THE CHEMICAL HERITAGE FOUNDATION

WALTER H. STOCKMAYER

Transcript of an Interviews
Conducted by

Jeffrey L. Sturchio and Peter J. T. Morris

in

Philadelphia, Pennsylvania

on

25 August 1986 and 22 January 1992

(With Subsequent Corrections and Additions)

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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Walter H. Stockmeyer

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(Revised 20 February 1989)

Walter H. Stockmayer, interview by Peter J. T. Morris and Jeffrey L. Sturchio at Philadelphia, Pennsylvania, 25 August 1986 and 22 January 1992 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0049).

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

WALTER H. STOCKMAYER

Education

1935	S.B., Massachusetts Institute of Technology
1937	B.Sc., University of Oxford
1940	Ph.D., Massachusetts Institute of Technology

Professional Experience

	Massachusetts Institute of Technology
1937-1938	Teaching Fellow in Chemistry
1938-1941	Instructor and Research Fellow
1941-1943	Instructor, Columbia University
	Massachusetts Institute of Technology
1943-1946	Assistant Professor of Chemistry
1946-1952	Associate Professor of Physical Chemistry
1952-1961	Professor of Physical Chemistry
	Dartmouth College
1961-1962	Professor of Chemistry
1962-1967	Class of 1925 Professor
1967-1979	Albert W. Smith Professor
1963-1967	Chairman, Chemistry Department
1973-1976	Chairman, Chemistry Department
1979-	Professor Emeritus

Honors

1935-1937	Rhodes Scholar, Oxford University
1946	Fellow, American Academy of Arts and Sciences
1954-1955	Guggenheim Fellowship
1955	Stas Medal, Société Chimique de Belgique
1956	National Academy of Sciences
1958	Bourke Lecturer, Faraday Society
1960	College Chemistry Teacher Award, Manufacturing Chemists Association
1966	Polymer Chemistry Award, American Chemical Society
1972	Honorary Doctorate, Université, Louis-Pasteur, Strasbourg, France

1974	Debye Award in Physical Chemistry, American Chemical Society
1975	High Polymer Physics Prize, American Physical Society
1976	Honorary Fellow, Jesus College, Oxford
1977	Honor Scroll, Massachusetts Institute of Chemists
1978	Humboldt Preis, Humboldt Foundation, West Germany
1982	Distinguished Lecturer Award, Polymer Science Department, University of Massachusetts
1983	Honorary L.H.D., Dartmouth College
1987	Service Award, Division of Polymer Chemistry, American Chemical Society
1987	National Medal of Science
1988	Richards Medal, Northeastern Section, American Chemical Society
1988	Polymer Chemistry Division Award, American Chemical Society

ABSTRACT

In the first interview, Walter Stockmayer describes early influences directing him towards the chemical sciences. Stockmayer first became interested in the mathematical aspects of physical chemistry as an undergraduate at MIT. A Rhodes Scholarship brought Stockmayer to Oxford, where he undertook gas kinetics research with D. L. Chapman. Stockmayer returned to MIT for Ph.D. research and pursued his study of statistical mechanics, which he continued at Columbia. He returned to MIT in 1943 as an assistant professor of chemistry and became involved in the theory of network formation and the gelation criterion. Stockmayer increasingly directed his attention to theories of polymer solutions, light scattering and chain dynamics.

The second interview begins with Stockmayer's Guggenheim Fellowship in Strasbourg, France, his first meeting with Hermann Staudinger in Freiburg, Germany, and his subsequent return to MIT. Stockmayer then moved to Dartmouth University in 1961, where he worked primarily on copolymers in dilute solution, established the journal *Macromolecules*, and collaborated with numerous Japanese scientists. He discusses his impression of the Gordon Conferences and the polymer community since the 1940s. Stockmayer concludes with his retirement and work as a consultant for Du Pont and other companies.

INTERVIEWERS

Jeffrey L. Sturchio is Executive Director, Public Affairs, Human Health—Europe, Middle East & Africa, at Merck & Co., Inc., where he is responsible for the development, coordination, and implementation of a range of policy and communications initiatives for the region. Before assuming his current position in 1995, he was Merck's Director, Science & Technology Policy, in the Corporate Public Affairs Department from 1993 to 1994; and Associate Director, Information Resources & Publishing, from 1992 to 1993. Dr, Sturchio joined Merck & Co., Inc. as Corporate Archivist in June 1989. He received an A.B. in history from Princeton University and a Ph.D. in the history and sociology of science from the University of Pennsylvania. He was Associate Director of the Beckman Center for the History of Chemistry from 1984 to 1988, and has held teaching appointments at the New Jersey Institute of Technology, Rutgers University, and the University of Pennsylvania as well as a fellowship at the Smithsonian National Museum of American History.

Peter J. T. Morris is currently at the Department of the History of Science and Technology of the Open University, where he is Royal Society-British Academy Research Fellow. Morris was educated at Oxford University receiving his B.A., chemistry in 1978 and D.Phil., modern history in 1983. He was a Research Fellow at the Open University from 1982 to 1984. During the period 1985-1987, Peter Morris was Assistant Director for Special Projects at the Beckman Center. He was the Royal Society-British Academy Research Fellow at the Open University, Milton Keynes, between 1987 and 1991, and Edelstein International Fellow in

1991-92. He is author of the monographs, *Archives of the British Chemical Industry, 1800-1914*, *Polymer Pioneers*, and *The American Synthetic Rubber Research Program*. Morris also co-edited *Milestones in 150 Years of the Chemical Industry* and *The Development of Plastics*.

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INTERVIEWERS: Jeffery L. Sturchio and Peter J. T. Morris

LOCATION: Smith Hall, University of Pennsylvania

DATE: 25 August 1986

STURCHIO: We know you were born in Rutherford, New Jersey in 1914, but we don't really know much else about your family or your early life. I wonder of you could tell us about your parents and what they did?

STOCKMAYER: My mother came to the U.S. as a twelve-year-old girl in the 1890s, the daughter of a man who, as a very young man, had come and fought on the Union side in the Civil War, went back home, begot a business and nine children, lost his money, and returned to the States where his membership in the Grand Army of the Republic got him a job, eventually at Ellis Island because he knew all the Eastern European languages. His ancestry was mainly German with some Swedish, but the name was Bostroem, which is a Swedish name. They lived in Estonia, which was then part of Imperial Russia. Anyhow, the family followed and my mother went to Hunter College and taught school in New York City.

My father was from southern Germany near Stuttgart, an organic chemist by training. He worked with a guy called Hell. If you ever remember the Hell-Volhard-Zelinsky reaction, well, that's the guy who was his Doktor-Vater, and he published a couple of papers out of his thesis. Then he went into the German dyestuff industry with a firm called Siegle, which had some connections in New York and I think a branch, so he came to the U.S. in 1909, intending, presumably, to go back to Germany after a few years. But he met my mother at a choral society in New York called the Liederkranz—the German choral society. And 1914 arrived, first I arrived, and then the war, and then it was hard for him to get back, and at least, I don't think he tried too hard. After the war, he seriously thought of going home, and I'm very grateful that when he made a trip in 1921 to go have a look, he decided that it was not going to be a great place to live, and we stayed in the U.S.A. I would have been cannon fodder for sure in Hitler's army, you know. He was in the States where he worked for a while in Cleveland for Grasselli, but got back into what became printing inks.

As a little boy, I remember his coming home from the lab in East Rutherford with beautiful stains on his fingers—red, blue, green, violet, all that—"Gee, what a wonderful job." [laughter] So, there was some sort of subliminal reverence for chemistry. Actually of course, in school you scarcely know that. By the time I hit chemistry in high school, I had begun teen-age rebellion, which was not very vociferous or open or anything, but for a while I felt that I didn't want to be a chemist just because my father was. When I got to MIT as a freshman, I simply liked it better than the other subject so I stuck with it.

STURCHIO: That's interesting. Do you recall any other early influences, for instance, books around the house that might have piqued your interest in science? Teachers in high school?

STOCKMAYER: Well, let me say that, for one thing, I guess there was certainly intellectual discipline around the house. There was music, a lot of piano playing and I would hate to leave the ball game at 5:00 p.m. and do the practice before my father got home because he didn't want to hear all those damn exercises. But I'm very grateful now for that. And I had to learn to read and write German. I spoke only German until I got to kindergarten. That made for some embarrassment in kindergarten, I guess, but no matter. So I had two languages to hand. I had to write German script to my grandmother back in Germany and read that damn Gothic. Half the young Germans today can't read that, really.

Specifically chemistry, I don't think the books around the house were of that kind, they were more general. Things for people of all ages to read. I had an aunt who worked in Brentano's in New York so we had a lot of good books around, thanks to her, in part. And what else? The teachers—well, in high school, the physics teacher was terrible, as was the text book. The chemistry teacher was very likable, and I thought the course good, and one of the math teachers, a woman named Julia Ross, was extremely able and encouraging. I didn't know what real physics was like or I might have tried that, but from the high school physics course, I had completely the wrong notion about what physicists did and what it was all about. I hated it at the time, but not after I arrived at MIT where I think the best teacher I ever had was my freshman physics teacher, Nathaniel H. Frank, who is well known as a pedagogue in physics. He died about two or three years ago. Slater and Frank, the famous *Introduction to Theoretical Physics* (1). He also wrote a couple of first year and second year texts—*Introduction to Mechanics and Heat* and *Introduction to Electricity and Optics* (2). We received those in mimeograph form because he was just writing them while he was teaching them, and very often, the calculus that was used in this text came to us in the physics course before we'd had it in the math course, which bothered a hell of a lot of people. Well, I didn't much care what the name of the course was called where I learned it, so it was all right, but it was tough, it was tough.

STURCHIO: I would like to talk some more about your under-graduate years at MIT, but I wanted to ask about brothers and sisters.

STOCKMAYER: I have one sister who lives in Connecticut in retirement, married.

STURCHIO: Did she go into science at all?

STOCKMAYER: No, she didn't. She studied languages at Middlebury and got married fairly early. She took a secretarial course at Katharine Gibbs and worked for a while that way and met her future husband through the work. No, I don't think she was scientifically inclined.

STURCHIO: It sounds as though it was always fairly clear that you were going to go on to college. Was it?

STOCKMAYER: I don't recall thinking that I wasn't, but I mean, it was not a conscious decision. The only question was, where did I want to go? I applied to Stevens [Institute of Technology] Tech., MIT, and to Rutgers, and made it to MIT. In those days, you had to take the college boards, and my mathematical weakness was in synthetic, Euclidean, and such geometry. The solid geometry exam, based on a not very good high school course, not from this favorite teacher, but somebody else; I didn't like the subject and I just about passed that bloody thing. I didn't get very good grades, but I made it anyhow.

STURCHIO: Why those three schools in particular?

STOCKMAYER: Well, by then, it was clear that I wanted to do science and Stevens was nearby and a lot of kids from Rutherford knew about it and went there. The father of a high school friend was a graduate from there and thought well of it. MIT because of its reputation from afar. I really didn't know anybody who'd been there. And Rutgers as a sort of back up, after all, the state university in my own state. Again, a lot of kids went there.

STURCHIO: You started to tell us about your physics course at MIT, and that it was tough. How did you find the atmosphere at MIT in general? When did you start these?

STOCKMAYER: I got there in 1931. Karl T. Compton had just been inaugurated as president the year before. It was a real upswing time.

I'm looking at MIT now from hindsight and from later membership on the faculty and knowing what my older faculty colleagues recall from those days and what a real boost it was when Compton came and promptly hired physicists like [John C.] Slater and [Philip M.] Morse to join the physics department, and started to encourage research. MIT wasn't <u>entirely</u> unknown for scientists rather than engineers. For example, Linus Pauling had spent a year there, or part of a year, as a visiting professor. Peter Debye had been there several times. It wasn't <u>completely</u> without what's become the modern point of view—the scientific rather than the purely technical school—but I think they were a bit behind Caltech all the way, and have remained so in some ways, certainly.

STURCHIO: Was that clear as early as 1934?

STOCKMAYER: For me, as an undergraduate, the outstanding thing was the incredible burden of work compared to a not very demanding high school. I couldn't believe it when the professor in a freshman English course curled his lip—I thought in scorn, but he was actual trying to smile benignly—and said, "Gentlemen, I would like you to write five hundred words by next Tuesday." I couldn't believe he demanded that much—that's more than two pages! Altogether the first few weeks at MIT were for me traumatic. I claim that I thought of jumping into the Charles River. What I actually did was to jump on to a bus and ride home on Columbus Day weekend, less than three weeks after classes had started, to flee into the arms of my mother and be comforted. I was sure I was going to be a failure. One had the feeling that all of one's six hundred thirty and something classmates were high school valedictorians, which I wasn't. They were all going to make it and I was going to be a big failure. Actually, MIT were very smart about that. You got a set of provisional grades after five weeks, again after ten weeks, and the semester was fifteen weeks. I did well after five weeks, I had met a couple of friends, and things started to look up. After ten weeks it was still OK, and then I hit that physics course for an "A" the first term and after that I was happy.

It was such a tremendous jump in workload. I tried to do everything, of course. This is what you have to learn—that nobody can possibly do everything you're expected to do. You have to learn to cut corners here and there. It's a terrible philosophy, but damn well true of anybody who gets through MIT.

STURCHIO: I was going to ask what other courses you took—you mentioned English and physics. Did MIT have a curriculum that was rigidly prescribed?

STOCKMAYER: It was pretty damn rigid. The freshman year was common to everyone, with the exception of the architects, who had a slight difference, but everyone at MIT then took one year of chemistry, two years of physics, two years of calculus, two years of what was called English and History. There were three or four different options in the sophomore year, and I was able to choose something that interested me, essentially the History of Intellectual Thought, although there were others, more political history, one European, one US, something like that. I chose something called the Randall option, because the textbook was *The Making of the Modern Mind* by John Herman Randall of the Columbia Philosophy Department (3). That again was an eye-opener. Thee are things that you don't learn at home as a teen-age kid, when you're starting to unfold. I thought I really had a set of dedicated teachers at MIT, and the reputation it has of being a factory uninterested in its undergraduates I think is false. It was false then and it's remained so. But it's a hard reputation to overcome.

There were so many people I met in the days when I was still on the MIT faculty and looking for kids to go there. In the town of Weston, Massachusetts, where we lived, the guidance counselor at the high school was a Harvard man, and he would tell every smart kid that he should go to Harvard instead because there was no intellectual atmosphere at MIT; it was just a grind, you know. That was a lot of horseshit; it was a marvelous place for intellectual atmosphere and from everything I've been able to see, the professors at MIT were more accessible than their Harvard counterparts. They might not all have been the world scholars that some of their Harvard colleagues were, but they were quite adequate for us and more. I've kept friends with some of them—Bill Greene, a long-retired professor of English. William C. Greene is still very much alive at age eighty-six or eighty-seven. We see each other now and then.

The chemistry curriculum, too, had a fairly strict set of courses. In your junior year you had organic and physical, and your sophomore year was all analytical, qual. and quant. There were also some specialty analytical courses, some of them leftovers from a more technical curriculum of the early 1900s. We all had to take one term of general biology, and there were supposed to be two terms of industrial chemistry. I got out of one of those by signing up for the theoretical physics course as a senior and by petition being allowed to drop one term of industrial chemistry.

STURCHIO: Who were some of the professors you had in chemistry?

STOCKMAYER: Well, in physical chemistry, Miles Sherrill; Miles Standish Sherrill, as it reads in the catalog. I found later that his middle name was not given to him as a boy but one he took when he came East. He figured it could help him, and it did. Sherrill was a fine man who made physical chemistry—the classical kind—pretty interesting. I don't think he was a Ned Frank. What I remember about Frank as a teacher was that he always closed a particular topic with great apparent reluctance. He would tell the class, "There's so much more to this field than we have time to study, but let me just open the window for you very briefly." And then in a couple of minutes he'd say, "If you were going on, the next step would be…." For example, when we learned simple elasticity in freshman mechanics; Young's modulus, Poisson's ratio and not much else, he would say, "The real way you would want to study stresses and strains would require you to know something called tensors. Now you've not had that in your math course and I can't teach it to you now, but that's the way. No matter how you try to deform something, no matter which way it deformed you'd be able to describe it in mathematical terms." And then he would go on to the next thing. But if you were curious about things, well, you'd go and look somewhere and try to find out some of those things. A great technique that I've tried to emulate myself, and it does work with bright kids—they want to know more.

On the other hand, there were some teachers around, perhaps the poorer ones, who in self-defense would say, "Well, that's not something that you're ready to learn in this course." And the answer might have been that they didn't know. An outstanding example that I recall was in my graduate school days and I won't mention all the names, but a very good fellow-student, a man who later became president of Arthur D. Little, Howard O. McMahon, asked the professor, "You know, we've been talking about rotational spectra, and in mechanics we learned about the conservation of angular momentum. Well, if a molecule rotates faster or slower after interacting with light where did the angular momentum go? It wasn't conserved." And the professor said, "Well, I can see you haven't had the proper background to take this course." The fact is, he didn't know the answer, so Mac went to Slater who said, "Well, photons have a spin and they can spin up or down like electrons and so on, although they have a quantum number of +1 rather than ½ and so on." In other words he started explaining about the special funny properties of photons that were needed to ensure conservation of momentum. A good teacher would have said, "Well, I'll find out and I'll tell you next time," or maybe give him a partial answer. I've always tried to say, "I don't know but I'll try to find out" when that's happened to me. I remember this case because we were all so unhappy.

MIT was a great place to learn things. There was a physiologist friend of mine at Michigan, Horace W. Davenport, who was a Caltech undergraduate, and he recalled the story about a freshman who went to whoever was then the dean of Caltech and complained about the caliber of the teaching. The dean said, "Young man, Caltech is a place for learning, not for teaching." A little bit cynical.

STURCHIO: The anecdote you told us about Slater and the conservation of momentum and rotational spectra took place a few years later, but in 1931, 1932 and 1933 when you were an undergraduate at MIT, those were the years when the new quantum theory was really first being applied to physical phenomena.

STOCKMAYER: Well, Slater in fact was one of those doing it, and he was right there. Of course, you know, in the undergrad curriculum we were given a course on atomic physics as seen by the physical chemists; it was part of the two-year physical chemistry sequence. We learned about the Bohr theory and a few things beyond that. I don't recall whether I learned any quantum mechanics there. When I got to Oxford I bought a Pauling and Wilson and read it on my own (4), but that wasn't my assignment over there, just trying to get ahead.

STURCHIO: That was published in 1935 or so?

STOCKMAYER: Yes.

STURCHIO: One thing that struck me about the first few papers you published at MIT was the way in which you handled a much more sophisticated variety of mathematics than most chemists would have come across in their graduate careers unless they were going into a fairly theoretical area, and I just wondered whether you had begun to take extra math. You mentioned the theoretical physics course, for instance.

STOCKMAYER: Well, actually, the theoretical physics—I'll confess to you what happened. I became president of my class as a senior at MIT, in fact had done that as a junior, too. I took this theoretical physics course and I was also growing up socially, and I dropped the course. I was allowed to drop it. After coming back from Oxford I took it again, as a graduate student. At that time the physical chemists in grad school at MIT were required, well, everybody had to do a minor and they were told, "This minor of yours had better be in physics, and you had better take the theoretical physics courses from Frank and Slater," so we did it as a matter of course. But I wanted to, anyhow.

But I had done some extra math as an undergraduate, because I liked it, and a couple of terms of advanced calculus and some vector algebra and such things that were not prescribed by the chemistry curriculum. I was a teaching assistant in the qual. laboratory. Qual. used to be a summer course. Every MIT chemistry major was required to stick around six weeks after his freshman year to take a really big course in qualitative analysis taught out of Arthur A. Noyes's book (5). In the summer after my junior year I was invited to be one of the teaching assistants in that course, so I could manage that and also take some courses on my own. That's when I took the advanced calculus, during the summer, so I hung around for the rest of the summer to finish that off and maybe loaf a little.

STURCHIO: It sounds like you, even as an undergraduate, began to get some of the background in math that later helped.

STOCKMAYER: Nobody pushed me, and I didn't really know which way I was going. If I had been clearer about that I might have tried to do a physics degree at Oxford or something like that, but, I was really slow at putting it all together.

STURCHIO: What did finally steer you in the direction you took finally? By the time you were a senior, say?

STOCKMAYER: Well, even though I was pretty good at organic chemistry, I found I didn't really have a great deal of skill in the laboratory, and subconsciously there may have been another thing. When I was a junior and taking these two terms of organic chemistry, a rather unusual freshman entered MIT. At the end of his first term (he had been made to take the usual curriculum of freshman chemistry), he also asked permission to take the final in organic chemistry since he had been doing a little studying on his own: he got about a 96. So he joined our class for the second term, and we all tried to eat his dust as he went by, because the name of this guy was Robert Burns Woodward. Bob Woodward was a very attractive and still very modest fellow as a freshman. He didn't remain modest, but then he didn't have to. He was so damned good just naturally; he knew his stuff once he read it and he was good in the lab. We had to end up the second term with a six week synthetic project of our own devising, and I know mine came to grief partly because I did something really foolish; instead of using freshly precipitated manganese dioxide (I hadn't read things carefully enough) I got some pyrolusite, which has huge crystals, no surface at all, and I cooked the damned stuff for practically five weeks and nothing ever happened, and I hadn't enough brains to do anything next because I...well, whatever. But Woodward—I remember he'd read somewhere that selenium dioxide was a good oxidizing agent for certain specific groups, maybe king ketones out of something or other, and he devised a project that was practically already new research. I remember the bright color of the selenium in there. Finely divided. Was it the oxide or the metal that as red? It was a red color, that stuff. It was a couple of benches away from mine. It's a sidetrack, but since Woodward is such a great figure that I can't refrain from mentioning him.

STURCHIO: I'm glad you brought him in, because one of the things we wanted to ask was who were some of your fellow-students as well as faculty members.

STOCKMAYER: Well, I liked organic all right, but I guess I liked physical better, I guess because Sherrill was to me a more inspiring figure. Our organic profs were Ernie [Ernest H.] Huntress, and Avery A. Ashdown, both good teachers. Huntress was not exactly slim, and he wrote so fast on the blackboard that he used to pant, I think just from writing so fast. That was our joke, anyhow, maybe he would have panted without writing.

