THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

HERBERT MORAWETZ

Transcript of an Interview
Conducted by

James J. Bohning

at

Polytechnic University

on

1 April 1986

Herbert Morawetz
JH
3/15/96

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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

HERBERT MORAWETZ

1915	Born in Prague, Czechoslovakia on 16 October

Education

1942	B.A.Sc., chemical engineering, University of Toronto
1943	M.A.Sc., chemical engineering, University of Toronto
1950	Ph.D., chemistry, Polytechnic Institute of Brooklyn

Professional Experience

1945-1949	Research Chemist, Bakelite Co.
1950-1951	N.I.H. Fellow, Harvard Medical School
	Polytechnic Institute of New York
1951-1953	Assistant Professor of Chemistry
1953-1958	Associate Professor
1958-1981	Professor
1981-1986	Institute Professor
1986-	Institute Professor Emeritus

Honors

1966-1967	N.I.H. Fellow, Universities of Naples and Rome
1980	Centennial Scholar, Case Western Reserve University
1984	Whitby Memorial Lecturer, University of Akron
1985	Polymer Chemistry Award, American Chemical Society

ABSTRACT

In this interview, Herbert Morawetz traces his early life prior to leaving Czechoslovakia on the Nazi invasion and resettling in Canada, where he studied chemical engineering at the University of Toronto. He describes his introduction to industrial research work and his consequent Ph.D. study at Brooklyn Polytechnic Institute and late postdoctoral fellowship at Harvard Medical School. Morawetz outlines the circumstances of his appointment to the faculty at Brooklyn and his research and scholarly activities there. During the course of the interview Morawetz reflects on some of his graduate students, on the future of polymer education and on international scientific collaboration.

INTERVIWER

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

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INTERVIEWED BY: James J. Bohning

PLACE: Polytechnic University

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BOHNING: Professor Morawetz, you were born in Prague on October 16, 1915. Can you tell me about your parents, their names and occupations?

MORAWETZ: My father, Richard Morawetz, was an industrialist who owned a textile factory in a small town on the northeastern border of Bohemia. The factory was engaged in flax spinning, jute spinning, and jute weaving. It was founded by my great grandfather in 1852, so my father was the third generation. When the World War I broke out, which was a little before I was born, it happened that all the managers on the family estate in eastern Bohemia were conscripted into the army, so my father was asked to manage the estate during the war because it was an essential food producing enterprise. So this is where I spent my early childhood, the first six years of my life. After that we moved to a small milltown in northeastern Bohemia and I lived there until I was high school age. Because there was no high school in that town, and as there were three other children who would shortly go to high school, and since my father had to spend about half his time in Prague, the whole family moved there in 1926.

I lived in Prague for eight years while I went to high school. After that, as the oldest son, I was expected to participate in the management of the plant, and so I moved back to this small town. One year after that I was sent to Belfast, Northern Ireland, where all the textile machinery we used was being produced. I was supposed to be there for a year, learning about the operation of that machinery. I returned to the small mill town in 1936 and I lived there until the German occupation, which was March 15, 1939. As soon as we knew that the country had been occupied we all realized it was a question of life and death for us to get out. Fortunately we had a German director in the company who was very loyal to our family; with his help I persuaded the German authorities that it was essential for me to go to Poland and buy some flax to keep the plant going. This is how I escaped and eventually came to Canada in late 1939 where I enrolled at the University of Toronto.

At first I tried to study chemistry, but since I had missed the first term, the head of the chemistry department decided that it was hopeless for me to enroll at such a late date. Luckily, the head of the chemical engineering department understood my desperate situation. At the time I felt that at twenty-four I was about as old as I could be to start as a freshman. So I studied chemical engineering. I might add that before the occupation of Czechoslovakia I had no particular interest in becoming a scientist. I was passionately interested in literature, in poetry, and I certainly was much more interested in creative writing than I was in chemistry. Of course, when I had to leave and had to learn a new language, it became obvious that any ambitions to be an author were out and that I would have to look for a new occupation. The choice of chemistry was motivated in a rather interesting way. My father had studied chemistry as a young man and had planned to become a chemist, but when he graduated he was told by the family physician that his father was dying of diabetes. To please his dying father he told him that he'd changed his mind and that he would take over the firm. Nevertheless, he always had a very nostalgic feeling for his youthful chemical ambitions. Even when I was quite young he would take me on walks and tell me about chemical experiments, and this undoubtedly influenced my choice.

At the time I was twenty-four years old. My parents also managed to get to Canada and my father at fifty-nine had to start a new business. Consequently, I was rather hesitant to tell him that I wanted to study, because I thought that he might consider it disloyal. On the contrary, my father was absolutely delighted because it somehow brought him back to the dreams of his youth. Even so, if I had realized at the time that I would have to go through graduate school to be able to do chemical research, which might take at least seven years, I would never have had the courage to start. I was very naive; I thought that four years would be sufficient to qualify me for what I wanted to do. I have been always very conscious of how old I was when I wanted to start something. Later on, when I went to graduate school at the age of thirty-two I would have never had the courage to make that choice if it hadn't been for the encouragement on the part of my wife, who is the daughter of a professor and who thought that an academic career would be much more suited to my temperament than the career of an industrial chemist.

BOHNING: I have a number of questions if I could back up for a little bit. We don't have the name of your family company.

MORAWETZ: It was the Upice Flax and Jute Works Limited. This is the closest translation from Czech into English.

BOHNING: What was it like growing up on the estate that your father was managing?

MORAWETZ: We lived in what used to be a medieval castle with no central heating. It had walls close to three feet thick, and in winter time there were very few rooms that could be heated. Now, of course, I don't remember much of that. I have very few memories of my early childhood, but we were really very isolated.

Apart from my two younger brothers and some cousins that I may have met occasionally, I don't think I had any contact with children. A sister was born after the war, when we had moved away from the estate.

BOHNING: What was the name of this town where the factory was situated?

MORAWETZ: Upice. The estate was in Svetla and the factory in Upice. Svetla was then almost like a village; I think it had a population of about three thousand. Today it's maybe ten times as large, after a glass factory was built. Well, it looks very different from what it looked like at the time when I was there as a child.

BOHNING: You completed your Gymnasium in Prague in 1934? Were there any teachers there that you remember? Anyone that had a special influence on you?

MORAWETZ: Well, we had a very gifted mathematics teacher. Certainly I was much more interested, if I was interested in science at all, in mathematics than in chemistry. I would say any inspiration I got for chemistry was more from my father than from my teachers. The mathematics instruction was really far superior to what you would get at an American high school.

BOHNING: Did you have any chemistry there?

MORAWETZ: Although all gymnasia were really elite schools, my school didn't have any laboratory. It was all book knowledge; I had never seen a microscope. It's very different from a high school in present-day America.

BOHNING: In 1935 you went to England and then to Ireland.

MORAWETZ: That's correct.

BOHNING: Are there any particular people or experiences during that year you spent there that you recall?

MORAWETZ: Well, I arrived in London on the eve of King George the Fifth's silver jubilee, the twenty-fifth anniversary of his accession to the throne. This was the last big celebration of the British Empire, with all kinds of Maharajahs coming to take part in the procession, with elephants and God knows what. On the night that I arrived in London there was dancing in the streets; I thought this was a feature of London life. I was very disappointed when I was told the next day that it happens once in a lifetime. [laughter] Now, my father had always insisted that I shouldn't take any English lessons in Prague because I would only learn bad habits, so I came to London knowing practically no English. As it turned out, I did take about ten lessons of English before I left and my teacher had this very strange idea that I should start by reading a book, and he selected the stories of Oscar Wilde. So the first English sentence that I read was "High above the city stood the statue of the happy Prince." [laughter] It wasn't very useful and when I arrived in London, for all practical purposes, I was unable to speak.

BOHNING: Were you able to pick up enough of the language?

MORAWETZ: My father's idea was that I should stay three months in London, and take a language lesson every day, which I did. After that I went to Belfast where I was very unhappy. It was the unhappiest year of my life. The atmosphere of Belfast was almost as bad then as it is today. There was an intense hatred between the Protestant and Catholic communities. I couldn't help, somehow, comparing the violence of that prejudice with the Hitler movement that, of course, was a source of great concern for us at the time.

BOHNING: Was there anyone that you recall, any individual that you remember from Ireland?

MORAWETZ: Only in a negative sense. There were some individuals that I took a very strong dislike to, for this reason.

BOHNING: You were at the company manufacturing the machinery that you used in Czechoslovakia?

MORAWETZ: That's correct. The idea then was that if you were the son of a well-to-do family it was not appropriate for you to take any salary, so I was there as a so-called volunteer. Of course, that meant that nobody was particularly concerned with teaching me anything; I really always felt that it was a waste of my time.

