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LEO MANDELKERN

Transcript of an Interview
Conducted by

James J. Bohning

at

Florida State University

on

28 April 1986

Leo
Mandelkern
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LEO MANDELKERN

1922 Born in New York City on 23 February

Education

1942 A.B., chemistry, Cornell University
1949 Ph.D., chemistry, Cornell University

Professional Experience

1949-1952 Research Associate, Cornell University
1952-1962 Physical Chemist, National Bureau of Standards
Florida State University
1962- Professor of Chemistry
1970-1974 Associate Director, Institute of Molecular
Biophysics

Awards

1957 Medal Award for Meritorious Service, United States
Department of Commerce, National Bureau of
Standards
1958 Arthur S. Fleming Award, Washington DC Junior
Chamber of Commerce
1975 Witco Award in Polymer Chemistry, American Chemical
Society
1984 Florida Award, American Chemical Society, Lakeland,
Florida
1984 Mettler Award, North American Thermal Analysis
Society

ABSTRACT

In this interview Professor Leo Mandelkern begins with his early years in New York City and his undergraduate education at Cornell University. This is followed by his service as a meteorologist during World War II. In the central portion of the interview Mandelkern describes his graduate education at Cornell, including his association with J. G. Kirkwood, Franklin Long, and Paul Flory. Particular emphasis is given to his postdoctoral work with Flory, and collaborative work with Harold Scheraga. The details of Mandelkern's career at the National Bureau of Standards include Bureau operations and management in the 1950s. The interview continues with more recent work at Florida State, including students and postdocs, and concludes with comments on methods of solving scientific controversies, especially as it relates to his role in the problem of the folded chain.

INTERVIEWER

James J. Bohning holds the B.S., M.S., and Ph.D. degrees in chemistry. He has been a member of the chemistry faculty at Wilkes College since 1959, and served as department chairman for sixteen years. He has been associated with the development and management of the oral history program at the Beckman Center since 1985, and in 1986 was the Chair of the Division of the History of Chemistry of the American Chemical Society.

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INTERVIEWEE: Leo Mandelkern
INTERVIEWER: James J. Bohning
LOCATION: Florida State University, Tallahassee, Florida
DATE: 28 April 1986

BOHNING: Dr. Mandelkern, you were born on February 23, 1922 in New York City. Can you tell me something about your father and mother, their names and occupations?

MANDELKERN: My father's name was Israel Mandelkern and my mother's name was Gussie Krostich. They were immigrants. I'm a first generation American. They immigrated to this country in the early 1900s, about 1910.

BOHNING: What did your father do?

MANDELKERN: My father was in the jewelry business.

BOHNING: Where in New York City were you living?

MANDELKERN: They tell me I was born in the Bronx, but I recollect living only in Brooklyn. [laughter] That's where I was brought up. I went to public schools there, and to Abraham Lincoln High School in Brighton Beach. It was a famous public high school in those days.

BOHNING: Did you have any teachers there that had an influence on you?

MANDELKERN: In high school, yes. Unfortunately, I can't remember their names, but I can remember their faces. When I went to high school in those days (1934-1938), it was the Depression. There were a lot of people teaching in high schools with a Ph.D. in their discipline, but not in education. I remember three people who had a strong academic influence on me. They were my physics teacher, my chemistry teacher, and my history teacher. I'm sorry I can't remember their names, but it was a long time ago. They all were Ph.D.s. They all were basically professional historians, or chemists, or physicists, who wandered into high school teaching because it was pretty hard to make a living doing something else professionally. At that time high school teaching was actually a very lucrative, relatively high paying, profession.

I actually went to Cornell as an undergraduate history major, although I had been interested in science. Cornell was very strong in American history and I won a New York State Regents Scholarship to Cornell. I may be off by a factor of two, but I think it paid \$400 a year. That covered the tuition.

BOHNING: Did your interest in science first develop in high school?

MANDELKERN: Yes. It developed through these high school teachers and their labs and lectures. They were very good. We also had advanced math, up until the beginnings of calculus. In other words, in a large high school like that, there was a diverse set of courses and curricula that one could pursue, all the way from vocational training to something like calculus.

BOHNING: What was it like growing up in New York City?

MANDELKERN: It was delightful. Everything was safe. There was never any concern for safety or the problems that I understand the people in New York now have. As a youngster in high school, I had no qualms about hopping the subway and going to Manhattan to see a movie or a show. All you needed was a nickel. There were all kinds of things to do, and I had friends all over the city. Brooklyn was very pleasant. Commuting on the subway was a very natural part of life. There were no problems.

BOHNING: When did you make the decision to go to college?

MANDELKERN: I don't remember consciously making that decision. I think that anybody who did well in high school could always go to the City colleges. Originally I had planned to go to Brooklyn College, but when the scholarship came through, I switched to Cornell. So, I can't remember making a conscious decision to go to college. It must have been made when I enrolled in the academic program in high school. You always had a choice. I think that anybody who was a halfway decent student pretty much aimed to go to college because the city schools, which then were City College, Hunter College, and Brooklyn College, were really considered to be top-notch undergraduate schools. If I recall correctly, Queens College was in its infancy or even on the drawing board.

BOHNING: Was the Regents Scholarship specifically designated for Cornell?

MANDELKERN: There were two scholarships. One covered as long as you went to any college in New York State. That was about \$100 a year. Then there was a specific one for Cornell. In high school, you took Regents exams in basic subjects at the end of your senior year. Those who were interested in Cornell would indicate that, but anybody could qualify for the Regents Scholarship that covered any college. That scholarship was based solely on your scores in certain selected areas like English or math. It was a very anonymous sort of thing that just used a mathematical formula.

BOHNING: You said you had three great teachers in physics, chemistry, and history. Why did you select history as the major area that you were going to concentrate in?

MANDELKERN: I don't know. It's hard to say now. I was sixteen years old and I just got very excited about all one could do with history. I was particularly interested in American history, and what one could learn from what had happened. This teacher had me reading all sorts of books. I remember reading Charles Beard (1). I felt that what you learned in American history could be applied to what was going on in the world at that time. It was a very tumultuous world at that time.

BOHNING: Had you given any thought to a career choice?

MANDELKERN: Not really.

BOHNING: When you got to Cornell in late 1938, how long did you remain a history major?

MANDELKERN: One semester. I went to Cornell for history because I was primarily interested in American history. But, I had to take other courses, including chemistry, physics, and beginning math. Chemistry, particularly freshman chemistry, wasn't very exciting. In fact, I had in the back of my mind what I had learned in high school.

In history they took a group of unsuspecting freshmen who indicated they wanted to be history majors and put us in a class of Ancient History. If you wanted to be a history major you had to start with Ancient History. That class was made up of a dozen freshmen, half a dozen seniors and half a dozen graduate students in history. I'm sure it was the greatest course in the world in ancient history, but hardly one for a sixteen and a half year old to try. This wiped me out, so to speak, as far as being a history major. It just turned me off completely.

In chemistry, I had a very nice lab instructor whose name I don't remember. So, I had this interest and decided to see what would happen. I switched over in the middle of the second of semester, before the first year was over, although I had made up my mind after the first semester. I passed that Ancient History course by the skin of my teeth. I figured I wasn't going to get very far in history if that was all I could do. So I decided to switch to chemistry and give it a try. Cornell was very good in chemistry in those days, and it turned out to be a very good experience.

BOHNING: Do you recall who you had for that first chemistry course?

MANDELKERN: Jacob Papish. At the time, they told me, he was a very famous inorganic chemist.

BOHNING: Let's talk about your four years at Cornell. What else did you take in terms of chemistry? What faculty did you have?

MANDELKERN: As I said, Cornell was a very good place for an undergraduate. They had two tracks in those days, although I don't know what they have now. One track was for those who were going to be chemistry majors and would go on to graduate school and become some sort of professional chemist. The other track was for those who were chemistry majors for other reasons-- premed, biology, etc. Unfortunately, I had started late so I didn't get on the track for the first year when they had a very good introductory course.

Simon Bauer was an instructor in the qualitative analysis course. They don't teach that anymore, but it was a course where you really learned chemical equilibria. That was a five hour course with labs. That meant you went to three lectures and three three hour labs a week or something of that sort. It was really a very difficult regime. That really made me a chemistry major, because I was very interested in the whole concept of equilibrium, going to lab, and doing the unknowns.

