix acids and bases; pour acid on metal to make hydrogen and then light it; electrolyze things and all that sort of stuff.

BOHNING: Did you do that in the home or did you have another place?

ZIMM: No, I did it at home. My father finally set up a wooden work bench so I wouldn't mess up other things.

BOHNING: You said that you had developed your interest in chemistry quite early. What kind of career plans did you develop?

ZIMM: Well, I thought I wanted to be a chemist.

BOHNING: Was there anyone you could talk to to develop that?

ZIMM: Not that I remember, except for my teachers in school. There really wasn't much one could do to develop a career at that age. You go to college after high school; in college you have to major in something. Well, I had already determined that I was going to major in chemistry. That was the only choice I really had to make.

BOHNING: Were your father and mother college graduates? Did you have that background in your family?

ZIMM: My mother had gone to Wellesley two years before she dropped out. My father had gone to art school, but not to college.

BOHNING: And how did you decide on Columbia?

ZIMM: That was through the headmaster of Kent School. He was a Columbia graduate, and he thought he could get me a good scholarship. Actually, he did help; it wasn't quite as good a scholarship as he thought, but that's how I got to Columbia.

BOHNING: That was 1938?

ZIMM: Yes.

BOHNING: Another question I meant to ask you earlier was about the effect of the Depression.

ZIMM: Oh, yes. A very decided effect. We were very conscious of money problems all the time . My father had one large commission that he got in about 1930 or maybe slightly earlier, when things were good, which lasted for about four years. It was to carve a number of stone reliefs for a church in Philadelphia. After that it was just one little thing after another. We were very short of money. I wouldn't say that we were poor in the sense of being impoverished, but we had to watch money very carefully. I only succeeded in going to Kent School and to college by the help of scholarships and from help from my mother's family.

BOHNING: What was Columbia like when you arrived in 1938?

ZIMM: I think very much the same as it is today. From my own point of view it was a bit of a bore in that the curriculum was very structured. Which was fine. I was glad to get it over with, but it inhibited any creativity. I didn't find anything very interesting to do in college. To raise a little money, I tried, with the help of my father, making and selling miscellaneous small art works.

BOHNING: What kind of art works? Were they sculptures?

ZIMM: Well, yes. We made ashtrays. For example, I put a Columbia Lion motif on an ashtray, with Columbia engraved around the edge. We made these out of pewter and I actually did sell a few for a total sum of about a dollar and a half each. Sold some of them through the college bookstore, and some directly.

BOHNING: I see. Do you recall how much your scholarship was?

ZIMM: Three hundred dollars a year. And my total expenses for a year were about nine hundred dollars.

BOHNING: So you got a little income from this activity?

ZIMM: Right, and I occasionally did some tutoring. But I didn't have any regular job, like a job in a cafeteria or anything like that.

BOHNING: The chemistry department had a real line-up in those days, in terms of people. Who was your first teacher in chemistry?

ZIMM: In freshman chemistry it was Ray H. Crist, a photochemist. In analytical chemistry there was [Joaquin E.] Zanetti, who taught qualitative analysis. Did you want to go through the list while we're at it now?

BOHNING: Sure.

ZIMM: Hal T. Beans, was quantitative analysis. [John M.] Nelson, we all called him Pop Nelson, was in organic chemistry. The laboratory was run by Charlie [Charles R.] Dawson; that organic laboratory was actually the first place where I really encountered any of the faculty to talk to. Previously everything had been on such large scale with large classes, except for freshman chemistry which was a small class. That was just a lecture class where the professor came in, lectured and went out. Then there is physical chemistry taught by a man whom I got to know quite well, Jake [Jacob] Beaver. Also I had an organic analysis course from Dawson .

BOHNING: I assume from what you say that there wasn't a laboratory with general chemistry in the first year.

ZIMM: Yes, there was. One afternoon a week, a laboratory which was not very interesting. Not that the experiments might not have been interesting, but there was such a rush. We had to go in, rush through the experiment, write up the notes and get out; there wasn't really any time to enjoy investigating anything.

BOHNING: Was Dawson the first faculty member you started talking to?

ZIMM: Dawson and then Beaver. Beaver ran a physical chemistry lab with a very personal approach so that he was somebody one could really talk to.

BOHNING: What did you discuss with him? Did you talk about...

ZIMM: Oh, well, principally about the experiments we were doing.

BOHNING: This would have been the extent of chemistry requirements for a B.S. degree.

ZIMM: That's right. Those were the minimum requirements for chemistry. Normally one would take more. It happened that, rather unexpectedly, I got through in three years and one summer session. And I continued right on in graduate school. I didn't even apply to graduate school until that last summer session. This, as I said, was unexpected because in those days you got extra credit at Columbia if you got an A in a course. By getting enough A's I accumulated sufficient extra credit so that I didn't have to take a full senior year. But I didn't know that was going to happen until very near the end. I didn't have any time to explore around for graduate school. Also the war was just starting, or was just about ready to start then. This was 1941, and I was in a hurry as the draft was already in operation. So I went directly on to graduate school and essentially didn't have any senior year. Otherwise I would have taken more [undergraduate] courses.

BOHNING: You didn't do any kind of research at the undergraduate level.

ZIMM: No. Columbia didn't actually have any such program at that time .

BOHNING: Did you take any math courses?

ZIMM: Yes.

BOHNING: Was that a better experience than what you had in high school?

ZIMM: Well, that's hard to say. I took analytic geometry and calculus first term, which was differential calculus. I couldn't take the integral calculus because of the interference of courses in the chemistry program. So the following summer I studied on my own and took a test to pass the requirement for the course, which Columbia allowed. Then I took the last term of calculus at the same time I was taking physical chemistry. So I really only had elementary courses, but the one thing that I did was to study on my own which was an interesting experience as it was the first time I'd ever done anything like that. I probably got more out of that than by taking a course.

BOHNING: And did you have any physics at this time too?

ZIMM: One year physics course, yes. A big lecture course with a laboratory session once a week. It was a good course and I found it rather interesting, although it was pretty much the cut-and-dried type of course. I think the most interesting part was electricity and magnetism, but again it was a course without much mathematics so from that point of view it was a bit dull. The fellow who ran the laboratory was a young instructor named Polykarp Kusch, who subsequently got a Nobel Prize. He ran a good laboratory and really kept you on your toes, even though it was very elementary material.

BOHNING: Did you have any interaction with him on a personal basis.

ZIMM: Nothing very significant. I mean I talked to him, of course, but nothing that you would call significant.

BOHNING: That summer session was the summer of 1941, and it was at the end of that summer with the war heating up, you...

ZIMM: Right. I applied to graduate school right at Columbia, and they put me to work as a teaching assistant to Beaver in the same physical chemistry lab that had I just finished.

BOHNING: What happened once the war broke out?

ZIMM: The war broke out that Fall but everything just kept on going for a while. Then effects gradually began to appear. A lot of people dropped out of teaching and went into the Manhattan Project. Harold Urey, for example, was teaching the course in atomic physics that I was taking that Fall. He started the course but he gradually disappeared from the lectures and other people came in to fill in.

BOHNING: He was chairman of the department then, wasn't he?

ZIMM: He was chairman of the department also.

BOHNING: I have read, I think in maybe one of those review articles that either you or Walter Stockmayer wrote, that, after the war started, Urey indicated that the war would be won by applied science and that people shouldn't rush off to join the military.

ZIMM: Yes. Stockmayer said that. He was already an instructor at the time, and I was just a beginning graduate student, so I wasn't in on those meetings!

BOHNING: Anyhow, you weren't about to rush off and join anyway.

ZIMM: No. I guess I registered for the draft that Fall and was waiting for them to come around.

BOHNING: I will get into what you did in terms of the light scattering in a moment, but in that first year you simply were an assistant, did some teaching, and took the standard introductory courses, is that basically it?

ZIMM: Yes. That's right. I studied for the qualifying exam at the end of the year.

BOHNING: What other courses besides Urey's course?

ZIMM: Oh. A thermodynamics course taught by V. K. La Mer. A course on the phase rule, taught by Jake Beaver while I was his teaching assistant. There was a course in physical organic chemistry, so-called, which was actually a course in macromolecules, though not under that title. That was taught by Charlie [Charles O.] Beckmann, who was a starch chemist.