I even elected to do a senior thesis outside the chemistry department with Hans Mueller, a physicist and a former Debye and Scherrer student from Zurich, and that thesis was a big flop, too. He passed me, but he shouldn't have. I didn't put in the time although it was a horribly tough project; I guess he thought I was better than I was. I was supposed to grow big single crystals of ice and then measure the Kerr effect, the electric double refraction. Well, hexagonal ice is already doubly refracting, and so you had to try to get a big single crystal and align it and measure the extra birefringence. I got nowhere, so at the end I said, "Gee, Professor Mueller, I better build something quick and measure some liquids," and so that's what we did. You can always measure more liquids than have been so far measured. It was not a devoted effort. There were directions on how to grow big ice crystals but I didn't have enough skill or facility for the lab work to do it right, and I never did get a single crystal. Every hunk of ice I ever froze was just scattering in all directions; it was just awful.

STURCHIO: It sounds like you had plenty of opportunity to do some research while you were at MIT.

STOCKMAYER: Everybody had to do a senior thesis, and you were supposed to put in fifteen hours a week for the whole senior year, close to five hundred hours. And some kids did beautiful things. I wasn't around when Woodward did his ever-faster accomplishment of formal studies, but some of my classmates did some pretty nice things.

STURCHIO: Have you remained in touch with any of your classmates?

STOCKMAYER: Yes, for example there's a man named Howard S. Mason. He is now professor emeritus at the University of Oregon in Portland. Howard stayed at MIT for his Ph.D. with Nicky [Nicholas A.] Milas and then he was Arthur Michael's last post-doc at Harvard, and I wish I could remember everywhere he worked. He had an international thing; he was in Cambridge for a while and worked with Todd.

[END OF TAPE, SIDE 1]

STOCKMAYER: Mason worked on oxidative enzymes later, but I remember when he worked on the active ingredient of poison ivy. That was kind of interesting to somebody who had had a few cases in New Jersey!

STURCHIO: That would be an interesting topic to work on. It would make me a little nervous. Were there other fellow students who went on to do chemical research?

STOCKMAYER: Well, there were only 15 of us who were chem majors. A few of went into industry directly after a bachelor's degree. A few went on to grad school. Another guy who stayed right at MIT for a doctorate, Paul Panagiotkos, ended as a professor at Lowell. He died a couple of years ago. Leo Epstein, a fellow physical chemist who stayed at MIT and took his Ph.D. under Scatchard and spent most of his career with General Electric at their atomic power lab out in California, and then eventually at Argonne National Laboratory. Leo had a lot to do with liquid sodium cooling of nuclear reactors. He's retired and living in San Francisco. I see him once every five years, roughly.

One of the very good ones, Percy Ehrlich, in a time when there weren't as many fellowships and everything as one would have wished, went to work after a master's degree at MIT and he's still in the Boston area.

STURCHIO: This, of course, was in the period when the Depression was at its worst, when you were starting college. How did you and your fellow students support yourself in school?

STOCKMAYER: Well, the answer is a lot of them didn't. The total MIT enrollment when I entered as a freshman in the fall of 1931 was close to three thousand two hundred. The number 3,193 sticks in my mind for some reason or other. Two years later it was down to about two thousand two hundred, and they weren't all flunked out; they simply couldn't afford to continue, and the Institute hadn't gotten scholarship resources for them. It was climbing back up again by 1935, but some of my classmates, too, dropped out because they couldn't afford to stay. Six hundred thirty-six of us entered as freshmen, and exactly four hundred got bachelor's degrees four years later. That's quite some attrition. As for me personally, my father was a German organic chemist with a doctorate, and I'd say he had a fair job. He was always essentially a technician; he was sort of the chief chemist of this small firm which eventually was incorporated into General Printing Ink, which later expanded to become something called Sun Chemical. Anyhow, he scraped together my tuition along with room and board. I worked at the dining hall because I felt I should contribute something, and he agreed that I could do that if I wanted. I applied for scholarships and was awarded a token amount; I suppose it was immoral to do so, but it didn't occur to me at the time. As I recall I got fifty bucks as scholarship one year, and twenty-five another year when the grades were actually better. Today, of course, a kid whose family resources are adequate doesn't get any scholarship aid, even if he's got all A's, but at that time they settled it by saying, "Well, if you apply we'll give you a token award."

STURCHIO: Was there a stipend connected with being a teaching assistant in that summer?

STOCKMAYER: Yes, it was something like twenty-five bucks a week, as I recall, which was pretty good. In fact that was a starting salary of most of my classmates when we graduated. Possibly I got a hundred fifty dollars for the six weeks, or it may have been a hundred twenty-five dollars. I don't know. I do know I went out and bought a used Model A for a hundred dollars. The first car I ever owned.

STURCHIO: You mentioned that when you went to Oxford you got a copy of Paulin and Wilson. Were there other things you recall reading in your later undergraduate years? Outside reading that had a big influence on you at the time?

STOCKMAYER: It would be hard to be too pretentious. I bought a lot of Schopenhauer and Nietzsche, but to this day I haven't read most of them. This was sort of a holdover from that sophomore intellectual history course. I wanted to learn more about these guys, but it was tough going. And also, despite the fact that undergrad physical chemistry was so heavily concentrated on thermodynamics, I felt after almost three terms I still didn't really understand it, so another thing I did at Oxford was to buy a Lewis and Randall (6), which had not been a prescribed text at MIT, and to work through the problems of that on my own. Why the hell I did this I don't know; I guess maybe I didn't like the experimental work I was assigned at Oxford and my mentor there was too reserved and perhaps too old to furnish much personal inspiration or contact. He was a very fine chemist named D. L. Chapman, who is remembered to this day. He independently did the theory of the electric double layer now known as the Chapman-Gouy double layer. It's essentially a one-dimensional Poisson-Boltzmann equation, which you can solve exactly in one dimension, non-linearly. And he formulated the adiabatic condition for a shock front, the so-called Chapman-Jouguet condition to describe an explosion. So, he's in the books all right. But he was suspicious of Americans. I don't know that he really wanted me, but he didn't have much choice. That was the college [Jesus, Oxford University] I was put into.

STURCHIO: We've talked so far about how you had taken some extra math, and you had taken some courses with Slater. You had been introduced to the kind of atomic physics that he and others at MIT were working on, and had plenty of background in physical chemistry.

STOCKMAYER: Yes, and there was another guy at MIT I should mention already because he remained an inspiration and later a friend as long as he was alive, and that was [John G.] Kirkwood. Kirkwood had taken his Ph.D. already in 1929 or 1930—very young. He had had a year with Debye's group at Leipzig as an International Research Fellow, and then (remember there weren't many jobs) he was back at MIT. He had worked already with Slater as a graduate student. He'd had to do an experimental thesis as a grad student on the dielectric constant of compressed ammonia, but he knocked off a paper with Slater on intermolecular forces (7), which is still quoted. He came back to MIT as a research associate in the chemistry department at, I suppose, a miserable salary. Those of us who were there just barely knew him but there he sat calculating, and one knew that he was doing tremendous things. [George] Scatchard mentioned him several times in class. When I came back from Oxford, though, he was gone; by then he was hired away to Cornell as an assistant professor.

STURCHIO: That's very interesting, because of course you began to work in areas that were congruent with the kind of work that Kirkwood was doing within a couple of years. How is it that you ended up at Oxford? Did you just apply?

STOCKMAYER: Yes, I applied for a Rhodes Scholarship. I was interested in going abroad in a general sort of way, and I thought it wouldn't hurt to apply. I had no notion that I could possibly receive one. In fact I was even in love with an American girl and thought, you know, the separation would be fatal to the relationship. Well, ultimately it was.

STURCHIO: Well, if one is faced with the opportunity of taking a scholarship abroad it's pretty clear what to do. But had you thought of going into industry or just going on to grad school in the U.S.?

STOCKMAYER: I definitely didn't know what I was going to do, and I was thinking maybe industry was the right thing to do. Thanks to my father I did get a job in the Sun Chemical or General Printing labs in the summer after graduation and again in the summer after the two years at Oxford. Those two occasions were sufficient to convince me that probably I wouldn't be an adequate industrial chemist. I didn't seem to have that kind of interest. Although, in 1939, when my doctorate was in sight, I did apply at Du Pont, went for an interview, and didn't get an offer. They hired me as a consultant some years later—six years later. At the time I wondered—maybe I didn't show enough *savoir faire* and interest in the really practical world. And there was one other thing that I often wondered about. On the train going down there I got into conversation with a man on the seat next to me, and he turned out to be a chemical engineer who had once worked for Du Pont and had been involved in an accident down in Charleston, West Virginia. He felt he had been ill-treated by the company. In my completely naive way I chose to mention this guy to one of the big shots I was being interviewed by. I thought he was a big shot, he may have been. Not that "X" thought the company had treated him shabbily, but I said I met "X" on the train, and he told me a few things about Du Pont. "Oh, is that so?"

STURCHIO: Do you remember who interviewed you there?

STOCKMAYER: The man from the Personnel Department was a charming fellow called Wade Reinhart, and I met him in later years. In the old Ammonia Department where I was interviewed, one of the people was [Alfred T.] Larson, who was one of the very well-known people when the ammonia synthesis and the high pressure work came to the U.S., and he published some papers in the area. He was nice enough. Maybe they just only had about three jobs to give out, and I was not one of the three guys. You know in 1939 there still wasn't a great deal of hiring going on. In any event, I got an instructorship at MIT and turned in my thesis during that year of 1939 to 1940.

STURCHIO: Let's go back to Oxford briefly. What was the atmosphere there compared to MIT, both as far as research goes and also socially?

STOCKMAYER: Oh, entirely different. I had an awfully lot of growing up to do. About the second night in college the captain and secretary of the college boat club called on me and asked me would I like to learn to row, I said, "Sure." Well, I had tried rowing a little bit at MIT as a freshman but I was too worried about my studies then and hadn't done much with it. Now I really took to it, and it was number-one during my two years at Oxford. Science was definitely second. Even though I did work the problems in Lewis and Randall and read some Pauling and Wilson, the river was the thing. I got fairly good at it. I rowed at Henley both years, once in the college crew and once in a university second boat, and I really had a shot at the Oxford crew. I was in the first boat for three weeks, and then they saw better. So I have an Isis tie, if you know what that is, tucked away somewhere. That's the sort of second boat.

At our 50th MIT reunion last June, we put together a crew. I had never rowed for MIT, but fortunately there weren't too many guys around so I got to row in the boat. We went out for about half an hour; it was fun, sure.

STURCHIO: So you were spending some time on the river, and also doing some work with Chapman. I wondered if the first paper of yours on the poisoning of palladium was written during your first years at Oxford (8)?

STOCKMAYER: Yes, exactly, that was my B.Sc. thesis. Someone previously had found this poisoning under certain conditions, and suspected carbon monoxide, since surface carbonyls could be very stable. So Chapman put me on to that, essentially to learn some glassblowing, but it was a trivial trick to load in known amounts of carbon monoxide and measure the length of the induction period as a function of the CO pressure. I got some fairly good preliminary results and was succeeded by another student named Max Burrows, who is the co-author of that paper. He got more data and finished it off.

There's a sequel to that. That paper simply gives the data and it gives an empirical equation that fits them, one that's too complicated to make any physical sense, but another one invites theorizing. By this time I was an MIT grad student, and I saw how with just ordinary Langmuir isotherms but with the proper ratios of the constants you could fit all these data, and I wrote a little manuscript and sent it to Chapman. He wrote back and said, "That's very interesting, but you can't prove this because it's only one possibility, so I don't approve of this being published," so I put it away and never did publish it.

I'll tell you two more things about Chapman, if I may. During the war, he wrote to me. This was before the U.S. got into the war, but England was already in it. It was a very cordial letter, completely different from any personal interaction he had ever had with me, where he was always grumpy and wondering why the hell I hadn't done more instead of being out rowing. It was essentially, I think, motivated by the notion that England was going to need some friends. I don't really know. Maybe it was just that he was unable to project himself face to face but could do it that way. And I think I answered it, I don't know. I didn't see him again until 1955, which was the next time I was in England. I went to see him; a very old man now, and I had called up Mrs. Chapman, who was somewhat younger and also a chemist who had worked with him. She arranged it all and I went out there and Chapman said, "You say you worked for me? I don't remember you." So I said, "I did, Mr. Chapman, and I'll tell you what I worked on. You communicated our paper to the Proceedings of the Royal Society. He told his wife, "See if you can find that," and she looked in a cabinet, and sure enough, out came a reprint. He looked at it and he looked at me and he said, "Well, I'm dashed. I still don't remember you." Then we both felt bad because I knew that I was such a lousy student that he hadn't remembered me, and he knew that his memory was going. He died the next year, as a matter of fact. It was a very unfortunate thing. Apparently he fell asleep in the chair in front of the fire, rolled off it and sort of got singed a bit. There was that physical suffering and all.

MORRIS: Could I ask a question about the Oxford system at that time, because you worked at the Leoline Jenkins laboratory at Jesus. When you left in 1937 the college laboratory system was almost coming to an end, but I can't remember now when the physical chemistry laboratory was built, but it wasn't long after that.

STOCKMAYER: It was in 1939. I was gone, but that's right.

MORRIS: What was it like to be in on the twilight of the old college laboratory system? At that time, of course, there was the very old Trinity laboratory that was beginning to pull ahead of the others.

STOCKMAYER: Well, I definitely felt something like a second-class citizen. When you get a Rhodes Scholarship, they thrust a piece of paper at you and say, "Here, this is middle December, college admissions have to be done very quickly now. Put down in order of preference ten colleges to which you would like to be admitted." Well, I had known this was going to happen but had done only a little homework. I knew Hinshelwood was at Trinity, so I put down Trinity one, Balliol two. I didn't care about Magdalen or Christ Church, which were socially prominent, and Brasenose was reputed to be nothing but jocks. Jesus had a lab so I put down Jesus third, and that's where they put me.

When I went to Oxford, I was armed with a letter from Scatchard to Ronnie [R. P.] Bell, who knew him through his work. I may have had one to [Cyril N.] Hinshelwood. I had one to "the prof", Lord Cherwell, [F. A.] Lindemann then, from [Frederick G.] Keyes. He received me graciously, but obviously that was only a call to convey Keyes's regards, and it lasted five minutes. I did hope that even though I was a Jesus man they would take me on at Balliol or Trinity. Bell was very kind, but he said, "Both Professor Hinshelwood and I already have too many students. I think that won't do." In the meantime they had been examining my notebooks. Well, kids at American universities didn't keep neat notebooks the way apparently English students of that generation did. H. B. George and Chapman, the two chemistry dons at Jesus, were horrified by these incomplete and childish notes with scribblings in the margin and occasionally a cartoon; irrelevancies of all kinds. They said, "Well, we're really not quite sure what to do with you. Maybe you should take a B.A." I said, "Well, I really think I should start and try to do some research." So they said, "Well, go to lectures for a term; we'll think it over." Actually that was good; I went to [J. H.] Wolfenden's lectures, the man who became my colleague years later; I went to Hinshelwood; I went to H. W. Thompson, who did the spectroscopy, and I went to Schrödinger, who was stopping by there before he got that chair in Dublin, where he spent almost a decade. I didn't understand much of what Schrödinger said, but my God, there he was, and I listened to him. After a term they gave back all but one of my MIT notebooks. I had a set of notes that Scatchard had written on colloid and surface chemistry. George never gave those back, and he was the University lecturer on colloid and surface chemistry, and I think he just damn well wanted to use them. Anyhow, they said, "All right; you can do research in the Jesus labs," and Chapman said, "There's a problem for you," and I spent the next five Oxford terms doing that. I wasn't ready to do a D.Phil. there; I really needed more of the American system for it to have made sense. It made more sense to go home after two years, something I hated to do because I should have been captain of boats at Jesus the next year, and I wanted to row one more year, but it never happened. So I went back to MIT and this time the Slater course was again a leap forward, a revelation; that was the thing, it was so wonderful. Frank was the lecturer in it that term, and Slater the following one, and [Julius A.] Stratton, the man who later became president.

STURCHIO: There were a couple of questions I wanted to follow up on at Oxford. Was [Keith J.] Laidler still there then?

STOCKMAYER: No, he wasn't. If he was, I didn't know that. I was not grown up enough to interact with the real research people, although I made a couple of friends inadvertently. One was Fred [Frederick S.] Dainton, who was doing his undergraduate part II research in Thompson's lab, and there was another American there, an interesting man named Milton Meissner, a fellow Rhodes scholar, a Lehigh grad who did take a D.Phil. in Oxford directly under Thompson. He went into the American chemical industry, had a career with Olin Mathieson where he got more interested in finance than in chemistry, and ended up as an ally of Robert Vesco. He has spent a little time in Luxembourg, and is now, I think, in Nassau or Costa Rica. No, he is in Costa Rica now. He might be in trouble if he came back to the States at this stage. Nice guy, smart guy, but something happened there. But anyhow, through Meissner I met Dainton and so on, but I did not know about Laidler. Hinshelwood did the lecturing in kinetics, and that was fun to go to.

STURCHIO: Did you have any interaction with Hinshelwood, aside from going to lectures?

STOCKMAYER: No. There was an Oxford University chemistry students' club called the Alembic Club, and I went to those meetings. The person I did meet that way and who was utterly sociable, friendly and charming was N. V. Sidgwick. Sidgwick was sort of an Americanophile; he had been the Baker Lecturer at Cornell, and a different guy. But Hinshelwood was a rather forbidding person.

MORRIS: How about E. J. Bowen; did you ever come across him?

STOCKMAYER: Yes, E. J. Bowen. He was one of my two examiners on my B.Sc. Bowen was one of them, the other one was B. [Bertie] Lambert; not James Lambert. I liked Bowen very much—Ted Bowen, he was called. They were kind to me. They passed me!

MORRIS: How many people were working in the labs with you?

STOCKMAYER: In the Jesus labs? Oh, six to eight, all doing part II's; one guy was doing a D.Phil. I saw a couple of them last year, last spring, at a Jesus dinner. I think none of them became really visible. The most promising of them, a really bright lad, was drowned, I think the summer after he got his degree, in a swimming accident.

STURCHIO: How did the labs compare with the MIT labs that you had?

STOCKMAYER: Well, more antediluvian, you could say. Chapman was more interested in the hydrogen and chlorine photochemical reaction than anything else; that was his really big life's work. But—I forgot—the really good chemical thing he did was the interrupted rotating sector photochemical technique. That allows you to get lifetimes of radicals, which Harry Melville first applied to polymerization. When that came out, I had known about it because Chapman had done it in the Jesus labs with a homemade deal in 1922 or 1923 to study the lifetime of the iodine atom, intermediate in the reaction between iodine and oxalate.

Because he worked with chlorine, he wouldn't have oil pumps, so we had mercury diffusion pumps, and these were backed by something called a Sprengel pump, which was in turn backed by an aspirator. All the glass was soda glass, and one blew it by pumping with one foot on a bellows and using a hand torch. So I learned glassblowing, although I never became very neat at it, but I could make things that would stick together. You had to anneal the hell out of the soda glass or it would crack, and you had to learn the hard way. Very primitive equipment. The reaction vessel was made of quartz, and I had to go to the University glassblowers several times when that developed a crack or something.

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STOCKMAYER: Chapman's standards of getting things pure and done carefully were pretty high, and he was quite jealous of Hinshelwood's fame, because he considered Hinshelwood's work sloppy, which, in retrospect, it could be said to have been in large part. At that time, Chapman felt that Hinshelwood rushed into print and really didn't do enough experiments. He would say, in this sort of Mancunian accent, which I can't really represent too well, but he would say something like, "He sticks his apparatus together with sealing wax." The old string-and- sealing wax story. I guess Hinshelwood, now and then in a hurry, did not make all joints with a blowtorch but with some sort of gunk. Chapman looked down on that.

Amongst the Oxford chemists, the friendliest was certainly Sidgwick, amongst those I happened to meet, and he was, of course, world-famous already. He was just then writing the big book, the two-volume thing (9).

STURCHIO: You went back to MIT, but, given the kind of chemistry that you had already done at MIT, and your exposure to kinetics at the forefront at Oxford, had you ever considered going to [Hugh S.] Taylor's department at Princeton, or out to Caltech?

STOCKMAYER: No, because I still wasn't that much in command of myself to know what I really wanted to do. I wasn't really sure. It was a very tentative return to MIT, but again, I found that with one more leap in the demands made upon one, that I could do it. So there I was, and I decided to stay there.

The man I wanted to do my doctorate for was Isadore Amdur, because he was just getting started on molecular beams, but this was still the Depression in 1937. He was an instructor, and he had one grad student, Harry Pearlman, and it was decreed that he should have no more. Professor [James A.] Beattie needed a pair of students to run the compressibility apparatus, and so another student and I were essentially told by Professor Scatchard that that's where you're going to work. He said, "You could argue with me, but I think at this stage your basis for judgment as to which field to work in isn't really well informed, and we want you to go and work there." I was pliable enough so I did it. I had one idea, which was based on kinetics in part. The old puzzle of the third order gas reactions, all of which known at the time seemed to involve nitric oxide. It was NO with oxygen, with chlorine, with bromine—I can't remember the fourth one—with hydrogen, maybe. People had already looked for N_2O_2 in the equilibrium properties of NO, but Beattie's apparatus was accurate for the time; it was probably as good as there was, and I thought, "Let's give PVT relations of NO another shot and see if I can see deviations from normalorresponding states and find some N_2O_2." That was the thesis I would have liked to do. Well, Beattie didn't want to do that, and besides, he said, "Now I have a grant to work on hydrocarbon mixtures, and mixtures are interesting, so why don't you work on methane/butane, which I'd like to do?" So I said, "Okay." I mean, I was pushed, and I wasn't a very aggressive pusher-back. I did what I was told, in a way. That was my German upbringing, if you like. I was scared of my father until I was well over 30, because, you know, he was an authoritarian father.

STURCHIO: How did he react to your beginning career in chemistry?

STOCKMAYER: Oh, he was very pleased, sure. I remember he gave me a copy of Taylor's treatise for Christmas (10), when I said I was going to be a physical chemist, and the following year he gave me a copy of Nernst's old book in German (11). I still have those, of course, with his writing in them. So, yes, he was pleased. And in the end he turned out to be not nearly as fierce a man as I had always thought him to be when I was growing up.

STURCHIO: Where did Beattie have the grant from?