The next course was quantitative analysis, which wasn't very exciting. And the next year, to catch up, I took both of what they called the long organic course and the long physical chemistry course. That meant three hours of lecture and three labs a week in each course. I guess the reason I could do that at Cornell was that they had lab on Saturday mornings. In organic, the lecturer was Jack Johnson, a very famous organic chemist. The lab was run by a young instructor named W. T. Miller, who then stayed on and became a professor and a fairly famous fluorine chemist. The physical chemistry lecture was taught by Professor Lynn Hoard. He was very quiet in demeanor and wasn't a very exciting lecturer in terms of his speech. But

his lectures and the way he introduced me to physical chemistry made me decide to be a physical chemist. We used McDougall's Physical Chemistry (2). In those days there weren't very many textbooks. It was a very logically written book and the thermodynamics was very interesting.

Having struggled through all of that in my junior year, and having taken the math and the physics that I needed, I was able to take graduate level courses in my senior year. That was really nice. I took J. G. Kirkwood's thermodynamics, which was a whole year course. I was still feeling my way around so I took an advanced inorganic course and an advanced organic course. I took Frank Long's kinetics course, and that indirectly introduced me to polymers as an undergraduate.

There were two things that happened in my senior year. One was Kirkwood's thermodynamics course which became the basis for the Kirkwood and Oppenheimer book (3). That was really a tremendous course. The only thing that was bad about it was the second term. Pearl Harbor occurred in December of that senior year. I don't know the details, but Kirkwood was in and out, and graduate students taught the second term. But the first term was the important term. Kirkwood was not great in terms of dramatics, and he was not a very good lecturer. But he was extraordinarily well organized.

Kinetics was a very organized course. We had to write a term paper. I was fishing around for a topic. He had given some lectures on free radicals and I talked to Frank Long who said, "They make something called polymers by free radicals. There are some books in the library. Why don't you get hold of them and write a term paper." There was a book by Mark and Raff on polymerization which I got and read vociferously (4). I wrote a term paper, but don't ask me what I got on it. That was my first introduction to polymers. It was a very backwards introduction.

BOHNING: Did you do any research as an undergraduate?

MANDELKERN: Yes. As an undergraduate they had senior research which I did with J. G. Kirkwood. He eventually turned out to be my undergraduate advisor. As strange as it may seem, he was doing some experimental work at that time. He turned me over to one of his more advanced graduate students. There were two of us assigned to the same lab. The other person was a fellow by the name of Chris Sporck. I have no idea what happened to him. He was assigned to Gerry Oster, and I was assigned to Fitzhugh Boggs. Boggs eventually married Elizabeth Monroe, who was one of Kirkwood's graduate students. Both Boggs and Oster were very nice to me. I was kind of green between the ears.

I did experimental work trying to verify the Kirkwood-Westheimer theory--essentially the second ionization of a dibasic acid. I remember it was terephthalic acid. We had this great big Wheatstone bridge and they bought the chemicals that we needed. I made the measurements. I don't think it ever got published but I do think Boggs used what we found in further developing his thesis. My senior year was a very exciting time except for the fact that the war broke out. It was a very exciting intellectual time as far as chemistry and science were concerned.

BOHNING: You were halfway through your senior year when the war broke out.

MANDELKERN: Pearl Harbor was December 7, 1941 which was right in the middle of my senior year.

BOHNING: What effect did that have at Cornell?

MANDELKERN: It didn't have a tremendous effect on me that year except for the fact that instead of graduating in June we graduated in late April or early May. It didn't seem to have a tremendous effect at the moment on the student body, at least not in my close circle of friends. It clearly had a lot of effects on the faculty. In my naivete I was not aware of that because they were in and out and rarely in residence. When the second semester started, you'd never know who would be giving the next class. For example, in the thermodynamics class Elizabeth Monroe and Fitzhugh Boggs taught most of the lectures, if I remember correctly. Once in a while Kirkwood would pop in. In retrospect, they were obviously getting involved in a great deal of war activity. But, for me that particular period through graduation didn't seem to be tremendously effected by the war.

BOHNING: Were there any other student colleagues of yours that you remember from those days?

MANDELKERN: There were really two. One was Chris Sporck. I know he went on and got his Ph.D. with Kirkwood. He stayed on at Cornell during the war, and then went on to Harvard Medical School for a postdoc. I lost track of him there. Another was a chemistry major, Richard Work, who was a very good friend of mine. His father was a professor of Vegetable Crops in the Agricultural School at Cornell. After a chemistry major, he switched to physics for his Ph.D. and went to academia. He recently passed away. He was a professor in the physics department of Arizona State and coincidentally, he was involved in some physical aspects of polymers.

BOHNING: What were you contemplating as a career?

MANDELKERN: I was thinking about going to graduate school. My experience with Kirkwood indicated that this is what I wanted to do. There was some confusion about the war and it wasn't so easy to get into graduate school. Although I had respectable grades, I wasn't a Phi Beta Kappa. Part of the problem was this catching up and this big jam up that I had in my courses. I do not regret that because I think I learned a lot. That was my goal, and in between I got caught in the draft. I spent four years in the armed forces working my way up to a first lieutenant.

BOHNING: When were you drafted?

MANDELKERN: September or October of 1942.

BOHNING: What did you do in the interim? Did you know it was coming?

MANDELKERN: I was pretty sure it was coming so I didn't do very much.

BOHNING: Where were you first stationed?

MANDELKERN: As I recognized that this was happening I tried to get into some program. The meteorology program looked appealing to me. I made applications for this but while these were pending, I was drafted. We were living in New York City and I got shipped to what was then called Camp Upton. It has a certain amount of history because in World War I Irving Berlin wrote some of his famous tunes about that camp. It later became Brookhaven National Laboratory. (In fact, many years later I was at a symposium there and since I was not an invited speaker, our quarters were in the barracks.) [laughter]

I hung around there for three, four, or five days and did nothing. Every morning they had a list and you'd line up. One morning I lined up and my name was on the list. They stuck us on a train and we wound up in Miami Beach. That's where I did my basic training. In fact, I did my basic training there at least three times in succession because I had the meteorology application pending. Somehow the paperwork got caught up and they weren't allowed to move me until something happened with my application. Incidentally, it wasn't very hard duty to take.

One day they called me and said that I was going to the University of North Carolina to their meteorology program. I said, "They don't have a meteorology program at Chapel Hill." They said, "Well, here's your orders. Go ahead." So I went. When I got there, I was the only one. Yes, they were setting up a program but this was going to be a premeteorology program. But nobody from the Army had arrived yet. So for about a week they put me up in the Carolina Inn. Then the program developed and I stayed in the dorm there. This was a premeteorology program which was sort of dull. It was the usual Army type of snafu. Most of the people had all the calculus and physics and types of courses that they wanted us to have.

Eventually we were transferred to the regular meteorology program. I was assigned to the University of Chicago. I spent six months there going through a rather rigorous academic program and the physical preparation to become a meteorologist.

BOHNING: Did you ever have a chance to visit the chemistry department while you were at both of those institutions?

MANDELKERN: A little bit at North Carolina. We didn't have much time to do anything at Chicago. Actually, it turned out that somebody I knew from way back was a graduate student in physics at North Carolina. So I spent some time in physics, but I also spent a little time in chemistry. At least I knew there were people around and what kinds of things they were doing. In Chicago there really wasn't too much time. They had us going from six in the morning until about eight or nine at night, and we were kept sort of isolated.

BOHNING: Where did you go after Chicago?

MANDELKERN: I was assigned as a meteorologist in Texas, and then at Norfolk, Virginia. Then they sent me out to learn how to operate meteorological equipment. There was no separate Air Force. The meteorology equipment part of the Army was given to the Signal Corp. So I was sent to the Signal Corp labs in New Jersey for a short five or six week course where we learned a little about how to handle the equipment. Then I was sent to the South Pacific, just about the time of the Phillipine invasion. My job was to keep the meteorological equipment in that theatre more or less functioning. That was not a trivial job because we had many problems with the high humidity and we didn't have the technology to keeps things dry that we have now. I spent the rest of the war doing that.

BOHNING: Was that in the Phillipines?

MANDELKERN: I reported to the Phillipines. It turned out the headquarters were in Australia. I did a lot of travelling all through there and then we all moved up to the Phillipines. I was overseas for roughly eighteen months. I spent about six months in Australia and the rest in the Phillipines.

BOHNING: When were you discharged?

MANDELKERN: Probably at the end of April, 1946.

BOHNING: Had you been giving thought to returning to graduate school?

MANDELKERN: Oh yes. I had been writing to Kirkwood and other people and there was no question in my mind that I would return. I started back in the summer session of 1946.

BOHNING: Did you do your Ph.D. thesis with Kirkwood or Long?