BOHNING: What was the orientation of that course? Was it mostly natural products?

ZIMM: It was really physical chemistry of macromolecules insofar as it was known at the time. We discussed random walks, I believe. He talked about spectroscopic methods and x-rays; more specifically x-ray and electron scattering. It was the first time I was exposed to Fourier transforms to any extent. What else did he cover? Oh, he talked about hydrodynamic methods, sedimentation and viscosity. At the time it all seemed a very <u>ad hoc</u> subject, but I guess, I absorbed some of it nonetheless. [laughter] He had an interesting style of lecturing. Not very rigorous: when a minus sign came out in the wrong place, he just changed it to a plus sign. [laughter] But he covered the material. I also had a course of quantum mechanics from George Kimball who was at the time helping to write the book, Eyring, Walter, and Kimball (1). But it was not yet out.

BOHNING: Did he use notes then?

ZIMM: He used notes. He gave a beautiful set of lectures from which was possible to take accurate notes. That's not usually possible. At the end of the term I ended up with a very good set of interesting notes. But that was really all; there was no homework so there was no chance to practice anything that he preached. It was essentially an exposure to a lot of beautiful stuff but without any real understanding.

BOHNING: How did he examine you or test you on it?

ZIMM: I suppose we had a written exam at the end of some kind.

I don't remember now. It couldn't have been anything very demanding.

BOHNING: This was all before you'd passed the qualifying exam.

ZIMM: The quantum mechanics was in the spring term and the qualifying exam was near the end of the spring term, so the two overlapped.

BOHNING: Any other texts that you may have used in that early graduate work.

ZIMM: Well, let's see. There should have been another course in there somewhere. Well, maybe that's all the courses I took that first year, because I was studying for the qualifying. I think that must have been just about it.

BOHNING: Did Urey or La Mer use any texts?

ZIMM: La Mer used Lewis and Randall (2). He had his own copy of Lewis and Randall, an interleaved edition. He had written his notes in between the other pages, so every once in a while he would surprise us by getting something out of the book which was not in our copy. After a while we learned what was going on. [laughter] Urey used books by Herzberg on atomic spectra and on structures and on molecular spectra and structures (3).

BOHNING: Was he rigorous in the classroom, in his lectures?

ZIMM: Well, it was rather descriptive material about atomic spectra and structures so that I don't think rigor is quite the word that one would apply. But I think he was accurate, as opposed to, say, Beckmann's course which was not always accurate in detail. I think Urey's was; I don't remember any time when we ever found that he was saying something that didn't agree with the book. In La Mer's course that did occasionally happen too. [laughter] Beckmann didn't use a text. Kimball didn't use a text. Beaver used a text called <u>The Phase Rule</u> by a man named Findlay (4). I think I still have it. It was just a catalog of interesting phase transitions of various kinds.

BOHNING: You mentioned that in the quantum mechanics you never had a chance to practice anything of what was being presented: did the other courses give you that opportunity?

ZIMM: Yes. We had a lot of homework in the thermodynamics. La Mer assigned a lot of homework every week. We had to come in and put problems on the board. He would assign one problem to a particular student to put on the board. We had a lot of practice in thermodynamics, which was a good thing because Lewis and Randall was not a very deep textbook. But it did have a lot of practical examples in it.

BOHNING: How many students were there in that graduating group with you?

ZIMM: Just about all of them were in each of these initial classes. As I remember, it must have been about twenty. That also included biochemistry students from P & S, that's the biochemistry group at the medical school; "Physicians and Surgeons". Probably something like twelve pure chemistry students.

BOHNING: I may be wrong here, but the appearance is that the department was concentrated in physical chemistry aspects rather than the organic. Is that true?

ZIMM: Well, there was a fairly strong organic group.

BOHNING: [Louis P.] Hammett was there then, wasn't he?

ZIMM: Hammett was there, right. A man named [Robert C.] Elderfield, who was the synthetic, I guess natural products, guy; I think he was pretty well known. Nelson had done some rather interesting work in earlier years but he was pretty old by that time. It's true I guess: probably the more outstanding people in the group were the physical chemists. Urey, La Mer, I guess they were probably the best known.

BOHNING: I was curious that you migrated towards physical chemistry, because I think you said earlier that it was the organic laboratory experience that gave you your first...

ZIMM: I found that organic laboratory very interesting, in fact, I found organic chemistry [as a whole] very interesting. I remember being rather uncertain as to whether I should go toward organic or toward physical chemistry when I went to graduate school. I'm sure I made the right choice but I don't remember exactly what influenced it. Probably just a lot of little things.

BOHNING: Now, we approach the summer of 1942. You've finished the first year. After passing the qualifiers, were you getting ready to select a thesis advisor? You selected [Joseph E.] Mayer. How did you make that selection?

ZIMM: It was a matter of going around talking to people, other students; seeing what they were doing and seeing what seemed most interesting to us; by us, I mean the first year class. Of the people I'd interacted with strongly, Beaver was doing practically no research, and he didn't have any research students. La Mer, whom actually I worked for in that summer on a war project on smokes, I found to be a difficult man personally. He was volatile, you never knew what he was going to do next. That influenced me to a considerable degree. He didn't have much of a research project emphasis at that time because the war work had really started up.

Whereas Mayer had some interesting things in several areas. Physical chemistry that even I, as a beginning student, could recognize as being good . I had met him personally, as he had actually come around to help with the physical chemistry laboratory. He came to help Beaver at times, so I had met him then. And he had a very good reputation among the students. So that was it. Beckmann might have been a choice, but at that time, except for the course he had given, I didn't know much about macromolecules. I didn't realize anything about their significance. It probably never occurred to me to work for him.

BOHNING: And Urey wasn't available.

ZIMM: Right. Urey was all tied up with war research. In fact, my friend Paul Doty had come to Columbia to work with Urey but Urey told him that that was just not going to be possible. So, Doty and I both went to work for Mayer.

BOHNING: Doty was a year ahead?

ZIMM: No. He was the same class.

BOHNING: Oh, he was in the same class.

ZIMM: We ended up sitting next to each other in La Mer's thermodynamics. La Mer arranged everybody alphabetically so he could keep track of who was who. I was at the end of the alphabet, Doty came in late so he sat next to me. [laughter]

BOHNING: I would like to talk about those first experiences with the war effort in the summer of 1942. Before we go into that, you had selected Mayer ...

ZIMM: No, I think we selected him by the end of the summer, actually.

BOHNING: The work you did with La Mer during the summer; was that by choice or was that sort of accepted as part of the war effort? How did you get into that particular project?

ZIMM: I don't remember whether he invited us in or whether we went looking. It happened very naturally, whatever it was. I think probably every graduate student at the time was being asked to work on something during the summer.

BOHNING: Well, could you tell me something about that work on light scattering and smokes?

ZIMM: Yes. The project was to work on smoke screens. I believe it had been initiated by OSRD, the Office of Scientific Research and Development, with, if not the initiation, at least the early involvement of Irving Langmuir. La Mer was brought in at the beginning to set up some of the experimental work. Doty and I got in on that quite early. At that point the interest was in looking at smokes of colored dyes to see whether one could get a smoke which would be opaque at most visible wavelengths, but which would be transparent at some selected, still visible wavelength. We worked on that all summer. I remember building generators to make smokes of these dyes. You heat them up, blow air over them to get a vapor and then condense it.

BOHNING: Where did you do this?

ZIMM: Where? In the Columbia laboratories, sixth floor of Chandler Laboratory. La Mer's laboratories, which he had been using previously for all his other work. They were just converted over to this project.

BOHNING: Did you have any ventilation problems or difficulties?

ZIMM: Oh, the place had good hoods, a good chemical laboratory set-up. We didn't have any problems in that way. I do remember we had a fire or two from oil baths which we put out right away with the portable extinguishers which were out in the hall. We had to build some sort of a spectrophotometer, a very crude one using auto tail light bulbs and colored filters to measure the transmission of these smokes. We had a copper tube about three or four meters long, extending most of the length of the laboratory. We introduced the smoke in one end and passed a light beam through two glass windows to see what kind of color they produced.

BOHNING: What kind of results did you get from that?