STOCKMAYER: Well, it's interesting enough, it was called the Polymerization Process Corporation. This was an outfit affiliated with M. W. Kellogg, the petroleum engineers, and the interesting thing is that the person who wanted to have this work pursued and who came to visit was a man called Manson Benedict. Manson Benedict was very famous in the history of gaseous diffusion at Oak Ridge during the war and was subsequently for many years chairman of the Nuclear Engineering Department at MIT, so we became colleagues later. He's now retired and living in Massachusetts. But Manson sort of made it clear when he came up that there were some interesting stakes here. What he had was, for its time, a super empirical equation of state. It was a modification of Beattie and Bridgman, with three more constants thrown in, so that he could fit vapor pressures into the liquid range if he wanted, as well as it could be done before the advent of modern theories of critical phenomena and non-classical methods. He wanted to see how it worked on some mixtures, and methane and butane was a fine test case.

STURCHIO: I noticed there was some collaboration with [Bruce H.] Sage and [William N.] Lacey out at Caltech?

STOCKMAYER: No, not by me. They were doing similar things n a way, that's right.

STURCHIO: Maybe it was just that you obtained some samples from them?

STOCKMAYER: Yes, we got the methane from Sage. He had a good source of pure methane. Beattie got it from him. That was about it. I never met Bruce Sage, or Lacey either. I know that Beattie was occasionally mad at me during the course of that research, because I was busy reading Fowler--that big fat book on statistical mechanics (12)—trying to learn about the theory of equations of state. I didn't get that much out of the course taught by Keyes, which was called statistical mechanics, but I have to say that it was not a good course. Kirkwood wasn't around any more, and so I was trying to learn this stuff on my own. And that cost time, because we had to get the thermostat up to the proper temperature. I would be reading in Fowler and suddenly that damned thing would be 25 degrees too hot, so I would turn it off and go back to reading Fowler, and Jesus, then it was 20 degrees too cold. The data didn't come as fast as Beattie would have liked. But in any event it paid off for me. He forgave me, we were friends and colleagues for a long time. What he didn't forgive me for was when I left MIT to go to Dartmouth. I think he felt I was a deserter. Beattie was an empiricist at that stage, he was himself not interested in intermolecular forces as such and if his equation, with some rules for juggling the constants, could be made to fit the mixtures, that was fine. It was a chemical engineering point of view, and that's what he had been originally himself. He'd had a classmate in chemical engineering called Robert S. Mulliken. Mulliken left chemical engineering, as we all know.

STURCHIO: What were the relationships between chemistry and chem engineering like at MIT when you got back?

STOCKMAYER: Again, I wasn't grown up enough to know much about it. We didn't take any of their courses because we had our own in physical chemistry, and the minor that we were expected to take was physics. I think they were cordial enough. There's a long history of that. There is a man at Princeton who wrote a paper about the history of chemical engineering at MIT (13). Yes, it was John Servos. I have a reprint of it. I hadn't known all of that stuff. I had known that it was all part of the chemistry department at one stage and then it eventually broke off.

It was much the bigger department at the time. We all supposed it was number one in the world at the time, whether it was or not. But Warren K. Lewis was certainly there. [William H.] McAdams was there, Tom [Thomas K.] Sherwood, [Edwin R.] Gilliland, a younger guy; Harold Weber; Fritz [Herman P.] Meissner (not Milton Meissner—that was the Vesco pal. Fritz was Miln's cousin.)

STURCHIO: One thing that prompted the question was that I noticed in one of the first papers you wrote with Beattie on equations of state that you had used some of Warren K. Lewis's work (14), so you were certainly reading that.

STOCKMAYER: Several of Beattie's students on the same apparatus, and therefore people he kept in contact with, were chemical engineers, really. William C. Kay, and I can't remember all the names. He and Lewis talked a lot, but I didn't get to know Lewis very well until I was a faculty member much later.

STURCHIO: Who were some of the people you were spending a lot of time with as a graduate student—fellow grad students?

STOCKMAYER: Well, my lab partner was Gil [Henry G.] Ingersoll, who went to Du Pont. He's retired from there now. I forgot to tell you one more thing about Oxford. My Oxford roommate for two years was Bill [William A.] Franta, a North Dakota Rhodes Scholar who became research director of Polychem—later called the Plastics Department. I'm going to have dinner with him tonight in Wilmington.

STURCHIO: Well, one of the questions I was going to ask you was how you ended up consulting for Du Pont. Did he have anything to do with that?

STOCKMAYER: No, in fact I helped him get a job at Du Pont. [laughter] He was in the Chemical Warfare Service during the war. No, but the Rhodes Scholarship had something to do with it. The guy who wrote to me was Robert E. Burk—I don't know if you know who that was. Well, if you go back to Western Reserve University—there was also a professor there named Robert E. Burk, who had been a Rhodes Scholar from Oklahoma who had taken a

D.Phil. under Hinshelwood at Oxford. Kinetics of something or other done with sealing wax! Bob Burk got very much into the practical side of kinetics and catalysis and all that, and during the war somehow left Western Reserve to go to Du Pont. After I wrote the first paper on gelation I guess he wrote to me, “Just through the Rhodes Scholar news [*The American Oxonian*] I knew that you existed, and now I see that you’re interested in polymers. We’re hoping to hire some scientists at Du Pont, and would you be interested?” I replied, “Offhand, I’m only getting my feet wet as an assistant professor, but, heck, it sounds worth a visit anyhow,” so I went down to Wilmington to visit him and John L. Brill, who was then the chemical director at what was the old lastics Department, where we spent a day talking. Later they said, “Well, would you consider leaving MIT and coming to work for us?”

So I thought a while, and then I did a very bold thing for me. “I guess I don’t want to do that, but possibly you’d like me to come down as a consultant?” And they bought it! I really was surprised. [laughter] So I started going down to Arlington, New Jersey, where the Plastics Department then had their research lab, and the most fortunate thing happened. They had just hired one of the first two students who worked under Peter Debye on the light scattering of polymers, Fred Billmeyer. You know him of course as the author of the book and he recently retired from Rensselaer (15). Fred went to work in the Arlington lab. He and Peter P. Debye, the son of the great Peter Debye, had built the first working light scattering apparatus in this country to study polymers with. Maybe not the first, since Bruno Zimm was working at Brooklyn with another one and also at Columbia. Maybe it was the first. So, there was Fred, and there really wasn’t anyone else for him to talk to down there. On my monthly one-day visits to New Jersey from Cambridge we got to have some common interests and in fact published a couple of papers together before we were through (16). Not great ones, but things we were both interested in.

Somehow I helped a little bit by consulting but was horribly dissatisfied with my performance. I felt that every time I failed to answer a question I was sure to be shown the door, until after years it turned out that not many questions are ever answered by consultants. There are always those who wonder why the hell they exist. [laughter]

STURCHIO: I suppose having a chance to bounce questions off other people can give a more detached perspective. Well, let’s get back to MIT. You mentioned that Ingersoll was one of your fellow grad students.

STOCKMAYER: Yes, and ward McMahon was another, the guy I mentioned in connection with the rotational spectra. Leo Epstein, who I mentioned before. And Barney [Bernard] Vonnegut, who has been a lifelong friend. He’s Kurt’s older brother. Barney is a cloud physicist in Albany. He’s done some remarkable research on thunderstorms. But Vonnegut is also, I think, the guy who really first did an experiment to measure the rate of nucleation of crystallization—a technique that many people have follow up on, but he first did it with tin. If you suspend the molten phase as fine droplets, once you get a nucleus, the growth finishes that droplet quickly, but the other droplets can’t start until <u>they</u> get a nucleus, so essentially the rate of crystallization is the rate of nucleation. He conceived that and carried it o in the case of tin in the early 1940s (17).

Who else? We didn't all finish within the same year. It may have been within one or two years of each other. A guy who was a little bit younger but who overlapped a little was Ed [Edward R.] Kane, who became the president of Du Pont for a while. He worked for Beattie also. Clarence A. Johnson was a man who went to work for Benedict, and also was prominent in the annals of the gaseous diffusion process for UF_6 isotope separation. He was a Canadian like McMahon, they both came out of the West. Keyes had connections with some Canadian scientists.

STURCHIO: This was about 1938, I suppose.

STOCKMAYER: Well, I got married in 1938. I had finished with the data essentially by the end of 1939 so I took a degree in 1940 and had a second year of instructorship there until 1941.

STURCHIO: By now you were growing up as a scientist and you were beginning to have a better sense of where the action was. I wonder if you would talk just a little bit about what sorts of things you were reading, and what journals. Did you keep up with the literature?

STOCKMAYER: Well, I tried. The *Journal of Chemical Physics* was the monthly Bible more than anything, and Joe [Joseph E.] Mayer was God, together with Kirkwood. There wasn't much action at MIT in these areas. Professor Keyes had been head of the department since about 1922 or 1923 and there was a fair amount of grumbling. In any case I felt I had to go try something else.

Columbia advertised an instructorship to teach evening classes or afternoons once in a while, it was for Extension students. I went down, interviewed and got the offer and I leaped at it because I had an office about four doors away from Mayer. George E. Kimball was on the same floor. It was something I wanted to do.

STURCHIO: Of course you discussed this in the article you did with [Bruno H.] Zimm (18). I'd like to ask you to expand upon some of the points you made there. I noticed in the papers you did with Beattie on equations of state that you had built extensively on some of Mayer's work?

STOCKMAYER: Not really built on any of Mayer's work, it was much older stuff. It was just standard second virial coefficient stuff. The polar gas thing I was kind of pleased with because I started that while I was a grad student. That's what made Beattie mad when I wasn't paying attention to the thermostat all the time. It was not much of a trick in retrospect, it was just bulling through a calculation on the second virial coefficient with dipoles on the molecules as well as having a realistic expression for the rest of the potential. Lennard-Jones plus dipoles. That had been used once before in a sort of half-assed calculation, when Henry Margenau had tried it enough to show that it made a reasonable contribution (19), but he wasn't interested in data-fitting and he never carried out all the terms. He was a real physicist, in other words: he wanted ideas and he didn't care about detailed fitting of the data, and I was a chemist who wanted to fit everything. Parameters!

STURCHIO: I didn't put it quite the right way. What I was trying to get at was, it was clear from these papers that you were aware of Mayer's work.

STOCKMAYER: I was, and I wanted to learn more about him, and to learn from him by being nearby.

STURCHIO: Well, at Columbia, with Mayer there, and his wife [Maria G. Mayer] coming down from time to time, and Kimball, who had been collaborating with [Henry] Eyring and [Harold] Urey...

STOCKMAYER: Sure, and with Harold Urey, and Louis P. Hammett, who had written the physical organic book (20). He was known also in analytical chemistry to anybody who had had to teach some of that, as I had. Also there were a couple of other nice physical chemists who weren't so well-known: a man named Jake [Jacob J.] Beaver who ran the instructional lab and taught some courses, who was a very nice companion. And there was Charlie [Charles O.] Beckmann, whom I mentioned in the article, a charming and bright guy with a German background like myself. We used to kid each other in German half the time. He was even lazier about writing papers than I turned out to be. [laughter] He did some good stuff. One of his students was a guy who became Urey's right hand man, a man called Karl Cohen.

STURCHIO: And Beckmann certainly had an important influence in moving both you and Bruno Zimm toward polymers.

STOCKMAYER: Yes, well, he had mentioned this polymer stuff. I had been reading *JACS* [*Journal of the American Chemical Society*] regularly; I started subscribing to *JACS*—I guess I joined the Society as an MIT senior. I've got my fifty-year pin and all that. I read the *JACS* and I saw, now and then, papers by Paul J. Flory on polyesters and the like. There was an equation or two connected with it, but I thought that was gunk, and I wasn't interested. But when he hit that gelation thing, gee, that was like a bombshell. That's all I can say.

STURCHIO: You mentioned in the article it was Tom [Thomas G.] Fox.

STOCKMAYER: Yes, I hadn't read the thing yet and during a journal seminar he got up and talked about it, and then I went and read the papers.

STURCHIO: We should come back to that whole series of papers when you started to explore that area. But Fox was there; Beckmann we heard about; Zimm and [Paul M.] Doty were also there?

STOCKMAYER: They were first year grad students when I came as a first year instructor. By the end of that first year they had both decided to do their thesis work with Mayer. So did Bill [William G.] McMillan. So that Mayer had that great triumvirate of McMillan, Doty and Zimm, all three in one year. It was wonderful.

McMillan to me at the time seemed perhaps the brightest of the three; he's had a strange career. Do you know who that is, William G. McMillan, of UCLA? He runs a consulting business to the Pentagon on the side, and he did a post-doc with Edward Teller. Teller was around Columbia at the time, too and Bill apparently really became a Teller disciple. Over the years most of his friends of his youth disagreed violently with his political views, but without coming to blows or him failing to be just as nice personally as ever, but he just didn't think the way we did. He was Westmoreland's scientific adviser in Vietnam for a while, too. McMillan took up a project that Mayer had suggested to me and which I dropped when gelation came along. That's the multicomponent formal statistical mechanics, the grand ensemble with many components that led to what's called the McMillan-Mayer theory (21). McMillan started out with an experimental problem that was almost impossibly difficult, although he pursued it for a while. When I dropped the McMillan-Mayer or at least shelved it for a while, as I thought, Bill needed a problem. He was damn smart so I said, "Fine, go ahead," and he polished it off in no time.

STURCHIO: By this time, also, you were doing a much more sophisticated kind of theoretical physical chemistry than you had been doing up until now. Was it the interaction with Mayer that helped?

STOCKMAYER: Yes, I think so. He ran a weekly research meeting, and he tried out on us his ideas on the multicomponent grand ensemble and the distribution functions. It was just after Elliott W. Montroll had left, who had worked with Mayer and wrote with him that seminal paper on molecular distribution functions (22). This meant that I was used to seeing these complicated notations and not so much afraid of them. I don't really think it's that much more difficult. It's just that the notation is forbidding, and to this day—I know I'm too old to learn too much new stuff now—but I feel very often that it's just the unfamiliarity that scares people. If they force themselves to read a little bit, all of a sudden it becomes intelligible, not much harder than stuff they can do already. Well, that's obviously a stupid remark if you take it too far.

STURCHIO: In a way, being familiar with that kind of discourse, if you will, made it easy for you to appreciate Flory's arguments on that?

STOCKMAYER: Oh, yes. Well, essentially, Flory had solved the problem in the general gelation condition he had given, and then he had worked out the distribution formulas for some special cases (23). My idea was that this is a phase transition, and it's one that's based on stoichiometry instead of integrals over forces at different distances, and so it can be done with algebra rather than multiple integrals that you can't solve anyhow. And couldn't I make it look more like Joe Mayer's theory of condensation, where the thing happened because there was a singularity in the partition function? I went at it that way and with that objective, and more or less succeeded, at the same time finding that I could solve many more model cases (24).

STURCHIO: The excitement you felt comes through in the paper in the *Annual Review of Physical Chemistry* (18). It must have been very interesting at that point in polymer science, to be able to apply—

STOCKMAYER: It was exciting to me. Of course, the alleged chemical examples in the first paper are all wrong. They were free radical cases and they just don't fit at all. But it was just window dressing as far as I was concerned. I would have been better advised to leave them out. Except for the polyesters; those were real enough, and Flory had already done experiments on those.

STURCHIO: This might be a good point to ask you a little more about your meetings with Flory.

STOCKMAYER: Well, that was quite wonderful. After the Tom Fox presentation I read the things and decided to go to work on this, I worked for quite a while and I had some struggles over the details of the combinatorics there. I remember Maria Mayer was particularly generous of her time and I think parried a couple of false starts I'd made. Eventually she said, "Why don't you really study the way we did it in our textbook for the imperfect gas case (25). You can probably use the same kind of imaginary device." Erector sets, you know, little frames with so many holes in them and then you had bolts, and some had washers and some didn't. Well, it's not the way things are done any more, but she essentially made me go and learn that, and then I found I could answer the problem that way.

[END OF TAPE, SIDE 3]

STOCKMAYER: I was hoping to bring along that first letter that Paul Flory answered me, we both wrote in longhand, it's in a file of older correspondence that I couldn't reach for in time, but it's somewhere in the basement. Anyhow, I remember something of what he said. I had asked him several questions about his work, and said I was working on another way of doing it, and I thought that he'd be interested, and I remember it was something like: "Dear Dr. Stockmayer: Your letter brings up several nettling questions connected with gelation theory," or something like that. I remember he used the adjective "nettling." He made comments on them. One of the things that I was concerned with was unequal reactivity of the groups, something I never went back to myself. Manfred Gordon was the guy who really fixed that one up years later in England. Flory said that he'd be interested in hearing about my method, and why didn't I come over? He was then in Linden, New Jersey, at the Esso labs. I don't remember whether there was any telephoning or not. By this time, too, this was all research done on the side, because we'd got into the war in December of 1941, and the first assignment aside from teaching classes, which were still around, was to work for Urey with George Kimball on the deuterium exchange equilibria, because heavy water was still in the picture.

STURCHIO: So that was taking up most of your time?

STOCKMAYER: Yes, or more. So I can't put a date on the meeting with Flory but it must have been some time in 1942. I went to his office when I got over there to Elizabeth. First, I think, he showed me the bank of osmometers where he was measuring the osmotic pressures of polyisobutylene. There was a very famous paper of his on the osmotic pressures and viscosities of polyisobutylene (26). What stands out was how accurately his data followed a Mark-Houwink law with a fractional exponent, 0.64 or something like that, over about three decades of molecular weight. He had gone from about 3,000 molecular weight to 2,000,000 with fractionated samples. He did this all with his own hands, I think. Well, he had maybe one assistant. Up to that time the theories had suggested that the dependence must start out first power and end up square root or something like that, but here the data were just on a log-to-log plot; it was really straight for three decades, and that's a lot.

Then he called me into the office and asked me to start showing him what I had been doing. I started writing on the board. After a while he said, "Wait a minute," and then he went into the next lab and talked to John Rehner, the man with whom Flory later worked on the theory of rubber elasticity and swelling of networks (27). He said, "John, come in here—I want you to hear this too." So Rehner came in and we were introduced, and then I gave my spiel again. We had lunch, and then I don't remember much else except he said, "That's very interesting; go ahead and write it up. Why don't you?" So I did.

A little later we had this disagreement, which, however, did not lead to unpleasant relations. As you know, in later years Flory was often an extremely bitter opponent of people who didn't agree with him. Well, I didn't see how the way he handled things after the gel point was passed could be right, because it seemed to put cycles in, and he had assumed there weren't any to begin with. Well, he thought physically that it was closer to the truth than what I was doing, which was to keep the rigorous condition that there can't be anything but trees all the way. The two methods do get different answers. Physically, he was better than I was. Mathematically, perhaps I could do mine more rigorously. His is a sort of strange Ansatz but it also leads to a definite result. It's far easier to apply to more complicated systems than mine would be. I think on balance we each had a little something to say; then years later many people went back and investigated under what conditions you'd get one or the other. I guess the real problem is still not completely solved, namely, how much ring formation is there, because that depends on real conformational problems. So I wrote it up, and of course I think I had enough decency to thank him. I certainly thanked the Mayers.

There was this joke when I came to handling more complicated cases which was when Harris L. Mayer had gone to work for Joe. I don't know what has happened to Harris Mayer—he disappeared into the Manhattan Project and he certainly never finished a Ph.D. with Joe Mayer afterwards either at Columbia or at Chicago. Mayer, Mayer, Mayer and Stockmayer. He and I thought that would have been real funny, but they didn't think that was such a hot idea. I even remember going in and saying, "Don't you guys want to put your names on this one?" Maria, especially, had sat with me for a few hours. And they said, "No, you publish it alone." So I did. They were wonderful people. We even lived in their house when they went off to Nantucket or somewhere for a week, and my wife and I were living in a hot apartment right near the Columbia campus. In exchange for mowing the lawn a couple of times we were allowed to go and live there for a week out in Leonia, New Jersey, which was maybe four degrees cooler than the city.

STURCHIO: Had you had any other contacts with people in polymers outside of the group at Columbia, and Flory at this time?

STOCKMAYER: No, except for Charlie Beckmann. He had an ultracentrifuge on the same floor, and there were three students working on problems with starch derivatives. Physical chemistry with the ultracentrifuge plus osmometry as a tool. Their names were Ralph I. Dunlap, who went later to Monsanto and had an industrial career. Hugh G. Bryce, who later went to 3M and had a good career there and Tom Fox, about whom you know a great deal. So I was friendly with all those guys, and used to go bowling with some of these characters as well as talk chemistry.

STURCHIO: Was there any travel back and forth on the subway to Brooklyn at this time to meet with Mark's group and others?

STOCKMAYER: The first contact with Herman Mark that I remember is actually at a nice symposium that the New York Academy of Sciences ran; that was before they got their building on the East Side, and so this meeting was in the Museum of Natural History. And it was quite a good meeting. Flory was there and gave his Flory-Huggins paper there, as I recall. Eyring was there, Charlie Price, Mark; I can't remember them all, but I went to hear those papers, all of them. Charlie Beckmann gave one, too, as I recall. He got me to go.

STURCHIO: Was that in 1942?

STOCKMAYER: 1942, yes. I certainly got over to Brooklyn at least once then. Doty and Zimm didn't finish until after I had left for MIT again. They had some sort of a signal as to how non-valuable my efforts to win the war were. There was no difficulty. You know, I was never a full-time Manhattan district employee, and going back to MIT didn't disrupt the Allies in winning at all. [laughter]

STURCHIO: How did you come to go back to MIT then?

STOCKMAYER: Well, I was asked to go back and become an assistant professor. By this time Keyes had been encouraged, let's say, to step down as chairman, and [Leicester F.] Hamilton was acting chairman. [Arthur C.] Cope was still a year away from becoming chairman, that was only after the war was over, two years later. They wanted me to help teach the Navy students and thought they had some useful work that I could do. After one abortive look at a project that I thought not to be worth doing I hooked up with [Arthur R.] von Hippel. The other one was by an industrial sponsor, and it was actually hedging for the post-war situation rather than war research. As soon as I recognized that, I got out of it, but I don't want to describe what or who it was. Von Hippel was another inspiring fellow, from Göttingen like Maria Mayer, whom he knew, of course. I guess they were both Göttingen children.

STURCHIO: Was that taking up most of your time: the war-related work?

STOCKMAYER: Whatever there was, yes, pretty much.

STURCHIO: Because you did find time to publish the paper on branched chains, then, the second one (28).