BOHNING: What responsibilities did you have within the company when you returned home?

MORAWETZ: I went back to the plant where I was supposed to be in charge of the technical management. I wasn't the only one; a cousin of mine looked after the commercial aspects of the operation. My father was president of the cartel of the whole jute industry which was headquartered in Prague. So he came only for inspection visits maybe once a month, which was always a source of great joy for me because I was extremely attached to my father, who was a man of innumerable interests; a very interesting man. His visits were the brightest part of the years in this mill town where I was rather lonely.

BOHNING: I believe that your father traveled a lot?

MORAWETZ: That's correct. In 1904 he came to the World Fair in St. Louis and traveled to the Yellowstone Park and to San Francisco. In 1911, he made a trip around Asia. He was in India and Ceylon and Indonesia, in what is today Thailand and in Burma. He went to Canton; he wanted to go to Peking, but he couldn't because there was a plague epidemic at the time. He was in China before the fall of the Manchu dynasty, he saw the old China. Then he went to Japan and he returned by the trans-Siberian railroad. It is characteristic of how times have changed; when he came to St. Petersburg and checked into his hotel, he was surprised to be asked for his passport. It had never happened to him before. When he asked why they wanted his passport he was told that every foreigner had to be reported to the police. He was so shocked by this that he took the next train home without seeing anything of St. Petersburg.

BOHNING: Did you have any particular feelings of how the political climate changed in the late Thirties? Had you given any thought to what you might be facing?

MORAWETZ: Sure; we were passionately interested in political developments. Every time there was an election in Germany we stayed out until late at night to see how things were going. It was very terrifying: we were intensely proud of the fact that our small country seemed to be the most determined barrier to this new barbarism. We were all very patriotic; we resented it very much if someone suggested that we should leave the country. My father would have never left if he hadn't actually seen the German Nazi troops in Prague. It's very difficult today to imagine this situation. Because, you know, President Tomas Masaryk, who headed the state until 1935, was not just a statesman, but was considered by most people to be the ultimate intellectual authority on almost every question that might turn up. He was often compared to the Platonic idea of the philosopher-king. He had been a professor of philosophy. In retrospect, one might question the depth of his philosophy, but by and large it was really in many ways like a golden age.

BOHNING: After the Germans took over, how were you personally affected and how long was it before you then left the country?

MORAWETZ: It took me two weeks to get out. The problem was to get a visa. By a fluke my parents had a French visa but I didn't have any visa and they were very difficult to get. First you had to get a visa and then you had to get a German exit permit; two conditions. The Polish visa I got more or less by accident, because of a friendship of an aunt of mine with the Polish ambassador. The German exit visa, as I told you, I owe to this German director of our company who went to the German authorities and persuaded them that it was essential for me to go to Poland. Now he understood, of course, that I wasn't going to buy flax; that was just an excuse. He just asked me to send him a cable from Warsaw saying that I had bought a lot of flax so he could go to the authorities and show it to them and he said they would then forget about it. So that's what I did. A lot of other people weren't sufficiently scared and stayed and paid for it with their lives. Even if you were scared it wasn't so easy to leave.

BOHNING: How long were you in Poland?

MORAWETZ: We were extremely lucky because my brothers and my sister were all abroad at the time. Otherwise the whole family would have never got out. I was in Poland for a week. The Polish government was fascist and would have been quite prepared to send me back; I got out again by a strange coincidence. I found out by accident that a man whom my parents had known while he was the French ambassador in Prague, was now French ambassador in Warsaw. I appealed to him and he was very kind to me and gave me a French visa.

BOHNING: When did your parents leave?

MORAWETZ: They left about four days after me.

BOHNING: And did they come to Poland or...

MORAWETZ: No, no. They went to France. Also on a fictitious business trip. We all met in France and then we went together to England. My parents went to Canada at the end of August and actually arrived on the day the war broke out. At the time I was planning to study chemistry in England, so I stayed behind. Of course, as soon as the war broke out, they wouldn't take on any foreign students, so I decided to go to Canada.

BOHNING: I have two questions. When did you decide to study chemistry at the collegiate level and, secondly, why did your parents select Canada?

MORAWETZ: Well, when I was a refugee in Paris I met a young man, a Czech refugee, who was studying medicine. He took me to the Cité Universitaire, where there was a compound of buildings for foreign students and since there was no particular building for Czech students, he was put up by the American students. I was so charmed by the atmosphere of the student community there that I decided that afternoon that although I was almost twenty-four years old, I maybe wasn't too old to start to study. Actually, I had at the time considered two careers, journalism or chemistry and I decided, I guess, very wisely, that since I would have to learn a new language, and my English would never be sufficiently idiomatic for journalism I decided on chemistry. Why did my parents go to Canada? As you may know, there was a long waiting line for potential immigrants to the Unites States. It depended on the country that you came from, for which there was a quota, and I have no idea how long my parents would have had to wait. Canada had a different policy. Anybody who had some money to start a business, or planned to be a farmer, was on a preference quota. This is how my father decided to go to Canada.

BOHNING: To start a business there?

MORAWETZ: Yes. He bought a partnership in a small business making overalls. Actually, there might have been another motive. I think that my father was somewhat scared that, since so many refugees would come to New York, there might be a reaction against them. I think that was also a motivating force which led to Canada.

BOHNING: You joined them in December.

MORAWETZ: I came on Christmas Eve, yes. With my sister on a blacked-out ship.

BOHNING: And your brothers?

MORAWETZ: Well, my youngest brother was employed in Belfast by the people with whom I had stayed, and he stayed there all through the war. My other brother, Oskar, who was studying music and is now a well-known composer in Canada, was stuck in France and then in Italy. He got out of Italy just in the nick of time, going to the Dominican Republic which was then governed by a very nasty dictatorship. But the dictator, Trujillo, to ingratiate himself with FDR was much more liberal in admitting refugees than more civilized places. Oskar stayed for about six months in the

Dominican Republic before he was able to join us in Canada.

BOHNING: When did you tell your father you were going to study chemistry?

MORAWETZ: Well, I must have told him already in England, because I know that in Montreal he went to McGill University to inquire about the possibility of me enrolling there. While in London I was staying with a cousin of mine who told me in no unmistakable terms that I was completely irresponsible in not joining my father in whatever he wanted to do. So that, although my father knew already that I had this notion, I certainly felt I had to bring it up again when I got to Canada, and I was overjoyed to see his enthusiastic reaction.

BOHNING: Was he in Toronto or Montreal?

MORAWETZ: He first came to Montreal but, interestingly, he did not settle in Montreal because he was so shocked by the antagonism between the French and British elements. He said he didn't travel over half the world as a refugee to find a situation reminiscent of the enmity between the Czechs and the Germans in Bohemia.

BOHNING: And you had experienced the same thing in Ireland too.

MORAWETZ: That's correct.

BOHNING: So there you were; in the middle of the semester and the chemistry department had turned you down for admission, because it was the middle of the term and they didn't think that you could catch up.

MORAWETZ: Well, the head of the chemistry department at Toronto, Andrew Gordon, was a well-known electrochemist but not a very pleasant man, very dictatorial in his ways. He just thought it was cheek on my part to think that I could be admitted in the middle of the academic year. I was very upset by his reaction but when I was walking across the corridor I ran into Professor James W. Bain, head of chemical engineering, and when I told him about my predicament he said, "Oh dear, oh dear. What can we do for you?" He was very sweet, and told me that I could enroll in chemical engineering and if I wasn't ready to write the examinations in April he would make other arrangements for me. As it turned out, I didn't need them. I passed my exams, just barely, but I passed them. This was really a Godsend for me. In those days you became painfully aware of how everything in life depends on lucky accidents, of meeting the right people and all kinds of other improbable strokes of luck.

BOHNING: So your degree was in chemical engineering from the School of Practical Science?

MORAWETZ: Yes. I got my B.S. degree in chemical engineering in 1943, and then enrolled for a Master's degree which I got in 1944.

BOHNING: I wanted to ask you some questions about the four years that you were there. You mentioned Bain; were there other faculty that had an influence on you?

MORAWETZ: Yes. I was very attached to the professor of physical chemistry, Frank Wetmore. He employed me one summer to prepare demonstration experiments. He was a very interesting man. I was also captivated by my professor of organic chemistry, Maitland C. Boswell, because of something he did in the first lecture of organic chemistry.

[END OF TAPE, SIDE 1]

Here we were, knowing absolutely nothing about the subject and in the first lecture he discussed unsolved problems. This made a very deep impression on me. So, when I decided to study for a Master's degree, I chose him as my thesis advisor, which turned out to be a very unhappy choice. It was the last year before his retirement, and he had some very naive notions about making a sensational discovery, but it was something which simply couldn't work; when it didn't work he accused me of sabotage. Yes, it was a very unhappy situation.