ZIMM: Nothing very interesting. We could get smokes of unusual scattering and transmission characteristics. We had for example a green smoke; I remember making a green smoke from a red dye. It would transmit red light fairly well and scatter the shorter wavelengths. It had to be a very dilute smoke because the particle size had to be very small in order to get these effects. If you went to a large particle size then the light was all absorbed rather than being scattered. It absorbed in these large particles, and then you lost the effect completely. It just looked like a pink smoke and it didn't scatter anything especially. In order to keep the very small particles, you had a very dilute smoke which meant that it didn't have much screening power.

BOHNING: You said that Langmuir had been brought in. Did you meet him at all during that project?

ZIMM: No. I didn't meet him then. I don't know what he was doing at that point. He never came around to this project.

BOHNING: Just as an aside, last November, I interviewed Hoyt Hottel at MIT. He was telling me about a meeting in which Langmuir was sitting next to him. Hottel had been describing some of the laws of jets; he was an expert on combustion. When Langmuir heard that he said, "That's just what I need for my smoke screens." [laughter] So I thought that I'd pass that along to you.

ZIMM: Well, that's... I don't know what Langmuir was involved in at the time. He didn't come around to our project anyway. He helped get it started but... So that was the summer of 1942.

BOHNING: Now did you continue that on into the academic year or did that cease?

ZIMM: No. At the end of the summer we went back to school, so to speak. I was a teaching assistant in the physical chemistry laboratory, and probably took one of these courses I had talked about. Oh, I know. I took a course in the physics department in quantum mechanics all that year.

BOHNING: Do you remember who you took it with?

ZIMM: A guy named Joe [Joseph M.] Keller. I don't know what has ever become of him. He was a fairly young guy at the time. Oh, and there was a course I forgot to mention before in mathematics. Differential equations. That was the other course in the first year. It was taught by a guy named [Harold W.] Webb in the physics department.

BOHNING: In terms of your mathematics, was it becoming more satisfying when you got to that course?

ZIMM: Yes. It was an interesting course, but it was a bit of a rush. They loaded a lot of material in in a short time, but I found it to be interesting material. I've used it a lot since.

BOHNING: This may again have been something that Stockmayer wrote but I'll ask you anyway. Mayer and Kimball had this lunch club during war: were you involved in that at all?

ZIMM: Yes, the whole research group. Both research groups and usually Stockmayer, who was an instructor, would go out to lunch. I think every Monday. Well, maybe not every Monday, but pretty frequently.

BOHNING: There's a number of people that I know you met during that time, and I want to ask you about them. How long did this kind of activity go on.

ZIMM: I think it kept on right through my graduate career at Columbia. After a while, Mayer got involved with the Aberdeen Proving Grounds, and only came up to Columbia on Mondays. The rest of the week he was down there. But we still had the Monday lunch club. And Maria Mayer got involved with the Manhattan Project, so she was around all the time. Kimball was there all the time.

BOHNING: Did you meet some of the people that showed up, Fermi and ...

ZIMM: No. I don't remember [Enrico] Fermi ever coming. [Edward] Teller came once in a while.

BOHNING: Any reaction about seeing Teller at that time?

ZIMM: Actually, several of us had an informal course from Teller on theoretical physics for a while. It short of petered out after a while because it was very informal. A very unorganized course. He would get up and discourse on whatever he felt like. Interesting but not a very good learning experience. [laughter]

BOHNING: Was his reputation such at that time...

ZIMM: No. He didn't have the reputation then such as he has since. No father of the hydrogen bomb or anything. He was best known for probably the BET equation (5).

BOHNING: You said that you went to him. Was he asked to do this or was he...

ZIMM: I don't remember how it got started. Whether somebody asked him or whether he offered. Or whether some of the faculty suggested it or what. But anyway, it did take place for a while, something like a couple of months and then it sort of died out.

BOHNING: Anyone else that showed up during the time of the war effort that. [John G.] Kirkwood?

ZIMM: Kirkwood was not around. Fermi by that time was in Chicago, I think. I don't think there was anybody else of the big names. None that I remember. They were all elsewhere.

BOHNING: Urey was still away?.

ZIMM: Well, Urey was the head of the Columbia Manhattan Project. So he was very much around, but he didn't involve himself in seminars or anything like that.

BOHNING: And were you aware of the Manhattan Project, and what work was going on?

ZIMM: Yeah. I didn't know what it was. Gradually one began to put things together. For instance,there were jars of uranium nitrate stacked outside the Pupin physics laboratories. During the spring vacation, I guess of my first year, they needed some help in a hurry, and recruited several of us to try a fractionation scheme on uranium. A water-ether two-phase separation; didn't tell us what it was for. I remember innocently asking, "What are we trying to do, separate something?" There was sort of a laugh but nobody said anything. I got the impression that was not the right question. [laughter] But I think it gradually percolated through that it was an atomic energy project.

BOHNING: Was Mayer involved in that at all?

ZIMM: No. He was at the Aberdeen Proving Grounds. He may have been involved in some way that I don't know, but he was not involved to a large extent.

BOHNING: How did his being away effect the Ph.D. work?

ZIMM: Very little actually, the laboratory was all there, there were some older graduate students who knew where things were. If it hadn't been for the fact that I'd had a lot of mechanical experience back there on the abandoned farm, it probably would have been very difficult but I knew how to put things together, to solder and so forth, so I didn't have any trouble. Mayer just suggested a project which was following the work of a previous student of his, measuring vapor pressures of alkali halides. In fact there was even the starting apparatus that the previous student had abandoned. So I could get right to it. I just talked to him when he was around about anything that happened to come up. Usually it wasn't a matter of much necessity, just a matter of getting advice once in an while.

BOHNING: Was Doty working in a similar area?

ZIMM: Yes. We both started there in the same laboratory at the same time. He worked on electron affinity of some halogen compounds and I worked on vapor pressure of alkali halides. It was very similar, using much of the same apparatus, the same galvanometers and so on.

BOHNING: Was this sufficient to keep you out of the draft at that point?

ZIMM: Well, I think it was more probably being a teaching assistant. That was sufficient for a couple of years, and then I got my Ph.D. just in time and got on to war projects again when the rules were tightened.

BOHNING: So you really didn't get back to La Mer's smoke work until after you finished your Ph.D.

ZIMM: That's right. 1944, I guess it was. I got my Ph.D. in that summer, and started with La Mer that spring. I started working on his project at the beginning of the spring when my own laboratory work was finished but before I'd written it all up.

BOHNING: Did you ask to go back to work on that project?

ZIMM: Yes.

BOHNING: Let me ask a more general question. What was the effect of the war on the graduate students' Ph.D. work? Were they able to combine any thesis work with war work, or was it separate?

ZIMM: There may have been cases in which they did. I don't know. In many cases the man had to stop and go over to the Manhattan Project, essentially as an employee. That happened to several friends of mine and in some cases they stopped and went into the services. Actually, several of those who went into the Project first were drafted, went into the service, and then the Army sent them back. So they came back in uniform.

BOHNING: They were fulfilling their military obligation by working on the Project.

ZIMM: That's right, yes.

BOHNING: Did you continue that work with La Mer until the war was over?

ZIMM: No. Not quite. I stayed there until about December of 1944. I knew about the project that Herman Mark was running at Brooklyn Polytechnic, because Paul Doty had gone there directly from his Ph.D. at the end of 1943. That seemed to me much more interesting work, so I managed, after a while, to transfer from one project to the other. Which was no trivial thing, as you didn't change jobs very easily during the war.

BOHNING: May I ask you how you managed that?

ZIMM: Well, I had to get La Mer's permission to leave the project. I had, of course, to get an offer from Mark's project and then I had to go and see somebody at the War Manpower Commission, I think it was called, in downtown New York, and get their permission to transfer. I managed to do all that by the end of 1944.

BOHNING: That raises another interesting question. That is, the transmission of scientific information during that time. Was it restricted to seminars and the like?

ZIMM: Well, certain things were, though we continued to have seminars at Columbia. One probably worth mentioning was a seminar that Charlie Beckmann ran on macromolecules which, aside from his course, was really my first introduction to the subject. And I remember having to give a talk on rubber elasticity. That was where I think I began to see macromolecules as a possible research field, as opposed to something just learned about in a course. Now that [seminar] was run without any restrictions. The restrictions, of course, came when talking about what was going on in the Manhattan Project. Nobody could do that.