MANDELKERN: I did it with Long. Kirkwood was sort of a nice, absent-minded guy. I had been corresponding with him on and off. He was a very nice gentleman. I told him that I wanted to come on my own. I had just gotten married and there was the GI Bill of Rights. I said that I didn't want to be a teaching assistant. I had spent four years in the Army and I didn't want to waste anymore time. I wanted to get going with my program and I would be able to finance myself between the GI Bill and my wife working.

When I got to Cornell in early June he was very nice and very apologetic. He told me that things had changed. He had not realized that the Cornell faculty had passed a rule that they weren't taking any postwar graduate students unless they were some type of assistant. It could be either a teaching assistant or a research assistant. A research assistant was a category that was completely foreign to me because when I left in 1942 there was no such thing. Then, you were either a teaching assistant or you were paying your own way, and there were a fair number of people doing that. He explained the facts of life to me and said that I could sign up with him and be a teaching assistant. That was no problem, and he could take care of that. Or, his friend Frank Long, whom I had known from my undergraduate days, had a grant and was looking for people. I didn't know what he was talking about because the concept was foreign to me. Kirkwood talked with me and said I could give it a try. If I didn't like it after a year, he would guarantee me a research assistantship with him for the following June.

Well, I didn't have much choice. I decided to take a chance with Long because I wasn't going to get much done the first year anyway, and during that time I could take all of my courses. It turned out that the final decision was not mine. It was Kirkwood's because at the end of the year he went to Caltech. For a lot of personal reasons I wasn't in the position to go out with him, so I did my Ph.D. with Long. There I did get a more detailed introduction to polymers.

BOHNING: What kind of graduate courses did you take?

MANDELKERN: Cornell had a tradition at that time that you have a major and two minors. Physical chemistry majors traditionally, almost without exception, took physics and math minors. So my graduate courses in chemistry for the first year consisted of statistical mechanics and quantum mechanics. The rest of my coursework was advanced physics and advanced math. I don't know what they do now but that was pretty much a standard thing. Physics through at least the physics quantum mechanics and math through what they called advanced calculus. That was really a course on mathematical methods for physics and chemistry. It was very highly advanced. And so, you worked yourself up to those courses.

BOHNING: Who were the instructors in these courses?

MANDELKERN: In chemistry, Kirkwood taught the statistical mechanics. That was the first term I came back. During the second term he took a leave. When he came back he said, "Anybody who wants to come with me..." He was very gentlemanly. I was included. He came to me and said, "I'm leaving, but if you want to come to Caltech, I have a place for you." I thought it over, but there were a lot of reasons I didn't go. He did the statistical mechanics, and some young fellow did the quantum mechanics during the second term. I think his name was John Bragg but he left and went to GE. In physics it was Richard Feynman, who was at Cornell at the time and taught the electrodynamics course. [Hans] Bethe taught the mechanics course.

In math, the advanced calculus course was taught by Mark Kac. These were great courses and great teachers. They certainly gave me a good base.

[END OF TAPE, SIDE 1]

BOHNING: When did you first meet Paul Flory?

MANDELKERN: I think I have to go back to my thesis. My thesis with Frank Long was on the sorption of vapors through polymer films (5). His interest in this area came about through some war work, and that's where the support was. Although the prime emphasis was on the kinetics of the process, I wound up with an equilibrium absorption isotherm and consequently I could calculate activity coefficients. The films we were using were cellulose acetate films. There was a new theory then called the Flory-Huggins theory. It was in the latter part of 1946 and 1947 that we started to get some data, but it wasn't fitting the theory at all.

Then I noticed that Flory was giving three papers at an ACS meeting in New York around that time. I listened to the papers and I tried to talk to him. This was a problem that always bothered me. I guess I was a rather precocious young lad and I figured, "Well, this theory of his is all wrong. He and I are going to have a little talk about it." But he was always surrounded by a large number of people and I couldn't get to him. I was precocious but also polite in a way. So I saw him for the first time in New York but never really got to meet him. I met him for the first time when he came to Cornell to give the Baker lectures. That was in the spring term of 1948.

BOHNING: We'll come back to Paul Flory, but now let me ask if there were there any other graduate student graduate that you remember.

MANDELKERN: There were a few. I'm sure I'm going to forget some. There was a fellow named Bill McDevitt who actually started with Kirkwood and switched to Frank Long because he couldn't go to California either. He finished maybe a month or two after I did. He went to work for Du Pont. I think he spent all of his days there and he recently retired. Seymour Geller was a fellow working with Lynn Hoard who did x-ray crystallography. As I said, Hoard taught undergraduate physical chemistry and was a very eminent crystallographer, particularly of boron compounds. Seymour went to Bell Labs and a few other places and wound up at the University of Colorado. I think he is still there. Then there was an organic friend of mine, Lou Verstandig, who worked with Johnson. He went to work for Chevron and then came east to work with a small company.

In Long's group there was Paul Drexel, who went to work for Hercules. There was a fellow Ken Coffin, who was from New England and worked with Simon Bauer. He married one of Frank Long's graduate students. Her last name was Dunkel, but I can't remember her first name. They went to work at the NASA lab in Cleveland. Those are the people I remember out of that time frame.

BOHNING: You received your Ph.D. in 1949, but you stayed at Cornell. Had you given thought to going elsewhere?

MANDELKERN: In my last year as a graduate student, I decided that I had to go out and get an industrial job and make money. Frank Long was very much opposed to that. We talked about this around the second or third year that I was with him. He said, "You ought to go and do a postdoc somewhere." He wanted to send me to Yale to work with Raymond Fuoss. I kept saying, "No. I don't want to."

Well, strange as it may seem, two things happened in 1949. First, I really got interested in what Flory was doing. Again, this whole idea of postdocs was a new concept. In 1949, I had two job offers, one of which I remember. It was in Tonawanda, New York working on inorganic phosphorous. I can't remember the name of the company. I had another offer of the same kind and I just couldn't see it. I wasn't interested. The last thing I was interested in was inorganic chemistry. A lot of people are and there's nothing wrong with it, but I wasn't interested in it.

Then, Paul Flory had come to Cornell during that academic year. Frank Long told me that he was looking for postdocs and he knew me from the Baker lectures. If I were interested Long thought that he could talk to Paul Flory. So my wife and I decided that maybe we better give this a shot. From a professional point of view that was probably the smartest decision that I ever made in my life. There were choice jobs that somebody might be interested in, but there didn't seem to be very much opportunity at least for me. 1948 and 1949 were very bad years in terms of companies hiring people.

As you know, sixteen to eighteen months ago my phone was ringing off the hook with companies wanting to hire people. Now my phone is ringing off the hook with people that are now looking for jobs. That's the way chemistry has always been.

So this is what I decided to do. I started in mid-June or July of 1949, and I stayed with Paul through the end of 1951.

BOHNING: Was this your introduction to polymer crystallinity?

MANDELKERN: I should go back and say that I finally got to talk to Flory during the Baker lectures. One of the lectures was on the Flory-Huggins theory. Then I could go down to his office, and I said, "Look at all this data we have." And he said, "The only reason it doesn't work is that cellulose acetate is crystalline." Of course I didn't know what he meant and some people say I'm still trying to find out. [laughter]

When I went to work as a postdoc, he had two basic supports. At that time I began to understand how things worked in academia. He had come with the old Rubber Reserve project. That's Speed Marvel, and you've read about this. Reconstruction Finance was still going, and Peter Debye had a big chunk of it. He had a grant from the Allegheny Ballistics Laboratory that made propellants for the government and was run by Hercules. They were a very enlightened management. They were interested in learning some of the basic properties and we did a lot of work for them on cellulose derivatives. We did one piece of work on nitrocellulose. That's how I got introduced into crystallinity. I did two things when I was at Cornell.

Flory published a very basic theoretical paper in crystallinity in 1949 (6). The substance of that paper was whether melting involved a first-order phase transition. Flory came to Cornell from Goodyear, and he had already some indications from work he had done at Goodyear on polyesters. It was my job not to say that it was, but to look more at melting points and the effects of diluents, the effects of copolymerization and things like that on melting points. We did a fair amount of work with cellulose derivatives, but we were able to branch into other polymers. In those days, there was no linear polyethylene. Branched polyethylene had just come along. They had it during World War II but its properties were just coming out in the literature during those days.

One assignment I had was to see experimentally whether it was or was not a phase transition. As he told me many times, you can set up a theory to go either way, depending on what you feed into it. He was pretty well convinced that this was the way it was, and it did turn out to be that way. So one of the things we accomplished from just a straight experimental point of view was to show that we did indeed have a bona fide classical, first order, phase transition (7).