BOHNING: Where did you stay when you were at the University?

MORAWETZ: I stayed with my parents. We were very dependent emotionally on each other. My children don't understand that I could have stayed with my parents until I was thirty. I remember during summertime when I worked in industry, some distance from Toronto and I came home for the weekend, I would look from the bus stop to see whether there was still a light on in the house, and if so, I would run. I just couldn't wait to be with the family. It was a very emotional tie which was natural under the circumstances.

BOHNING: You mentioned you worked in the summertime. Did that help pay your tuition and your expenses?

MORAWETZ: Well, I can't say that I saved any money, maybe a little. But I worked two summers. The first summer I worked for Imperial Oil in Sarnia. That was a very unfortunate situation because they really didn't know what to do with summer students, so it was rather boring. Later I worked for the Welland Chemical Works, close to Niagara Falls. They were making only one product, nitroguanidine, which was to be used in anti-aircraft shells. The atmosphere there was delightful because it was generally assumed that this was just a wartime occupation, that nobody would stay there after the war, so nobody was trying to make a career. There was a very free and easy camaraderie. I was very, very happy there.

BOHNING: Is there anyone there that you became friends with?

MORAWETZ: Well, I can't say that I kept up a friendship with anybody there, but I was very happy. The plant operated on three shifts, so I quite often worked from midnight to eight in the morning. Things were rather primitive when I remember today what the control arrangements were like; something out of the middle ages.

BOHNING: Were you involved in a production process or in a laboratory?

MORAWETZ: In a laboratory.

BOHNING: What specifically were you doing?

MORAWETZ: I tell you frankly, I don't really remember.

BOHNING: Then you, I guess it would be 1943, returned to Toronto to work on your masters?

MORAWETZ: Yes, that was in the summer of 1943. That's correct. I returned to get my Master's degree.

BOHNING: Also in chemical engineering.

MORAWETZ: Yes. Then I tried to get a job and there was at the time such intense anti-semitism in Canada that, although I had graduated at the top of my class, I was unable to get a job. Strangely enough, I was much less upset by it than my mother; she was absolutely desperate. Eventually a neighbor of ours, who was a Rotarian, told my father that a fellow Rotarian was the president of the Canadian Bakelite Company and that he would help me. Sure enough, I got a job with the Bakelite, but the people there were so furious that they had been forced to hire me that they kept me sweeping floors for six weeks in the hope that I would resign.

BOHNING: This was in Canada?

MORAWETZ: That was in Toronto. Well, I didn't resign. Then they had some technical problem, I don't remember what it was, but they called in somebody from their New Jersey plant to straighten them out. This man immediately recognized what my situation was and he asked if I would be interested in transferring to New Jersey. I jumped at that. I went to New Jersey in September 1945, and was very happy there.

BOHNING: Was that in Bound Brook?

MORAWETZ: Yes.

BOHNING: You were at Bakelite for four or five years?

MORAWETZ: Well, I started in September 1945 and I took a leave of absence in September of 1949 to finish my dissertation, so I really wasn't there all that long. I had started in night school in September 1947, so I went to night school for two years.

BOHNING: Was that at Brooklyn?

MORAWETZ: Yes. But then I decided that to do a dissertation part-time was pretty hopeless. By that time we had one child and were expecting a second, and although my father offered to help me financially to study full time, I felt it would be undignified at my age. So I did my Ph.D. by two years part-time study and then with this one year full-time.

BOHNING: Let's go back to your experiences at Bakelite for a moment. What were your responsibilities there?

MORAWETZ: I was put into a group which was supposed to work on polyethylene. It was a rather ridiculous situation. When I was hired, I was given a pep talk where I was told that polyethylene was a material that had been used in wartime for radar cable insulation, but now the war was over. There was a U.S.

production capacity of twenty million pounds a year, and they didn't know what to do with the stuff; we were supposed to find some new uses for it. Today the production is about four hundred times as large and still growing, but then none of us knew much about it. The group consisted of three people, all of us I think, very ignorant. Later on I worked on the heat stability of polyvinyl chloride; there I think I made some reasonable contributions, but of course I wasn't allowed to publish it.

BOHNING: Do you remember any of the people you worked with?

MORAWETZ: Oh, yes. The group leader was Clayton Myers. The division head was Richard F. Clash, but these people did not have any influence on me. The man who really influenced me most was Dr. George J. Dienes, who was a physicist and who is now at Brookhaven National Laboratory; he is a rather well-known solid state physicist. In particular, he told me that companies were not particularly encouraging their staff in spending time on publication and that I couldn't be sure what the future would hold for me; it would be his advice to try to publish as much as I could. As it turned out, I published only two papers while I was there, but he gave me the right idea; that I really couldn't tell where I would end up.

BOHNING: What was the impetus behind your going back to school in 1947?

MORAWETZ: I think the impetus came largely from my wife. My wife was working for her Ph.D. in mathematics and I used to make a joke. I used to say that when our daughter grows up, the phone will ring and she will ask, "Who do you want to speak to; Dr. Morawetz or Mr. Morawetz?" My wife didn't think that was very funny. As she was the daughter of a famous mathematician and had herself scientific ambitions, she didn't think that the kind of work I was doing at Bakelite was what I should do for the rest of my life. And as it turned out, she was right. It was also important that I found out that I could study at night at the Polytechnic Institute of Brooklyn and that Herman Mark was teaching there. Of course, when I finally opted for an academic career at the age of thirty-six, I was scared stiff; I really wasn't sure I could make it.

BOHNING: Were you living in New Jersey and commuting to Brooklyn for night school?

MORAWETZ: Yes. I would work at Bound Brook from eight in the morning until four in the afternoon, and I took the train twice a week for the classes here which went from six to ten in the evening. Parenthetically, one disadvantage of this was that, since I knew that physically I couldn't stand more than two nights a week of this kind of life where I would return home at midnight and get back to work at eight in the morning, I took only the classes given on Monday or Thursday between six and ten. As a result, I never studied a number of subjects which I should have studied; I always thought that I'd make it up later on, but, of course, one never does.

BOHNING: What courses were you taking?

MORAWETZ: Polymer courses and I guess I had to take some physical chemistry and some organic chemistry. I took organic chemistry from Charlie [Charles G.] Overberger the first semester that he taught here. I took some courses in modern physics where again I made a great mistake. There were two sections, one for chemists, the other for physicists; I was sufficiently arrogant to think that the section for chemists would be too watered down so I enrolled in the one for physicists. As a result I really didn't understand what was going on, and I could have never made it if my wife hadn't helped me with the homework.

BOHNING: I was going to ask you about your math background.

MORAWETZ: Well, my math background is so limited, that no chemist could get a Ph.D. today with the kind of mathematical training that I got. Of course, I had calculus, but I never had a course in differential equations, for instance, which is really rather shocking.

BOHNING: I suppose for the record we should have your wife's name.

MORAWETZ: Sure. My wife is Cathleen Synge, and she's the daughter of a very well-known mathematician, John L. Synge, who is still alive and who lives in Dublin.

BOHNING: Where did you meet your wife?

MORAWETZ: In Toronto. Her father was head of the applied math department in Toronto. Incidentally, she's now director of the Courant Institute of Mathematical Sciences at NYU.

BOHNING: When did you first meet Turner Alfrey? You did your dissertation with him.

MORAWETZ: That's correct. Well, I enrolled in a course on introductory polymer science taught by Herman Mark, but of course, he was very often out of town and Turner Alfrey was substituting for him. The first time I had a lecture from Turner Alfrey, I had absolutely no doubt that that was the man I wanted to work with.

BOHNING: What was it about Turner that you found so attractive?

MORAWETZ: He was the most brilliant teacher that I've ever known. Like many brilliant teachers, he often gave students the impression that they understood things which it turned out later they didn't really quite understand. But he was not only a brilliant teacher, he was a passionate teacher. In other words, although he had the largest teaching load of anybody I've ever known, if a question came up in casual conversation and he found out that his student was weak in some area, he would schedule a series of impromptu seminars on the subject. I remember that he gave such a series of seminars on tensor calculus, because he thought it was something we should know about. He was incredible in that way. Of course, he had a lot of students and very often you had to stand in line in front of his office to get a few minutes with the great man. He was a few years younger than I was, so it was kind of a strange relationship. But I was very grateful to him, apart from my very pleasant relationship with him, for the fact that he approved my thesis after only one year of thesis research because he understood that I was thirty-five years old at that time and I was very self-conscious about starting a career so late. I guess he figured that if he kept me any longer it would really not increase my qualifications; he was not like so many other supervisors who take advantage of students by keeping them on when they are productive.