BOHNING: What had Doty been telling you about what Mark was doing. What was it that intrigued you?

ZIMM: It was a bit of a sideline from the main project that Mark was running, which was working on polymers that were of use in films and coatings; things like this briefcase, which the military was using for raincoats, gun covers, all kinds of stuff. In the process you'd need to know what the polymer molecular weight was. This was something which I wrote up in that article you referred to in the <u>Annual Reviews of Physical Chemistry</u> (6). Anyway, there was this new method that Debye had developed to measure molecular weights by light scattering; unpublished [work]. But Mark's project had heard about it. When Doty heard about it he came back and told us because he and William G. McMillan, who was also a student of Mayer, and I were interested in light scattering because it seemed like a good way to look at the critical phenomena that Mayer was interested in and which McMillan was doing his thesis on. So when this light scattering method for molecular weights came up, we were all attuned to understand it. We had the apparatus which Charlie Beckmann had had sitting around for years; when he heard us talking about it he said, "I've got the apparatus." We'd already used it to look at critical points in McMillan's triethylamine/water system. All we had to do then was to get some polymer solutions, which Doty could get from Mark's project, and measure them.

BOHNING: You did that at Columbia then.

ZIMM: At Columbia, yes. Well, Doty was already at Brooklyn, but he came back and forth. I was still at Columbia, just finishing up my thesis.

BOHNING: What was the offer from Mark's group? You just went through the mechanism for making the transfer.

ZIMM: It was to join their project on polymer films as, I guess they called it, a research assistant; I think that was the title of the job.

BOHNING: Were you given an assignment then when you joined that group?

ZIMM: Yes. To look at polymer degradation, specifically polyvinylchloride degradation. They were using polyvinylchloride in films and it degrades easily in both sunlight and in heat. Or both together. My assignment was to study that process.

BOHNING: Could you tell me something about what you found at Poly when you went there in 1944?

ZIMM: Well, it was a physically dilapidated old establishment. The Columbia Laboratories weren't so wonderful, but the Poly ones were considerably worse. In fact, the lab that I had was part of the old morgue of the hospital that the building had been before. At least that's what they told me. [laughter] But there was a very lively spirit among the people that Mark had brought in. He had brought in some young people, and the older faculty members of the Institute were happy to see us around, which was a good thing. It might not necessarily have been that way, but it was.

BOHNING: Which of those people that Mark had brought in did you interact with most?

ZIMM: Well, with Doty a great deal. And with Turner Alfrey. The three of us had many interests in common and we were talking to each other all the time.

BOHNING: And what about Mark. Did you get to see him?

ZIMM: Yes, but he was then, as now, traveling a lot. In the case of Mark, it was a matter of his being at the talks that we were giving, conversations in a corridor once in a while, and so on. But not a matter much of discussing day-to-day research.

BOHNING: Can you tell me something about Turner Alfrey? Your experiences with him?

ZIMM: Let's see, he had got his degree with Mark a year or two before and at the time I'm talking about, he was part-time on this project and part-time working for the Monsanto Chemical Company in Connecticut. He commuted back and forth for a while between the two. I'm not quite sure what kind of an arrangement that was. I think later in the project he came down to Brooklyn full time. He was a thorough polymer chemist; that was how he had been brought up, so to speak. And that was important from my point of view because I hadn't had much contact with it [polymer chemistry] aside from the seminar I told you about. So I learned, I think by osmosis, quite a bit of polymer chemistry from him. He was a very easy guy to talk to, full of good humor and lots of original, bright ideas.

BOHNING: In addition to your interaction with the people who were working on the project, did you do anything else at Poly in terms of getting more polymer background? Courses?

ZIMM: No. Learning polymers there was a matter of learning by doing and by osmosis from other people. There was just a lot of work going on in addition to the people I mentioned, there was some organic work going on. Peter Hohenstein had a group which was, I think, also associated with this same project. Several graduate students synthesizing polymers, I've forgotten just what.

BOHNING: Was [Charles G.] Overberger there then?

ZIMM: Overberger was not there yet, that was later. [Isidor] Fankuchen, who was an x-ray man, was there. We occasionally used his equipment to take a picture of a stretched piece of polyvinyl chloride or something. Let's see, were there any other polymer people? I think not at that time. Except students of Mark and of Doty. There was a chemist named Kurt Stern, a biochemist. He came there at that period, and his lab was set up right next to mine in the basement. He had an ultracentrifuge, and he was looking at DNA; my first acquaintance with DNA.

BOHNING: I wanted to talk about that some.

ZIMM: One of his graduate students was Seymour J. Singer, who is now a colleague of mine at La Jolla. I guess Singer was officially a graduate student of Mark, but he actually worked with Stern.

BOHNING: Can you tell me something about the degradation. Could you tell me something about what kinds of experiments you did or what...

ZIMM: We never published any of it in the open literature. There was a big report written at the end of the project describing the things that were found. But never any papers. We found a number of things which have since been verified by other people. There were also a number of things which were known to chemists working with polyvinylchloride at the time which I don't think had been much written down anywhere, but were just general lore. The effect of HCl, for example. If you stored a bag of polyvinylchloride films in a warm place or in sunlight, in a bag which itself was impermeable to gas, when the bag was opened there was big smell of hydrochloric acid. The PVC had split out HCl. That was well known empirically. I did experiments with that, put pieces of polyvinylchloride film in sealed tubes, exposed them to light or heat for a while, opened them, and analyzed them for HCl. There's an autocatalytic effect, presumably because of the low pH of generated by the HCl, which would catalyze the splitting out of more HCl. If you plot a rate curve, it would take off. So, I spent some time examining that effect and seeing what the conditions were that would bring it on, using very simple equipment. I had ultraviolet light sources, sun lamps and a big arc light source called a Weatherometer which they had there for other studies. I tried looking at the molecular weights of the PVC as it degraded, but didn't come up with anything very interesting because we know now that PVC is not completely soluble in ordinary solvents when you work at room temperature, so that the molecular weight measurements are really of aggregates and not of individual molecules. Anyway, I was able to keep busy with lots of interesting experiments. Every once in a while we'd have to give a report to a bunch of industrial people who were associated with the projects. They'd all come in on Saturday morning and we would get up and show slides of all these latest developments. I think they were entertained and found some of it perhaps of some use.

BOHNING: What companies, do you remember?

ZIMM: Union Carbide, Du Pont. They were certainly two of them. U. S. Rubber. I don't remember what others, but there were a number of them.

BOHNING: Did you do any teaching? Or were you strictly research?

ZIMM: I did a little bit of teaching. That was sort of after hours. Taught a physical chemistry course one term and I taught a course in atomic physics for chemical engineers. [laughter] One term.

BOHNING: That must have been an interesting experience.

ZIMM: Yes. It was a good group actually. One of them wrote me years later, saw my name in Chemical Engineering News in connection with something. He said that at the time he couldn't see why we were learning all this stuff about atoms. Then in the summer immediately afterwards the atomic bombs were exploded and they suddenly understood. [laughter]

BOHNING: This continued up through the end of the war?

ZIMM: I stayed there until the end of the summer of 1946.

BOHNING: Had you given any thoughts to what you were going to do once the war was ended?

ZIMM: Well, nothing very serious. I was still very interested and involved with that project. Actually, through Joe Mayer, who is a Berkeley Ph.D., I think, I got offered a job at Berkeley, which, of course, I was very happy to accept. I didn't have to do much looking.

BOHNING: You went there in the Fall of 1946?

ZIMM: 1946, yes.

BOHNING: Let me back up for a moment. You have recounted rather clearly the work on light scattering that developed out of Debye's early work and then you, Mark and Doty published a quick paper. Then you published a rather lengthy paper that followed later.

ZIMM: First, we published two letters to the editor (7). Then we followed that with a paper in the Journal of Chemical Physics, giving more details (8). This must have been 1944 and 1945, I think. Then we had a review article in something that Mark had started called the Polymer Bulletin, which is no relation to the current Polymer Bulletin (9).

BOHNING: I was trying to get at how this led you into light scattering work once you went to Berkeley.