There was something else that was of some interest to me as we started exploring crystallization kinetics. If Flory thought you had anything at all that was halfway sound, he would at least let you try it out for awhile. We were doing our melting point work dilatometrically, which is a pretty good classical way of doing it. In fact, we're going back to doing some of it that way even now. As we did the melting, it seemed interesting to me to see how these things crystallized. We would set up a melt and keep them at some temperature. We had a whole bunch of constant temperature baths.

Flory had a small group, and he believed that if his postdocs were any good, he got them assistants. I had a couple of assistants, but only one to start with. He didn't give them to you all at once. We had these baths and one time he assigned me one of his graduate students to be paid on the grant for the summer. That summer we worked out the melting process pretty well. It was getting to be routine. You had to decide what polymer, and make your diluent mixtures. It took a lot of time.

Once we had a good sample in the dilatometer, particularly the pure polymers, we just looked at the inverse process.

This was in 1950-1951 and we found this fantastically strong negative temperature coefficient. In other words, if we were close to the melting point, nothing happened. If we went down a few degrees, it would start to go, and at ten to fifteen degrees it would go very fast. I must say, it certainly puzzled me. I would say if he were here, he would agree, if he remembered, that it puzzled Paul Flory at the time. We were sitting on this. I did the kinetics with polyethylene oxide. He said, "Well, try it on some polyester." And I tried it on a polyamide that somebody had made and it did the same kind of thing.

About six months before I left Cornell, we became aware of [David] Turnbull's work on the nucleation of mercury droplets, and only then did we clearly understand what we had. We learned about Turnbull's work when he gave an ACS tour lecture at Cornell. In those days they used to be in the evening. Now you have to remember that in the usual physical chemistry curriculum, polymers, or even the classical concepts of nucleation, were never discussed. You have to look far in the back of a textbook, if you even find it. We went to this evening meeting, and there's this very nice lecture on droplets of mercury, the temperature coefficient, and what was happening. Flory came to me and said, "Let's get together with Turnbull tomorrow morning." [laughter]

It became abundantly clear what we had. We did a few more experiments and I did the analysis. I took the materials with me and wrote it up when I went to the National Bureau of Standards. It was an interesting example. I suspect we would have found out sooner or later what we were dealing with, but it may have been later, because neither Flory in his experience nor I in my limited experience had ever run across nucleation phenomena which has this very characteristic. So I think it would be fair to say that we were the first to quantitatively establish the important role that nucleation processes played in polymer crystallization way back in 1951. I think the two papers came out in 1954 or 1955 (8). I wrote the papers up very early when I went to the Bureau of Standards. That's how I got introduced to crystallization.

We also did some other things which I think were important and which I haven't done much of recently. There were a lot of things going on in the lab, and I did some work on solutions. At that time period, 1949-1950, Flory had been doing a lot of work with Tom Fox. Their major focus was on the relationship of intrinsic viscosity to molecular weight and the thermodynamic nature of the solvent, the intramolecular excluded volume effect, and the theta temperature. Things of that sort were evolving, and he and Fox had started to publish. The lab was already functioning when I joined it. I think they had started some of this work at Goodyear. Fox came with Flory from Goodyear. They were publishing and of course we had reprints of the papers.

Flory made sure everybody knew what was going on in the lab.

There was a nice set of four or five Fox/Flory papers from 1949 to 1950 in the Journal of the American Chemical Society (JACS) on the viscosity relations to molecular weight and the intramolecular excluded volume effect (9). Flory was teaching a polymer course one term. I asked him if I could sit in on the course and he said, "Don't waste your time." One day he came back from his lectures with a book under his arm and said, "You know Leo, I think that the sedimentation velocity might be treated the same way as Tom and I treated the intrinsic viscosity." (Fox was still there but he was involved in glass temperature work among other things.) So my first question was, "What's sedimentation velocity?" He said, "Well, why don't you read this." It was a chapter in a book by Svedburg (10).

I dug through the literature and it turned out that you could treat the sedimentation velocity the same way you did the intrinsic viscosity. Then we found out that you could combine the sedimentation velocity with the intrinsic viscosity and have essentially universal behavior. There were a set of constants then that were universal to all chain molecules.

There was a young instructor at Cornell, Harold Scheraga, who came in 1947. At that time they were having polymer seminars at night. There were ten to twelve professors and students interested in polymers. So maybe every other week we would have a seminar, and that is how I got to meet Harold. We got to learn about what was going on. He suggested to me that maybe we could apply these ideas to the hydrodynamic properties of proteins. So we did. We set out to look at this and it didn't take very long because it wasn't really that complicated. We were able to do that, and we worked out what is now known in the textbooks as the Scheraga-Mandelkern equation for the sedimentation viscosity relation.

During this work Harold called me up one Sunday morning about seven o'clock. He worked pretty hard. After we talked he said, "I think I see how we can do it now." He likes to work pretty hard. We asked Flory to join us as a coauthor because we had been talking to him on and off. The idea was between Harold and myself. Flory would just say, "Well, that sounds good. Keep going.", or something like that and just pat us on the back. He was the senior person and I guess ninety-nine people out of one hundred would have said, "Sure. Stick my name on it." But he said, "No. You did it all yourself." We had a lot of battles with the referees but we battled it all through. Harold and I and our families have all been friends since that time which is almost forty years ago.

BOHNING: It's number twelve on your publication list, "Consideration of the Hydrodynamic Properties of Proteins", published in 1953 (11).

MANDELKERN: The work was actually done while I was still at Cornell. There was a battle for about a year with the referee. I had to go back to Cornell with Harold. W. Albert Noyes was the editor of the JACS. Flory said at one time, "Do you want me to interfere?" We were two young kids. We said, "No. We're going to do this ourselves." Somewhere I have the correspondence. Anytime I get discouraged I just look at that because that was actually a good lesson. There were a lot of entrenched feelings and a lot of misconception.

Also, we were obviously very naive and very green in the paper. Maybe it was not as clear in the first three versions, until we got it right. But I would suspect that maybe Noyes was checking with Flory behind our backs. Noyes would say, "Come back and have another go with the referees." We finally figured out who the referees were and then we could respond, seeing where they were coming from. So it took at least a year before we got the referee situation settled. That's why it came out in 1953. We actually still did the work while I was at Cornell. That paper caused quite a stir in some circles but that was part of the work that we had done. It was sort of an open lab, so we just took these trends that were in the lab.

BOHNING: Was this part of the resistance to the whole concept of polymers that had developed even earlier than that?

MANDELKERN: No, I don't think this was a development of resistance to the concept of polymers. This was in part the referees not understanding. They actually had become pretty ardent. Also there was a whole school that had a very strong vested interest in a certain kind of idea of how you treat a solution of a protein. I think it was a combination of that and mainly that they didn't understand the polymer work that we were doing. That's understandable because it was very, very new. I think that once they understood it, there was no problem. Part of it is that we were green and we didn't write a very clear paper. I just remember Noyes being tolerant. He just let us come back and said, "As long as you want to keep making a stab at it and revising it, I'll be with it."

BOHNING: You actually spent three years as a postdoc. Wasn't that a long time?

MANDELKERN: No, it was two and a half years. I came in June or July of 1949 and left at the end of 1951. Maybe it was a long time in those days. I know most postdocs today are between two and three years. The problem there was that financially, Paul Flory treated us reasonably well. We didn't make any money, but we weren't starving. When I took my first job at the Bureau I went there for a little less money than I was making as a postdoc. Those days at Cornell were very exciting times. It was

very hard to leave. I realized that I had to leave and I'm very glad that I did leave. Some people stay on and on. If I have a good postdoc here, I'll tell them after six or seven months that unless something unusual turns up, two to three years is a good time to stay because you really get cranked up and you can develop your own reputation. You mature a lot and that keeps you on. It was very hard to leave.

I'm sure I could have talked Flory into letting me stay another year or two but I felt that the time had come. We weren't hurting in the sense that we were able to make ends meet. The pay was about four or five hundred dollars more than I made at my first job at the National Bureau of Standards. But he was fairly liberal and he gave you a lot of help help in the lab. He was very nice.

BOHNING: Did he come into the lab every day?

MANDELKERN: Maybe not every day, but he was very close. Or he would call you to come into the office. You felt as though you had no problems. If you knew he was around, and if you just stuck around long enough, he would talk to you. He always kept his groups small, at least in the days at Cornell when I was there. There was Bill Krigbaum, Tom Fox, and myself as the postdocs, Alan Schultz was a graduate student, and I think there was one other graduate student that has drifted away from polymer science. I've forgotten his name. So I think he never had more than four or five people, either students or postdocs, working with him in those days.