BOHNING: You weren't in this building at that time. You were in a different building.

MORAWETZ: No, we were on Willoughby Street.

BOHNING: What was it like at Brooklyn Poly in 1949 when you came back full time?

MORAWETZ: I can't tell you much about it because I was so overwhelmed with the idea that this was the last possible time in my life when I could study. Because I was so terribly old I didn't look to right or left; I just worked like crazy. I have practically no recollection of who else was in the lab. I hardly ever spoke to anybody. I was completely immersed in finishing up as quickly as I could. Don't forget, by that time we had a child; well, halfway through that year we had a second child. We lived in a one-bedroom apartment in Brooklyn, of course, we were very anxious to have the whole thing over with! [laughter]

BOHNING: Yes, I can imagine. When you came back full time did you have any kind of assistantship?

MORAWETZ: Yes. I've forgotten what it was called. But I had some kind of assistantship.

BOHNING: Did that involve your doing any teaching?

MORAWETZ: No. As I recall it was an Office of Naval Research research fellowship.

BOHNING: I think you mentioned earlier that someone had indicated to you that you were suited more for an academic career.

MORAWETZ: Well, this was my wife. Well, actually what happened was that when I finished, I went for some job interviews and I also applied for a post-doctoral fellowship at Harvard Medical School. My feelings were rather ambivalent because when the fellowship came through, I felt almost trapped. My wife was very annoyed with me at the time, because she thought this was really a great deal. She certainly didn't want to go to Buffalo; I think it was a Du Pont plant near Buffalo where I had an offer. So then I went to Harvard Medical School for a year where I worked in the blood protein fractionation laboratory. That was during the Korean war and I had some romantic notions that I was going to do something for the alleviation of human suffering, but I found out very quickly that people who work on biochemical and medical problems are just as involved in technical problems as anyone else. From that point of view it was very different from what I expected. It was a very interesting experience; there were a number of very gifted people there. A number of people there made very big careers and became quite famous. I think in retrospect that I wasted the opportunity because I should have joined one of those great scientists and worked on their problems, but I was so involved with this feeling that at thirty-five I had never worked on my own ideas, that I insisted on working on a problem which I had formulated, which was really a rather trivial problem; so from that point of view it was really a great waste. The one thing which I got out of it was a lifelong interest in biology and biochemistry. In that way I'm sure that I've become very different from most polymer chemists.

BOHNING: Yes, that's a point I wanted to touch on later. But how did you make the move from your thesis work?

MORAWETZ: Well, you see, my thesis was on amphoteric polyelectrolytes, polymeric electrolytes carrying both positive and negative charges. Originally, they were conceived as models for proteins, but they are poor models; it was a rather naive notion. They are really not models of proteins, but it seemed natural for me to try to work on the real thing.

BOHNING: I see.

MORAWETZ: And besides, Professor Isidor Fankuchen, the x-ray crystallographer who was here, was a friend of Professor John T. Edsall, who was second-in-command of this blood protein laboratory at Harvard Medical School. I think he might have even written to Edsall about me. Anyway, Edsall became one of my culture heroes; he's an absolutely wonderful man. I've always regretted that I didn't choose to work on one of his problems. This would have certainly been the right thing to do.

BOHNING: Has he not also become involved in writing history?

MORAWETZ: Yes. After he ceased to work on biochemical research, he became a historian of biochemistry and this certainly gave me, to some extent, the idea of doing something similar. Actually he wrote to me recently that he's reading my book and that he's very happy with what I did. But apart from that, he's a great humanist; I think I'm not the only one who says that he's a culture hero. I think he's one of the most admired scientists in America as a human being.

BOHNING: Did you have much contact with him when you were at Harvard then?

MORAWETZ: Yes, he was very sweet to us. He offered us hospitality in his home and so on. But I didn't have any specific scientific contact with him.

BOHNING: So you really worked on your own problem?

MORAWETZ: I worked with Walter L. Hughes, but on the problem which I had brought and which, as I say, was a rather trivial problem.

BOHNING: You returned to Brooklyn then in 1951. Had you planned to return?

MORAWETZ: Maybe I should say something about what I had planned to do at Harvard. I rather naively thought that I could use synthetic polymers to fractionate antibodies. At that time, very little was known about the chemistry of antibodies. Antibodies had been studied for a long time, and from many points of view, but it was, for instance, unknown that different antibodies had different amino-acid sequences. It was a rather naive notion, but it was something I had dreamt up and I was not to be done out of it.

BOHNING: Did you publish anything on this?

MORAWETZ: We published a paper, as I say, not a very important paper (1). As a matter of fact, one of my first students here worked on a continuation of that work (2). We could fractionate proteins after a fashion, but it really wasn't a very interesting approach.

BOHNING: What things did you have in mind beyond Harvard?

MORAWETZ: Well, it is not quite right to say I came back to Brooklyn. Of course, I realized that once you are a post-doc at Harvard your opportunities to get jobs are orders of magnitude greater than when you are a graduate student at Brooklyn. In particular I had a very tempting offer from the General Electric Company. However, I told them that since my wife had just got her Ph.D. in mathematics, I couldn't go to Schenectady unless they had something for her. They invited her for an interview, but they treated her in such a condescending fashion that she came home most unhappy. That was the end of General Electric. It was really unbelievable. Many years later, I mentioned this incident to Dr. Arthur M. Bueche, who was then vice president of General Electric. He laughed and said, "Oh, today we would give our eye teeth to hire her." [laughter] But this was a different time when women had many problems. Not always from employers; other women were very hostile to women who wanted to be professionally active. So there were great complications. I considered going back to Bakelite, and I went Bound Brook for an interview and I asked them what would they have me do if I came back. They said, "Why of course, to continue work on the stability of polyvinyl chloride." I said, "Well listen. I'm tired of that. I'd like to do something else." "Such as?" "Well, for instance, I thought that heterogeneous catalysis might be an interesting field." They thought this was a rather far-out idea. This was a little before Ziegler made his famous discovery, so, while I don't claim that I ever would have gotten anywhere, it was rather ironic that this was how we parted company.

Now, I should say that when I was a graduate student here, I first thought of teaching, but it was very difficult to get academic jobs. I applied to the North Dakota Agricultural College and they didn't even deign to reply to my application. I thought it was completely hopeless. And then one day, whilst I was still at Harvard Medical School and was in New York attending one of those famous Saturday morning symposia at Poly, and as I was walking out, I felt somebody's hand on my shoulder. I turned around and found it was Herman Mark who said, "Would you be interested in an assistant professorship with us?" Well; I was so flabbergasted I almost fainted. Then he invited me to his home and we discussed it; of course, the pay was terrible. There was such a big discrepancy from the offer from GE that I hesitated and then something really lucky happened. The research director of the Eli Lilly company met Turner Alfrey and told him that they wanted a polymer chemist as a consultant or something like that. Anyway, he came to Boston and offered me a consultantship that made up the difference between the Poly pay and the GE pay. There's a sad sequel to that. He outlined to me a problem which was essentially what became later gel electrophoresis. I agreed to try it out. I had the correct idea of cross-linked polyacrylamide, but unfortunately I used a catalyst that created nitrogen bubbles during the polymerization. When I saw this, I just threw up my hands and gave up; I didn't understand the importance of the problem. This, I think, is one of the most spectacular failures of my career because this technique has revolutionized biology.

[END OF TAPE, SIDE 2]

BOHNING: What was your first pay at Brooklyn?

MORAWETZ: Well that's a funny story. I got a letter from the president saying that my pay would be $7,000 a year. I accepted; I thought it was great. Then a few weeks later, he wrote me another letter 'Dear Professor Morawetz, I'm sure you understood that there was a clerical mistake in that this was predicated on you having a grant for summer research. Your academic salary will of course be $5,250.'

BOHNING: This was 1951?

MORAWETZ: Correct.

BOHNING: What were your first assignments here?

MORAWETZ: Well let me tell you why I got the job. I got the job because Turner Alfrey decided to leave and go to the Dow Chemical Company. Apparently he told Mark that he would recommend me for the spot. Now Mark probably didn't know me from Adam at the time. So it was, again, a series of lucky coincidences. There was a Gordon Research Conference to which I went to show Turner Alfrey the draft of my thesis. In my thesis was a theoretical part on the distribution of counterions around charged rod polyelectrolytes. The chairman of the conference was Raymond Fuoss. Turner said, "Fuoss would be interested in this, would you mind if I tell him about it?" I said, "No." So, Fuoss then asked me to give an impromptu talk. Herman Mark was there and this was undoubtedly the first time that he knew who I was.