ZIMM: Doty actually had the main responsibility for that, but I helped with it some. We had one joint student there who got a master's degree, Bill [William A.] Affens. He worked on light scattering from polystyrene solutions. And we published that too. That, I think, was in the Transactions of the Faraday Society (10).

BOHNING: But when you went to Berkeley, you picked up light scattering work rather quickly.

ZIMM: Well, I had been working on it, you see. Although there were other things. My main effort at Brooklyn Poly was this polyvinylchloride stuff. Light scattering was one of the techniques which we used, which was not very useful for polyvinyl chloride, but I was at least familiar with the technique. I also developed an osmometer while I was at Brooklyn Poly for use on the PVC (11). That was with a student named [I.] Myerson. When I went to Berkeley, it was just natural to continue that work.

BOHNING: The first thing you did was develop a new apparatus, is that correct? That was, I think, one of your first publications (12).

ZIMM: Yes. That was the first thing. To build a photoelectric as opposed to a visual apparatus.

BOHNING: Could you tell me something about what led you into that.

ZIMM: We had been talking about a photoelectric apparatus at Brooklyn. In fact I think we had a photometer, which was sold as a complete piece of apparatus. It was called a photovolt photometer made by, I guess, the Photovolt Corporation. But it wasn't sensitive enough. It used an ordinary phototube. What one needed for light scattering was the multiplier phototube which had just become available. I don't remember now for sure but I guess we had talked about building such a thing at Brooklyn. When I went to Berkeley I found an electronic stockroom and people with electronic expertise, it seemed like a natural thing to do. Particularly when I got a couple of graduate students who needed projects to work on.

BOHNING: Did those graduate students start working for you right away?

ZIMM: Not right away. I guess they showed up in the spring on 1947. And I was just starting to build the apparatus at that point.

BOHNING: How long did that take?

ZIMM: Building the apparatus and getting it working? I had a very simple version of it going in a few months, but to build a good reliable instrument took longer. Must have taken about a year. We published results on it in 1948 (13). I went there in 1946, but didn't start building until the spring of 1947, so I think it must have taken about a year to get it going.

BOHNING: You also collaborated with Stockmayer, I think around then. It wasn't that long after you went to Berkeley.

ZIMM: It was after I went to Berkeley. We had talked about collaborating when I was at Brooklyn. He had left Columbia by that time and gone to MIT. But he got back to New York once in a while, and I went up to Boston once. I guess we corresponded. Anyway we talked about writing a joint paper using Mayer's statistical mechanical methods on macromolecule solutions. Actually we never got to doing that together, and I wrote it myself while I was at Brooklyn (14). But then we did succeed in collaborating on a theory of branched polymers. That was a paper I think we published in 1949 (15). We did that pretty much by long distance. I would write something and send it to him and he would write stuff and send it back.

BOHNING: Your work is interesting in the sense that you were highly theoretical on one side and yet quite practical in building a number of instruments along the way.

ZIMM: Well, I've always found that the two complement each other very well in a field like macromolecules which at that time was quite undeveloped. It needed both kinds of work. So if you could do both kinds of work, it was good.

BOHNING: Which did you feel more comfortable with and more satisfying?

ZIMM: Both of them, equally so. I always liked doing things with my hands. On the other hand, I found I also liked mathematical physics. When you can do both, one leads to the other. You do an experiment and then you need it interpreted, or you start developing a theory and then you need experiments. It's very hard for a theoretician to persuade some experimentalist to do his work for him. It's not usually so hard for an experimentalist to get a theoretician to help him out if he finds an interesting phenomenon, but it's fun to do it yourself if you can.

BOHNING: In 1950 you had a paper with Melvin Calvin (16).

ZIMM: Oh, yes.

BOHNING: You mentioned DNA once before, but I'm looking when...

ZIMM: That was not DNA of course, that was a protein.

BOHNING: Right. I'm looking for when you moved into biological molecules.

ZIMM: Well, I'd been exposed to it in several places. I mentioned Kurt Stern working on DNA with his ultracentrifuge in his laboratory right next to mine at Brooklyn. At Berkeley I became acquainted socially with someone working on Calvin's project, Walter B. Dandliker. He was essentially a postdoc at that time. I think his wife was a schoolmate of the wife of one of the assistant professors in the department, [William D.] Gwinn, a spectroscopist. Gwinn and I shared teaching physical chemistry laboratory courses and we lived a few houses from each other in Berkeley. So we got to see a lot of them and Dandliker socially. Dandliker was working on this project for Calvin, and I think that's how I got involved in it.

BOHNING: And what was your portion of that? Was it physical properties of ...

ZIMM: I did the light scattering and they did the ultra-centrifugation. They had the first Spinco ultracentrifuge. Literally. Instrument # 1.

BOHNING: Where was that made?

ZIMM: That was made at Spinco, over in Palo Alto. I remember driving over with Dandliker to see it when it was in the building stage. We drove over in a university car, a pre-war car in which the engine kept missing all the way there, and we didn't know if we were going to make it. That was probably in 1947 when that protein work was done. Well, anyway, I'd always been interested in biology. Just as an intellectual interest. As a kid I collected snakes, and I told you my father was interested in geology and that we collected fossils. One summer when I was in high school I helped with a research project that a professor at Columbia Medical School, who lived in Woodstock, was running. He'd gotten some money from a foundation to do this project. It was a psychology project. It had to do with asymmetry in the nervous system, and what it amounted to was to look for asymmetry in people's reactions to startling stimuli. So we had various psychology-type black boxes. Flashing lights and stuff. We'd go around getting volunteers from the community to expose themselves to these things. Then we'd tell them we were doing various other things when what we were really looking for was to see whether they used their left eye or their right eye, whether they turned left or turned right. [laughter] I think I got paid seventy-five cents an hour for that project. Maybe it was seventy-five cents a day. I don't remember now.

BOHNING: When did you meet Calvin?

ZIMM: Well, even before getting to Berkeley, I'd met Calvin at an American Chemical Society meeting. I guess he was probably looking me over as a new faculty member. Everybody at Berkeley knew everybody else in the department.

BOHNING: I was going to ask you what Berkeley was like when you arrived there as a new faculty member.

ZIMM: Well, it was a very friendly place. G. N. Lewis had died just the year before, but his influence was still very strong around the place. There was a tremendous esprit de corps. And as I say, it was very friendly. There was no business of somebody being above somebody else or somebody standing on their dignity. Everybody was always glad to talk about whatever you were interested in. I very quickly got to know a lot of people, including Calvin.

BOHNING: What were the facilities like at that time?

ZIMM: As far as I was concerned they weren't very good. A lab bench and the usual electricity and gas and water. I had to build things up. I made several osmometers to start with and I started building that light scattering apparatus. Got some glassware from the stockroom to polymerize polymers in, and went out and bought a couple of fish tanks for thermostat baths. That sort of thing. I remember having to put in my proposed annual research expenditures at one point and I estimated two hundred dollars for the apparatus I thought I might need.

BOHNING: You were there for six years. Did you get any external support for your work during that time?

ZIMM: At the very end. ONR had started up and I got a grant from the Office of Naval Research.

BOHNING: What about consulting? Did you do any consulting during that period?

ZIMM: Very little, if any. I remember going to Dow Chemical Company in Midland [Michigan] once. There may have been a couple of other things but they didn't amount to very much.

BOHNING: Would that have been on light scattering work as well?

ZIMM: Yes.

BOHNING: Were they doing any of that?

ZIMM: They were.

BOHNING: Of course, Alfrey was there by that time, wasn't he?

ZIMM: No, that was before Alfrey was there. It was [Raymond F.] Boyer I actually went to see. I remember that they were doing light scattering and it wasn't working very well. They wanted me to come in and see what I could do about it. My recollection is mainly that their solutions were dirty.

BOHNING: In that article you mentioned Mayer's "Derby-Hat" theory (17). I guess it was around this time that you came back to it and did some more experiments. You and Doty had tried something much earlier, hadn't you?