STOCKMAYER: That was finished at Columbia essentially. I wrote it up after I had gone back. In fact there's a chemically irrelevant personal story on that. I was doing the last derivation and I was proud of that paper because I never had a formal course on complex variables in mathematics, but I used some Cauchy tricks in there that seemed to work. I finished them the morning my first son was born, and I'll tell you how that was. My wife started in labor about 4:00 a.m. and we got in a taxi and took her up to Presbyterian Hospital, and I asked, of course, "Should I stick around?" "Oh, no, it'll be hours; go away; don't come back before 11:00 a.m." I said, "Are you sure?" "Oh, absolutely." So I went down to the lab; I couldn't go back to sleep. I finished a couple of derivations, and then I went back up there on schedule. The baby had been born an hour before, and where the hell was Daddy? [laughter] It's something you never quite live down, and I haven't, really, I think. My wife understood, but at the same time she didn't, you know. "Why wasn't I there?" This was long before fathers were encouraged to hang around and watch deliveries. They were simply not welcome.

STURCHIO: Well, it happened to my father and me, so I can appreciate it.

STOCKMAYER: I did a couple of other things in my spare time; the copolymer paper (29), based on, you might say, a simplification of Simha's general result that he didn't do much with (30). Then there was a paper on SO_3, which was a sideline, and that was based entirely on a lecture course. The first time I ever taught statistical mechanics myself was during the war, there were two or three students, George M. Kavanagh and Harold S. Mickley, one other guy and me. Mickley became a chemical engineering prof. He said, "The other day with Doc Lewis, we wondered about the heat capacity of SO_3, and I don't think anybody ever measured it." We had just been talking about calculating thermodynamic functions from spectra, and he said, "Do you suppose we could try that?" I said, "I'll think about that." And I came in the next day and said, "O.K., throw away the rest of the problem assignments for the term. You guys are going to do this instead." I turned the course into a little research. There were only two of them by this time as the third guy—I can't even remember who that was—had dropped, so instead of formal lecturing by me we just all worked together on that, and it worked out all right, at least temporarily. I think in the end some of our interpretations of the rather crude spectra were wrong. But it was the best treatment of the thermodynamics of SO_3 for quite a few years (31). It was good for the students, too.

STURCHIO: I wonder if you'd talk a little bit about what the chemistry department at MIT looked like just after the war, and how things were changing. You were at MIT when it was the end of an earlier era. In the post-war period, not only did things change at MIT, but things changed nationally in terms of support of research, etc.

STOCKMAYER: Yes, that was beginning. The Office of Naval Research, of course, was the first granting agency before NSF [National Science Foundation]. NSF didn't get established until 1950. ONR [Office of Naval Research] was going a bit earlier than that. Well, as far as MIT was concerned, by this time I just loved it anyhow as a sort of home. Also the fact that they had also hired Dick [Richard C.] Lord to come into the lab so we had another molecular spectroscopist, and I could teach the kind of courses I enjoyed. So Lord is another colleague.

The older tradition: Beattie was still there; Gillespie had died. Keyes had essentially retired. He was still around, but he didn't teach any more. Scatchard was still very vigorous, and moving into more contact with the Harvard medical people on proteins, quasi-polymers. I had always gotten along well with George Scatchard, so that was a temptation. Indeed, Slater was still around, not that I had any contact with him any more. In fact, I guess the physicist who next really gave me an important hand-up was Laszlo Tisza. I'll come to that when we discuss the light scattering paper.

STURCHIO: Looking back on it, do you feel there was a real change in the atmosphere for research at MIT from 1945 on?

STOCKMAYER: I do, yes. Compton had brought in an earlier era to the Institute as a whole. Of course it arrived in physics sooner and, with the change of administration in the chemistry department on the advent of Cope, it happened there. He brought in some brilliant organic chemists very quickly, young colleagues, namely Jack [John D.] Roberts, C. Gardner Swain, and John C. Sheehan. The older guard in organic wasn't particularly happy about that, in fact several of them had fancied themselves as possible chairmen of the department until Cope was brought in from outside. Suddenly organic, which hadn't been anything special at Tech, had some really able young practitioners. Cope was not all that old himself. I had known Art Cope at Columbia, you see. He came to Columbia from Bryn Mawr as an associate professor when I was there as an instructor, so we already knew each other. In the war he went off to OSRD, the Office of Scientific Research and Development, and then ended up at MIT, but we were already friends. He was chairman for twenty years and I thought for about the first fifteen he did a great job, and then it didn't go so well any more, I thought.

STURCHIO: Was that about the time you decided to go to Dartmouth?

STOCKMAYER: It had something to do with it. It wasn't an out and out... I was going stale anyhow, but this didn't contribute to my wish to stay. So, yes, a big change at MIT. Of course the students, after the war, were so damn good. There were so many of them. They were older and wiser and more experienced than the average graduate student; they were the people who had been in the armed forces and were coming back for grad work. Classes were challenging; they were good students. It was an exciting time to be teaching anywhere.

STURCHIO: Well, you picked up some of your early grad students at that time, too, for instance Jacobson.

STOCKMAYER: No, he was another Columbia guy. I already knew him at Columbia. Homer Jacobson was a young star, about eighteen years old. He graduated from Caltech already and came to Columbia; may be that he was nineteen.

STURCHIO: I forgot that it was a paper delayed in publication (32).

STOCKMAYER: It was delayed because the two advisors were myself and Charlie Beckmann, neither of whom was a real whizbang in getting things out on time.

STURCHIO: That was part of that stuff on intramolecular reactions?

STOCKMAYER: That's right. The ring formation thing which is still useful, although it's been greatly improved.

STURCHIO: You continued to work with Zimm on the same kinds of problems in polymers—in this case dimensions of polymer chains (33)?

STOCKMAYER: Yes. Well, Bruno was out at Berkeley. He and I had been friends when he was a grad student with Joe, but there was no direct working together then, although we tried something when he was in Brooklyn, and I was at MIT. He was already thinking of using Joe Mayer's cluster expansion on the problem of two chains, the second virial coefficient. He wrote a paper in 1946 which develops that (34), and which has a sort of plaintive acknowledgment at the end saying that it was originally planned to write this paper in collaboration with Stockmayer but that, as he put it so genteelly, "The difficulties interposed by the distance between Cambridge and Brooklyn proved to be insurmountable." [laughter] The fact was that the difficulty was that I had two young kids and didn't have enough gumption or energy to hold up my end of the bargain. That's all. So in the end he went it alone. But we talked about that a lot together.

The first real collaboration came when he was in Berkeley and I was at MIT and we just corresponded. He said he had been making calculations about certain dimensions of some branched molecules and rings, and I said, "Gee, I've been doing some of that, too!" We exchanged what we had done and they dovetailed beautifully. He had done practically nothing of what I'd done and vice versa. So we had a paper about twice as big and twice as full of results as either one of us might have had alone at that stage (33).

STURCHIO: And then shortly after that or roughly around the same time you started working on light scattering?

STOCKMAYER: Yes. I'd like to talk about that because there's one especially human story involving Kirkwood. I had started being aware of light scattering early because of friendship with Paul Doty and Bruno Zimm; correspondence or occasional visits. And consulting with Fred Billmeyer, who was building a machine at Du Pont in Arlington, and had started measuring Lucite and things like that and proving that the molecular weights were about ten times as big as what they'd always thought they were. Previously the Staudinger law was used. Maybe they had done the work themselves, but Staudinger's old trick was to assume that the intrinsic viscosity went exactly as the first power of the molecular weight. That was the Staudinger rule and it was calibrated with one freezing point [depression] measurement on a rather low molecular weight sample. So you may have a curve that goes like that; that has some fractional power. [Stockmayer draws a diagram]. This is intrinsic viscosity versus molecular weight, linear, not a log-log plot. What they would do was to say that it's got to be first power and calibrated with that point. So it goes through the origin, here's the line, and by the time you're up at high molecular weights you can see you can be off by a hell of a lot. The Lucite molding powder that Du Pont made was always called 15,000 molecular weight: when Fred measured it, it was 150,000. But it didn't matter to the guys in the plant or even to the engineers. Only relative values mattered, both went up and down together. The viscosity is a fine relative index no matter what you think the absolute values are.

But back to light scattering. The reason I got interested in the multicomponent problem was that John Edsall, a friend of Scatchard and by now a friend of mine, although we got more friendly years later, was beginning to work on protein solutions. To do decent light scattering on the protein, and to avoid nasty problems having to do with charges, you put in salt. Now you already have a three-component system, water, salt and protein. You might have a buffer present as well. Paul Doty had come to Harvard by now, too, and he was doing light scattering there. He arrived at Harvard in 1948 or 1949: he had a brief time at Notre Dame right after the war, after leaving Brooklyn.

So the problem existed, and there was nothing much in the literature until Debye published something with one of his own students named [John R.] McCartney and with two U.S. Rubber Company chemists (35). He used to consult with U.S. Rubber. [Roswell H.] Ewart, [Charles P.] Roe, Debye and McCartney. They had done some light scattering in mixed solvents and found that you generally get the wrong molecular weight if you make believe the solvent isn't a mixed solvent but treat it like a single component. They gave a non-formal, a non-statistical mechanical, but more physical theory based on relative adsorption of one of the solvent components to the polymer, which, physically, is completely the right idea. As far as infinite dilution was concerned, their theory was completely adequate. An equilibrium constant is as good as a free energy as far as I'm concerned; they're different ways of saying the same thing. But more generally, you want to do a bunch of concentrations and you'd like to be able to handle umpteen components. There was still something to be done and so I started out after that, and I think I had the result by using a standard grand canonical ensemble, but it took me a long time to get around everything. I'd been sort of socially friendly with Laszlo Tisza, a Hungarian physicist and at one time a co-worker with Teller at Göttingen. Nothing like Edward Teller; far more likable, if not quite such a brilliant human being. Laszlo and his wife Vera, and my wife Sylvia and I were friendly because of mutual interests of various kinds. He was a real good theoretical physicist and he suggested that maybe I should try a different approach. Since ultimately I wanted systems under constant pressure rather than at constant volume, why didn't I try a constant pressure ensemble. Guggenheim had written about that.

I don't actually remember whether Laszlo also put the bee in my bonnet about what you could call a semi-grand ensemble where you keep the number of moles of one component fixed but you keep the potentials of the others fixed. In the grand ensemble you use chemical potentials as the independent variables rather than the mole numbers, this is sort of a bastard of the two. Whether the idea came from him or not, I do not know. Again, I worked it out but he wasn't interested in being a co-author on the paper, so I thank him in it. This was the quickest road to what I wanted and it's an ensemble that has survived and had some other uses. Well, Terrell Hill put it into his textbook (36), and gives me credit for it, which perhaps Tisza should have had. I must say, I'm a little hazy as to how all that went, except that he didn't want to be part of the paper. I do know that very recently Bob [Robert A.] Alberty at MIT has found that he likes to use it in connection with some equilibrium problems of a series of homologous hydrocarbons. It was interesting in its own right as a sort of formal exercise in statistical mechanics. Basic, but it's just kind of a trick application. The results were available to Edsall already in 1948, but I was still having real literary constipation with the kids being young. We had bought a house out in Weston; an old house requiring lots of attention and a long commute to MIT, and I couldn't seem to get anything written.

[END OF TAPE, SIDE 4]

STOCKMAYER: So Edsall used it; he fitted the protein, salt and water system and then he went off to lecture in California. It must have 1949 some time, early 1949. He gave a talk at Caltech, when John Kirkwood was present, and Kirkwood said, "Well, my student [Richard J.] Goldberg and I have gotten the same theoretical results, and we've sent a manuscript in to Joe Mayer just last week. Do you know how far along Stockmayer is?" Edsall said, "No, but I'll tell him the moment I get back home," which he promptly did. So here I was with one of the things I'd done of which I'd felt really proud, and a letter came from Kirkwood. He'd known me since I was a senior student, although only to say hello to. He said, "I don't want to scoop you if you've also done this, as Edsall assured me; I saw the results he was already using and they're the same. If you've just about got your own manuscript finished, I'll ask Mayer to hold up a little while so we can publish them in the same issue, or if you're nowhere near that, here's a copy of our manuscript and I invite you to join us as a co-author." I had one hell of a temptation to do the latter, because Kirkwood was one of my idols both as a human being and as a scientist, and a chance to be a co-author with Kirkwood—Jesus! Why the hell didn't I just throw mine away?

On the other hand, he and Goldberg had used the conventional grand ensemble and I had used this trick ensemble which got there faster—about one-third of the space—and I thought it was interesting and I was proud of it. I was about two-thirds finished with my manuscript, believe it or not, I really was. So I wrote back and said, "Thank you; I'll work like all hell." And in a couple of weeks I had finished the manuscript. There's the impetus, you see.

[laughter]. He had written Joe and said, “Wait, don’t print this until you hear from me again.” And I sent Kirkwood a copy of my manuscript and he agreed, “Yes, that’s different.” So we both sent them in and they were published in the same issue (37, 38). That’s an example of a great man being very kind to a lesser man under circumstances where he needn’t have bothered.

Well, the funny thing: there’s a sequel. The month before our papers appeared, another paper appeared in the *Journal of Chemical Physics* (39). The authors were J. J. Hermans and I don’t even remember who the co-author was, now [H. C. Brinkman; ed.]. It wasn’t Overbeek, with whom Hermans had worked on poly-electrolytes. But they had also discussed the same problem, although they hadn’t taken it quite as far toward the final applied results as either Kirkwood and Goldberg or myself, but they pointed out that all the basic theories of fluctuation in many-component systems were published by Zernike in 1915 in his doctoral thesis in Amsterdam (40). All of our flurry in the late 1940s when the basic theory had been done over thirty years earlier! In retrospect you could say it should have been known, but we thought it was pretty hot stuff. Strangely enough, the Hermans paper is rarely quoted, and Kirkwood and Goldberg and mine got more mileage than his for some reason. I think Hermans is one of the underrated or forgotten men, and I don’t know if you’ve ever gotten him on your program.

STURCHIO: We’ll have to look that one up.

STOCKMAYER: There’s a lot to tell. In part the reason he was obscured was that the Dutch were so isolated during the war and lots of his best work was in that six years from 1939 to 1945.

Concerning the light scattering equations, Harry E. Stanley, who was one of my first grad students—and the father of H. Eugene Stanley of Boston University, a theoretical physicist—built the light scattering apparatus which he and my later grad students used. He did just about all the building of it and made some measurements on solutions of two different polymers in the same solvent, a field that was dormant for a long time but today, with polymer blends, is big stuff. The first measurements ever made by light scattering on that kind of system were by Harry. A teeny little article, a Letter to the Editor (41). He was a very practical man. He knew how to build apparatus; he was not too much younger than I was. Gene was already there, a seven-year-old boy, the same age as my own kid, even in fact older, because Harry had married young. He went back to Du Pont worked on textile fibers, dyeing of fibers, very practical research for the rest of his life, mainly at Chestnut Run. Died relatively early of some nervous system disease. It wasn’t the same as Lou Gehrig’s disease, but it was bad stuff.

STURCHIO: You’ve now mentioned three or four people who had built light scattering apparatus at this time, and I wonder if you would talk a bit about the networks involved in passing on this tacit knowledge of how to use the technique. Doty and Zimm were working on this in New York for several years, eventually doing this with Mark. Each group had to build its own apparatus and learn how to use it. Did this all go back to work during the war?

STOCKMAYER: Yes. Debye started it, as far as the awareness of it goes. I think Mark learned of it from Debye and got Doty and Zimm interested. They were interested earlier, as Zimm explained (18), in doing light scattering because of McMillan's problem of the binary critical point, so they already knew something about it when they heard about Debye. I learned a little bit about it from them and then from Billmeyer. When Bruno went to Berkeley after leaving Brooklyn, he decided that the apparatus up to that time such as Beckmann's old turbidimeter which he had used at Columbia were too elementary. The Debye apparatus, which Billmeyer essentially imitated, required far too much sample for anything other than industrial scale operations. So Zimm built what, as soon as he'd described it, seemed state of the art.

MORRIS: Billmeyer had actually worked with Debye. At the beginning had anyone actually tried to build a machine from the paper?

STOCKMAYER: No, because Debye didn't write that up. I guess Billmeyer built another like the one that he and young Peter Debye had designed in Ithaca; it was very similar. When Zmm went to Berkeley—since he really knows how to build apparatus—I think he just was very inventive himself, and he sent us all sorts of information because of our friendship. There were Gordon Conferences already, you know, and other meetings, and the people who work in the Rubber Reserve heard what Debye had to say at their meetings four or five times a year. Most of them were just polymerizing Coke bottles full of GRS, but anyhow, some of them got that message.

MORRIS: So it was a question of people building their own machines from their own ideas?

STOCKMAYER: They had to. The first commercial apparatus was the Brice-Phoenix. Brooks A. Brice here in Philadelphia at the regional lab of the Department of Agriculture, designed what seemed like a good practical compromise between the fancier Zimm research machine and maybe some others that by then had been published. I guess he got the Phoenix Company to go into making them and selling them, and they were a big success. This was in the early 1950s.

MORRIS: What did they cost?

STOCKMAYER: I don't know; I never bought one because we had built our own, which by the way has been rebuilt several times, and my younger colleague and one-time student, Bob [Robert L.] Cleland, still has it. Someone from my own lab, where light scattering passed into only occasional characterization found an unused Brice-Phoenix that Peter von Hippel, who was then in biochemistry at Dartmouth Medical School, was discarding, so I got that, and my students lately have used an old Brice-Phoenix. Well, there are no more students now doing that. Yes, it was necessary to build your own. Zimm had some great ideas, but Harry Stanley had enough experience so that he made what I thought were some damned useful modifications. Edsall must have built an apparatus of his own.

MORRIS: From my reading of the Debye report, the complexities of measuring turbidity were pretty great.

STOCKMAYER: Yes. You remember all the multiple reflections? Of course you're talking to a man who is a natural slob, and high precision wasn't my primary interest, but by the time we had got into it Debye was already offering to send to anyone in the light scattering business a sample of polystyrene, which of course he'd got from Dow in a large amount, and which was known as "the Cornell polystyrene," because Debye had measured it and he and his students said, "That's the molecular weight," using their own calibration. It was Bruno Zimm who felt that more careful calibrations were needed, and he and Clide I. Carr out at Berkeley really produced a step upward in that. A man at Case Western called Sam [Samuel H.] Maron, whose interests were more analytical, I would say, than theoretical, also produced some very fine calibrations, and eventually the so-called high values reigned supreme. But it took a while. Harry and I never tried any absolute calibration; we got a sample from Debye, and that was our calibration. Plus, independently, some pure liquids, and we checked up with other people who had by now had been doing those, and when Zimm and Carr came out we didn't have to worry about it. We were close enough, so we didn't change anything. But Harry Stanley and I decided that it was interesting.

When he came as a graduate student, we had quite a different original plan. It was based on the fact that on paper at least the polypeptides made from N-carboxy-amino acid anhydrides were living polymers with a narrow [molecular weight] distribution. You know, ring opening and the CO_2 poops off, but you regenerate the amine end group that's the active end. Just like any other living polymerization, but this was 1947. The Leuchs reaction had been around for a long time but it had been rediscovered by Robert Burns Woodward, who was lecturing about it. I heard him at the Harvard Medical School and he was going around, and a lot of people heard about that. And Harry and I were wondering. He had decided he wanted to work with me and do something in physical chemistry of polymers. Well, if we can make high molecular weights, we should be able to have really narrow distributions, and we can investigate lots of things with narrow distribution polymers, if we can prove that's what we've got. Light scattering itself wasn't yet in the picture.

Woodward claimed to have made a polyphenylalanine of molecular weight of a few millions by the Leuchs reaction with a very high intrinsic viscosity in toluene, something like ten deciliters per gram. Well, it turned out that this was one of Woodward's very few errors in judgment. He never published it, because before it got that far (he had just lectured about it a little bit) somebody discovered that if you put in a few drops of some really good hydrogen bond breaker, the whole thing turned to soup. The high molecular weight was really a couple of thousand. Harry and I worked on this for a while. Well, he did all the experiments. He made a number of carboxy anhydrides, polymerized them, but never could get the molecular weight above a couple of thousand. Inherently it was just too hard to make anything good because of the effect of impurities. So by 1948 we had decided to bag that and said, "Gee whiz, light scattering is going to be around for quite a while, so why the hell don't we get going on that." By then I had the theory for multicomponent systems and had the idea we should study polymer-polymer interactions. So that's what Harry did. The real living polymers, of course, the anionic ones, like those of Szwarc, were more than ten years away.

MORRIS: Could I take up the question of the Gordon Conferences? You were talking about the group you were working with at Columbia, and you were saying that were as good a group of polymer chemists you could work with at that time.

STOCKMAYER: No. Physical chemists.

MORRIS: When did you first realize that you were going into a new specialty called polymer chemistry? Were you ever aware of crossing a line?

STOCKMAYER: I think it was when Homer Jacobson joined Lester Weil as my second grad student. Now I had two grad students and that was the only kind of problem I was thinking about that I could give them. Lester certainly wasn't a theoretical chemist, and Homer wasn't sure what he wanted to be yet. He's done some good theory, but anyhow, all of a sudden, yes, here I had two co-workers and they were doing polymers, so I guess that's what I was doing myself.

MORRIS: What date was that?

STOCKMAYER: That would have been 1942 and 1943. During the war for a while there weren't any more grad students, and so there were three or four years when it faded temporarily. Then, when the grad students started coming back I almost naturally turned to polymers. I had kept with it and thought of lots of problems. I kept with it partly because the gelation work had been published and was of interest and I kept thinking about problems like that.

STURCHIO: We were talking earlier about you contacting Flory and the other people in the New York area who were working on problems of this sort. But I thought your comment was interesting that it was really physical chemists you were working with, and you probably saw yourself as a physical chemist.

STOCKMAYER: Yes, sure. I still like to think of myself as an old physical chemist, no longer in the swim.

STURCHIO: Your work certainly still has that character to it. But by the early 1950s you were publishing mainly on problems in the polymer area, and you were presumably by then going to the Gordon Conferences in polymers, and publishing in the *Journal of Polymer Science*.

STOCKMAYER: Yes, I guess I was chairman of the Gordon Polymer Conference in 1954.

STURCHIO: So things had changed within a ten-year period, not only in your own interests, but also in the polymer community.

STOCKMAYER: Yes.

STURCHIO: I wonder if you could just talk about that a little. You've alluded to one aspect, namely that after the war there were more students all of a sudden, and they needed problems. Just demographically, there were more people going into polymers.