So that was very lucky. When Turner Alfrey left (as did Arthur D. McLaren) two slots opened up. The other was taken by Gerald Oster. I taught Turner Alfrey's course on polymerization kinetics. I must have also taught a course on the physical chemistry of polymer solutions because I remember preparing it during the summer before I came here.

BOHNING: What about your research?

MORAWETZ: As far as my research is concerned, again, it was an extremely lucky situation because that was the time the Korean War ended. We had more students than we could accommodate and there was a lot of research money. I very quickly got a grant from the Office of Naval Research and Herman Mark turned over to me one of the men who had applied to him. Riad H. Gobran became my first graduate student; he was a very gifted man. It wouldn't have been so easy for me to pick up a graduate student at the time; Mark was very generous that way.

It was a time that if you were willing to work hard, you could collect money and students and this made it possible for me to start at a late age and yet to make a go of it. When I compare it to the present situation I think my situation would have been utterly impossible.

BOHNING: You started to work on solid-state polymerization in 1953.

MORAWETZ: Yes. This is also an interesting story. I mentioned previously Jay Dienes. Dienes at that time was at Brookhaven where they were interested in possible chemical uses of radioactive waste products. So they invited my colleague, Robert B. Mesrobian and myself to come there and discuss it with them. During the discussion, Mesrobian made the very sensible suggestion that there was no point in using gamma rays to polymerize monomers that can be polymerized easily in other ways. They should look for processes which cannot be studied in any other way. He suggested two; one was radiation grafting and the other was the polymerization of solid crystalline monomers.

He started to study that but he was already considering getting out of the academic life. He had a student who did some very superficial work on this. But I got very intrigued by this idea, not because I was trying to make a polymer in a new way, but because I thought it would provide an insight into molecular mobility in crystals. It would be really a rather basic problem. I started some work on this and I met a lot of skepticism because the work was largely on acrylamide. Now acrylamide melts at 86°C, so it was thought that some of our monomer melted and that all the polymerization took place in the molten state. We had to try to disprove that notion and I think the first clear disproof was when we decided to work on different salts of acrylic and methacrylic acids, where the polymerizable species, acrylate or methacrylate would always be the same, but the crystal structures would depend on the cation. We found that potassium acrylate polymerizes at 0°C much faster than sodium acrylate at 100°C (3). This could not be explained except by recognizing that the crystal structure plays a crucial role in the polymerization.

Even then, as I recall it, I didn't have many takers for this proposition. Then something interesting happened. I think it was again Dienes who got me in touch with a group of people who were going to publish a monograph on the organic solid state. It was suggested that I write a chapter on reactions in organic crystals. However, that was difficult to do because solid-state organic reactions were not considered an orthodox field so there was difficulty in surveying the literature. Certainly you couldn't use Chemical Abstracts. So I wrote letters to leading organic chemists, asking them if they had ever heard of such a thing, and I got several responses. I wrote a chapter which was incorporated into the book (4); in the chapter was a particular section where I reported on some work that had been done in Germany on the reactions of diacetylene derivatives and in one of the papers was a footnote indicating that one of these diacetylenes seemed to polymerize in the solid state. The authors suggested a structure for the product but one I could not believe. Anyway, I included it in my chapter although it turned out to be a mistake. But I always say that this mistake was probably more productive than most of the correct things I've done in my life because Dr. Gerhard Wegner in Germany read the chapter and it stimulated him to do work in that area. In fact, he made a career of it; he is now the director of the Max Planck Institute for Polymer Research in Mainz. This has really become a big thing. The first time that you could convert a macroscopic monomer crystal into a macroscopic polymer crystal. So that was a strange and unexpected by-product of this activity.

BOHNING: How long did the resistance to the concept last?

MORAWETZ: I would say really until Wegner's work. There is also a man in Japan, Masaki Hasegawa, who did work in a somewhat different area (5). Then of course, there was another Japanese named Seizo Okamura. He discovered in 1961 that you could take a single crystal of trioxane and convert it not only into a polymer, polyoxymethylene, but that the polymer would be highly oriented in the crystallographic direction of the monomer (6). This rather sensational finding was reported at an IUPAC meeting in Montreal and I remember that some people then told me that I was just working on the wrong monomer. Certainly Okamura's result was much more spectacular than anything I had done. From then on, the solid-state polymerization of vinyl compounds became of somewhat less interest.

BOHNING: Did your association with the Brookhaven people continue?

MORAWETZ: No.

BOHNING: You had mentioned that you started out consulting with Eli Lilly in the early 1950s.

MORAWETZ: Well nothing really came of that. They were very helpful in getting me started in academe. I'm sorry to say that I never really did anything much for them.

BOHNING: In 1956, you went to the Weizmann Institute. How did that originate?

MORAWETZ: In 1953 I attended my first IUPAC meeting in Stockholm and there I met Aaron Katchalsky. He invited me to come to the Weizmann Institute. He said that in 1956 the IUPAC meeting would be there. So I started applying for a fellowship to finance that visit. I had planned to go there with my whole family for a year but, as it turned out, the country was in an uproar at the time because of all of these fedayeen attacks. Both my parents and parents-in-law were very upset about us taking our small children there. So instead of all of us going for a year, I went for half a year by myself. As it turned out, I was personally somewhat incompatible with Aaron Katchalsky so it was not a very fruitful decision from a professional point of view.

BOHNING: What did you work on when you were there?

MORAWETZ: I really didn't work on anything in particular. I took an interest in some of the work of Katchalsky's graduate students. He was going to write a book on polyelectrolytes, so I was asked to contribute a chapter to that book on specific associations of polyelectrolytes and counterions. I wrote my chapter but he never did anything about the book. So I eventually published the chapter as a journal article (7).

BOHNING: I have a note here; origin of the term microtacticity in 1958. Did you originate that term?

MORAWETZ: Yes.

BOHNING: What were the circumstances?

MORAWETZ: This is rather technical. I have to go back a little bit. As I told you, my thesis was on polyelectrolytes and I was particularly interested in the electrostatic potential around a highly charged chain molecule. At that time I lived in New Rochelle and since Brooklyn Poly was rather far away, I used the Fordham University library. While going to Fordham, I met a young biochemist called Milan Bier. Watching Bier in his laboratory, I became acquainted with a procedure which is very common with enzyme chemists who use a reagent which changes color after enzymatic attack. In particular they use nitrophenyl esters which become yellow when they are hydrolyzed.

I had the idea that if I took a long chain polymeric acid acid and attached to it a very small number of nitrophenyl esters, small enough so that it wouldn't change the nature of the chain, since the hydrolysis of the ester is catalyzed by negatively charged hydroxyl ions which would be repelled from the polymer, the factor by which this hydrolysis is reduced would reflect the electrostatic potential in the neighborhood of the chain.

Well, we tried the experiment and to my amazement not only wasn't the rate reduced, but it was a million times accelerated. We were completely puzzled by that for some time until it became obvious that the reaction mechanism had changed. The reaction was no longer dependent on an attack by the hydroxyl ion but was controlled by the attack of a neighboring charged carboxyl group.

That got me interested in neighboring group activations because this was obviously related to enzymatic activity. I spent a lot of my time studying situations of this type. One day, instead of using polyacrylic acid, I took polymethacrylic acid. I found that although I had only a very small number of these ester groups attached to the chain, some of them reacted ten times as fast as the others. I was completely puzzled. As a matter of fact, the publication which first related this observation in a footnote was garbled (8). If you read it, you don't even know what it's about. But what I wanted to say in the footnote is that this is what we found and that we have no idea what it means.

Then one day I was having a bath; this was about a year after Natta had discovered isotactic polymers and suddenly it occurred to me that the stereochemistry of these two kinds of esters and their immediate environment must be different. So we made some molecular models and proved that this was really so. Of course, at that time the proof of tacticity was only by crystallographic techniques which are only applicable to a crystalline material and a polymer would crystallize only if it had a very extensive stereoregularity. Suddenly we had a method by which you could demonstrate differences in stereo configuration even when there was a small deviation from randomness as far as the stereoregularity was concerned (9).

I was quite excited by it and I said let's call it microtacticity. I'm surprised that you know about it.

BOHNING: That was just something that I came across while going through your work. It's a fascinating story.

MORAWETZ: Of course, I should add that the use of this reactivity as a handle to prove microtacticity is not very practical and at this time nuclear magnetic resonance is the method of choice. But it was certainly the first time that anybody had found a technique by which such things could be demonstrated, even in a primitive way.