ZIMM: Yes, with McMillan. That was what we were doing when Doty heard about the Debye experiments. We were looking for the "Derby-Hat" in McMillan's critical solutions of triethylamine and water, and we didn't find anything much that looked like it. So we didn't know quite what to make of that and didn't do anything with it. Then the Debye stuff came along. About the time I got my apparatus working well at Berkeley, somebody organized a symposium on the critical point for the American Chemical Society, and I was invited to participate in it. So I set to work. I think the talk was to be at the end of the summer and I set to work during the summer to make as many measurements as I could. And that was some of the early, I might say, rigorous light scattering from critical solutions (18). But I had really one of the first quantitatively accurate apparatuses.

BOHNING: How did Mayer react to this?

ZIMM: He was very generous. It must have been an unpleasant thing, I think, for him to find that experiment didn't show any critical point "Derby-Hat" phenomenon. On the other hand, the experiments did show there wasn't a classical Van der Waals critical point either. There was something more complicated, so I think he found that interesting, although he must have been disappointed that the "Derby-Hat" didn't appear. He was very generous about it. There was no suggestion of his being bitter, or anything. I suppose the fact that I was one of his students probably helped.

BOHNING: Around the same time, around 1950 I think, you spent a year at Harvard?

ZIMM: 1950-51. I was a visiting lecturer there. [George B.] Kistiakowsky had a sabbatical. When somebody took a sabbatical, Harvard would bring in someone to teach his classes for that year. That year it was me.

BOHNING: I see. How did that arrangement come about? Had you been looking for something?

ZIMM: No. I hadn't been looking. I don't know how it came about. I guess I got a letter in the mail asking if I'd be interested in doing it. They had been looking for somebody a year or two previously, and were interviewing people. They had interviewed Doty and they interviewed me among others. Doty actually got the job and went there. That was only a couple of years before and they probably remembered that when it came to finding somebody to substitute for Kistiakowsky. I guess I got either a letter or a phone call asking if I'd be interested in doing it.

BOHNING: Did you do any research while you were there?

ZIMM: I did some theoretical work on the critical point (19), and also wrote up some of our other previous work.

BOHNING: Were there any other benefits that accrued from your spending that year at Harvard?

ZIMM: Well, I of course made a lot of new friends, which is always a benefit. Kistiakowsky, E. Bright Wilson, and various other people at Harvard, to a lesser degree. To get the chronology right; actually, I had gone to General Electric Labs for a month during the previous summer before going on to Harvard.

BOHNING: I noticed there was some overlap in those dates.

ZIMM: Right. That was a get-acquainted month, essentially. Their interviewers had come out. Well, to go back one. I had actually interviewed for a job at GE just after I had gone to Brooklyn since that was not a permanent job. So we were acquainted. That was when I met Langmuir although I made much more of a contact with Ray [Raymond M.] Fuoss at GE. He was a consultant at Brooklyn and came there quite frequently. So I was well acquainted with him. Then the GE interviewers came out to Berkeley in 1950. I was not entirely happy with the situation as it was developing at Berkeley. It looked like there was going to be more and more work and less and less play as time went on. Well...

BOHNING: Could you expand on that a little bit?

ZIMM: Well, yes. Berkeley was very nice for a beginning instructor. They left you pretty much alone to do your research and teach your course, whatever it was, and didn't have to do much of anything else. The courses took some time, but they were not excessive, and you had a lot of time. Then as time went on, you got more and more into the workings of the department, and that meant you had more and more things to do. You got on committees. I remember I got on a committee planning a new building. I could see where this was leading. The older faculty at Berkeley were hardly directly involved with research themselves. They all had graduate students doing it and that was not what I particularly wanted to do. So I was interested in exploring alternatives. That's why I talked to the GE people who said, "Why don't you come around next summer, and let's get acquainted." So I did that before going to Harvard. I think the Harvard thing developed about the same time. I went to GE for a month in the summer and then went to Harvard for the year. During the course of the year I was offered the job at GE, took it and went there instead of going back to Berkeley.

BOHNING: How did you react to the move from academe to industry? Had you done that purposely or was it just that the GE people were there and you had been talking to them?

ZIMM: Well, I went and saw the GE atmosphere during that summer. Actually I had seen it once on the interview trip before. It looked very interesting. It was a very unrestricted type of atmosphere, and of course very good facilities. If you had an interesting project to work on, usually they'd let you do it. In that respect it wasn't very different from academia. You could spend much more time on your research, in place of having to teach a course and sit on committees. Once in a while they'd come around with some particular problem that needed immediate work. That was the way you paid your way. At the time when I was there it was a very good place for doing that kind of research. For a while after that it got much more applied, but now I think it's gotten back more toward the fundamental type of work. It was a rather unusual industrial laboratory in that respect.

BOHNING: This was in Schenectady?

ZIMM: Yes.

BOHNING: What kind of administrative arrangement did they have in the lab?

ZIMM: I guess much the usual kind of things. Starting at the top there was a vice president/ director of research, and then the lab was divided into, I think five departments, of which chemistry was one. Each had a director. The chemistry department was divided into several sections, one of which was physical chemistry and that was the one I was in. About fifteen people in it with a manager who was busy all the time going to meetings. [laughter] The rest of us were down at the bench where we put all our time into science.

BOHNING: Do you remember who the manager was?

ZIMM: The physical chemistry manager was a fellow named Herman Liebhafsky, a Berkeley Ph.D. of some years before. The chemistry manager, Liebhafsky's superior, was Abraham Lincoln Marshall. The laboratory director was C. Guy Suits who was an applied physicist, with some fifty patents, I think, to his name. They were a good group of people. Marshall was somewhat hard to get along with personally; some people found him very difficult, but I never had any problems. Some people found it hard to know what he was thinking, and I guess that created problems. It was hard to know what he was thinking but good thoughts came out of most of his thinking.

BOHNING: What about others in your group?

ZIMM: At GE? Well, I shared a laboratory with several people. All the time I was there, I was with Fraser P. Price. We all worked in polymers. There were other people who came and went; one was John D. Hoffman, who is now at Midland, the director of the Michigan Midland Institute.

BOHNING: I met him about two weeks ago.

ZIMM: Oh, did you? Well, he hasn't changed much. [laughter] Julian H. Gibbs was there for awhile. Julian became the president of Amherst last year. And then there was Ralph W. Kilb, with whom I collaborated with on a paper. He's now in Santa Barbara, but not working for GE anymore. He's working with some nuclear chemistry outfit, I think.

BOHNING: Were you given any particular assignment when you first arrived there, or were you free at that point to...

ZIMM: They usually left you alone to develop your own project, unless you wanted to go directly into some existing project. In my case, I was left alone to work in polymer physical chemistry so I started out trying to see if I could devise new techniques for molecular size and shape. Light scattering by then was pretty well established and there wasn't much obvious left to do with it. Of course we didn't have any lasers, so there wasn't any laser light scattering. I started looking into various dynamic methods which I got into because of something I'd done out at Berkeley on the Kerr Effect; electro-optical layer birefringence. That was at Berkeley with Chester T. O'Konski (20). He's still at Berkeley. That was an outgrowth of the light scattering, I just want to add.

BOHNING: Were you sensitive to practical applications while you were developing this? In terms of working for a company rather than being...

ZIMM: Well, my feeling was that if I could get a good alternative method for molecular weights, size and shapes, which were always very tedious methods, they would be useful in the laboratory, because there was a lot of polymer work going on there. Marshall had some thirty chemists and most of them were working on polymer projects. But I didn't have real immediate, money making practical ideas in mind. One of the disappointments of my interaction with GE is that nothing of that kind ever really did develop. I got one patent while I was there, for a process of making polymers of uniform molecular weight, but it was not a commercially interesting process (21).

BOHNING: I noticed that during that period which you were talking about, eight years, I guess...

ZIMM: Nine years, actually. 1951 through 1960.

BOHNING: ...that you published at least twenty-five papers, maybe even more, which strikes me as being a rather large number from an industrial research lab.

ZIMM: Well, as I told you it was a pure research environment and only occasionally did we have to get involved in projects that wouldn't lead to publishable results.

BOHNING: Was that unusual for the time, or was that common throughout other laboratories?

ZIMM: That was unusual. GE and Bell have always been unusual that way. More recently in IBM laboratories and maybe a few other places have been like that. But in most industrial laboratories that sort of thing is quite unusual. People do publishable, more or less fundamental, work on the side whereas at GE you could make that your main effort and do other things on the side.