STOCKMAYER: Yes. But also, I guess there were more obvious questions that people were interested in. I'm talking now of the academic people. In the industry, of course, they didn't need to wait like that. I'm not sure I quite know what to say.

MORRIS: There may be one way into it. When you went into polymer science were you aware of building up a new kind of circle of people that you were interested in, that you worked with, I mean? You were speaking of Flory and Mark. Did you find you were coming into contact with new people?

STOCKMAYER: Well, no and yes. I think I still felt that the main message was that the big molecules obey the same basic laws as little ones, and this was the great message that Flory was carrying also, although he worked with polymers. But you felt that you were a physical chemist who found good problems with big molecules but you weren't a polymer scientist exclusively. For that reason plus natural inclination, perhaps, I never tried to build up an organized program in polymer science at MIT. The only kind of organization there was was a very applied plastics laboratory. Al [Albert G. H.] Dietz, the director of it, a very pleasant colleague from the Department of Building Construction—his interest was in the use of plastic materials in construction—encouraged some interdepartmental contact. Actually, Herb Lauterbach, one of my early grad students, was supported through that set of funds. MIT never got an organized program until the last couple of years. And as you know, they now have some pretty fancy brochures with gray and orange covers, and finally it seems to be going.

MORRIS: Were you aware in the period 1944 to 1945, from the point of view of your scientific work, of a certain community, a new discipline, coming into being?

STOCKMAYER: Well, I don't know that I felt that pretentious about it, but I was certainly aware that more and more young people were getting into this field and we were going to the Gordon Conferences and talking about things to each other. There was only one Gordon Polymer Conference, and the old complaint was, of course, that most of the attendees were synthetic and organic chemists, but the new synthetic and organic polymer chemistry was done in industry and could not be disclosed at Gordon Conferences. And so they had to rely more on talks by physical chemists, and they didn't enjoy those, except for the few of us who were doing it. Turner Alfrey, Paul Doty, Arthur Tobolsky, Bruno Zimm and myself were in this group. I could add Bill Baker, until he rose high in the Bell Labs administration. He was very active in the Gordon Polymer Conferences.

A man I would like to recall with affection and great admiration is Gilbert W. King, I don't know if you know that name? Gil King, born in England, also a quasi-boy genius, MIT undergrad degree in 1933 when he was about 19 and a Ph.D. probably by 21 or 22. He had one of the very few National Research Council fellowships in the middle 1930s, which he spent two years of working successively with Pauling, Van Vleck, Onsager, not too bad a choice of mentors. But there weren't many teaching jobs, and I guess Gil wasn't the greatest teacher in some ways. Anyway, he didn't get a teaching job and he went to Arthur D. Little for some years and then to IBM, and eventually to ITEK Corporation. He was the man who first used digital computers on physical-chemical research problems. King, Hainer and Cross made the tables for the asymmetric rotor energy levels (42), and he went on from there. Gil King did the first Monte Carlo work I ever knew about on chain conformations on lattices, and on the dynamics of change on lattices. Published as A. D. Little reports, but never published in the open literature. He was a friend from my undergrad days and, again, when we went to Gordon Conferences, became more so.

STURCHIO: Do you recall the first Gordon Conference that you went to?

STOCKMAYER: Of course! It was then a Gibson Island Conference. I think it might have been 1946. I was asked to come and talk on gelation. I think the chairman was Cal [Calvin S.] Fuller of the Bell Labs. I think the only time I've ever played golf in my life was with Frank Mayo and Turner Alfrey and I can't remember who the fourth person was. I was not a success. [laughter] I loved Turner; he was a marvelous guy. What a tragedy—he must have suffered a great deal.

STURCHIO: You may have seen the collection of his papers (43).

STOCKMAYER: Yes, I have. Ray [Raymond F. Boyer] wrote some nice things in there, except he gave me one inaccuracy. He had me moving to Dartmouth about ten years earlier than I actually did, and he thought it was because I wanted to ski, but I'm a terrible skier. [laughter] No, I'm no great intermediate skier, let's put it that way; I'm not the worst, but that's not why I went. Ray Boyer was another one of those who came early to the Gordon Conferences, but I guess the Gibson Island people found the club was too populated by scientists. Anyhow, they were glad to have us move to New Hampshire.

STURCHIO: You might be interested in knowing that Charlie Price docks his yacht on Gibson Island now.

One reason Peter and I were both interested in the Gibson Island, later Gordon, Conferences was that just as you enumerated some of the people you used to see and discuss problems with, they served as a very important informal mechanism by which new problems would be discussed. We've been finding in our conversations with people that we really do want to focus more on the role of that kind of informal institution. Well, it's not an informal institution; it later became a formal institution, but it had a very important effect in the gelation, as it were, of polymer science.

STOCKMAYER: It sure did. Indeed, before Atlantic City went bad, the ACS meetings at Atlantic City were particularly good too, because you could meet people so readily on the Boardwalk and get from one meeting to another. I thought those meetings were more valuable than any ACS meetings I've been to in any other location for purely strategic reasons.

STURCHIO: Of course there were thousands of polymer chemists close to Atlantic City, many more than anywhere else.

MORRIS: Could we go back to that question about the social dimension? The one thing that struck me—for example from the people you've mentioned—is the way that polymer science, particularly in this period, was a way that people from organic chemistry, physical chemistry, biochemistry, could discuss similar problems.

STOCKMAYER: That's true. I would certainly have forgotten my organic chemistry even more completely had it not been for the polymer input. No question about that.

MORRIS: What about the input from biochemists? One thing biochemists often complain about is that historians of polymer science often completely ignore the biochemical aspect of it. You mentioned John Edsall, for instance. What kind of influence did the biochemists have on you?

STOCKMAYER: On me, very little. That's my own fault, I'd say. When Francis O. Schmitt ran the 1958 scientific program in biophysical science at Boulder, I was fortunate enough to be invited. The hope was that some of us would see more clearly the problems in biophysical chemistry. Doty and Zimm had already seen and didn't need conversion. I found that I turned everything back into a physical problem after all and didn't have enough curiosity left to really embrace modern molecular biology as I perhaps should have. It's a real deficiency, and I have to admit that it's stupid not to have gone deeper into it than I have, even for my own understanding, just for philosophical purposes, let alone work. But there is no question that now and then somebody will say, well, that's got an obvious biochemical application—okay, so you can work on it. But I'm not a biologist; I don't know how to be one, I guess.

[END OF TAPE, SIDE 5]

STOCKMAYER: You say the biochemists complain that their contribution hasn't been recognized? You mean in the case when they preceded someone else in some discovery, perhaps? Well, that's certainly true of what you could call size exclusion chromatography, where it was rediscovered by John C. Moore at Dow who did it with styrene/divinylbenzene gel, but the biochemists were already doing gel filtration and it was a well developed technique. What others? Actually, I don't know enough myself.

STURCHIO: Well, maybe we could go back to some of the other work you were doing in the early 1950s. After the light scattering papers, you took up the excluded volume effect.

STOCKMAYER: In a way that was certainly a part of light scattering. And then my most brilliant student of all time came along, Marshall Fixman. An absolutely tremendous fellow.

STURCHIO: Did he come to MIT just to work on the kinds of problems you were working on, or did he sort of stumble into your lab?

STOCKMAYER: If I understood what Marshall told me, I think one of his teachers at Washington U suggested that he come to MIT and look me up, and I'm not sure who that was. It might have been David Lipkin, who had once worked for G. N. Lewis, and whom I may have met but never really knew. But whoever it was, Marshall said he was urged to go to MIT. And Al [Alfred M.] Holtzer, who was his classmate, and who is now a prof back at Washington U, was urged to go to Harvard and work with Paul Doty, so they both kind of came East at the same time. However that may be, it didn't take Marshall too long to decide he wanted to work for me, and for me to decide that he was an absolutely fantastically bright guy. One who bet against Kirkwood and won when he did his postdoc.

Well, it's in Bruno's account. It's the light scattering calibration; the lower or big values? Some people said well, "The Einstein theory is wrong, because there's an internal field problem that must be taken into account," and Einstein just sidestepped that, as a matter of fact successfully, but he didn't do, you might say, a complete molecular theory. Should it not be investigated further? I remember when Marshall was in his last year with me at MIT, that was 1953. Jack Kirkwood came by for a visit. Marshall had already won a Jewett fellowship, which was a post- doc the Bell Labs were giving, and had asked Kirkwood to take him on. Kirkwood had already agreed, so Marshall was bound for New Haven shortly thereafter. Kirkwood stopped by to talk about what they might do and it was agreed that Marshall would try to work on the molecular theory of light scattering all the way back from basic principles and see whether Einstein was right or not.

I remember Kirkwood saying, "No, I think there'll be a correction to Einstein." Well, as it worked out, there wasn't. But it was Fixman who first did the molecular theory to the stage where one could see that (44). Later there were other papers from Peter Mazur in Holland and Bob Zwanzig (45, 46), that took it a little farther, because Fixman had cut off after a couple of terms; enough to see that it was going to go, but he did a lot of work. He was thinking on his own. In fact the first paper in which he is a co-author is by Zimm, Stockmayer and Fixman on excluded volume (47).

The story on that one: Bruno and I had been corresponding for years, and when he came back from Berkeley to General Electric, well, I was being wooed by GE and the summer I spent there working with Bruno was partly a tryout. Not that I had real notions of leaving academia, but every so often you want to ask yourself whether there isn't something else you don't know about, and we had this problem to work on: the excluded volume problem, because we had both been referees of a paper that gave a wrong answer. We decided that we could improve on it. We were pretty sure it was wrong but we didn't know how it was going to come out. So we spent the summer and we got an answer that is published in that paper. That was Zimm and myself. I think it was the summer of 1951 that I spent over there. When I came back, I told Fixman about it and showed it all to him and he said, "Gee, that seemlike kind of a long way around the barn to get the result. I wonder if there isn't a better way to do it." This is the more straightforward cluster expansion that he eventually published in 1955 (48), but it's in his thesis. He produced that in a couple of months, and we quickly enrolled him as a co-author to give his partial result, and I guess to save ourselves a little bit, but he didn't mind that at all. Then he went on and did the general theory and published that alone.

STURCHIO: At this point, most of the calculations you were doing were still the old-fashioned way, weren't they? They were just brute force calculations of these various long expansions.

STOCKMAYER: Oh, yes.

STURCHIO: I remember a very interesting conversation I had with Paul Flory a year ago—just about six months before he died—in which he said that he really began to appreciate the impact of computers in the late 1950s and the early 1960s when he could begin to tkle problems that he had had to put aside years earlier because he realized there was no way to make the calculations.

STOCKMAYER: Yes, that's right.

STURCHIO: Did you come across the same kind of possibilities in your work?

STOCKMAYER: Actually, the person who realized that about the excluded volume was Fred Wall. He was already publishing things in the middle 1950s that were very important in that connection. Frederick T. Wall. And some of those results have stood up pretty well, particularly on the dimensions of chain molecules for the lattice model.

Actually I remain to this day pretty much a dunce on the computer, although my collaborators have been using it for over 20 years. Peter H. Verdier, who implemented Gil King's notion of watching chain dynamics by little hopping motions on the lattice, came to me as a postdoc at MIT in 1959. Sometimes you get a good postdoc for the wrong reason. Peter was an E. Bright Wilson student and a microwave spectroscopist, and at that time thought of polymers as gunk, just as most physical chemists did when they got started into it. His wife, Marilyn, wanted to take a master's degree in inorganic chemistry at MIT with Al [F. Albert] Cotton, so Peter needed a job while Marilyn finished her graduate work and he asked me if I'd take him on. There was one problem he worked on a bit which he found kind of dull, and then he thought of this thing. He developed programs to do the first rudimentary chain dynamics on the lattice, something, by the way, he's still doing at the Bureau of Standards. He had a manuscript that I wasn't going to put my name on, but then I contributed a big connection to reality so I decided after all to put my name on the paper (49). I had mixed feelings about that one. Maybe he should have been allowed to publish it alone, I don't know. The method was clearly his invention, but I think the interpretation of his results was mine. It was a collaboration, all right. After that, there were other things; the work with Karel Solc on the shapes of chain molecules, that couldn't have been done without a computer (50).

[END OF TAPE, SIDE 6]

[END OF INTERVIEW]

INTERVIEWEE: Walter H. Stockmayer

INTERVIEWER: Peter J. T. Morris

LOCATION: Philadelphia, Pennsylvania

DATE: 22 January 1992

MORRIS: I went through the transcript and all the materials from the last interview.

STOCKMAYER: So did I. [laughter]

MORRIS: One of the things you mentioned was the Flory correspondence, because you had some early correspondence with Paul Flory. But you were going to clear it with Emily Flory first.

STOCKMAYER: I've not done a thing, but I've still got it.

MORRIS: Did you send copies to the [Beckman] Center?

STOCKMAYER: I haven't even done that. No, I'm sorry. The editing of *Macromolecules* (I'm one of the Associate Editors) has gotten to be an ever-more demanding chore, at least with respect to my energy and abilities. I'm hardly lecturing at all. I'm still trying to write papers. All of a sudden I've got a couple of small research ideas, now that I have no collaborators. [laughter] I do all of the calculations myself.

The Flory project should have been done a couple of years ago, and I just haven't got around to it. It's on schedule for the spring to do that, so then they'll all turn up. I've got the material, even the first letter he ever wrote to me in 1942.

MORRIS: You know the Flory papers are here?

STOCKMAYER: Yes. And I think the material ought to be sent here. I can easily make copies of anything that I consider important.

MORRIS: Good. I was also thinking in terms of the joint memoirs you're writing. Who are you writing it with?

STOCKMAYER: [William S.] Johnson and [Henry] Taube, who were both his colleagues at Stanford. In fact, Taube has already written something for the American Philosophical Society (51).

MORRIS: That was what I'm thinking of.

When we stopped the last interview, I think for lack of time, as I recall, we were around 1953. You had been talking about your collaboration with Marshall Fixman.

STOCKMAYER: Fixman, who was my student. I thought we'd got a bit beyond that because he left in 1954 to go to Kirkwood and then into the army. There's something at the very end about Peter Verdier and the work that was done in 1959 and 1960. We stopped somewhere at MIT.

MORRIS: We touched upon the consulting. We sort of did go backward and forward a bit. I thought one good point to start this interview would be to talk about your Guggenheim fellowship to Strasbourg in 1954, which we didn't discuss.

STOCKMAYER: That's correct.

MORRIS: How did that come about, and why did you pick Strasbourg in particular?

STOCKMAYER: I picked Strasbourg partly because I had met Henri Benoit, who had a year with Paul Doty at Harvard. I didn't know [Charles] Sadron personally. Paul Doty, who was at Harvard and a good friend since his grad school days at Columbia when I was an instructor, said, "Sadron is just the kind of fellow you'd enjoy working near. Why don't you go to Strasbourg? It's a good place." So I wrote to Sadron, who said, "Sure, if you want to come here, we'll have a room for you. In fact, the new building is going to be ready in the fall of 1954."

I applied for the Guggenheim and carried out some of the work I'd planned to do. As usual, it never turns out the way you'd think. [laughter]

MORRIS: What was your experience like in Strasbourg? What sort of work did you manage to achieve while you were over there?

STOCKMAYER: There were a few papers (52). I got to know a whole new coterie of people. Travel wasn't all that common then, but some of the big shots would come over reasonably frequently. I got to know a nice bunch of young Frenchmen and a few Germans on some trips there.

Because of my father's south-German origin, I still had cousins and an aged aunt at that time. My own father and mother came over in the fall of 1954. It was his first visit to Stuttgart since 1921. He was bitterly disappointed that the world had not stood still over there. Things were different.

Going to Alsace was partly a cultural idea. I didn't yet want to go to Germany; it was too soon after the war. There was a feeling that I didn't want anything to do with ex-Nazis, and I didn't know how Americans would be received or what kind of relations there would be. In the event, they turned out to be marvelous, but, in any case, I was not anxious to stay in Germany. Alsace was reputedly and indeed a mixture of French and German. They used to joke that the cooking was French in quality and German in magnitude of the dishes. [laughter]

MORRIS: Presumably you knew German anyway because of your family background.

STOCKMAYER: I knew a great deal of German. I spoke it as a child before English, and I'd done quite a lot of French in school. So I wasn't afraid of doing French. In fact, I lectured in French here and there, usually observing a few wry little smiles or remarks when I said something foolish. [laughter]

MORRIS: Did you meet [Hermann] Staudinger when you went over there.

STOCKMAYER: Indeed! He was in Freiburg, and I called on him. My Guggenheim stipend came from the U.S. in dollars. I had a friend, Henry Faul, who was a geologist working in Strasbourg. He had a Fulbright and was paid in French francs. So monthly, we drove together to Basel and did some currency exchanging. I did a certain amount of changing dollars for francs directly. We both benefited from the deal somehow. I needed French francs and he wanted something else. I really don't remember the details.

But anyway, one way to go from Strasbourg to Basel, not the shortest way, is to pass through Freiburg. I had written Professor Staudinger, and he said, "Come on over." So I did. The place was still an old, old chemistry institute. He had just two assistants, people named [Hans] Krässig and [Walter] Hahn. Hahn eventually came to the United States and worked for Du Pont in Virginia. I never saw him again that I recall. Krässig also disappeared somewhere into German industry. There were these two guys and Staudinger.

MORRIS: Were they the only three people in the department at that time?

STOCKMAYER: As far as I know. There might have been some grad students as well, but I was introduced only to these folks. Staudinger must have talked to me for about half an hour and handed me some reprints of recent work. He seemed most annoyed that the Americans had not honored the royalties on his patent for a coffee substitute. [laughter]

MORRIS: I think he did that work with [Tadeus] Reichstein (53).

STOCKMAYER: I think so.

MORRIS: What was your impression of Staudinger as a person?

STOCKMAYER: Well, I guess, a grand old man is all I can say. He was very affable, and he was not about to start quarreling all over again with Herman Mark at the time. When Staudinger eventually did come to the States, I think it was 1958, he did come by MIT and gave a lecture in German. It was my job to translate every five minutes' worth from German to English, which I was pleased to do. By that time he had given up the notion that all polymers were rod-like, and had even sort of made up with Herman Mark.

MORRIS: I would like to ask you about Staudinger and Mark a bit later on.

I was going to ask you another question about your stay in Strasbourg. What was your impression of European polymer chemistry, compared with what you had known in the United States? Did they approach it in a different way? Was it as sophisticated?

STOCKMAYER: I don't think that it was all that different. Benoit was a very good statistical mechaniker with natural ability, whom I met right away. Sadron, although he was now much more of an administrator, was certainly doing top-flight work. Over in Germany, Arnold Münster, who eventually left polymers for more formal statistical mechanics, was doing polymer work, and I visited him several times. It turned out he was a musician too. [laughter]

In Germany I found sort of two camps. There were some really industrially-oriented people sitting in university slots, and I had not very much in common with them, in terms both of my own background and natural interest. I traveled around, visiting the polymer chemists in Paris: [Georges] Champêtier and [Michel] Magat. In Belgium, in Liège, there was Victor

Desreux, who was one of the early people doing first-rate light scattering.

So I thought the caliber was pretty damn good. At the opening of Sadron's new building in September of 1954, he had a really starry crew assembled to give lectures. There was a small volume published on that (54). Melville (later Sir Harry), was there from England; so was [J.A.V.] Butler, who was working on DNA at the time, and Sadron had got interested in it. Herman Mark, G. V. Schulz, [Werner] Kuhn, and Aharon Katchalsky.

There's a funny sideline. Katchalsky is the only one who didn't turn in a manuscript, but they taped the lecture, just as you're taping this. Unfortunately, although Katchalsky spoke tolerable English, he did so with a strong Polish-plus-Israeli accent, and none of the French people could figure out what the hell he was saying. [laughter] They asked me, as someone whose native language was English, if could I sit and commit to paper the Katchalsky lecture. I did, but I had to play the thing about twenty-five times before I could figure out some of the things he was saying. But, of course, the science was understood. He was talking about polyelectrolytes, which were very new then and exciting. A great man, very interesting.

MORRIS: Let's talk a little bit now about your final years in MIT, because you came back in 1955 and you had roughly five more years at MIT.

We talked in the last transcript about the people you knew at MIT. Arthur Cope, for example. Is there anything about those final five years that you feel might be worth mentioning? For example, in the last transcript, you did say that you thought that Cope's influence was waning in that area.

STOCKMAYER: My own feeling was that his center of interest had shifted from the banks of the Charles River to Washington, D.C. He was heavily involved in ACS affairs. He had been president, he was about to become chairman of the board. He'd done a major job of building the department up into a good one from what hadn't been that, and I think perhaps he felt everything was going all right. But in any case, he didn't seem to hear what I was trying to say.

We were a group of eleven faculty members in physical chemistry and we had a couple of very bright young people. The super-bright one was John Waugh, later a Wolf Prize winner, as you may know. He is one of the really expert NMR people of the world. He essentially invented the pulse-sequence NMR that made solid-state NMR possible. He didn't get the Nobel Prize; the guy from Zürich did, who developed it further [Richard R. Ernst, 1991], but John was really in the running, I should have thought.

John Waugh had a nasty little laboratory in the basement and he was getting the best students. It was crowded to beat hell, and he needed more space. I was then on my three-year term as a rotating chairman of the physical chemistry subgroup in the department, so I said to Cope, "John Waugh is an absolutely first-rate guy. He needs more space, and I know just the lab and just the place you could give him. Here's this thing that used to be for qualitative analysis; it's nothing but a repository of samples now. For God's sake, give it to him for a laboratory." Cope said, "Well, I'll think about it." Nothing happened. The next thing I knew, that very room had been converted to an editorial office for Fred [Frederick D.] Greene and the *Journal of Organic Chemistry*. John Waugh was still in the basement. So that got me a little bit disappointed.

The other reason that I can discern for moving off, was that I really felt I was going stale in research. I had some very, very good students. Never again another Fixman, but some very competent people. I was disappointed that I wasn't giving any problems that seemed really exciting, either to them or me anymore. I don't want to mention any more names, it's irrelevant. I thought, "Well, maybe I've done all that I've got a chance of doing, but I know I'm a good teacher." I knew Dartmouth through John Wolfenden, an English-born physical chemist at Dartmouth. I'd gone to his lectures at Oxford and got to know him then and since.

So when the sort of quite accidental chance came to go up there, I went and had a look, and said, "Maybe this is really what I ought to do, because I know I can teach these undergraduates. They're bright; they're not all science-directed. It's a different place. It's a new atmosphere. I like mountains and it's closer." That coupled with the fact that I really felt that I couldn't seem to make things happen in the department at MIT, so I decided to go.