BOHNING: I'd like to move a little bit ahead into the 1960's and your first book, <u>Macromolecules in Solution</u> (10). You made a comment in the introduction which you enhanced in the second edition ten years later. `...the realization that macromolecules which retain in solution precisely defined conformations are not confined to the realm of biological macromolecules, the de facto separation of the fields of natural and synthetic macromolecules became an absurdity...' What kind of reaction did you get to that statement in 1965?

MORAWETZ: I don't recall any reaction to it. I don't think that any biochemist bothered to read my book. And of course, synthetic polymer chemists knew that Paul M. Doty and Elkan R. Blout had discovered helix-coil transitions of synthetic polypeptides (11). I've always said that the golden year in polymer science was 1953-54 because of three completely unexpected phenomena discovered within about twelve months; helix-coil transition in solutions; chain folding in single crystals (12) and stereoregular polymerization (13). I doubt that there will ever be another year in polymer science in which three such revolutionary concepts will be established.

BOHNING: By 1975, you repeated that statement. Has any impact been made on the biochemists in the past ten years?

MORAWETZ: No. I don't think so. I'm not sure that my efforts to get polymer chemists interested in biochemistry have been very effective. People try to stick close to their bread and butter. As I say, I have been very deeply influenced by this year at Harvard Medical School which led to great pleasure in my scientific career.

BOHNING: What about your colleagues here at Brooklyn? Do they share your enthusiasm for this?

MORAWETZ: I don't really think that it has any influence over anybody around here. I wasn't trying to proselytize anyone. For many years, I've tried to follow the biochemical literature. Now, it has become so technical that I can't read it anymore.

BOHNING: Do you see an increasing specialization in science that makes it more difficult for people to cross over?

MORAWETZ: Of course; I think it's very obvious. Let me give you an example. Two gentlemen got the Nobel Prize in Chemistry last October. I knew the name of one, but I had never heard of the other. For another instance, there are now six chemists who have been awarded the National Medal of Science. Of course I knew Mark, Westheimer and Marvel. I knew of Harry Gray although I couldn't really tell you much about his work. The other two I had never heard about. This may show that I am parochial but I think most people are as parochial as I am. Some of my colleagues seem to know almost everyone in all kinds of fields. How deep they know what they are doing, I don't know, but at least they know their names and they know something about them. Maybe it's partly a reflection of my age that I'm not making as much of an effort to go outside of my immediate interests as I once did.

BOHNING: Well, in an interview with him last summer, Carl Djerassi made that very same point. One year there was a Chemistry Nobel Prize winner that nobody in the chemistry department at Stanford had ever heard about.

MORAWETZ: It's funny that you should mention Djerassi because ten minutes before you came, a colleague of mine came in and said that he had met Djerassi last night and that Djerassi mentioned that he knew of me. This colleague was very surprised because Djerassi had no interest in polymers; but he said, "Oh yes, Paul Flory had told him about you." Some people manage to pick up things like that, but by and large I think this is becoming more and more rare. As a matter of fact, I'll tell you something. I have people tell me that they are surprised that I bothered to publish in the Journal of the American Chemical Society because they publish only in macromolecular and polymer journals as a way of advancing their careers. They are not interested in publishing in journals their colleagues don't necessarily see. I must admit that in recent years I haven't made a particular effort to read all of the polymer journals. I should be ashamed of it but...

BOHNING: The volume of literature today has grown so much.

MORAWETZ: That is an interesting point; let me tell you something about that. Many of my colleagues are overjoyed with computer literature searches. I hate this whole concept because to me one of the great pleasures of a scientific career is to go to the library and to browse through journals and come across things that you never thought about and to get into all kinds of areas purely by accident. Obviously, if you are going to search the literature by computer you know ahead of time what you are looking for; you're not going to get anything that's unexpected, that is going to stimulate you to branch out into something different. And as far as I'm concerned, much of the charm of science will go out with this kind of approach.

BOHNING: I can see the day when journal literature will probably be only on computer. It may not exist on paper.

MORAWETZ: Then I'm happy that I won't be around to be part of it.

BOHNING: I share a similar feeling in dealing with material on microfilm. It's the same thing. You can't browse. You go just to the point you're looking for and nothing else.

MORAWETZ: True.

BOHNING: It is very disturbing. During the time that you wrote your first book in 1965, you made a comment about the support that your department gave you; a reduced teaching load, I believe. What was the impetus behind your writing the book in the first place? What brought you to sit down and write the book?

MORAWETZ: This is very difficult to remember. I don't know how one decides to do a thing like that. It comes out gradually. I know that, much earlier, Herman Mark once asked me to do a book on molecular weight determination with him. I wasn't very keen on doing it and as my reaction was rather negative nothing ever came of it. Eventually, I wanted to write something more broadly based, particularly to present a unified treatment of biological and synthetic macromolecules. I had very good reactions to the book but then writing the second edition was as much work as writing the original book. It was rather disappointing that it sold very few copies; very few people realized that it was an entirely new book.

BOHNING: I think you remarked in your introduction that about half of it was new material.

MORAWETZ: Yes, but nobody believed it. I am sorry that many people use the first edition and are unaware of the second edition. But this is natural; I think that if I had a book and I heard about a second edition, even a revised edition, I wouldn't quite believe it. It takes you a while to buy it.

BOHNING: Maybe if you had changed the title.

MORAWETZ: Maybe.

[END OF TAPE, SIDE 3]

BOHNING: While we're talking about the books, why don't we move on to your book on the origins and growth of polymer science (14).

MORAWETZ: This came about as follows. I got a letter from Mrs. Magda Staudinger (Herman Staudinger's widow) to say that the hundredth anniversary of the birth of her late husband was coming around and that Dr. Wegner had recommended me as a suitable person to write some kind of an appreciation. Now, I should tell you a little bit of the background which you will not find in the book. When Staudinger died, his wife, who lives only for the memory of her husband, collected all of his papers and sent them to the Deutsches Museum in Munich with a request that they should create a Staudinger Archive, which they did. The result of it was not at all what she expected. A young man there called Claus Priesner planned to write a Ph.D. thesis on the history of science and his advisor suggested that a good subject would be the controversy between Staudinger on the one hand and H. Mark and K. H. Meyer on the other. He wrote a book called Staudinger, Meyer, und Mark which is rather damaging to Staudinger's memory (15). It shows him as a rather vindictive person while Herman Mark was always courtesy incarnate. Staudinger's reaction often was just unbelievably hostile. Mrs. Staudinger was obviously very unhappy about this book so her request was an attempt to counteract this unhappy outcome.

I knew about all of this and I wasn't going to get myself involved so I wrote to tell her that I was too busy and so on. However then she sent me ten volumes of Staudinger's collected works. So I gradually got the idea that, while I was not going to write about Staudinger himself, I might start from the beginnings of polymer science. And that is how I wrote the whole book. But then I had great problems persuading Wiley to publish it; they told me that nobody cares about science history. I think it was finally Eric Proskauer who persuaded them to go ahead with it. Even so, they had the idea that they were going to advertise it as a book from which one could learn polymer science, which of course is nonsense. Somebody not knowledgeable about polymer science won't be able to read five pages of it. I was originally going to call it a History of Polymer Science but they wouldn't take it. Then I asked if they would take "Polymers -The Growth of a Science" but they were very reluctant to agree to that. Finally, when I changed it again to "Origins and Growth", they were really furious with me. "Now it really sounds like a history." I said, "It is a history." They were very unhappy about it but in the end they accepted it. I must say that the book has had marvelous reviews. I don't know whether you've seen them.

BOHNING: Yes. I've seen some.

MORAWETZ: Still, I must admit that Wiley were right from the commercial point of view as the sales have been very disappointing. The fact is that people are only interested in what happens tomorrow, and are uninterested in what happened one hundred years ago. But from my point of view: first of all I told you that as a young man I was much more interested in literature than in science; this was a return to my old love in a way because it was half literature. Also, I believed, and still do, that if I did not write such a book, nobody would. My wife felt that I should plan to write this book later, when it would not interfere with my research. Maybe I should not have gone so fast so that it's finished before my retirement. This problem of what I'll do next is still on my hands.

BOHNING: How long did it take you to write it?

MORAWETZ: About four years.

BOHNING: Did the format of this book come slowly?

MORAWETZ: I never make an outline when I write something. I just start out and see what happens. Of course, I found out so much in writing it that I had no idea about. For instance, the whole business about Berthelot giving a lecture on polymers in 1863. I found that out by sheer accident. I was looking for some lectures of Pasteur and suddenly there I see the heading "La polymerie" and I almost fell over. 1863! It was great finding it (16). This is like Columbus. [laughter] It's really very exciting when you come across something like that.

BOHNING: I have told people that sometimes it can be as exciting in the history of science as finding something in the laboratory. You've just given an example of this.