BOHNING: Did you socialize with Flory?

MANDELKERN: Yes. He used to have parties at his house. Cornell was a very congenial sort of place. We got to know his wife and she was very helpful in terms of helping us with our first child at the time. She was very pleasant. He had a boat, and he would invite me and other people from the lab to sail on it. We had picnics in the summertime. At Cornell it is pretty hard to do anything in the wintertime. I remember one Christmas, although we don't celebrate it, he knew that we were in Ithaca by ourselves and said, "Why don't you come over Christmas afternoon and we'll have something to drink." He showed us some movies and that was very nice. In that way he was very nice.

But he really wanted excellence in everything. You learned how to do an experiment because if you had something, even if it substantiated his theory, he would say, "Let's try it this way to see if it's right." He really wanted to be certain of everything. He was a real perfectionist. He sat on theories and experiments until we did it this way and that way and then got another fraction and extended the molecular weight range. That's

the way you have to do things and I think there are still a few people who do it that way. He was very demanding in that sense. Not demanding that he snapped a whip at you. I got misunderstood in an article in Chemical and Engineering News. He was demanding in the sense of doing everything right. He told me as I found out, "You work all you want. I can tell if you're working or not." [laughter] I tell that to my students here. You don't have to stand there and hold a whip over them.

So in a sense, he really wanted you to be excellent. In all the people who were closely associated with him, he really developed in us a passion for excellence. I don't know if we've achieved it, but we certainly got that instilled in us. Taking away what he did scientifically, I think that is his hallmark-- his passion for excellence.

BOHNING: How did the position at NBS come about? Did he give you the advice you give your students or did you leave on your own?

MANDELKERN: In terms of leaving, it was a mutual decision. Jobs were not too bad at that period. Working with Flory got me a lot of interviews and I visited a lot of places. I went to Du Pont and they had two jobs. One was in the newly developed polyethylene terephthalate fiber group that they had there. The second was in this place that they were going to run for the Atomic Energy Commission at Savannah River. I forgot which one they offered me but I was seriously considering it when Flory told me that L. A. Wood at the Bureau of Standards was looking for somebody. Wood was one of the early pioneers who did anything quantitative in polymer crystallization and polymer transitions. Flory said I ought to think about going there. I thought about it. The pay there was a lot less than that at Du Pont but then again he explained a lot of things to me and strongly urged me to go to the Bureau of Standards.

BOHNING: So he urged acceptance of that position.

MANDELKERN: Yes. He obviously recommended it very strongly.

BOHNING: You had a number of papers that you were writing after you left Cornell. Did you continue any work with him at the same time?

MANDELKERN: No. We essentially did not formally collaborate after that time. I may be wrong and I'd have to check the list. When he came back from his leave in England, he had published this very beautiful paper in JACS on contractility and dimensions that really got me excited (12).

I didn't understand it all and I talked to him about it. I think he may be a coauthor on that first paper with Roberts and myself on the dimensional changes in rubber as a consequence of cross-linking (13). That might be the only other time we actually formally collaborated. The idea was that if you stretched a polymer and cross-linked it when it was stretched, when you relaxed it it would still have some additional length in the amorphous state. It was a very intricate type of logic and was very difficult for me to follow then. I remember going up to Cornell and we just figured out how to do the experiment. I went back and did the experiment at the Bureau and it did turn out to be correct. We talked a lot and interacted a lot over the years on many things. That's my recollection. There are some things early on at the Bureau of Standards but they were left over from Cornell. We worked very hard during those days, and we did a lot of work.

BOHNING: Yes. There's quite a number of papers that came out during this period.

MANDELKERN: He also believed in having his postdocs work with each other. So I would work together with Bill Krigbaum or with Jack Kinsinger. We would both be authors but we each had made substantial contributions because the ideas naturally crossed each other.

[END OF TAPE, SIDE 2]

BOHNING: What was your first position at NBS when you went there in 1952?

MANDELKERN: In those days the NBS was organized into divisions. There was a heat and power division, an electricity and magnetism division, and there was an organic and fibrous material division. Each division was subdivided into sections. In 1952 all the sections were named essentially along commodity lines. There was a textile section and a leather section and a plastic section and a testing section and a rubber section. L. A. Wood was chief of the rubber section. Among these commodity sections the rubber section was somewhat unique because about half of its activity was basic research connected with polymers and rubber.

There was a long tradition there. The other senior person was Norman Bekkedahl, who's still living in Pompano Beach. L. A. Wood was a physicist and Norm Bekkedahl was a chemist. Both had Ph.D.s. Around 1932, there are some papers by Bekkedahl on calorimetry and specific heats of natural rubber. Then Wood came along and he got interested in transitions and melting. They were just pioneers. They didn't understand what a polymer was in

those days. But their work still stands up and is extraordinarily good data.

They actually did the first crystallization kinetics using natural rubber. They did some work on melting temperatures and the dependence of melting temperatures on crystallization temperatures. They were quite cognizant of the importance of this kind of work to what their main objectives were--being able to set standards for rubber tires and gaskets. That's what was happening at the other end of the funnel, so to speak. So the section was split roughly in half. This commodity influence prevailed everywhere. It was very frustrating in many ways because it was difficult to get across to some of the leaders (except for Wood and Bekkedahl) who were the older people that there's something called polymers. It's ten years old now and this is what you're dealing with.

On the other hand, for a young fellow, my first few years at the Bureau were great years because I was on my own. They put very little restraint on me. There were some administrative restraints because a lot of the rules of the government were a little hard to live with. But scientifically and intellectually, they put very little restraint on me for the first few years. That was my first assignment.

Then, to jump ahead in the organization, they finally recognized that there was something called polymers. So they set up a new section called the Polymer Structure Section which eventually Norman Bekkedahl got to head. He was the second head. This was probably around 1956 or 1958. He asked me to join him and it took a little bit of behind-the-scenes maneuvering so I could move without getting too many feathers ruffled. There was a different building for me to move into the Polymer Structure Section, and that essentially took me away from any of the commodity things. But I didn't feel tremendously stifled in the Rubber Section to start with. Although, if I had wandered too far away from transitions--crystallinity and glass temperatures--I think there would have been some mutterings.

So for the first five or six years, they let me do what I wanted. They promoted me in proper order and gave me some help in the lab. There were no restraints on publications. They encouraged it.

BOHNING: You also have a patent (14).

MANDELKERN: That was sort of a hobby. We were always getting visitors. One of the things I didn't like about the Bureau of Standards was the fact that even with or without appointments, somebody would call up and say John Jones from so and so is here and he's interested in crystalline polymers. Well, you worked for the government, he was a citizen, and you had to do that. Also, a lot of people liked to come to Washington so even with

appointments you had to be nice. People would come from all over the world which is all right in moderation, but when you had to do three or four visitors a week in the springtime, it got to be a little too much.

That patent was an interesting scientific thing but it was that somebody from industry came along and said, "You ought to patent that." I called up the Bureau's patent people and they did what they had to do. We can get to what's involved there which is part of why I left. It was one of the reasons but not the major reason.

The Bureau to start with was very good because nobody bothered me. I didn't have to worry about anything administrative as long as they gave reasonable leeway. They gave me two technicians.

BOHNING: What kind of areas did you pursue?

MANDELKERN: Primarily in the crystallization area and some work on glass temperatures. When I moved into the Polymer Structure Section on solution work there was some collaborative work. Several of the people were doing solution work and, without bragging, I felt that they needed a little guidance and so we got along pretty well. There was a paper on sedimentation equilibrium and sedimentation velocity (15). I was like a consultant. I contributed in terms of saying, "Well, you're making this effort. Why don't you do it this way and learn scientifically from it?"

But basically the heart of what I did was work on glass temperatures, but mostly on the thermodynamics and kinetics of crystallization. Those were the two main thing we did. We did a whole series of papers on thermodynamics of natural rubber, gutta-percha, and polyethylene (16). We just established some more of the principles and did a lot of work on kinetics, and worked on glasses.

BOHNING: Who were some of your coworkers?

MANDELKERN: In the Polymer Structure Section, there was Bekkedahl and Wood, who were very nice. They were actually people you could at least have a reasonable scientific discussion with. There was Bob Marvin. He may have just retired. He was a rheologist and a student of John Ferry. He moved over to some other part of the Bureau to set up a general rheology section somewhere along the line. Then there was Herb Lieberman who passed away at a rather early age while I was still at the Bureau. He was a rheologist doing things like creep. Again, this was all very pioneering work for that time. People weren't doing these things on a systematic basis. Anybody who made a

rubber knew there was such a thing as creep. But nobody was really trying to study it from a fundamental point of view.