But I didn't play around with it. I didn't say, "Well, I'll stay if" or anything like that. I made up my mind, and I told them I was going. I don't like this business of bargaining. Although, there are times I suppose when it does make sense. But the situation was so different here; it wasn't a sideways move, it was something different.

I can mention that in 1959, I did go out to La Jolla at the invitation of Harold Urey, who had gone there, and Jim [James R.] Arnold, who was there. The campus that became UC San Diego was essentially non-existent. There were very few people there. They asked me if I'd like to go out there and join the department. It was tempting in a way. I'd always admired Urey greatly and I like Arnold. Roger Revelle was then the local chancellor, I guess you could say, and he painted a picture that was destined to remain only a vision. It was going to be a campus limited to only six thousand students, almost all of whom would have science or engineering as their interest. In other words, another western Institute of Technology to rival Caltech, or something like that.

That didn't happen, of course. It's a twenty five thousand-student-plus branch of the University of California. Urey said, "We want you to come out here, and you'll be the next chairman for a while, and you'll be able to design the new building." I hate designing buildings! I couldn't imagine doing it. [laughter] That was the end of that.

MORRIS: Is that why you turned it down?

STOCKMAYER: Essentially that was the main reason. Plus I thought, "Well, it's not so bad at MIT." But within a couple of years I had really convinced myself that I wasn't doing good research just then, and that maybe I should move.

MORRIS: Who made you the offer to go to Dartmouth? Did they make an attempt to get you?

STOCKMAYER: They did in a way. I don't know how interesting this is. I have a friend named Manuel Morales; he's a biochemist. He was at the Dartmouth Medical School then, as chairman of biochemistry. He moved to UC San Francisco, not so many years after that. He had earlier done some light-scattering theory while he had been at NIH, so we'd had some correspondence. We were both at the summer study in Boulder, Colorado, in the summer of 1958 on Biophysical Science, which Francis O. Schmitt of MIT had organized. There's a big volume of collections of lectures about that (55).

Schmitt's idea was that he could get people in various branches of biology and biophysics to talk to each other and to physical scientists who had perhaps an interest in learning more about what they could do in a biological area. Doty was there, Zimm was there, as well as Stuart Rice and John Ferry, among people I already knew. But there were a hell of a lot of biologists, and some real physicists. Charlie [Charles H.] Townes was there; he's essentially the inventor of the laser.

Morales was there, and when I first saw him, I said, "How are things going at NIH?" And he said, "Oh, I'm not there any more, I'm up at Dartmouth, in the Medical School." I said, "Are you? You lucky guy." I thought of mountains. I'm in love with the White Mountains. Apparently, he went home, having remembered that remark. Dartmouth had gotten a slug of money from the Rockefeller Foundation to increase the pure-science component in the medical school. But they had a promise from the president, John Dickey, to do a little more with appointments in the physical science departments of Dartmouth College. He talked to Wolfenden, and Wolfenden was the person who got in touch with me. They asked me up.

There was another connection. In the Dartmouth physics department, there was one of the most successful physics textbook writers of the century, Francis W. Sears. Sears had taught me physics for one term when I was an undergraduate at MIT. He'd gone up to Dartmouth on a sabbatical year, liked it very well, they liked him, and he stayed on. So I went to see him and asked him, "Francis, how do you like it up here?" "Oh, it's marvelous!" He really sold me on it. Then I went home with a notion that this was something worth considering.

I do remember sitting down with my wife and trying, perhaps foolishly, to quantify all of this. Let's consider all the factors that would go into such a decision and let's give them each a weight of so many points and then adding up to a hundred and award the points to the institution or the location that would come out ahead. Everything from professional colleagues, friends, living conditions, you name it. Financially, there was no consideration. They made an offer. It was a five hundred buck raise, so there was not too much competition. [laughter] It came out fifty-five for going and forty-five for staying. It was that close. I tried to re-do the calculation, but no matter how I did it, it roughly came out sixty to forty, fifty-five to forty-five; so we went.

One postdoc came with me. He was a Japanese named [Michio] Kurata, and he became a professor in Kyoto and a leading figure in Japanese polymer science. Michio Kurata had come to MIT on a two-year fellowship. I wish to hell I could remember now which auspices were involved. It was a large program of two-year foreign fellowships for people in all the disciplines, not just chemistry. But Michio had from March until August still left, and I moved up in March of 1961. I said, "You can certainly stay at MIT, and I'll be in touch with you. You've got your place here and you're among friends. What would you rather do?" He said, "I would like to come with you." So he did.

While he was there we finished a monster review article for one of the first volumes of *Advances in Polymer Science* (56). It was in there that the error occurred, which I have to talk about sometime. This was an enormous collection of data on the conformations and solution viscosities of polymers of all kinds in solution. It was an enormous job, and he did ninety percent of the work in putting all this together. We discussed the theory of light scattering and of conformation. We included the excluded volume effect, the whole bit, and then data on all sorts of polymers. Amongst them was one particular one, polyoxymethylene. Delrin® wasn't on the market yet, but they were working on it. There wasn't much data on conformations because they hadn't found solvents for the big polymers. Staudinger had some very old data in his book (57), but not much. Some Japanese had measured some dipole moments and we calculated out of those.

In this case, where the gauche conformation is favored, the thing likes to make helices—the so-called "pentane effect", a gauche-plus, gauche-minus interference. It is a relatively minor thing for a great many of the vinyl polymers, but it's just an enormous thing here, and we left it out! As a result, we got a numerical answer for this polymer that was just ridiculous. It said it would essentially roll up into a ball.

When I realized that, the review had just been published. People hadn't called me on it yet, so I decided I would publish a correction. I don't know where the whimsy came from, but I used a pseudonym, which was slightly indecent. There was a good physicist named Rudolph Eisenschitz. He was a collaborator of [Fritz] London in pre-Nazi Germany, and then he had a professorship at University College, London. I had fun with the idea of using another element in the periodic table and I coined the name Waldemer Silberszyc, which was W. S., my initials. This was a high-school-level joke with the indecent word. I spelled in a slightly Polish way with the "szyc" instead of "schitz."

I wrote a letter to the editor of *Polymer Letters*, Charlie [Charles G.] Overberger, pointing out the error in this review article and citing some of the old Staudinger data, along with a back-of-the-envelope first approximation, putting in the pentane effect, and getting a more reasonable answer. At the end of the article I said, "The writer thanks W. H. Stockmayer for a useful soliloquy." [laughter] [see following two pages] It was accepted and published (58). The address was my P.O. box in Norwich, Vermont, which some people think is a college town. There is a Norwich University, a military college in the center of Vermont. It had started in Norwich, but when the buildings burned down back in the nineteenth century they moved over to another town far away, Northfield. But a lot of people still think, "Oh, Norwich, sure. That's a college town."

MORRIS: Did you get any correspondence on that paper?

STOCKMAYER: I got a request for a reprint from none other than Anton Peterlin. [laughter] I sent Peterlin a copy of with a florid message subscribed to by the author. I never heard any more about that. And I heard at least one person quote it in a lecture, perfectly seriously.

MORRIS: How well known was the fact that it was you?

STOCKMAYER: I guess only among my pals for a long time. I don't think I did anything special about advertising it. "Stretch" Winslow and I had fun with the name later. I had to leave the associate editorship for a while when I had to be department chairman again, and Bob Cleland sat in for me. When I came back to being an editor, there was part of a year where there would be a blank on the masthead under the editorial advisory board members.

MORRIS: What journal was this?

STOCKMAYER: *Macromolecules*. My name had been there, but now I resumed being an associate editor. That left a blank on the advisory board, so we put Waldimer Silberszyc up there for a few months. [see following page] I guess Charlie Bertsch knew about it. I'm not sure he liked it. [laughter] It's kind of silly. It's a puerile episode.

MORRIS: Before we move on to discuss a couple of your papers, one person that you never mentioned in the first transcript was Avery Morton, despite the fact that you were both at MIT. I noticed in your bibliography that you did do one paper on the Alfin polymers (59). Unfortunately, he died since we last spoke. Did you know Avery at all?

STOCKMAYER: Oh, sure. In fact, we remained friendly, which you can't say of too many of Avery's colleagues. I thought his chemistry was really empirical in making the polymer this way. His ideas about how it worked were pretty nonsensical, I think. The reason he quarreled with Roberts was partly over that, but many other things. [laughter] The reason I was a co-author was that I said, "Avery, at least we should be able to do some polymerization kinetics in the case of the isoprene polymer, which doesn't go too fast, and it's reasonably soluble. So we'll get some reaction rates." Two of the people who worked on his projects and did some of the work were Robert L. Letsinger and Eugene E. Magat.

[END OF TAPE, SIDE 1]

MORRIS: We were at the end of the Alfin paper, and then we started on the polystyrene.

STOCKMAYER: Yes. Well, that was not published; it was in Cleland's thesis. The solution properties were not detectably different from ordinary free-radical made polystyrene, the part that we could study. But there was this insoluble portion, which all appeared somehow near the beginning of the reaction. If we plotted the total amount of insoluble stuff as a function of conversion it went down. So it was all there near the beginning and that was all. But we threw it away.

Some years later, after [Giulio] Natta, a man named [Jack L. R.] Williams at Eastman Kodak went back and polymerized styrene with Avery Morton's Alfin catalyst, the same one. He looked at this stuff that wouldn't dissolve, and it was isotactic polystyrene (60). Well, Cleland had it already in 1950, as a master's student! If we'd known what the hell we had, we might have gotten it first.

MORRIS: What was it a block polymer?

STOCKMAYER: Well, I can't recover his samples; they were all thrown away. But we presume because Williams made them the same way, that what we had was some isotactic polystyrene, which has a high melting point and is not room temperature soluble in anything. I suspect that we had a stereoregular polymer and didn't have enough brains to look for it. We thought it was cross-linked.

There's another strange thing about that. There's an early paper by Natta, of which I have seen only the abstract (61), because the original one is in the *Atti accad. Lincei* [*Att. della Reale Accademia Nazionale dei Lincei*] (62), in which he reported on electron diffraction from surfaces of some polymers, including polystyrene. In it he says the pattern suggests a regular three-turn helical structure for ordinary polystyrene, which could not have been made in 1936 by a route to isotacticity. He never followed up on that, and that article was never once cited by Natta in any of his later work, which was real stuff. So conceivably he decided the measurements simply were not reliable.

MORRIS: That sounds very interesting.

Let's talk about one paper that you've marked as being important, and one of your more significant papers in the 1950s. That was the paper you did in 1955 on copolymers in dilute solution (63) with Fixman and [B.N.] Epstein and [Louis D.] Moore [Jr.].

STOCKMAYER: Yes. I think it was the first paper to look systematically at the confirmations of a random copolymer in solution. It was marked with a Roman numeral one. There was some later work, Roman numerals two and three, that could have been written, but never were. They were given at ACS meetings, and I wasn't ashamed of them, but I'm simply not that energetic a writer-up of things. The first paper, the one that was published, had a theoretical section in it, in which I pointed out how the light scattering from a copolymer needed to be treated, going back to the multi-component light scattering theory. Essentially, in our "azeotropic" styrene/methyl methacrylate copolymer, we did not take account of these extra terms. But Henri Benoit saw that you could turn it around and turn it to advantage; namely, if you measured a given copolymer in a series of solvents with different refractive index, the evaluation of these extra terms would give you something on the breadth of distribution of composition in the copolymer. It was something which I could see but hadn't bothered to pursue. But he did it systematically and made a very good thing of it, both writing out the theory in perhaps more transparent form and then guiding some measurements with his students. I think it's important because that's where it was. I did it there, and then it was taken on from there by Benoit.

MORRIS: You yourself did not do much more work in that field.

STOCKMAYER: No. I sort of got interested in other things. If I had been a better organized person and maybe have had a couple of more energetic people, we might have done more.

MORRIS: In the last transcript we spoke a bit about the move into computers around 1960, which led to your Monte Carlo paper (49).

STOCKMAYER: That was Peter Verdier's work in 1962. I was only a guardian angel.

MORRIS: We didn't talk specifically about the paper, so I thought you might like to tell us a bit about Peter Verdier.

STOCKMAYER: I'll go back and say again that the idea originally came from Gilbert King, whom I mentioned in the earlier transcript, and then Peter took it up. I think he rather independently decided he wanted to do something like that. He invented the flip move, which is the basis of it, and with the resources of that day had a few results to show. There was the question of how you fix the physical time scale onto a number of cycles in the Monte Carlo program. Peter hadn't solved that problem, and I did, snowed in one night in the Buffalo airport. It suddenly came to me that we could use the translational diffusion coefficient as the scaling factor to get to real physical time. Having contributed that, I felt that I had made a good enough contribution, so I put my name on after his. In retrospect, I wish I hadn't, because too damn many people always tend to take the senior author as the person who did everything. I'm sometimes considered to have been the guy who invented that kind of process. Well, it's Verdier and Stockmayer.

MORRIS: Could you tell us a bit more about him? He's not a name that I'm personally familiar with.

STOCKMAYER: He's still at the National Institute of Science and Technology in the Polymers Division. He's a Caltech undergraduate and a Harvard Ph.D. with E. Bright Wilson in microwave spectroscopy. I think this does appear on the last page of my previous interview. He asked for a job with me, I think almost entirely because his wife Marilyn needed to finish up a master's thesis with Al Cotton in inorganic chemistry at MIT. So I took on Peter, and first I asked him to work on a problem that we never got too far with, but Fixman and [Gerald] Wilemski solved it a few years later at Yale; namely, the kinetics of the two ends of a polymer chain finding each other.

Verdier didn't seem to like that problem. He was a microwave spectroscopist, all right, and a good one. But he did invent this other thing. He thought of that one day, and I said "Fine, that sounds like fun. Go ahead." So he did that the rest of the time that he was with me. Then he went back for a while to Bright Wilson, getting more microwave data, and had horrible luck. A workman who was cleaning the windows fell into the spectrometer and smashed the whole damn thing up to beat hell. It took five or six months to rebuild it.

Peter then had a job for a while with Union Carbide; they had a place up in Westchester County for a few years. Then from there he went to the Bureau, where he's been ever since. He's still flipping polymers on and off lattices, more sophisticated versions of the problem. He was for a while the chief of the Polymers Division in the Bureau; maybe fifteen years ago he had a term at that.

MORRIS: I noted that you didn't mark any other papers from the 1960s in your bibliography. What other lines of work were you pursuing during that time?

STOCKMAYER: I don't recall what I marked. I can't give you a specific answer.

MORRIS: Were you particularly busy at that time as head of the department?

STOCKMAYER: I was chairman from 1963 to 1967.

MORRIS: Did that take up a lot of your time?

STOCKMAYER: Sure. There were some good postdocs around, but, again, I think the dielectric work that was resumed for a while was in abeyance. I don't know that I can point to anything I was too awfully excited about.

I want to give a lot of credit to John Hearst. I haven't mentioned him so far. John Hearst is professor of biophysical chemistry in the chemistry department at University of California at Berkeley. He had come from Caltech and had a National Science Foundation postdoctoral fellowship. He'd worked with [Jerome R.] Vinograd on the density gradient sedimentation just at the time when [Matthew] Meselson and [Franklin] Stahl and Vinograd did that crucial experiment on the DNA, using nitrogen-15 labeling to prove that one strand was the template for the other. John was in on some of the physical theory on DNA, and he wanted to work more on the theory as well as the dynamics of semi-flexible chains. I don't remember if he called me or wrote, but in any case, he said he'd like to come work with me. I pointed out to him that I had no graduate students, and we didn't have a doctoral program at Dartmouth as yet.

MORRIS: Really?

STOCKMAYER: That didn't start until 1965. I told him he'd have only me to talk to. But he said, "That's okay. I like skiing, and I'd like to come." I said, "I'll take a chance." As I said, I felt that my research ideas had sort of gone downhill for a while, and one of the reasons I left MIT was maybe it was the thing to do at the time.

Hearst kind of got me started again, because we did do a useful paper. He did a couple just under his own name, but I think I got them started, and then we did publish one together, on the diffusion and sedimentation of semi-flexible chains, which is still quoted (64). It's a good paper. To John, I owe this extra credit because he got me interested in research again.

Then the next postdoc I had was Hyuk Yu, whom you may know as a professor in the chemistry department at Wisconsin. Korean-born, very bright, already with a master's in organic chemistry from Southern Cal with Jerry [Jerome A.] Berson, now at Yale, and then a Ph.D. at Princeton under Arthur Tobolsky. Then he wanted to come work with me as a postdoc, and I said, "Okay." We got some dielectric measurements started. His name's on one paper, which was published rather later (65). We did a theoretical one, too, which turned out to be wrong. Here's another marvelous story of something that didn't pan out.

MORRIS: How do you spell his name?

STOCKMAYER: Yu. And his first name is Hyuk, and if you don't pronounce it carefully, people misunderstand what you're saying. [laughter] He has fun with that when he lectures to freshmen.

Yu and I did a theoretical calculation of the intrinsic viscosity of what we call the once-broken rod, a thing with a hinge in it, thinking of a semi-flexible chain model that we could solve. The paper got accepted (66). In 1966, we were both at an IUPAC symposium in Japan, and there was a little post-symposium meeting in Kyoto, where we gave this story on the theory. [Hiroshi] Fujita was there, and he said, "I've just had a student make some measurements on a polybenzyl glutamate. It's polymerized onto a diamine, so it has a flexible hinge and it has these two rods. Our data are in pretty good agreement with your calculations." We both thought that was just great.

The sequel is that our theory was wrong. We made a rather fundamental mistake. There's just one place in the calculation where we didn't move the center of gravity in the molecule the way we should have when the thing bent.

MORRIS: It wasn't a crucial error, was it?

STOCKMAYER: It was crucial enough to change the calculation. That was fixed later and generalized by Gerry Wilemski. In Fujita's data, it later turned out, the joint wasn't fully flexible. Wilemski, a later postdoc of mine who had been a Fixman student, did the theory for joints of any degree of flexibility and sure enough, we can sort of confront the data and reinterpret it with a correct theory. This is a significant example—the wrong theory that fits experiments that are also misinterpreted. [laughter] It's not uncommon.

MORRIS: When you say that you didn't have a doctoral program at Dartmouth until 1965, it emphasizes that one tends to forget that Dartmouth is a fairly small college.

STOCKMAYER: It is.

MORRIS: What was your impression of Dartmouth, after being at the sort of powerhouse of MIT, with Harvard alongside?

STOCKMAYER: I felt more isolated in some ways. My colleagues were very pleasant. I loved the teaching, and I had some extremely good students each year in physical chemistry. I also taught a lot of freshman sections. So I wasn't completely isolated.

We had then a very energetic dean of the small engineering school there, the Thayer School, a man named Myron Tribus. He was enamored of statistical thermodynamics, as taught from the point of view of E. T. Jaymes, using Bayesian statistics, rather than the conventional way. He ran a "peripatetic seminar on thermodynamics" to which people were invited from everywhere. Sears gave a talk on the Carathéodory thing. He had a simple model that showed where it was really useful—the Carathéodory way of telling the second law. And Mark Zemansky, a famous pedagogue from City College, who had written several great physics books, came and spoke.

It wasn't complete isolation. I could drive to Cambridge in two and a half hours or so, and I still had several graduate students finishing up down there. So I wasn't that isolated, and I came down here now and then. I've never felt complete isolation, especially because I've never had a large and demanding experimental program. If I had to rely on nuclear magnetic resonance in the early 1960s, we didn't have anything. Paul Shafer, after a sabbatical with Jack Roberts, did get us going on that.

But today, we're behind again. We have a three hundred megahertz machine, but the people we'd like to hire in biophysical chemistry want to do protein structure, and they want protons with five or six hundred megahertz. So we're looking for another half a million bucks or so. You see, that's the trouble with a smaller place. You can't do everything, and you shouldn't <u>have</u> to do <u>everything</u>. Of course, you make choices.

I didn't feel as isolated as I expected, and I found the students, well, certainly more varied. In the freshman course there were some incredible dunces, from the quantitative point of view, that thought they were premeds and had to be discouraged. Several of them, by the way, have become distinguished professors of English literature and thank me for having flunked them. [laughter] That's the way it goes. But the best Dartmouth undergraduate science majors, well, they've produced people in the past like [Frank] Westheimer and John Waugh, most obviously—they're tops! And Ed [Edward] Lorenz, the meteorologist at MIT who gets credit of discovering chaos in mathematics through his modeling of the atmosphere, was a Dartmouth undergraduate math major.

The standard reputation of the place is that it's a hairy-chested, outdoor atmosphere, and it's not for intellectuals, but for relatively unintellectual people. That image persists, and of course the kind of people who apply tend to perpetuate something like that. We have women now, and that's produced some changes for the better, I certainly do believe. But sometimes I feel since the women went and made sororities and all that, that they're misguided in part, too.

The place is a lot bigger now. Our graduate program in chemistry runs between thirty-five and forty people in all the branches of chemistry. That's a small outfit with fifteen faculty, but it's not impossibly small any more. My first grad student, Bill Gobush, came in 1966 and got his doctorate in 1970. He was a theoretical chemist, and he did some nice things on statistics of chains. We published some of this (67) and some of it never has been published. He works for a golf ball company, and has a patent on the dimples on golf balls. It made *The New York Times* not too long ago (68). Bill had a great stuttering problem, and I think he would have been a fine academic except that it was impossible for him to lecture, really.

MORRIS: In the 1960s you started to get involved with some outside activities. For example, you were on the NRC Army Research Advisory Panel from 1961 to 1965. With my work on the American Synthetic Rubber Research Program, I got to know Paul Greer, who was in charge of the Army Research Office at Durham. Did you know Paul Greer at all?

STOCKMAYER: Not very well, but I remember him.

MORRIS: What did your work on the research panel involve?

STOCKMAYER: The only thing there was to judge the research proposals. We did a lot of it by mail. We met once a year in a meeting, but that was about it. It was usually in connection with an ACS meeting.

MORRIS: You didn't go down to Durham.

STOCKMAYER: I don't recall ever going to Durham for that reason. In those days the ACS met at Atlantic City very, very often. I really almost shed tears when Atlantic City went by the board, to use no pun. [laughter]

MORRIS: Did the Army fund much research on polymers in the 1960s?

STOCKMAYER: Yes. They had funded some of mine, in fact, in the 1950s already, at MIT. I wish I could remember all the people whom the Army Research Office in Durham did support. They funded some very, very good stuff. I think some of John Ferry's work. I don't really remember.