BOHNING: During that eight or nine year period, was there much movement within the group or was the personnel pretty steady?

ZIMM: There was a fair amount of turnover. As you noticed I mentioned that Price and I were always in the same group there but three of the other people left. Hoffman, then Gibbs and then Kilb. So there was a fair amount of turnover. There was a fair amount of turnover in the rest of the physical chemistry group too, as people switched around from one project to another and new people came in and so forth. Yes, I think it was unusual for anybody to stay on one project for more than three or four years, though it did happen.

BOHNING: Did Hoffman go to NBS after he left GE?

ZIMM: Yes he left GE and then went to NBS.

BOHNING: You published at least one, maybe more, papers with him.

ZIMM: At least one. We published a paper on thermal diffusion together (22). I think we must have published one or two others. I'm not quite sure now just what. When Hoffman was there he wasn't much interested in working on polymers. He was interested in smaller molecule dielectrics. Polymers took a hold of him, after he left. [laughter] He is very firmly established in polymers today. But at that time our research interests didn't overlap very closely even though we were in the same laboratory.

BOHNING: I'm not clear on the dates now, but you started some work on DNA about the time that you left GE I'm not sure if that started at GE or after that.

ZIMM: It was just at the end. Just before leaving GE I went to Yale for the spring semester as a visiting professor.

BOHNING: During the spring of 1960.

ZIMM: Spring of 1960. I guess it was then that I wrote the first theoretical paper on DNA and also met and collaborated with a fellow who was at Yale. William Ginoza was doing experiments on DNA as a transforming principle, that is, carrying genetic information from one organism to another. I helped him interpret experiments he was doing on the inactivation of this transforming principle by heat (23). In other words, a simple physical phase transition.

BOHNING: What led you to leave GE?

ZIMM: Well, it was a growing interest in biochemistry on my part. I began to feel that polymer chemistry was moving pretty slowly. And certainly things like light scattering that I had done for a long time were well past.

BOHNING: How did you set about looking for something new?

ZIMM: Well, I had a little acquaintance with biochemistry through contact with Stern at Brooklyn Polytechnic, with Calvin at Berkeley, and with Doty the year that I spent at Harvard.

Doty was already working on DNA at that time. Then I got interested in helix-coil transitions from the theoretical aspect because I was invited to give a talk on it at Brooklyn Poly sometime in the late 1950s. So I had to go to the literature and look up what there was and found that there wasn't very much and realised this was a subject that ought to be amenable to some fairly straightforward statistical mechanics. I got into working on that and that rekindled my interest in the whole biopolymer field as opposed to synthetic polymers. Then I got invited to join a month-long workshop at Colorado which was run by NIH in the summer of 1958. That really brought me up to date on what was going on, so I determined to go into physical biochemistry, or polymer biochemistry, whatever you want to call it, leaving synthetic polymer chemistry behind. It seemed that it would probably be a good time to leave GE because, while they didn't object to my doing this, there wasn't anything similar going on there, and it really didn't look to be something that would fit comfortably into the laboratory. So I started to look around to see what opportunities there might be.

BOHNING: What were some of the places you were considering?

ZIMM: Well, there was Yale. And I re-established contact with Berkeley. Then, on the way out to see the people at Berkeley, where I was invited to give a talk, I stopped off at Chicago to see Mayer. He said that he was seriously considering going to the new California campus at La Jolla, so I worked that into the schedule, flew there from Berkeley to see what it was all about. I also looked into Stanford too. Those were enough places to look at. [laughter] I had to make up my mind!

BOHNING: You went there [La Jolla] then in 1960, is that right?

ZIMM: Yes, that's right. There was also a fellow going there from the biochemistry group at Yale, David Bonner. That was one of the reasons I went to La Jolla.

BOHNING: What were the conditions at La Jolla when you were there?

ZIMM: Very interesting. It had been just an oceanographic station until then. They had a couple of new buildings; the school of science and engineering was officially established but had hardly any physically existence yet. But at least they had it formed on paper, and they were recruiting chemists and physicists and biologists. We came there and moved into a new oceanographic building, kept the oceanographers out for a few years, and started the school up pretty much from scratch, except for the fact that there was an oceanography graduate program already going, so we had something to build on. But we had to set up the curricula completely, advertise around and get graduate students.

BOHNING: Did you find yourself back on committees again at this point?

ZIMM: Of course! [laughter] Yeah. I think by that time I'd forgotten what it was like to be in a university.

BOHNING: Were you able to extricate yourself or have you continued to be active in this?

ZIMM: No, I haven't. It's pretty well impossible to extricate oneself completely, but after a few years I did succeed in toning it down somewhat.

BOHNING: Where were your sources of external support coming from?

ZIMM: First NSF and then NIH.

BOHNING: And consulting?

ZIMM: Not to amount to anything. An occasional consulting trip. One or two times a year someplace. Or somebody would come into town and they'd want to consult, instrumentation usually. I remember I went up to Beckman's operation in Fullerton once and a guy came down from Hewlett Packard; several other places like that. Aerojet General, I remember. All individual consulting things, no long lasting relationships.

BOHNING: Could you tell me something about the graduate students?

ZIMM: Well, the first graduate student in the department and the first one I had at La Jolla was Don Crothers, who is now a professor at Yale. I guess he came partly because of my connection with Yale. He'd been an undergraduate at Yale and I think he was in contact with people at Yale. I got a couple of postdocs who had been graduate students at Yale, Jerry A. Harpst and Neville R. Kallenbach, both physical chemists. Harpst is now at Case Western Reserve and Kallenbach is at the University of Pennsylvania. Rodney E. Harrington was also a postdoc. He was from the University of Washington at Seattle. He's now at the

University of Nevada at Reno. They were the initial group at La Jolla. And then other people came in gradually.

BOHNING: I see Harrington's name here and it's in polymer degradation work (24).

ZIMM: Yes.

BOHNING: But you started building instruments again too, didn't you?

ZIMM: Well, we needed instruments. My old light scattering instruments were still at Berkeley in a storeroom, and I went up there with my station wagon in June of the first year, and with the help of a couple of people there, dug it out of the storeroom, loaded it in the car and brought it back. What there was left of it anyway, which was most of it. I needed other things as you can't do experimental work in physical chemistry without instruments. There was no very good way of measuring the viscosity of DNA solutions at that time. DNA is very difficult thing to measure by viscosity because the molecules are so big they just deform very easily in flow.

We were trying to think of a good way of doing it. I was washing the dishes at home one night, and I had drinking glasses in the sink full of water. A glass happened to be sitting inside a somewhat larger glass; the inside one had a very heavy knob of glass on the bottom so it was properly weighted. It was just floating there and slowly turning around, and it occurred to me that the only thing that was opposing the turning was the viscosity of the liquid in between. That was the genesis of the rotating cylinder viscometer which Crothers and I then succeeded in putting together in the laboratory (25). It worked very well for DNA solutions. People still use it once in a while.

What else did we do? At GE I had spent quite a lot of time on viscoelasticity. I'd started to talk about that to you before, dynamic methods of measuring molecular size and shape. I thought of various possibilities for doing this. Viscoelasticity was probably one of the most practical, because it produced a big effect to measure. I actually built an instrument at GE and developed some theory to understand what was going on. After went to La Jolla, a couple of the later postdocs had thought it would be interesting to follow this up with DNA. Actually the guy who was most interested in doing that was Stanley J. Gill who was taking a sabbatical from Colorado. He had done some viscoelasticity experiments when he was working in an Army research laboratory somewhere shortly after the war. It occurred to him that it would be interesting to do it on DNA. In anticipation of his coming to La Jolla, I bought a flow birefringence instrument which uses a very closely related phenomenon. He and another younger postdoc named Doug [Douglas S.] Thompson from MIT succeeded in using it to measure dynamic flow birefringence with DNA solutions (26). This suggested that it would be good to measure dynamic viscoelasticity, and Thompson actually started to modify it. But then he left, and a graduate student, Lynn Klotz, took it over for his thesis and did a very good job of it, and we got a really good working instrument out of it. We were able to measure DNA relaxation times (27); a sort of long culmination to something I started at GE ten or twenty years before. After that time I succeeded in really using it as a technique for size and shape measurements on DNA. So that was a long haul.