Also in the Polymer Structure Section there was Sam Weissberg, who was doing solution work. Don MacIntyre joined us. He's now at the University of Akron. We had John D. Hoffman with us for a little while. He moved over to another part of the Bureau to set up his own group, and that is another story in itself. Leo Wall was another very important person who did organic synthesis and studied the mechanism of degradation. He died in a sailing accident a few years after I left the Bureau. He was one of the senior men. He wasn't old. He was about my age at that time, but he was one of the more productive scientists in the place. He was also in the Polymer Structure Section. We were in two parts. There was the physical chemical part and the organic synthesis degradation part. Leo made some major contributions to the degradation mechanisms of polymers and then got interested in fluoropolymers.

BOHNING: You said that Bekkedahl was the second head of the Polymer Structure Section. Who was the first?

MANDELKERN: The first head was a fellow by the name of Irl C. Schoonover. At one time he was head of the Dental Material Section. Schoonover moved on to be an associate director somewhere in the director's office. He was rather influential in running the Bureau. During the last four or five years that I was there he was probably the most influential person in running the Bureau from an operational point of view. The other people above him were pretty much dealing with Congress and the president.

When he moved upstairs, so to speak, Bekkedahl replaced him. Bekkedahl and he were very good friends. It was a horrible Bureau in terms of the people who were maybe ten or fifteen years older than I was. It was a very close-knit corporation. There were a bunch of people that had grown up together. They knew each other quite well. They lived together and drank together and played together. It was a pretty closed corporation.

BOHNING: Were there many new people like yourself coming in at that time?

MANDELKERN: There were some. They didn't have a big push that I understand they have had lately. The newcomers really weren't interested in what these people were interested in. We were interested in trying to do science and we got very frustrated sometimes in their directions. These people were more into the politics of science. Not Norm Bekkedahl, but all the other people in that age group. So the people that were coming were like myself. They were hiring a lot of physicists and they were

more interested in science.

BOHNING: You mentioned that the patent was one of the reasons you ultimately decided to leave.

MANDELKERN: Well it wasn't the patent itself. It was the subject matter of the patent. Actually there were a lot of reasons. One of them was that I was beginning to develop a strong interest in teaching. I began to realize around 1959 that I had been away from the university for almost ten years. You don't teach or read or go to many seminars. You go to meetings. But you didn't have a weekly seminar. So you're beginning to lose touch. You're beginning to make a right-hand thread better and better, and that's the way I used to express it. One time I wrote a paper and made a lot of stupid mistakes. I thought something was novel and it wasn't, really. It was almost elementary thermodynamics, and the referee pointed this out. It dawned on me that maybe I'd better start studying and refreshing thermodynamics and statistical mechanics. Well there's no way you can do that unless you go back and teach. That was one of the reasons.

The other, in terms of that patent, involved contractile fibers. We followed up the rubber work with highly oriented polyethylene. We found out that if you cross-link highly oriented polyethylene then you could make a fiber that changed dimensions reversibly just by melting it. The Bureau was nice to have all of these facilities for radiation and Leo Wall was helping me with that. This led to building the machine that was the patent. The point is, that led us into biological fibers and the whole subject of contractility. We did a lot of work in the Bureau on keratin and collagen. We could justify it a little bit because they still had the commodity sections. The Bureau did not have its reorganization in terms of what it is today until several years after I left.

So I was getting more interested in this. They hadn't really said anything. Bekkedahl was fine but I was getting concerned about the people above him. It was very difficult to be a GS 15. That's what Leo Wall and I were. We were the only two in the Bureau at that rank who didn't have administrative responsibility. Ugo Fano was made a consultant to the director, and his office was in the administrative building. My office and Leo's were right in the lab, like this one. There may have been others. I don't know. But he and I were the only two to have parking places and had lunch in the director's lunch because of our rank, with absolutely no administrative responsibilities.

But it was getting more difficult. They would say, "Why are you doing this and not directing a section?" Also the kind of work I was doing was also getting away from them. They were muttering, "Well you ought to be at NIH doing this." That wasn't the only thing we were doing but it was looked on then, and it

still is, as a very important kind of approach. We were basically using polymer principles to tackle problems in biological polymers, particularly macroscopic systems. Not the solution characterization.

There were essentially three reasons, and they all sort of came together. In the crystallization work, this whole problem of the folded chain started to rear its head. That essentially brought John Hoffman and myself into direct conflict. He was originally in the Polymer Structure Section and we got along pretty well. Then he moved over to head, with major help from the higher administration, what was then the Dielectric Section but really it was a device so that he could set up his own research program. This was in another division. Without going into a great deal of technical difficulty, this became a scientific issue which in itself could have been resolved. But it became basically a local political situation of rather serious import. I was feeling quite harassed in my isolated position.

In that area there was a complete misconception, either deliberate or accidental, on the misuse of nucleation theory which I felt very strongly about and had been using all of the time. Perhaps unfortunately, I let my opinions be known loud and clear. Some people weren't going to let their potential fame rest or be halted by something like that. So there was a great deal of harassment, internal problems and politics. The scientific problem became intermingled with some peoples political ambitions at the Bureau. The situation became intolerable.

I started looking around and this Institute in this University was just developing. Professor [Michael] Kasha invited me down and made me an offer which was financially about the same as I was making at the Bureau. But the rest of it looked very attractive. In 1962 things were looking very good, and I have no regrets. It was the second best move I made. Although, the Bureau was fine. If this harassment of the folded chains didn't come along, I probably could have handled the other things. I was beginning to make contacts with people at NIH, and we could have collaborated. Driving down Wisconsin Avenue wasn't that big a deal at the time.

In fact, in that period I did do a paper with Kollmer Laki on muscle (17). If I wanted, I could have gotten some teaching in the Washington area. But everything came together. If this science-political problem hadn't come I probably would still be at the Bureau. It became very serious but the scientific problem has finally been resolved. I had never been down south before, and I have no regrets. I enjoyed every minute of it-- professionally and personally. I've always told my students they should never leave a job for one reason. There has to be a set of reasons or circumstances that make you want to change.

Do you have any questions about this? That's the Bureau, period. I still have mixed feelings. I still see Wood. I still converse and have interactions with Norm Bekkedahl. He's probably in his eighties now, and retired in Pompano Beach. Wood was through Florida and was here about three years ago. Except for one trip back shortly after I left, I've never been to the Bureau and I haven't been invited there. I've been invited all over the world. I had one trip back after three or four months to clean up things. I was supposed to be back on a regular basis to clean up, and a lot of things never got cleaned up. That's the way life is.

BOHNING: Did you discuss the situation at the Bureau with Flory?

MANDELKERN: Oh yes. We discussed it on and off. We both moved at about the same time. He moved one year ahead of me and he was having major problems too. In fact, we could commiserate with each other in a certain sense. After all, I was on my own. I wouldn't see him everyday, but he knew there were problems there. He encouraged me and thought it was time for me to leave the Bureau if I felt as strongly as I did that I wanted to do academic work. He moved to Stanford about a year to a year and a half before I moved. I think he went in 1961. He knew I was coming down here before they even went to Stanford because I remember going out to see him in Pittsburgh before he left. He was talking to one of his daughters and he said I was going to be a professor at Florida State.

There was a half year interim before I came down. I just had too much going. It's not like you're at a university where you bring your graduate students with you and take your equipment. I just had too many things going so the deal we struck here was that I came down in January rather than in September. I think I accepted the position in April or May. It took me two trips down to convince me that I could move my family down to Tallahassee. At that time I had a lot of interactions. I had three or four technical people at the Bureau. They had a postdoctoral program in which every year somebody would come and work for me as a postdoc. I cleaned up just about everything. There were a few things that could have been finished up but eventually they got straightened out, and I started over again.

BOHNING: Was Florida State the only place you looked at?

MANDELKERN: I looked at several other places but this was the only one that made me an offer that I considered reasonable. This was a very exciting place then. It was young and had a little under ten thousand students. The faculty in chemistry was very good and I presume it's still pretty good. It had a pretty good reputation.

BOHNING: I wanted to ask you about the origin of the Institute of Molecular Biophysics here, and you said it was forming when you came.

MANDELKERN: It had been formed. The person who basically formed it and evolved it was Professor Michael Kasha, whose office is upstairs. He's a fairly eminent physical chemist and spectroscopist. His perspective is very similar to mine. He wandered into some area of singlet-triplet oxygen and found out how important this was. He found out that there were biology and biochemical people puttering around with things that he could help them with. I don't know all the details, but there were some good acts of coincidence that he was able to get an institutional grant from what was then the AEC [Atomic Energy Commission].