MORRIS: In 1968, you were chairman of the ACS Polymer Division. Was that a particularly arduous year from your point of view?

STOCKMAYER: No, the harder work was the year before, when I was vice chairman. Then you're the program chairman, and you work getting symposia and getting people to organize them. That was more demanding, but not hard. It's sort of not unrelated to the editing process, in a way. You know the community and you know what they're working on. You form opinions about who would be good to have for this or that purpose. When the journal [*Macromolecules*] started in 1968 I signed on out of curiosity, to see how it would go. It's been something that I've really enjoyed and now, in my now declining stage, find it's extremely useful as a way of superficially being aware of what's going on.

MORRIS: How did the journal come to be founded?

STOCKMAYER: All that has been written in the very first issue this year of *Macromolecules*. There is a little piece called "Twenty-five Years of Macromolecules" (69). It has pictures of Winslow, who's edited the whole time, and of Frank Bovey and of myself, as we've been Associate Editors almost the whole time.

But the journal was founded in response to some agitation led by Paul Flory. He felt that the *Journal of Polymer Science* was not doing an adequate job. I think he felt correctly that there were times when Herman Mark's very permissive attitude toward manuscripts was misguided. Herman would publish just about anything. He felt that the open literature would decide if it's good or not. Paul wanted a Society high-standard journal in polymers, and he got the ACS committee on publications, which was then chaired by Charlie Overberger, to finally get the thing approved. Overberger himself was already an editor of the *Journal of Polymer Science*! Nevertheless, I think he saw that something was necessary, and he led the search for the Editor.

MORRIS: What about the fact that the *Journal of Polymer Science* wasn't owned by the ACS?

STOCKMAYER: That may be a piece of it too. I don't know that there were people in Washington who said, "We've got to own the thing." Prices to individual subscribers were high, except zero to people who were put on the editorial advisory board. That's a custom which I guess is inherited from European publishers and of course [Eric S.] Proskauer and Wiley-Interscience had that philosophy. To this day, I get *Journal of Polymer Science*, both *Polymer Chemistry* and *Polymer Physics*, free, because I'm still on the advisory board. About twice a year [Edward F.] Casassa or somebody asks me for an opinion on a manuscript. I think it's immoral in a sense. But it's by no means a new immorality; it's a custom that's been around a long time.

There were many good papers in that journal, because one did publish there, although really more physical things went to the *Journal of Chemical Physics*. But the really new fundamental developments in chemical physics made the latter journal expand. For example, nuclear magnetic resonance, laser spectroscopy, the computer and therefore the burgeoning of quantum chemical calculations, all meant that although there were still some interesting problems in physical polymer chemistry, it was no longer the forefront. It was proper to have a more specialized journal at this stage. *JACS* didn't have room; *J. Polymer Sci.* didn't have the standards.

MORRIS: You had close connections with the *Journal of Polymer Science* and so did Overberger. Were there any bad feelings when the new journal started?

STOCKMAYER: I've never been aware of any, and I've been to some of the board meetings. Proskauer and Mark are the symbols of Viennese politeness all the way. They always are extremely friendly. Ed Casassa is my own former student. I've known Hershel Markovitz since he was a grad student of Joe Mayer's. You know, these people are my friends, too, and I guess they all recognized it had to happen. I don't know what to think about the *Polymer Physics* edition of the *Journal of Polymer Science*. I thought for a while the standards of the papers they published were very high. Casassa's an extremely conscientious editor; he doesn't publish junk. Somehow the time lag was greater over there. It may be one of these almost inexplicable rolling snowball things. People want to publish in *Macromolecules*.

I wish we could turn down more. I had a funny letter from a Japanese colleague about a year ago, but I won't mention his name. He said, "I'm concerned that the *Polymer Journal* (Tokyo) (that's the blue covered Japanese polymer journal) doesn't have papers of a high enough standard. Our scientists seem to want to publish so many of them in your journal. I wish you'd turn down more of them so that we could have them." [laughter] Now, that's a mission to Tokyo that would have succeeded!

[END OF TAPE, SIDE 2]

MORRIS: In the last transcript we talked about a formation of a polymer community in the 1940s. You said at one point that you felt you were working with physical rather than polymer chemists. Then, more or less one day you woke up and realized that you were in polymer chemistry.

STOCKMAYER: I think that's true, yes.

MORRIS: Now in the 1960s. We've talked about the starting of a complete new journal for polymer chemistry. The 1960s were probably the heyday of American polymer chemistry in particular, the period when the Americans were still far, far ahead of everyone else.

STOCKMAYER: Yes.

MORRIS: What were your impressions of that period? Was it a happy period for the community, do you think?

STOCKMAYER: I think so. I don't know how to expand on that. I attended Gordon Conferences whenever I could and the ACS meetings and there always seemed to be quite a lot going on, even though I wasn't contributing as much in the way of having a research stable anymore.

I had postdocs right along, and then when the graduate program came I had a few excellent students. Finally, a theoretician whom I consider second only to Fixman amongst my students, and that's Marc Mansfield, now at MMI [Michigan Molecular Institute].

MORRIS: One person said to me that he was always a little bit disappointed at the way the Gordon Conferences developed because back in the early 1950s they were very intimate, and relatively speaking, a very small number of people went to them.

STOCKMAYER: Yes.

MORRIS: By the late 1960s they were inviting a much larger number of people to the conferences, so you lacked that kind of think tank, the workshop aspect.

STOCKMAYER: That's undoubtedly true, and I think it's a result of the money becoming available and the feeling that attendance at these meetings was worthwhile. It was also a sort of natural expansion. The Polymer Physics Gordon Conference was created largely by one person who worked very hard on it. That was Robert S. Marvin, who was a rheologist at the National Bureau of Standards; John Hoffman was there at the time. Those two had a great deal to do with starting the Gordon Conference on Polymer Physics. I can't remember the date of the first one of those anymore. You would know that. That broke up what used to be just Polymers, then Polymer Physics and later there were others. There's now one on Water Soluble Polymers, there's Biopolymers and all that.

In the middle 1950s, though, people didn't have big travel budgets. When I was chairman of the Gordon Conference on Polymers, in the summer of 1954, the total budget I had to make available to speakers for travel expenses was six hundred bucks. And I wasn't encouraged in any way. The companies were supporting it anyhow; you know the whole idea of that. So the idea of going to companies and getting them to promise more money or to promise to schedule visiting speakers from abroad simply wasn't done in those days. Tobolsky had been my predecessor as chairman, and I think Ray [Raymond M.] Fuoss the year before that.

In any case I did sort of what they did, and I brought G. V. Schulz to America, possibly for his only visit, and used five hundred of my six hundred bucks on Schulz. But he was a success; he was a legendary figure from the Staudinger era in the 1930s and one of the real founders of polymer physical chemistry, along with Kuhn in Europe. He came, and we even got him into a softball game; he hadn't the slightest idea what was going on, but we made him hit the ball once and got him on base, and made him score a run. He remembered that for many years. [laughter]

MORRIS: I suppose one problem that the polymer scientific community had in the 1960s was one of success. The numbers must have grown enormously in the 1960s.

STOCKMAYER: Yes, although the growth was mainly in industry. By then I think academic departments of chemistry were becoming indifferent to polymer chemistry, as distinct from the more fundamental things that I mentioned which were inevitable because of those advances. The whole business of spectroscopy—without the laser where would it be today? How enormously detailed and successful it is! When Herb [Herbert S.] Gutowsky and a colleague wrote an *Annual Reviews* article on NMR, they called it "an evergreen," and right they are (70)! It's still yielding new tricks.

So polymer science developed. I have a feeling it's sort of repeating or mimicking the history of metallurgy, which was all in chemistry departments and some in physics departments for a long time and eventually had enough science plus applied science to form its own discipline. There are both pros and cons to that. Eventually the chemical engineers saw that polymer science rightfully belonged partly to them, and of course materials scientists often now call themselves departments of materials science, rather than just metallurgy.

MORRIS: My impression is that in a way polymer chemistry or polymer science is somewhat being subsumed into materials science, which has a much broader agreement?

STOCKMAYER: That's true! Absolutely. My research grants for the last decade did not come from the Division of Chemistry of NSF, but from the Division of Materials Science. Norbert Bikales has supported a lot of academic, really theoretical types.

MORRIS: You see that in the Royal Society of Chemistry, which has never had a division of Polymer Chemistry. But it's now creating a division of Materials Science.

STOCKMAYER: With another journal, I suspect, just as we have the *Journal of Materials Research*. I haven't really paid much attention to that. It's over in the engineering library a few blocks away. Hildebrand's rule is that the interaction with the journal varies as the inverse fourth power of the distance from your office. [laughter]

MORRIS: Do you think that the conversion of polymer science into materials science is going to be a good or bad thing for the study of polymers?

STOCKMAYER: Both. They'll learn from people who work with non-polymeric materials, but perhaps the cross-fertilization from chemistry and physics research of other kinds will necessarily decrease. I can't help thinking that the size of effort is so much greater now than it was when I was young that I sometimes don't regret being older. It would be so much harder to start off nowadays. There's more competition and much more to learn.

The computer and modern instruments mean that research can be produced so much <u>faster</u>. I have an almost respectable total of almost two hundred papers now, but that's not a very high rate of production over a forty-odd year span—a fifty year span now, as a matter of fact. It's not. Today it would not be adequate for tenure in most university departments.

MORRIS: Really? Amazing.

STOCKMAYER: Sure! Of course, Lars Onsager only published eighty papers all his life; but count quality, and count all the things he solved that he put in the drawer and never wrote up!

MORRIS: What sort of work were you doing in the 1970s? There were no particular papers which you've highlighted. One of the things I did notice about the 1970s is the increase in number of what appear to be Japanese scientists coming to Dartmouth.

STOCKMAYER: One particular man was Keizo Matsuo, who came as a graduate student, got his doctorate, and then stayed on as my Research Associate for almost eight more years. Generally, sort of one year at a time, I'd say, "Keizo, isn't it time for you to look for something else now?" "No, I'd like to stay another year."

The experimental program that was carried out was in large respect due to him. For example, I even got back into polymerization. There's a couple of papers on vinylidene chloride polymerization (71). I was really pleased with that one. He carried it out, he and a couple of undergraduates who helped. The monomer had been around for a long time, and there wasn't a solvent for the polymer under conditions where you could do proper homogeneous reaction kinetics. Eventually [Ritchie A.] Wessling at Dow found something, and with his consent, we quickly did the kinetics. It was kind of fun to get back into that. But there were also some theoretical things.

There were other Japanese. Hiromi Yamakawa came on a sabbatical year. He is an outstanding theoretical polymer scientist in Japan and the author of the book called *Modern Theory of Polymer Solutions* (72). He is a prodigious worker, a real workaholic, and an extremely good mathematician. He's still producing absolutely first-rate stuff. We did some work on semi-flexible chain statistics while he was there and it is still being used a bit (73).

Wilemski was another super postdoc, who came from Fixman. Unfortunately, he didn't get the academic job he wanted, at the time or later. He's with a consulting firm in Andover, Massachusetts, called Physical Sciences, Incorporated, PSI. He's done very good work on fuel cells and more recently on colloidal stuff. I would have like to have seen him a professor, because he was a very good teacher.

MORRIS: Another name that came up a few times in your 1970s papers was G. Tanaka.

STOCKMAYER: Genzo Tanaka. He was a Yamakawa student who came over and worked for a while. He stayed long enough to get off the Japanese track. He wasn't a member of a pyramid anywhere and he couldn't get back on. He's still in the United States. He's finally got his green card and he's working for Goodrich out in Brecksville, Ohio, between Akron and Cleveland. Essentially, what he's doing is the sort of thing that Biosym does—computer software, polymer properties and correlations, telling synthetic people what to try next, and so on.

MORRIS: How did you find working with the Japanese? Did you find that they approached problems differently from the Americans?

STOCKMAYER: Mainly in terms of the conversations that go with it. In the laboratory, Matsuo is without doubt the most careful chemical worker I have ever had. Among the theoreticians, [Michio] Kurata, [Akira] Miyake, Yamakawa, [Yoshiaki] Chikahisa and Tanaka are all very skilled mathematicians, very neat and very industrious.

The problem is in discussing problems as they develop, because they don't want to suggest to their teacher that he ever is wrong. I had a hell of a job having these people tell me, "Oh, no, you're crazy. That's the wrong thing to go." They would tend to say, "Oh, yes. Yes." I eventually learned that when they say, "Well, maybe," that meant, "Oh, you're absolutely full of it. Just completely wrong." [laughter]

MORRIS: My impression is that the Japanese are doing very well in polymer science now.

STOCKMAYER: It's mine, too.

MORRIS: They're possibly even in danger of overtaking the United States.

STOCKMAYER: I'm not enough of a synthetic polymer chemist to be able to say that, but they certainly seem to be publishing a lot of first-rate stuff in that area. As for physical polymer theory, the revolutionary ideas have come from DeGennes and his school and from physics from a different background. The Japanese have not, on the whole, gone into that very heavily.

MORRIS: Are there any other aspects of your work in the 1970s that you think might be worth mentioning?

STOCKMAYER: I should have mentioned Alan Jones. This is Alan Anthony Jones, because there are two Alan A. Joneses. He's a professor at Clark University, an NMR guy. He came from Wisconsin. He knew enough NMR, and I knew enough polymers, so we made a very good team in interpreting chain dynamics for polymers in solution with NMR methods. We used the sixty-megahertz machine because that was good enough for doing fluorine research at fifty-six megahertz. We did a lot of that with fluoro polymers and then some others. I am very grateful for that collaboration. That's when we got started on these olefin sulfones, which I'm still interested in.

But then another big boost came from my last sabbatical. I went to Freiburg. I forgot to mention the previous sabbatical, because that's where [Ronald] Koningsveld comes in. Shall we do it in historical order?

MORRIS: All right. If you want to.

STOCKMAYER: Ron Koningsveld, the pianist, composer and polymer thermodynamicist, came to visit me after a meeting in the States in 1967 on polymer characterization. He was working for DSM [Dutch State Mines]. He wrote and asked if he could come up and visit me at Dartmouth. I had long ago written one teeny-weeny bit of a paper in which I had derived the critical conditions for a poly-dispersed fluid system, which hadn't been done elsewhere (74). He wanted to meet me.

When he was having dinner at our house we discovered that we both liked music. During the course of that evening, when he confessed that he composed, I said, "Why don't you write something for the two of us—we were playing four hands by then—about polymer molecules?" He said, "I'll think about it, but I haven't much time." A couple of years later he had the time because he was in hospital for about a month. I can't remember whether it was when he lost his finger, or another time. Anyhow, he composed a two-piano suite in three pieces, and we played them at the IUPAC meeting in Leiden in 1970. That was the beginning of the Polymer Music Suite, which now has six movements.

On the polymer thermo theme, I sort of had a dormant interest in that. I decided I'd go to Holland for my next sabbatical, and DSM gave me a place to sit. I worked with Ron on some attempts at shoring up what we call the mean-field thermodynamic theory of polymer solutions. It seemed moderately successful at the time (75).

We also decided we'd write a book, which to this day doesn't exist. This book on phase equilibria in polymer solutions, which was conceived in 1972, has not yet appeared. There are to be three authors now: Koningsveld's former student, Eric Nies, from Eindhoven, is a third collaborator. There's been an awful lot of work. I thought that the definitive thing was practically ready. Oxford Press has agreed to do it.

In the fall of 1987, when I was in Japan for a few weeks, I finished what I thought was the definitive work, but it hasn't appeared yet. Ron keeps dragging his feet; he wants to re-draw some polymer phase diagrams, mainly. It's a textbook; it's not a bloody treatise! But he keeps wanting to put new things in.   He's also had a great deal of personal intrusion which needn't concern us here, but it means that he hasn't been able to put in the time. But it's in his hands. The last time I saw him, we pounded the table with each other and agreed that the Oxford Press would have everything by July of 1992.   We'll see!

Thermodynamics was god at MIT when I did my studies, and I enjoyed polymer thermodynamics from the moment I read Flory's and [Maurice L.] Huggins' papers. Getting back into it through Ron has been fun, and very recently I'm back in it again in another way. I hope that book does come out before we all depart for other places. [laughter] My sabbatical with Koningsveld was in 1972. Freiburg was 1978.

I wanted to go there because now, at last, I wanted to go to Germany. I had gotten acquainted with Walther Burchard at a few meetings, and I knew we could enjoy talking solution properties there. But there was also a fellow doing NMR called Wolfram Gronski, and I thought I would do more polymer NMR while I was there. When I got there, it turned out that Burchard had recently become really overwhelmingly interested in dynamic light scattering, which was then still relatively new. This is time-dependent light scattering, intensity-fluctuation light scattering, quasi-elastic light scattering, whatever you want to call it.

He had some theoretical problems in that, and Gronsky was very busy and, by nature, very retiring. He didn't especially push me or welcome me in. I don't mean to say he was unfriendly; quite the contrary. But I dived into the quasi-elastic light scattering business and the papers that [Walther] Burchard and I wrote there were based on that (76). Then his student, Manfred Schmidt, got one of these Feodor Lynen fellowships from the Humboldt Foundation and asked if I had the NSF money to complete the stipend; it's a bilateral affair. I said, "Sure, come on over." So Manfred came, and we did some more work in that area. Schmidt is now just gone to Bayreuth as a professor after some years at the Max Planck in Mainz.

MORRIS: The last paper that you highlighted was one that was evidently done when you were in Freiburg, the quasi-elastic light scattering paper (77). Perhaps you'd like to relate that to your time in Freiburg.

STOCKMAYER: It was another natural extension of being interested in the motions of chain molecules, the Brownian motion theory of chain molecules, but there were some interesting mathematics to be done. I'm proud that I solved the problem, which was a pure mathematical problem that had eluded several previous workers in the area. It's not mentioned as such in there, but it's kind of fun to do something like that.

MORRIS: What about your co-author on that paper? Can you tell us anything about him?

STOCKMAYER: Burchard is a professor in Freiburg. He's not one of the directors of the Institute. He's what's called an extraordinary professor. He came from East Germany, where he was a boy during the Second World War. He came to Freiburg after a year in another university, maybe Frankfurt, and ended up working with Elfriede Husemann, a former Staudinger student, who had succeeded Staudinger as director of that Institute. He worked on the solution properties of cellulose derivatives. He was really trained as a physicist. He had taken his bachelor's degree, I think in nuclear physics, so he had a more mathematical background than the other polymer chemists around. He has published a lot of good work on solution properties.

MORRIS: Did you get on well with him, yourself?

STOCKMAYER: Very much. During my sabbatical, he was quite ill. He had a heart valve problem, and he had to spend more than half of the time I was there in hospital. I had to take his lectures in polymer physical chemistry, in German, which I was able to do, with the help of the students who knew enough of the words that failed me to make it go. [Hans-Joachim] Cantow could have done it, but Cantow was himself off on sabbatical, and then when Burchard suddenly took sick, I was the only other person in the Institute who could have done it. So I did. I have a nice letter from the dean of the University of Freiburg thanking me for having stepped in and done this.

MORRIS: Did you have a good time there?

STOCKMAYER: Wonderful, yes.

MORRIS: I mean, you did enjoy yourself?

STOCKMAYER: There were several cousins who were still alive. My favorite one has just died a few weeks ago at age eighty-one. They were still in the Stuttgart area. We lived outside of Freiburg in a small village. The Black Forest was right outside the door. There was cross-country skiing in the winter and hiking in the fall. We had a marvelous time and enjoyed really embracing the German culture when the Nazi business was gone, essentially, as far as one could tell. The young people knew more than you sometimes believed, and they certainly didn't like it. They were ashamed of it and resolved that it wouldn't happen again.

So I did enjoy everything about that visit in Germany, and I travelled around quite a lot. I lectured in a lot of places during that time—Konstanz and Heidelberg and Zurich and Strasbourg, which wasn't far away. It's less than an hour over to Strasbourg and my friends over there.

MORRIS: When you came back from Germany, you basically retired.

STOCKMAYER: At that time sixty-five was the mandatory retirement age, so I became professor emeritus in 1979, having been already permitted to take a sabbatical in my last regular year. That is something that's not regular, as you can imagine. But it was already clear that I wanted to keep working for a while, and in fact my colleagues asked me to work three-quarter time, with the dean's consent. For a few years I taught a three-quarter load and then it became a half, and it's now down to eight percent; I'm giving ten lectures this years. [laughter]

MORRIS: Not bad for twelve years after retirement.

STOCKMAYER: That's right.

MORRIS: Perhaps you could just say a little bit about your so-called retirement activities. It seems to me that you have remained fairly productive during your retirement, and I dare say you probably have more time to do other things as well. What have you been up to in the last five years?

STOCKMAYER: Not enough other things. No, the editing has become an increasing chore because of the volume of manuscripts coming in. Even though we're up to a total of seven editors now, six Associate Editors and Winslow as Chief Editor of *Macromolecules*, we each seem to have more manuscripts. All you need to do is look at the size of the journal. We do our best to keep out junk, but still they keep coming, and you have to consider them at least briefly. That's one thing.

For the rest of it, as long as the NSF for a while was willing to give me a modest research grant, and I was willing to have a postdoc or two, I was able to do that. Klaus Huber was another Feodor Lynen Fellow who came just a few years ago. He and Schmidt were both Burchard students who came from Burchard to me for a postdoc here.

MORRIS: Is there any kind of regular link between Freiburg and Dartmouth, or is it just a purely informal thing?

STOCKMAYER: What there is between Dartmouth and West Germany is a professorship that's now been endowed by American donors, William P. and Dewilda N. Harris. It was called the West German-Dartmouth Distinguished Visiting Professorship. I imagine it's just German now. People are invited for one hopes at least two weeks, but sometime some of the political scientists who have come have appeared for only two days and gone off. But they are invited to come and give lectures.

We had Helmut Ringsdorf last year; he's a friend of mine in organic polymer chemistry from Mainz. You probably know him, too; he's an old Staudinger product. Helmut came and gave us six lectures on his work, essentially mimicking biomembranes and things like that with synthetic polymers. Plus a lecture on wine-tasting.

[END OF TAPE, SIDE 3]

MORRIS: Is there anything else you wanted to mention about your retirement activities?