BOHNING: Some of those papers were published not too many years ago, right?

ZIMM: Klotz's thesis was published in 1971, I think. The work was followed up by another postdoc, Ruth Kavenoff, who succeeded in doing more spectacular work measuring chromosome sizes (28). She succeeded in measuring the sizes of DNA from chromosomes of Drosophila fruit flies which showed that one piece of DNA went with one chromosome which, up to that time, was something geneticists liked to believe, but for which there was no demonstration.

BOHNING: Let me back up for a minute. There is one thing that you wrote that intrigued me. At the very end of the Annual Reviews of Physical Chemistry article (6), you said that the circumstances surrounding the determination of the molecular weight of polymers are curious from a bibliographic point of view.

ZIMM: Well, as I say there, the war confused the whole publication situation. Most of Debye's work, for example, is in unpublished reports and there was just one short paper in Journal of Applied Physics (29) and a later paper considerably after the war was over (30). There was verbal stuff. Debye came and gave a lecture at Brooklyn, for example; I remember it was after I'd been there a few months. We took the best notes that we could from his lecture.

BOHNING: How was he as a lecturer?

ZIMM: Debye was an excellent lecturer, one of the best I've ever heard. He was in a very high class, above ninety percent or more of lecturers. Beautifully organized, very fluent; the only problem was that you thought you understood everything while he was talking, but after he got through and you went back and tried to repeat it to somebody else, you realized that Debye understood it, but you didn't. [laughter]

BOHNING: In this paper you traced through, starting back with Einstein's 1910 formula and then some things that are somewhat overlooked. I was also intrigued by the fact that in 1946 you said that you received unexpected reprints of Raman's paper (31). Did you ever find out what precipitated that?

ZIMM: Not directly. I can only guess that he saw or heard about our work, maybe from Herman Mark. I know he visited Mark one time. Maybe Mark saw him in India. Anyway, all of a sudden a reprint arrives in the mail. I think Doty got one also, maybe Mark got one. I know that other people besides myself [got reprints. Yes, it was way back there whenever Raman wrote it, in the late twenties or something.

BOHNING: 1927.

ZIMM: It could have been used at that time. It was all there.

BOHNING: But like Gibbs in thermodynamics, it was not in a publication that was terribly...

ZIMM: Well, it was in the Indian Journal of Physics or something. I suppose hardly anybody saw it. The person who could have used it would have been Mark, for example. I guess he never saw it. Mark had a research group in Germany, had several research groups. He could very well had someone to work on this [light scattering] if they had seen the paper.

BOHNING: What changes have you seen in your career in polymers; what would you say are the most significant changes that you've observed?

ZIMM: Well, there have been a number of new polymers of course. There have been new polymerization methods. The Ziegler-Natta thing came on in the 1950s. When I started out working in polymers, about the only way anybody ever polymerized anything was with radical polymerization. So there have been a great many developments on the synthetic side. And then there have been a lot of technical developments, gel permeation chromatography, the use of NMR, but actually there's been remarkably little that has changed. There's been a lot of additions to the things we used to do back in the 1940s, but then, a lot of them are still done. People still use light scattering, they still use osmometers, they certainly all still use viscosity techniques. I think scientists from the 1940s could walk into one of these sessions today and not feel very much out of place.

BOHNING: That's an interesting comment.

ZIMM: Well, he would find a lot of things that were new, but he would find a lot of things that were very familiar too.

BOHNING: To you think a lot of it has been just refinements of existing techniques?

ZIMM: Oh, yes. There's a lot of refinements. There have been these new techniques. But a lot of the old stuff has not gone out, and you still see a lot of papers on polystyrene.

BOHNING: Yes. And one wonders after all these years of polystyrene that there's still more to...

ZIMM: Yes. Well, there seems to be new things that turn up. Of course the isotactic and syndiotactic polystyrenes added a new dimension to the old polymer. Even so, it's a nice test material, and people are still using it to test their theories and so on.

BOHNING: The polymer division has been involved, and certainly Herman Mark has, in terms of both publications, and I wanted to ask you about your book that you mentioned earlier, and also with polymer education.

ZIMM: Very important thing. Universities are not doing a good job on polymer education. Just no doubt about it. At least not in this country. Many of us for years have been concerned as to just why. I think it's pretty clear. The science of polymers is not fundamental to chemistry. Chemistry is fundamental to polymers, but not vice versa. Not fundamental to physics either. The traditional pure science departments consider polymers to be out somewhere on the fringe. The result, if they teach it at all, is that they usually have one person in the department for whom polymers are kind of an avocation, and he teaches maybe one course. The natural home for polymers really is in the place where it's appreciated, in the engineering departments. I think it's relatively recently that even engineering departments have taken it up. It's been more with the growth of material science departments that polymers have entered the academic curriculum. But, for whatever reason, there are not a great many advanced students in those departments, so that there's not much Ph.D. level training in polymer science in the United States.

BOHNING: And yet with the increasing emphasis on biomaterials, you would thing there'd be a natural place for the two to come together.

ZIMM: Yes, well, there is of course a great deal of work these days on biochemistry which deals with biopolymers in the sense that proteins and nucleic acids and so forth are polymers, but the emphasis there is on other things. Different from what synthetic polymer chemists are much interested in. I don't think biomaterials have made a very large inroad at universities yet either. They are more industrial.

BOHNING: You had mentioned to me earlier that you and Michael Szwarc were writing a book. Can you tell me something about what your project is?

ZIMM: Well, the project is to write a comprehensive textbook on the chemistry and physics of polymers. We're still developing a concept. We, Michael Szwarc and Murray Goodman, have to decide for example, exactly what audience we're aiming for, what level. Elementary students, more advanced students or monograph. We've pretty well decided that it's at the advanced elementary level; graduate students and seniors. That makes it a textbook rather than a monograph. The aim is to write a book which will, we hope, be interesting, authoritative, and comprehensive enough to cover anything that one might want to put in a polymer course, extending over a period of a year, but not to go too much beyond that; for reasons of expense, and for fear of intimidating the students. So that is the aim. The three of us, being sort of experts in different fields of polymers, feel we can write an authoritative text. There really hasn't been anything of that kind since Flory's 1953 book. There have been some joint-author texts in which the authors were authorities in their individual fields. By joint author, I mean more the symposium-type texts, where there are five or six authors. Those texts,of course, have difficulties in coherence. There have been a number of texts written by an individual, but it's just not possible for one person to be an expert on everything these days, and the books show it. So we feel there's a need.

BOHNING: Do you have such a course at UCSD now?

ZIMM: We have only a small course. Our academic year is divided into three quarters. And we have a one quarter course in introductory polymers which is given to graduate students and senior level chemistry majors. So that would essentially represent one-third of this book.

BOHNING: Is that course required?

ZIMM: No, it's an elective. It's taken by something like a quarter of the chemistry majors and most of the chemical engineering majors. And some graduate students. So we have registrations running from fifteen to forty in different years. Depends on whether it happens to conflict with another important course. [laughter] We always have those problems.

BOHNING: What do you see happening in the future?

ZIMM: In the polymer field?

BOHNING: Yes. I know it's difficult to say.

ZIMM: I hesitate to predict. You know the interesting things that will happen will be unexpected things. Those are not ones that I'm going to guess now! Certainly there are going to be more refinements. As we get better and better instrumentation, we will get more and more into understanding the details on a physical level of what polymers do. That's one thing that's certainly going to come. What the molecules are doing when the gross polymer sample is doing something. That's what a lot of people are working on. I suppose there will be new synthetic methods; there have been in past years, and there probably will be some more. So I certainly expect that there are going to be a lot of developments. Whether something really unexpectedly new comes along, like the Ziegler-Natta catalysis or Szwarc's living polymers, is something nobody knows.

BOHNING: Well, I know you have another engagement this afternoon. Is there anything else that you'd want to add at this point?

ZIMM: I think we've covered things pretty well. I don't really know what else we should go over.

BOHNING: I'm looking at my notes and I think I've covered most of the things I had in mind. Well, I would like to thank you very much for taking time to spend the afternoon with us. Thank you very much, Dr. Zimm.

ZIMM: You're certainly welcome.

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