The theme originally was to be radiation chemistry and biology but that changed before I got here. He got money from the state to put this building up. This is a unique kind of situation. It was then and I still think it is now. This Institute is only a physical facility. It is not an academic enterprise in the sense that there is no professor of molecular biophysics. You're either a professor of chemistry or a professor of biology and you happen to be working in the Institute for Molecular Biophysics. There's not enough space here, and we have members who are working elsewhere.

The cornerstone was a rather substantial institutional grant from the AEC. If I recall correctly it was the order of about one million dollars in 1962. I was associate director for several years and ran the nuts and bolts of the place. We were working with a budget from the AEC then of \$700,000. The state put up the building with some matching funds. This was actually the first scientific academic building on this part of the campus. The only other thing were these dormitories that you saw over here. There's biology next door and chemistry came some years later. They were all on the other end of the campus.

We also give a degree in molecular biophysics at this University which started in about 1969. It is a joint degree awarded by the biology and chemistry departments and of course the Institute per se because of the fact that we are associated with the focus of that program. But again there are people with students in that program who are next door here. We profit by having interactions in theory and having central facilities. Some years ago we lost the central grant and everything is now running on its own.

BOHNING: So each of the faculty involved in the Institute would traditionally supply their own support.

MANDELKERN: We all were doing some then. But as a consequence of how we originated, we have a set of central facilities here. We have the shops, offices, people who take care of the bookkeeping and paperwork, so it takes a burden off of the department. It works both ways. If they took the five chemistry professors who are in this building and dumped them in to chemistry, that would cause a great deal of problems for chemistry and vice versa. There are about eight or nine people here. Four or five are from chemistry and four or five are from biology. It fluctuates slightly as people leave and somebody else moves in. When I first came here this building was just going up and I had temporary quarters in the old chemistry building. Kasha, [William] Rhodes and me are actually the original people who are still in this building. Rhodes was a young assistant professor and a theoretical quantum mechanician. There are still some people in chemistry who were here but they moved into chemistry when the new chemistry building was completed.

BOHNING: You were director of the Institute?

MANDELKERN: I was associate director for about four years, from 1970 to 1974. I took care of things. It was a time consuming job because you had to make decisions on how to distribute a fairly good sized chunk of money. You have a lot of interactions with the people of Washington and Gaithersburg. It took a lot out of me physically.

BOHNING: Did you appreciate the administrative aspects?

MANDELKERN: Not particularly. Here it was not that difficult. There was a pretty congenial crew here and we got along reasonably well. We got pretty straight answers from the people in the AEC. We embarked on a program of trying to reach a steady state with the AEC in financing, and then working up the rest. It wasn't too bad except that in any large state university when you get into something like that, maybe anyplace, you just get into everything. The actual obligation wasn't so bad. I could make a couple of trips a year to the AEC and work with the crew here on how we would distribute the money. We had administrative people and they were pretty good.

But it's the everyday thing of having to go to two or three meetings every week or every day. I used to have two secretaries. One was doing just my work. I would go home and stay up all night doing my work. You have to go to the science area committee. Then you would have to go to the dean for this and president for that. I don't mean social functions. Most of them were a waste of time. You would have to go to the building committee. If you didn't go, then someone would take half of

your building away. That part was what killed it and almost killed me. I got sick and was out for a year.

But the basics, what you really had to do, I really enjoyed because I had good people. We had sufficient flexibility. People were going to get hurt but nobody was going to get hurt in a meaningful way. I had a colleague who was on leave in Sweden. In those days, making a long distance call was quite an effort. It looked like we were going to get a big budget cut and I just called up Bob Fisher and said he had better be prepared when he came back, because he may not have all of the money he thought he was going to have. People could work with that and that part I was able to do all right. We had some people who had technicians and I told them, "You had better tell your technicians that in the next year or two they should find another job because it looks like we're not going to be able to keep the crew that we have." Nobody ever got really hurt. We had a congenial group of people to work with and we had competent people help us downstairs.

But this other stuff killed me. I just couldn't take it. I felt the responsibility very strongly and knew that if I didn't go to a meeting of the building committee somebody was going to take a lab away from us.

BOHNING: You've been called Mr. Crystalline Polymer. In 1964, you wrote a book on the crystallization of polymers (18). What led to the writing of that book?

MANDELKERN: I haven't thought about that in some time. There were several intellectual and scientific reasons. There was also the physical situation. The timing was right. At that time, with the work that Flory had done and the work that I had done with him and a lot of other people, one could begin to see the basics of the subject. And at the same time, there was a lot of new work on morphology. That had a lot of elements of merit in it, but it also was clouding the issue. I thought the perspective of the problem was being lost. There was very basic science that could be discussed and I felt that I was obviously close to the subject.

I had been working on it since 1949. I thought that the new work on morphology and structure on the lamellar crystallites, which were important, were incorrect. It turned out that most of them were. At that time a lot of ideas were being propounded without really any substantive thing. I thought the whole subject was getting out of perspective. Since I knew I was going to be moving sooner or later, I started the book in 1961 and came here with a first draft. The labs weren't ready when I came here so I had a little cubby hole in the library and finished it up.

[END OF TAPE, SIDE 3]

BOHNING: How was the book received?

MANDELKERN: Originally it wasn't received too well because of the climate. But I would say now everytime I go to a meeting people say, "When are you going to put out a new addition?" [laughter] So I feel vindicated in many ways. The only person who really gave it a good review was Maurice Huggins (19). He realized what I was trying to do, although he didn't agree with everything I said. To be perfectly honest, it was really lambasted by several people. It sold out eventually. (They don't make massive printings.) But it was not originally received well. Certainly it disappointed me. But I feel quite gratified because it was in essence correct. It's hopelessly out of date and I hope I last long enough to get around to work it over. It has to be worked over from stem to stern, so to speak. But I feel quite good about it right now because when people introduce me at a meeting, they introduce me as the author of this book. If you go back and look at some of the reviews that appeared in some of the journals--

BOHNING: Which ones were they?

MANDELKERN: Well, I don't want to say. Historians can look it up. All I want to say is that Huggins didn't agree with me completely, but at least he saw what I was trying to say and wrote a very objective review. There were reviews that were saying, "This guy doesn't know what he's talking about. He's back in the dark ages. He should recognize what everybody knows that chains are regularly folded. Obviously you can see them in the electron microscope." Well that was a lot of incorrect stuff. It's all clear now where the problems are. Unfortunately, I think we were subject for about a fifteen or twenty year period to what might be called propoganda if we were in Russia or advertising if we were in New York. It's one of those things which is very sad in science.

For a long time I was sort of sitting almost alone with perhaps only two or three other people in the world who were saying that this stuff just can't be right from experimental results. Flory had one theory, and and a more recent one sort of wiped out the regularly folded chain concept and Hoffman's nucleation theory. But the experiments just didn't add up to folded chains. It was very difficult to be heard for a long time. The propoganda was fantastic.

BOHNING: What about when you were to give papers at meetings?

MANDELKERN: You weren't invited to give too many papers, but you could go and give a contributed paper. They would listen to you, I guess. But there were many meetings in the 1960s from which we were just excluded. I think that history will straighten out the past. It's all since been straightened now. It was the subject of a real advertising propoganda. It was all that one can imagine to be bad about science.

There was a meeting in Germany in the 1960s published in the Kolloid Zeitschrift. Somebody wrote me a letter and asked if I would like to come and be a chairman of a session. I said, "No. I can't come to Germany and be a chairman of a session on a subject which I have been studying for twenty years. I have things to say and if you think I'm not worthy of making a presentation, I won't come." I went to that Bristol meeting in 1960 that Frank and Keller held. My time got chopped away until I had about five minutes left to talk. That was a very difficult period for me. But I feel quite good about things now.

BOHNING: Where did the support come from eventually?

MANDELKERN: Do you mean from science or from people?

BOHNING: Both.

MANDELKERN: This was the approach that I was taking. First, the properties did not allow regular chain folding to happen, but this was a deductive thing. This was probably one of the reasons for the difficulties.

Secondly, chain statistics wouldn't allow regular folding. Then Flory had one paper in 1962 which I think was pretty much ignored by most of these people (20). It explained why you would form the lamellae without having to fold the chains and why you didn't have to invoke the nucleation theory of Hoffman's to do that.