STOCKMAYER: Yes, I do. I've always had hobbies of music and of hiking in the mountains, and I guess those haven't decreased perceptibly, although I'm not as fast a hiker as I was. I'm proud to say that this fall I completed, since turning seventy, once again, the ascent of the forty-eight 4,000-foot hills in New Hampshire. I first did that back in the 1950s and I've done most of them three more times. But after turning seventy, I noticed that I had quite a few of them again! Why don't I try for the whole lot? So I've done it. Now I hike with my grandchildren. But I'm slow.

As to the music, that's continued. Not only with Ron, but I have some local colleagues, not professionals, and we have a piano quartet or quintet performance generally once a year. So

I've done some of the really great chamber music works. There's a weekly amateurs' concert in the music department, and I'm allowed to take part in that.

Research, since there are no more co-workers right now, it's necessarily less, but in the last year somehow more things have happened. I got an idea about thermodynamics of associating polymers in solution as the result of reading a couple of papers in *Macromolecules*, and that's led to one paper (78) and now to a collaboration with another Japanese (Fumihiko Tanaka). It's been long distance by correspondence, although I'm going there in May. Last year I was made an honorary member of the Society of Polymer Science in Japan. Michael Szwarc was, too. There haven't been too many from the U.S., but there is Mark, of course, and Flory, Huggins, George Smets from Europe, maybe a handful of others. Some obviously should have been, but aren't. Dick [Richard S.] Stein, for example, has had far more contact with the Japanese than I, but he's not old enough yet; he's only sixty-five. [laughter] I couldn't go last year to accept it, but they've set up a place for me at their annual meeting this May, so I'm going over.

MORRIS: Do you have any new ideas in polymers that you want to discuss?

STOCKMAYER: New wrinkles on old ideas. I wouldn't say they're new. In thinking about these associating polymers in solution, I've come across a small riddle in the theory that I'm trying to iron out. But it's not worth talking about.

On the polysulfone side, for years the dielectric properties made us (and, independently, Allen Fawcett in Belfast), think there must be helices in them, but since they don't crystallize and the optically active ones aren't optically active enough, we haven't been able to demonstrate in any direct way that there are helices. However, I have a colleague over in biological sciences, George Ruben, who's a very skilled transmission electron microscopist. He's got some very new techniques and can see down to very small dimensions of the order of ten Angstroms. We now see the helices where they ought to be. I've got a paper coming at the ACS meeting in April. George will give the paper; I won't. That's a ten-year search that's culminated in apparent success. Of course, it again may turn out that it's wrong (79). [laughter]

I'm still trying to write up old stuff, but I'm <u>awfully</u> slow at that. I've never been able to dash off something the moment it should be done. There are periods when all of a sudden you can dash off anything, and it goes on and on successfully, but then in between there are these long dry spells. I just so admire people who can discipline themselves to write promptly and regularly. John Ferry and Paul Flory are two examples. John Ferry is the outstanding one; he always wrote up everything promptly and on time. I can't do it.

MORRIS: One other thing I wanted to deal with in detail was your consulting work for Du Pont, because we touched upon that in the last interview. You basically indicated that you more or less fell into working with the Du Pont plant at Arlington in New Jersey. Maybe we could try and deal with this more completely here. How exactly did you get involved with the Du Pont people?

STOCKMAYER: That's in the other interview. They even asked me if I'd like to be interviewed for a full time job. I did listen but decided, thinking also of my previous small experience working summers in an industrial lab, that I probably didn't have the right temperament for that. But I was bold enough to say, "If you want an occasional consultation, maybe I can help that way." They bought it, and they said, "Come twelve days a year. Come to Arlington. Billmeyer is coming down; he was a student of Debye; you and he can certainly talk to each other," which turned out to be eminently so. So I got started on it. When the old Plastics Department joined with the Ammonia Department and became Polychemicals and the research part of it was moved down to Wilmington, I started coming to Wilmington. And it's gone on and on.

MORRIS: What kind of problems have you dealt with at Du Pont over the years? Is there a pattern of things that they tend to come up with?

STOCKMAYER: To the extent that the methods of polymer characterization have wanted to be talked about, my own experimental experience, such as it was, plus knowledge of the theory, I could give them some useful hints now and then. For example, back to our funny polyoxymethylene. When Delrin was made, and they found some solvents for it—hydrated hexafluoroacetone, for example—they gave me some samples. I had a master's student, before our doctoral program, that did light scattering on those solutions. The correct data on polyoxymethylene that was finally obtained in our laboratory (80) came only because I knew the Du Pont people; they told me that there was such a solvent before that was published. (Before we completed our work, it was published.) They gave me the samples of various chain lengths; we could never have made that polymer in our kind of rudimentary laboratory.

So I would say there was a two-way street, and it was very helpful academically. Of course, you enjoy making contacts for good students. And there are other people looking for jobs and often at Du Pont, they might be useful.

MORRIS: Have there been Du Pont chemists that you've been particularly associated with over the years?

STOCKMAYER: Billmeyer and Beasley, for example, with whom I've published papers (16). John Beasley, who's now retired, was an MIT Ph.D., so I knew him already from grad school. In fact, I used to say the reason that Polychem and then Plastics kept me on as a consultant so long was because I had convinced Beasley that's where he ought to go to work. He was an outstanding man. Best graduate student of his era at MIT, and he wanted to work in industry. He'd been a chem engineer by background and apparently worked at Tennessee Eastman at the end of the war. He knew he wanted industrial research and interviewed at Du Pont in several departments, but not Polychem. I said, "John, I think that's the place for you. Why don't you ask them if you can interview there too?" So he did and he liked it and he got the job. I claim credit for selling Du Pont and Beasley to each other. He did so well for them that I used to joke and say that's why they've kept me as a consultant. [laughter]

MORRIS: Anyone else?

STOCKMAYER: Among the others over the years, there is Howard Starkweather, who's still working. He's been there ever since the late 1940s. Among the people I didn't do science with but knew as friends and were more involved with management was my old Oxford roommate, William A. Franta. Ed [Edward S.] Bloom was for many years the sort of personnel director there, who is retired now. Also, Ed [Edward B.] Cooper, a physicist, long retired. It's been varied, and most of them are not long, continuous things.

I must say there's another problem, a mathematical problem that I solved thanks to the interest of two people at the Philadelphia laboratory of the Fabrics and Finishes Department, where I would go once in a while to consult. They were working on emulsion polymerization and the people were Seymour Hochberg and Werner Zimmt. They wanted to talk about the kinetics as a function of the particle size and all that business. I read up on that a bit, and there was this paper by [Wendell V.] Smith and [Roswell H.] Ewart, which was sort of a fundamental paper on the problem (81). They had a mathematical relationship that they hadn't solved except for the limiting case. I remember riding the train from Philadelphia to New York and on up to Boston, and on that train I got the idea to solve that problem.

MORRIS: Drawing on your experience with Du Pont which now covers some forty-five or forty-six years, what are your thoughts on the nature of academic and industrial collaboration? Is it profitable for both sides?

STOCKMAYER: I'd like to change the question a little bit. What I've observed mostly, and I guess it's obvious to lots of people, is that the range of problems that an industrial research organization chooses to contemplate seems to be very much a function of the marketplace, and even of the management in a particular laboratory.

At the Du Pont Experimental Station, in what used to be called departments (the name Department has disappeared and I don't know what's taken its place) you may find that in one building the people who are running the show didn't like mathematics, they didn't like theory. They just wanted people to get in there and polymerize more bottles of this and that or else make more test pieces and improve the tensile strength. It was really an empirical thing.

At the other extreme, at times it's a period when people thought, "The way to solve all our problems is by outdoing the academic people at being an academic." Some of them were very good and could do that. Others who tried to imitate it produced only second-rate academic kind of stuff. I've seen that there too. Where does the consultant fit in? Obviously, when they're on an academic kick, he's much more involved personally and able to talk to what's going on. Some of the other times, and in fact I could say recently, I've felt I haven't been so useful to them. When you are useful, you may say something in such a way that later they get an inkling of something that came out of the way they presented it to you.

It's clear that there are some people like Roger Adams and Speed Marvel who, to the predominantly synthetic chemical fraternity of the Du Pont outfit, have been of transcendent value. Jack Roberts, I fancy, has been very valuable in that way. I never could think of myself in that ballpark. They haven't fired me, and I call up now and then and say, "I haven't been down lately. Do you want me?" The answer now, and it's done by a person who is not herself a scientist who says, "I'll send around a notice, and if I get enough response we'll have you come down." I did this at fairly short notice for them when I got your message about doing this interview, and it worked out. So they do have me for a day tomorrow.

MORRIS: Let's talk a little bit about the mechanics of consulting. To you it's probably fairly obvious, but this sort of thing doesn't always get on the record.

STOCKMAYER: It's varied. Now and then I was told, "This is the area where we want you specially to be useful, so please keep abreast of things going on this area. We'll meet regularly whenever you come with a group that deals with these things." Whenever I've done that before, it didn't seem to last, perhaps because I wasn't successful at keeping my end of the bargain. But there have been other times when I've seen the same person year after year, and generally over in the area that's the scientists' area. The work there changes faster than my own, but sometimes I've got enough breadth so I can stay with it. Some people with whom I do seem to be able to exchange ideas have wanted to see me, but it's their choice whether they want to see me or not.

In my case, I would say that probably the majority has been short term shopping. People are supposed to know what I may know and say they would like to talk to me. Occasionally, people have come and expected me to know something, and I haven't had the slightest idea; well, they haven't come back. But other times, people have come from laboratories, or people have come from Virginia up to the station when I'm there or from Philadelphia, and I've been able to help them because they knew what I knew.

MORRIS: Do you talk to them much on the phone? It seems to me that consulting always seems to take place face to face. You don't get people from Du Pont calling you up in Dartmouth and asking if you help them on something.

STOCKMAYER: Once in a very great while something like that has happened, or somebody's even come up. But that's rare. No, it's mainly face to face. There was something only a couple of years ago, when I was asked in fact to look into a certain matter. Predictably most of those have had to do with some sort of a patent case. Explore a certain idea, is it feasible, is the opponent's or the rival's statement sensible? What can you say about this whole area? If I have to spend a few hours in the library, I'll produce a calculation and send it to them. I'll tell them how many hours and then they'll send me a check. I don't know what my hourly charge is; I don't recall. It goes *per diem*, so if I have half a day, I get half a day. That's about it.

MORRIS: Has Du Pont ever funded any research for you?

STOCKMAYER: No, they haven't. For years, certainly when I was most active, their policy was that they fund chemistry departments all over the United States, and they give no special consideration to people who happen to be their own consultants. That was their policy for a long time. I don't know whether that's changed. It may well have.

MORRIS: Has your relationship with Du Pont sort of varied over the years? Have you detected a greater enthusiasm for academic contacts at some times than others?

STOCKMAYER: Oh yes; it's a function of the marketplace, or in part maybe of the person who's running the show.

MORRIS: Have you ever consulted for other companies apart from Du Pont?

STOCKMAYER: Yes, with their knowledge, because they were first. For a few years (and you will be amused by this) there was the American Chicle Company, the company that makes chewing gum.

MORRIS: Robert Woodward was a consultant for a few years for American Chicle Company.

STOCKMAYER: Here's how it happened to me. One of the people who had been an instructor of organic chemistry at MIT when I was an undergraduate became their research director; Robert Heggie was his name. (He was also one of Woodward's instructors!) Around 1950 they were having trouble getting an acceptable polyvinylacetate from the manufacturers, who I think were mainly Union Carbide, but there may have been a couple of others. They wanted a low molecular weight PVA, which was part of the gum base. What was furnished to them was inevitably tainted with the flavor of acetaldehyde, and they couldn't get the people there to do a washing process afterwards; it wasn't a large enough volume of business as far as Carbide was concerned. So they decided to purchase a semi-works plant that they knew of on Long Island, hire a chemical engineer to run it, and make their own polyvinyl acetate. Which they did, but there wasn't anybody, including this chemical engineer, who'd been working on a quite different area, who knew anything about polymerization.

This was before Natta and Ziegler came along and made polymer chemistry too big for me. I knew enough about free- radical polymerization that Heggie said, "Could you help us out designing this plant, or at least telling us what variables to play with in running it?" So I did, and that's how I got to go down there. I was at MIT in those days and I lived out in the suburbs. I could get on a sleeping car in the suburb of Newtonville, not far from my home, and sleep overnight and wake up in New York in the morning and go out to Long Island City to the chewing gum plant.

MORRIS: Did that last for very long?

STOCKMAYER: Maybe five or six years. They did what they wanted, and then they kept me to talk about some other problems and things they were trying to develop.

MORRIS: What do you use polyvinyl acetate for in chewing gum?

STOCKMAYER: It's part of the gum. Originally the gum was based on the gum from a tree. But during World War II they couldn't apparently get all they wanted of that, and it just turned out that this polymeric stuff was a quite acceptable consistency.

MORRIS: I read somewhere, although it seems very improbable to me, that modern chewing gum is a styrene-butadiene copolymer.

STOCKMAYER: A certain amount of it's in there too. Yes, some of the standard styrene-butadiene stuff. [laughter]

MORRIS: Are there any other companies you want to mention?

STOCKMAYER: General Electric Company. I spent a summer there working with Bruno Zimm; that was in 1951. That work was later published in a paper with Fixman (47). I mentioned that in the previous interview. They had once before asked me to join the research lab, and I said no. This time they asked me again if I might like to come there because my friend Zimm was there. I came and spent a summer, and liked the atmosphere quite well enough. But there didn't seem to be any obvious reason to leave MIT, which I liked very well then, and I had my superb bunch of students at that time and a home, and my kids were growing up. There was no reason to move. Now and then I would be asked to go over and consult a day with them, so in the later 1950s I did that a few times. It was essentially a minor activity. It didn't conflict in any way with what I was doing at Du Pont. That's about all.

MORRIS: We're coming toward the end of the interview now. I thought that you might want to talk a little bit about Paul Flory because, of course, you wrote that very good account of Flory's work for *Science* when he got the Nobel Prize in 1974 (82), and you're working at the moment on the obituary for the National Academy of Sciences.

STOCKMAYER: Yes, a so-called Biographical Memoir.

MORRIS: Perhaps you'd like to talk a little bit about your relationship with Paul Flory.

STOCKMAYER: I'm only sorry that I didn't see more of him during our lives, because we never worked in tremendous proximity. I would see him at lots of meetings, and we corresponded about science on occasion. We didn't always agree, as I've already mentioned in connection with the theory of the gelling polymer after the gel point has passed. There was another thing in later years, when he didn't like the way Kurata and I were over-interpreting some viscosity data in certain systems. When Paul disagreed with somebody, he generally was fairly quick to say so or even to write so. Somehow, I think because he really enjoyed our friendship, he didn't do that to me. If I did something of which he disapproved, he simply ignored it rather than attacking it. That, of course, can be annoying too, if your work is not mentioned. But in this case, he was right and I was wrong. It was wrong stuff and he did well to perhaps not mention it. But he was always encouraging. He was not all that much older than I was, four years or so. But he definitely, I think, thought most of my work was good. Of course, he hired Fixman at Mellon Institute for a while after Fixman's instructor term at Harvard was over, and that was a wonderful two years for Fixman.

MORRIS: You got on with Flory very well personally?

STOCKMAYER: Always, yes. I once went to lecture at Goodyear, when Paul was at Goodyear. He lived way out in Kent then, which was some miles away. Kent State University is there, the one where the unfortunate incident occurred, where the students were shot. Paul lived in Kent, and I remember being taken out there to visit for the evening. Later, when he'd moved to Cornell from Goodyear, I went to lecture at Cornell, and I was actually a house-guest in the Flory home. The two girls and young Jack were still all schoolchildren. All were very cordial. We discovered then that Emily was an ardent Unitarian, and, since my wife Sylvia and I had joined a Unitarian congregation back in Massachusetts, we had a common interest in that.

We exchanged confidences about what we were going to do. I remember when he decided that he was going to leave Cornell and go to Mellon, he told me very quickly about that. I don't really have the full story of why he wanted to go away from Cornell. I do know he felt a positive obligation to go to Mellon. Maybe he's mentioned that in his own interview. He had told the people at Mellon what he thought they ought to be doing, because he was a member of their board. Then they said, "Well, you're the man to do it. Come on." He said, "I felt obliged; I had outlined this program for what they should do. I'd better go there and do it." Then they were unable to keep their end of the bargain financially and he decided to go back to academia.

The story of how he got to Stanford instead of back to Cornell is one that Johnson tells in the article that we're going to publish, but maybe it's already been told by Flory. Has it? I don't know.

MORRIS: I don't really think so. It's a bit of an untold story.

STOCKMAYER: It's a very funny one. Essentially, at a meeting of organic chemists, Johnson overheard one of Flory's Cornell organic chemical ex-colleagues say, "Well, we're about to re-hire Flory." Johnson said something like, "Has he agreed yet?" Johnson apparently immediately called the provost back at Stanford (or maybe he waited till he got home) and was quickly given permission to contact Flory and invite him out to Stanford before Flory went back to Cornell. So he did come out to Stanford and after several visits decided that's where he'd go. But it was all an accidental remark somewhere at the social hour of a meeting, or history might have been different in that respect.

[END OF TAPE, SIDE 4]

STOCKMAYER: I want to say one more thing about Flory if there's time. It's just that I thought what was remarkable was the way in the relatively later stages of his career, he suddenly embraced and really expanded upon the matrix algebra method of handling the partition functions of a chain in the rotational isomeric state model. Flory's mathematical training was probably less than mine. He had marvelous physical insight, but in terms of the perhaps meaningless phrase, "mathematical elegance," he had not particularly distinguished himself that way. It was the combination of not too obscure mathematics for the reader with marvelous physical insight that characterized all his great works. When he got onto this conformational business, and he clearly saw the computer coming too, these methods were the way to go. He not only taught himself how to at least read what [M.V.] Volkenstein and several other people were doing at that time, but then really building and improving upon them and becoming the master of that thing. And he did this at a relatively late stage in his life. He was certainly past fifty, and it was a completely new area for him. That's remarkable. I just wanted to mention that.

MORRIS: His early death must have been quite a shock. I was in Akron at the time the news came out, and it was quite a surprise to hear that he had died.

STOCKMAYER: Oh, yes. It was hard to believe.

MORRIS: There was no sort of indication that he had a medical problem.

STOCKMAYER: There was no indication. He was still hiking in the hills near his Big Sur place, now and then. He seemed to be in good health. He was still swimming. As you know, he was a very good swimmer.

MORRIS: For the last topic on the tape, let's move on briefly to your family. You mentioned in the last transcript that you got married in 1938, but that's all we've heard about your family so far. You might like to say a little bit more.

STOCKMAYER: I met my wife Sylvia through a colleague in the chemistry department. I was a grad student and he was a young postdoc, Clark Stephenson. He introduced me to Sylvia, who knew some MIT people. She was working in Boston as a fashion model for Jordan Marsh Company, one of the big stores in Boston. We enjoyed each other's company quickly, so after a relatively few months we were married. She's still my wife, and it's now fifty-three and a half years. Sylvia has pursued many interests, especially in public affairs and environmental issues. In 1969-1971, she served as State President of the League of Women Voters of Vermont.

Our children didn't come along for a few years, until after five years and seven years. We have two sons, neither of whom became a scientist. The older boy [Ralph] is very much more practical than I. After a hitch in the army, which interrupted his college years, he got a degree at Syracuse University and then worked for the Mobil Oil Company. He is now the distribution manager for the Freihofer Baking Co., in Albany, which now is owned by General Foods, which in turn is owned by Philip Morris. He's been a successful businessman. He has three children, one of whom, his second daughter, is a junior at Yale and is doing a major in mathematics with an economics minor. I'm proud of her among the others, but I'm proud of them all.

My younger son, Hugh, is a bookseller in Boston—old books. He works in the Brattle

Book Shop, of which he is the manager, along with the owner. He never finished college, but he never forgot anything he ever read. He was more or less a rebel, and I think he felt a tremendous burden to perhaps do something that his father would like. I never demanded that either of these people should be scientists or anything like that. Hugh had a quick understanding of mathematical concepts. I saw this from his school work, when we used to talk about it. I would encourage him to beat hell, to go on with this. Well, he understood, but he wasn't sufficiently interested. He had a tremendous combination of what I'll call pessimism and laziness for a while that reinforced each other. So it took him many years to find what he wanted to do. But he's very happy in it now. There are five grandchildren; four women and one male. The boy is about to enter college; he's the third child of my older boy. That's about it. The younger son is in Boston, and the other one is in Albany, so we see them reasonably frequently.

The daughter-in-law married to my older son works for a book publisher as an editor, and the other one is a research assistant in Tufts Medical School. She's a marine biologist by training but is working in a project on Alzheimer's now. So there's a slight science connection there. But I have no idea what the next generation will do.

MORRIS: It will be interesting to find out.

STOCKMAYER: I should also mention Dainton (now Lord Frederick Dainton), is a prominent chemist in another country with whom I've remained friends for a long time, without having ever collaborated, although I was very interested in his earlier polymer work, and he in mine, when he was a professor at Leeds.

MORRIS: What happened to him afterwards?

STOCKMAYER: He became Vice-Chancellor of the University of Nottingham. Then he went to London to be chairman of the Science Research Council, during which I guess he was knighted at some stage or other. Then he went back to Oxford as the Doctor Lee's Professor of Physical Chemistry, successor to Rex Richards. After a few years of that, he decided that scientific statesmanship was really more fun than running a research lab, so he resigned the chair and went back to London as Chairman of the University Grants Committee. He was succeeded by my friend John Rowlinson, with whom I've climbed in the Alps quite a lot. Rowlinson did a little polymer work when young, so our interests crossed there for a while, too.

MORRIS: John Rowlinson was my professor of physical chemistry.

STOCKMAYER: Have you gotten the recent blurb of the Fifty Years of the Laboratory?

MORRIS: No, I haven't.

STOCKMAYER: Heavens! What a terrible thing. Well, you should find it when you get home. It was written by Barrow [R.F.] and Danby [C.J.] They put together this history of the lab from 1941 to 1991 (83).

MORRIS: I'd be interested.

STOCKMAYER: Wolfenden is quite prominent at the beginning of it, too.

MORRIS: I know John Rowlinson quite well, because his son Paul did the history of science at the same time as I did. I got to know him also through the Alembic Club because when the Alembic Club had speakers, John Rowlinson always used to invite us to his office to have sherry with them.

STOCKMAYER: Lovely.

MORRIS: Thank you very much.

[END OF TAPE, SIDE 5]

[END OF INTERVIEW]

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