MORAWETZ: Oh yes. There's no question about it. The other thing I found very exciting was that Lord Kelvin had formulated a theory of rubber elasticity and had got everything wrong. It wasn't until afterwards that I found out that Flory knew about it, so I wasn't the first one to find out about that.

BOHNING: You spent some time in Europe examining manuscripts.

MORAWETZ: Well, two years ago I was in Germany for a month in Freiburg. I thought that being close to the place where Staudinger worked would give me some insight. I used that opportunity to visit some of the chemical industry. It turned out that Hoechst in particular has the most fantastic archives. Of course they destroyed everything that dealt with their activities during the war, but, other than that, they have a big building that is completely devoted to their archives. They gave me complete freedom to ask for anything that interested me. They have published little monographs based on their collections. Something for which there is absolutely no parallel in America. I don't know whether you know but, unfortunately, the chemical industry lawyers here tell them, "better shred than read"; so all the old records are destroyed.

BOHNING: You commented also that you really got to know Herman Mark when you were writing the book.

MORAWETZ: That's correct. When I tried to decide whether to come to Brooklyn, my wife and I sat down and wrote out on one sheet of paper all of the advantages and on another sheet, all of the disadvantages. One of the advantages was the collaboration with Herman Mark and a disadvantage was the need to teach. But, it turned out exactly opposite. I like teaching and I have had almost no scientific interaction with Herman Mark. I don't know why but it just turned out that way. He's more interested in the broad sweep of things; details don't particularly captivate him.

But the moment I started to write this book, he became fascinated. I don't know how often he came to my office with little bits of information. I tell you a story about that which is really characteristic of his charm and kindness. I applied for a Guggenheim Fellowship to write the book and in my application I wrote that I was aware of one great handicap in that I've never been trained as a science historian. I gave Mark as one of my references and he later sent me a copy of his recommendation. In this he wrote, "It is true that Dr. Morawetz has not been trained as a historian, but one should remember that Thucydides was a general and Winston Churchill a politician." [laughter]

BOHNING: You also commented that you used only first hand citations. Second hand citations are incredibly unreliable.

MORAWETZ: That's correct.

BOHNING: Did you find that as you started?

MORAWETZ: No; I don't want to mention any names. There's a prominent polymer chemist, one you have interviewed, who wrote an article about a certain development. I went through the literature to check it out and found it was completely incorrect; he never bothered to look at the sources; it was just his vague recollection. This happens all of the time. As a matter of fact, it can happen to me. Some time ago, I looked at Chemical Abstracts and I read an entry and thought, well this is an interesting abstract. When I came to the end of it, I found that I had signed it. [laughter] Memory is faulty.

BOHNING: We had mentioned earlier the materials that you had collected in writing that book.

MORAWETZ: I am not a Xerox fan. I have practically no Xerox copies of the things that I write about, I just sit in the library and I make notes. I have a collection of some of these publications of Hoechst; that is really most of what I have. I don't have much.

BOHNING: Maybe at this point we should talk about the Flory correspondence.

MORAWETZ: This is an interesting story. I don't quite remember the year. Yes; in 1977 I got a letter from Academician Andrianov in which he invited me to be chairman of a session at an international polymer meeting in Tashkent. I replied that as long as Soviet colleagues whom I admire are not free to come and visit me, I don't think that I should come. Of course, I never heard from him again. What I did was a little more than that. I had about one hundred copies of that letter made and I sent it to various people who I thought might also be invited, among whom was Paul Flory. I did it with some trepidation because I knew that he had been invited to give a keynote address; I thought he might be offended when I tried to interfere.

I didn't hear from him for some time and then he wrote me a letter in which he said that he had provisionally accepted the invitation but that if the Russians misbehave sufficiently, he would feel free to change his mind. Next spring there was this trial in the Soviet Union of Orlov and Shcharansky who were put into concentration camps. I wrote to Flory, "Did he consider this sufficient misbehavior?" For some time I didn't hear from him. Then he sent me a letter which he had written to his Russian host in which he said that he would be glad to come but that he wanted assurances that he would be free to discuss matters which, although not directly related to science, are of great importance to the lives of scientists. Something to that effect. Of course, he never got an answer. Not only didn't he go, but he added insult to injury by going to Peking instead. From then on, his name was taboo in the Soviet Union.

This boycott spread and to my great amazement Herman Mark, who is the most non-political person that I've ever known, decided to join it. He didn't go; neither did Murray Goodman or Walter H. Stockmayer. This was the outcome of it. In the end, there was a rather unfortunate talk on Soviet television in which Flory was compared to Don Quixote.

BOHNING: I heard about that just a few days ago. Will it be possible for us to get copies of that correspondence?

MORAWETZ: Yes. Next there was a great deal of controversy because of this talk given by one of our colleagues. Dr. Edwin J. Vandenberg, who was then chairman of the ACS Polymer Division and who thought he was going to smooth over the roughened water, had the unfortunate idea to write to Flory about this Russian television show suggesting that the case had been had overstated. Flory just blew up. [laughter]

BOHNING: I can imagine.

MORAWETZ: It really was very funny. But it's interesting how naive some people can be and how they really don't quite understand. Vandenberg contacted me; he was a man who would really try to keep everybody happy. He couldn't understand why Flory was furious: I could understand.

BOHNING: Well, if we could get copies of that.

MORAWETZ: I will try.

BOHNING: Because in the Flory papers that the Center just acquired, I know there is some correspondence relating to the Tashkent meeting. This would be a natural to go with that.

MORAWETZ: I think that's the only thing I have in my correspondence which is worth preserving.

BOHNING: I have some questions about a few of the things in your scientific work. You were the first to introduce the use of fluorescence spectroscopy for polymer research.

MORAWETZ: No. This is not correct.

BOHNING: Well, your article in Science was the first (17).

MORAWETZ: No. Let's put it this way. Fluorescence had been utilized very effectively by biochemists in studies of macromolecules. The great pioneer was Gregorio Weber, who started the work in 1951. Also, my colleague Gerald Oster was very interested in fluorescence and he started some work with one of his most brilliant students, Yasunori Nishijima who is now President of the University of Kyoto. He has had a tremendous career and he has continued to work in the area of fluorescence of polymers. There were some other scattered contributions.

I got into this because I wrote Macromolecules in Solution and was so impressed by the extensive application in biochemistry, so I wondered what could be done with synthetic polymers. I know that many scientists tell you that they have twenty research proposals in their drawer and they just don't have enough students to work on all of their ideas. This has never been my feeling. I have always run scared; I've always felt that the thing I was working on was the last good idea that I would have in my life. Particularly in that I hit upon this fluorescence business rather late in life; I was almost sixty years old, so it was really a windfall. Certainly, it's been a lot of fun over the last few years. But that's how I got into it. As I said, I might have got some stimulation from Oster but it happened independently.

BOHNING: That's interesting. So it came out of your book.

MORAWETZ: Altogether I have the impression that many people believe they can't get involved in writing a book because it is so much effort at the expense of their research work. First of all, it's fun writing a text. Secondly, you are doing something that will last a little longer than a research paper. Lastly, you will get a lot of research ideas out of it. This is certainly one of the benefits of writing a book.

BOHNING: In a number of your publications, you talk about crankshaft-like motions. In 1980 you have a paper (18) in which...

MORAWETZ: Their non-existence?

BOHNING: Yes.

MORAWETZ: One of the referees told me that it couldn't be published with that title. At that point I wrote to the editor-and-chief, Stretch [Field H.] Winslow and said that as far as I'm concerned the title is the most important part of the paper but I would submit to the judgment of Walter Stockmayer. He backed me on it. I would like to say one particular thing about it. I've been working on this whole problem of whether there is or isn't a crankshaft motion for a long time. As you know, its first models were not very definitive. I started working on it in 1965 but I think nobody believed me. Everybody ignored this work and it was only when Gene [Eugene] Helfand did some computer simulations which lead to similar results that it was believed (19,20). This has always puzzled me; it seems to me that while computer simulation is a very important and powerful method, experimentation should certainly be considered equally important.

I would like to say that I have had one other run-in with a referee which is also interesting. Some years ago I studied the problem of the rate of reactions of two chain molecules, one with an attached catalytic group and the other with an attached reactive group. The results turned out to be completely unexpected so in the paper I wrote that we didn't fully understand the results (21): I still don't understand them. One of the referees commented that this was an interesting paper but the authors "would be well advised to conceal their surprise." I blew my top and said that the surprise was the whole point of the paper. This is quite essential to my whole philosophy of publishing scientific work. I think the attitude is too common that research is not publishable unless you can explain your observations. This is a great mistake because very often one author's unexplained data might stimulate someone else to look for an explanation.