Then I think the real thing that broke it open was neutron scattering. There were several papers starting with George Wignall and people from ICI who showed that the radius of generation did not change from that melt (21). They looked at molecular weight relations. In other words, if you had a regular folded chain, it's high school alegbra to calculate what the radius of generation would be. They're not even close, by order of some magnitudes. You couldn't explain that. I didn't do it, but I think that's what turned a lot of people who didn't want to believe. And a lot of people who are not involved in polymers or crystalline polymers just look at things superficially. So people were not too convinced by the kinds of experiments that we had done. But I think the neutron scattering were more dramatic and more direct. That was probably started in the 1970s.

BOHNING: In 1972 you wrote a book called Introduction to Macromolecules (22).

MANDELKERN: That was a different kind of thing. That was a labor of love for a different reason. Several of us had been concerned for some time about the lack of an introduction to polymers (I'm not talking about proteins and nucleic acids) in high school chemistry, and certainly on an undergraduate level. The Education Committee of the ACS Polymer Division has a program to do this. It used to bother me and it still bothers me now. One of the questions was, "How can you get students at a very young age, seniors in high school and freshmen in college, who are interested in learning, to pick up a book like this? How can you get them to learn that there are such things as polymers and macromolecules, that they are as interesting as argon or ethane, and that they could be treated in the same kind of way." So this promoted it. I really enjoyed doing it.

BOHNING: How was that received?

MANDELKERN: That was received very well. That was very interesting. Several publishers I approached wouldn't touch it. Konrad Springer has a friend on the campus who he was visiting one day and somehow word got around. He popped in to see me. They were very much interested and it was a pleasure working with Springer-Verlag in New York. This book has been well received. I don't want to brag but all of the reviews were very favorable. In fact, my head swelled in some ways. It's been selling at a steady pace. It's not adopted as a textbook but it does sell. It ran out of the first printing and I just made some relatively minor changes. I added a section on liquid crystal type polymers and the work on fibers that come from that, and a couple of pages on genetic engineering and DNA. Rather than have them do just some more printing, I figured I would put in a little more effort to make a second edition. That was quite different, since it was very well received by both students and the faculty who teach general chemistry courses.

BOHNING: You also have a paper in the Journal of Chemical Education showing how to introduce polymers in the physical chemistry course (23).

MANDELKERN: Yes. The ACS is putting out a book that is being edited by Tom Lippincott at the Institute of Chemical Education in Madison. I have a chapter in that. It's just an amplification of that paper in the Journal of Chemical Education. In fact, a lot of it is lifted straight from it. But there is a lot of work now going on in this kind of thing both in physical

chemistry and organic chemistry. We have a seminar in Gainesville in two weeks at the local ACS meeting on this subject. I am very happy with the paperback.

BOHNING: What about your work at Florida State?

MANDELKERN: We've covered a good part of it. Here our work has been primarily involved in different aspects of crystallization. A good part of it has been on what we just talked about-- properties and mechanisms, addressing this folded-chain business. During the first fifteen years, although not so much in the last few years, we worked on contractile fibers. One of the reasons that motivated me to leave the Bureau was that we did an extensive amount of work on keratins, collagen, and muscle fibers some of which has been appreciated, and some of which time will tell.

The only thing that we did out of the general crystallization area was some work on the conformational analysis of polypeptides. I use the word crystallization in a very general sense. We talk about contractility or structure or properties of a fiber. That's a crystalline entity. We did follow through until the last few years, when my interests have changed back to the synthetic polymers, the contractile fibers, and the fiber proteins. That's one of the things that I studied at the National Bureau of Standards. I got interested in some of the conformational work on polypeptides over the last few years, but I don't know what the future holds for us. I've been pretty much working on synthetic polymers.

BOHNING: Are there any specific graduate students of yours that stand out?

MANDELKERN: I don't really distinguish between graduate students and postdocs. I've had some really outstanding students. Wayne Mattice is now moving to an endowed chair at the University of Akron. He had been at LSU for fifteen to eighteen years. He's quite outstanding. He came here to work on a polypeptide problem. Jose Fatou is in Madrid at the Institute of Rubber and Plastics. He and R. Kitamaru, who's a professor at Kyoto, were my first postdocs. They came within a few months of each other. Fatou is a major mover in Spanish polymer chemistry. He's been following pretty much along the lines of the work we did here. Kitamaru has really developed in the last five or six years and has gone into solid state NMR. He's made some very interesting and important contributions. He didn't do that here but I'd like to believe he learned from his work here. Those stand out in terms of people who are in academia. I haven't had a lot.

I had Scott Zimmerman, who's now a professor at Brigham Young and who was a very bright student. I believe he's in biochemistry there. I had Roberto Benson, who's in material research at the University of Tennessee. He just moved from the University of Utah in material science. A large number of my students are in industry here and abroad. I had a student who is now with ATO in France, Michel Glotin, who was a recent postdoc. He's going to be very important to French industry before too long. I had a postdoc named Armand Dekmazian who left here about four or five years ago. He heads up a major group at Exxon's Linden Labs in New Jersey. I had a postdoc in NMR, Rich Komoroski, who went to industry working on NMR and polymers for a while. He had a responsible position at Goodrich and has left or is in the process of leaving to do NMR imaging. He's going to be a professor at the University of Arkansas Medical School. I had a student, Ricky Allen, who's with 3M. I have a lot of people in industry but I can't keep track of where they all are. They changed around a lot.

I had a very good student named Gary Stack who is at the Naval Research Laboratory Branch at Orlando. They have a lab there concerned with underwater problems. They have just gotten into polymers and he's doing very well there. He's one of the better Ph.D.s that we put out of this institution. I had a student, Harry Lader, who used to be with Du Pont and actually did his postdoc with Bill Krigbaum. He is now with the Nordson Corporation in Ohio. They make a lot of machinery for polymer processing. We've gotten heavily into gels and the fellow who started that was a Ph.D. here named Charles Edwards. He's in Akron in what used to be called General Tire. I believe it is called GenCorp. I had a very good student from Turkey named Ertugal Ergoz. He did some classical work here on crystallization kinetics. He's manager of some rubber manufacturing plant in Turkey.

BOHNING: Is there anything else that is on your list that we haven't covered?

MANDELKERN: I just made some notes from your list. I hope I made clear that I think what is important historically is this scientific problem of the folded chain. I want to emphasize that I think the issues are pretty well decided now. It was personally a very difficult, harassing situation for a long time. The fact that it persisted for twenty some odd years is not the question. The question is that it was a rather difficult personal time for me to keep working. I do appreciate several companies that kept me on as a consultant and thought that I could help them despite the fact that in the early 1960s our work on crystalline polymers was held in disrepute. I particularly want to mention Exxon Chemical Company in Baytown, Texas. I think that would be a fair statement to say.

My book came out at that time, and I feel quite good about it now. Not that I'm bragging, but I get introduced as the author of the book and everybody tells me how much they read it and how much they want a new edition. But I do think for historians looking at this from all points of view and not just mine, it was a very difficult period and a very bad period for polymer science. It was a good lesson of how controversies in science should not be solved. They should not be settled by political innuendos, by keeping people off of programs. I don't want to go through all of the harassment at the Bureau of Standards. That's not the way. The solutions just get delayed. I think this is a point I would really like to make. As you look at these things over the different aspects, there are very standard ways that scientific controversies have been settled from time immemorial. It doesn't have to get involved and deprive people of things to do.

There is a very classic example of how two scientific gentlemen in the same institution and the same department can have a major disagreement and still go out to lunch, enjoy each other's company, and have the problem settled in the proper way. What I'm talking about occurred during the 1949-1951 period. On the subject of the intramolecular excluded volume effect, Flory had one position and Peter Debye had the totally opposite position. You could have seminars where they would argue and their students would argue and discuss these things. It got hot and heavy at times, but it was never personal and vitriolic. I've seen many times when Debye and Flory would argue in the morning and then they would go off for lunch together. They had tremendous admiration for each other intellectually and personally.

The way in which the problem was solved is not important, but that's the way things should be done. This is something I learned back in 1950 and I think that's the way gentlemen and scientists settle their affairs. They had very deep-seated disagreements on whether the excluded volume effect went asymptotically with molecular weight or it continued indefinitely with molecular weight. That had very practical effects on how you interpreted almost all the solution properties of polymers. It took a few years to get it straightened out, but it did get straightened out. To me, that is a classical example.

As we look back at it it was a very hard experience but I think the science is straight and that's what's important. And I was also doing other things so I kept myself from going completely crazy. I think that this is basically what I wanted to say.

BOHNING: Then we shall close with that. Thank you very much for your time.

MANDELKERN: Well, thank you for coming down.

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