I've had controversies like that with people who would tell me about some results and I would say, "But look, this is illogical." They would say, "What explanation do you have for it." I would say, "I have no idea." They would say, "Well, it can't be published unless it is explained somehow." I see an awful lot of that. People just cook up explanations because they think it won't get past a referee otherwise.

BOHNING: That's unfortunate.

MORAWETZ: Very unfortunate.

BOHNING: Another area that you did some important work on was the cyclization process (22).

MORAWETZ: Much of my work has been involved with kinetics of polymer reactions. There are, of course, two kinds of reactions which depend on cyclic intermediates. The ones where the activation energy is high so that these two groups will meet each other and diffuse apart billions of times before something happens. In that case the reaction rate depends on the statistical probability that you have the cyclic structure required in the transition state. The other case is that pioneered by Professor Mitchell A. Winnik at the University of Toronto: the case where there is practically zero activation energy so that the rate of the process depends on the rate at which the two groups come together because the first time they come together something happens. These are two different problems; possibly Winnik was stimulated in his work by some of ours. His work is superb.

[END OF TAPE, SIDE 4]

BOHNING: In 1962 you were in Rome for a year.

MORAWETZ: No. It was in 1966-67. I went to spend a sabbatical year with Alfonso Liquori who was then at the University of Naples. Halfway through my sabbatical he moved from Naples to Rome and I moved with him. Again, it was really the same thing as with Katchalsky. I met somebody; I was captivated by him and in the end it turned out that we did not work together.

BOHNING: What did you work on at the time you were there?

MORAWETZ: Only literature work. I didn't do any experimental work. Incidentally, doing literature work in Italy is quite an experience. You have to be in a place like Italy to appreciate how easy life is in America. Even a poor school like the Polytechnic has library facilities which are practically unknown over there.

BOHNING: Is that common throughout Italy?

MORAWETZ: It's common throughout most European countries. You know the common American situation is that you just go to the stacks and pick up any book that you want. That doesn't exist over there. In Rome, if you want to go to a first class library, there's an organization, I forgot what it's called, but you have to fill out a slip with your name and address and the name of your father and God knows what. Then you wait for half an hour until you get the book. After all, in literature research I sometimes look just for one number in a paper, so I take it out, make a note and then look for something else. This is impossible in Italy; impossible.

BOHNING: I have a number of things which sum up what we've been talking about in a more general fashion. I want to ask you about your graduate students.

MORAWETZ: I've had a number of brilliant students. The three most brilliant students were: Jules Shafer, who became, I believe, the youngest full professor at the University of Michigan Medical School. When he finished here he went for a post-doc with Westheimer. Incidentally, when he first came to me as a very young man to ask for a thesis topic, he said he liked the topic but he was concerned that it would only qualify him for academic work. That's not what he wanted to do as he wanted to make a career in industry. I told him, "You are a very young man and you never know." Of course after he was through with Westheimer for a year, he wasn't interested in anything but an academic career. He's a very prominent man and he has remained a very close and devoted friend of mine. We have a wonderful relationship.

There are two others that come to mind in the same way. The second one is Jerry [Jerome B.] Lando who for many years was head of the polymer group at Case Western Reserve University. He is going to chair the symposium of the American Chemical Society one week from now at which I am to receive the Polymer Chemistry Award.

The third one is Chong Sook Paik (now C.S.P. Sung). When she was with me she was a young woman who had just come from South Korea. She's now at the University of Connecticut in the Department of Materials Science and she's absolutely brilliant. She has done extremely well and I'm sure she's going to continue to do very well. Of course I have many other very good students but these three really were the stars.

It so happens that right now I have a student from Peking, China who I think is absolutely outstanding. He is Yingcai Wang. It's a pleasure to work with people like that but I want to say quite frankly that I've also enjoyed working with the other students. I had over fifty of them and it's been a great life.

BOHNING: Do you have any patents?

MORAWETZ: Well, I once consulted on a beer additive problem. I know I was asked to sign a patent application but I never found out if the patent was granted (23). Frankly, I really don't care.

BOHNING: You did start some consulting with Lilly. Did you do any other consulting over your career?

MORAWETZ: My big consultantship was with Dow. I was with Dow for twenty-seven years. I enjoyed it but, quite frankly, I don't think Dow knew how to take advantage of consultants. I think there were some ideas which would have been good if they had been acted on. The thing that I was particularly sorry about was that in a number of cases I found out about a development at Dow only when it was practically complete where I might have possibly been useful earlier. Another thing is that very often I was asked to spend an hour with a man who was working on some project on which I had absolutely no expertise; that was a complete waste. I really think that it's a great art to use a consultant but I don't know how many people are good at it. I enjoyed my experience at Dow very much, possibly because of my friendship with Turner Alfrey and others in the company. But I don't think that they really knew what to do with me.

BOHNING: Did you consult with anyone else?

MORAWETZ: I was always reluctant to consult with several companies because I was always afraid that there might be a conflict of interest. I gave a number of industrial lectures over the years. About three years ago, I was contacted by Technicon which is a company in Tarrytown making machines for medical diagnosis. They had heard about a certain development here and they retained me for a year to develop that. This was interesting; something that I had never done at Dow, to try to follow up a certain idea up to a commercial product.

BOHNING: I have two questions to tie this up. What changes have you seen in polymer chemistry during your career?

MORAWETZ: I really don't know how to answer that question. I would say that the field has become much more specialized. Today, when I pick up a polymer journal there are relatively few papers in it which would interest me. Everybody seems to be in a rather narrowly defined field. I guess it's because chemistry has grown so much. As I told you, I don't think we'll ever see anything comparable to 1953-54. There are a lot of people who say that we're heading for huge discoveries. It could be. I don't know. But, it's not obvious to me that the field will be revolutionized in any way.

BOHNING: What about polymer education? Training students?

MORAWETZ: As you know, the major schools in America don't teach polymer science. I was told by a number of my friends at Dow that they had been to a number of leading schools to urge for polymer courses but they were never provided. My good friend Rudolf A. Marcus who was on the staff of the University of Illinois chemistry department actually tried to persuade them to hire a polymer chemist but, in spite of his tremendous prestige he was never able to bring it about. The talents of Paul Flory were never taken advantage of by the Stanford faculty. He was, I think, quite bitter about it; they made no effort to replace him. Princeton made no effort to replace Arthur V. Tobolsky. This is a sad situation. I gave a talk at a prominent chemistry department a few years ago and I went to lunch with some of my colleagues. They were talking about this topic and they said, "It is difficult for us to hire polymer chemists because while we interview students from Harvard, Yale, Princeton, Berkeley, and Caltech they don't produce any polymer chemists." What they were saying exemplified the vicious circle which makes it so difficult to change the situation.

I always look at it this way. It is absolutely obvious that you can never define polystyrene as rigorously as you can define a benzene molecule. So if scientific rigor is the only criterion of the worth of a science then obviously polymer science will be rather low on the pecking order. But no two mice are identical. When you go to biology you throw this notion to the wind. There's something illogical about it, but this is the way it is. Of course the moment a polymer is considered biological, say when it's a nucleic acid or a protein, then you're in a different league. To me it is strange that companies like Du Pont make no effort to change the situation. They take the line, "We want people to be well trained physical or organic chemists and we'll teach them about polymers." Well, I'm not sure this is the most realistic attitude but as long as we live with this situation, things will not change.

BOHNING: There seems to be development in polymer engineering.

MORAWETZ: That's correct. In many chemical engineering departments; but it's ridiculous. For instance, my good friend Curtis W. Frank at Stanford University is in the chemical engineering department, but what he's doing there is not, by any stretch of the imagination, engineering. Why doesn't the chemistry department at Stanford pick him up? It's completely ridiculous. But this is the way it is. I think it's a little better in Japan and maybe in Germany, although the Germans also complain about it.

BOHNING: So it's just not endemic to this country?

MORAWETZ: No. It certainly is a fact in England. There are some powerful polymer theoreticians at English universities but not very many who really work with polymeric materials.

BOHNING: My last question; what do you see for the future in polymer chemistry?

MORAWETZ: I don't believe in prophesying; many people have been so disastrously wrong in prophesying in science. Of course, there are very many people working on conducting polymers; some of them have been dreaming about superconducting polymers. I don't have the impression that it's very probable that we will get them, but I don't know. Should it happen it would be a completely new field. But I'm really not in the business of prophesying.

BOHNING: Is there anything else that you would like to add at this point that we haven't touched on?

MORAWETZ: I don't know. By the time you write this out it will be much too long anyway.

BOHNING: Oh I don't think so. [laughter]

MORAWETZ: I really don't know. I've had a good time of it.

BOHNING: Well, with that I'd like to thank you very much for your time. I certainly have enjoyed it. Thank you very much.

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