THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

HERMAN MARK

Transcript of Interviews
Conducted by

James J. Bohning and Jeffrey L. Sturchio

at

Polytechnic University, Brooklyn, New York

on

3 February, 17 March, and 20 June 1986

(With Subsequent Corrections and Additions)

CENTER FOR HISTORY OF CHEMISTRY

Oral History Program

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(Revised 6 February 1986)

Herman Mark

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(Revised 20 February 1989)

Herman Mark, interview by James J. Bohning and Jeffrey L. Sturchio at Polytechnic University, Brooklyn, New York, 3 February, 17 March, and 20 June 1986 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0030).

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

HERMAN MARK

1895	Born in Vienna, Austria on 3 May

Education

1921	Ph.D., chemistry, University of Vienna

Professional Experience

1921-1922	Instructor in Organic Chemistry, University of Berlin
1922-1926	Research Fellow, Kaiser Wilhelm Institute of Fiber Chemistry, Berlin-Dahlem
1927-1928	Research Chemist, I.G.Farben Industrie
1928-1930	Group Leader, I.G.Farben Industrie
1930-1932	Assistant Research Director, I.G.Farben Industrie
1927-1932	Associate Professor, Karlsruhr Technical University
1932-1938	Professor of Chemistry and Director, First Chemical Institute, University of Vienna
1938-1940	Canadian International Paper Company, Hawkesbury, Ontario
1940-1942	Adjunct Professor, Polytechnic Institute of Brooklyn
1942-1964	Professor, Polytechnic Institute of Brooklyn
1946-1964	Director, Polymer Research Institute
1961-1964	Dean of Faculty, Polytechnic Institute of Brooklyn
1965-	Dean and Professor Emeritus, Polytechnic University

Honors

Honorary Degrees

1950	University of Liège, Belgium
1953	University of Uppsala, Sweden
1954	Free University of Berlin, West Germany
1955	Technical University of Berlin, West Germany
1956	Lowell Technological Institute
1960	Technical University of Munich, West Germany
1962	Gutenberg University, Mainz, West Germany
1964	Karl Franzens University, Graz, Austria
1965	Technische Hochschüle, Vienna, Austria
1965	Polytechnic Institute of Brooklyn
1965	Charles University, Prague, Czechoslovakia
1971	Jassy University, Rumania
1973	Universidad Autonoma Madrid, Spain
1975	Israel Institute of Technology, Israel
1976	Long Island University, New York
1976	Montan University, Leoben, Austria
1979	University of Nottingham, Great Britain
1980	University of Vienna, Austria
1982	University of Massachusetts
1987	Universite Claude Bernard, Lyon, France

Elected and Honorary Membership

1933	Austrian Academy
1934	Bucharest Academy
1936	Chemical Society of Madrid
1937	Chemical Society of Bucharest
1937	Austrian Society of Textile Chemists and Colorists
1937	Austrian Society for X-Ray Research
1938	Budapest Academy
1943	New York Academy of Science
1944	American Institute of Physics
1947	Royal Institution of Great Britain
1947	Max Planck Society
1949	American Leather Society
1949	Amsterdam Academy
1950	Vienna Physical Chemistry Society
1950	Textile Institute of Great Britain
1950	Indian Academy of Sciences
1951	Austrian Association of Paper Chemists
1952	Austrian Society for Wood Research
1952	National Institute of Science of India
1953	Weizmann Institute of Science
1954	Italian Chemical Society
1956	American Academy of Arts and Sciences
1961	National Academy of Science
1962	Phi Lamba Upsilon Honorary Chemical Society
1965	Plastics Institute of America
1965	Austrian Society for Plastics Technology
1965	The Fiber Society
1966	Soviet Academy of Sciences
1968	International Academy of Wood Science
1971	The Franklin Institute
1972	Society of Polymer Science of Japan
1973	Indian Chemical Society
1974	Croatian Society of Plastics Engineers
1976	Plastics Hall of Fame
1977	The Chemists Club
1977	The New York Academy of Sciences
1978	Chemical Society of Japan
1978	American Institute of Chemists
1979	Yugoslav Society of Plastics and Rubber Engineers
1979	Indian Society for Polymer Science
1979	The Royal Institute of Chemistry
1980	Gesellschaft für Chemiewirtschaft, Vienna
1981	American Society for Testing Materials
1985	The Textile Institute of Great Britain, Honorary Fellowship

Orders, Medals and Prizes

1928	Hertz Medal, Germany
1934	Exner Medal, Austria
1937	Medal of the Austrian Society of Textile Chemists

	and Colorists
1948	Harrison Howe Award, American Chemical Society
1948	Franqui Medaille, Belgium
1953	Honor Scroll of the American Institute of Chemists
1953	Legion d'Honneur
1954	Medal of Honor, Milan Polytechnic Institute
1955	Honorary Fellow, University of Vienna
1955	Golden Honor Medal, University of Vienna
1955	Trasenster Medal, Association of Belgian Engineers
1960	Nichols Medal, American Chemical Society
1961	Distinguished Service Medal, Syracuse University
1962	International Award, Society of Plastics Engineers
1962	Gold Medal, Indian Association for the Cultivation of Science
1965	Polymer Chemistry Award, American Chemical Society
1965	Olney Medal, American Chemical Society
1966	Austrian Honor Cross in Science and Arts
1966	Cresson Medal
1968	Swinburne Medal, Plastics Institute of Great Britain
1970	City of Vienna Prize for Natural Sciences
1970	Distinguished Service Award, Polytechnic Chapter Sigma Xi
1972	Scientific Achievement Medal Award, City College Alumni Association
1972	Chemical Pioneer Award, American Institute of Chemists
1975	Gibbs Medal, American Chemical Society
1975	Austrian Grand Silver Medal with Star
1975	150th Anniversary Prize, Aachen and Munich Insurance Association
1976	Plastics and Coatings Award, American Chemical Society
1976	Harvey Prize, Israeli Technion
1977	Distinguished Service Award, Polytechnic University of New York
1978	Humboldt Prize
1978	Plastics "Vision" Award, Society of Plastics Engineers
1979	Wolf Prize, Israel
1980	Perkin Medal, Society of Chemical Industry, Great Britain
1980	National Medal of Sciences
1980	Jabotinsky Centennial Medal, Israel
1980	Silver Medal, International Commission for Fiber Science Research, France
1980	Colwyn Medal, Plastics and Rubber Institute, Great Britain
1982	Gold Award, Society for Plastics Technology, Vienna
1982	Polymer Education Award, American Chemical Society
1984	30th Anniversary Lecture Medal, Milan Polytechnic Institute
1985	Gold Merit Medal, International Center for Research on Synthetic Fibers
1986	Mayor's Award of Honor for Science and Technology, City of New York

1987	Bronze Medal, Universite Claude Bernard, Lyon
1987	Medal of the City of Lyon, France
1987	Bronze Medal, Conseil General du Rhone, France
1987	Mayor's award of Honor for Science and Technology, City of Vienna, Austria
1988	Heyrovsky Medal, Czechoslovak Academy of Science
1988	Goodyear Medal, American Chemical Society

ABSTRACT

In this first of three interviews Herman Mark starts with his study of relatively stable free radicals under the direction of Wilhelm Schlenk, first in Vienna and then in Berlin. After a post-doctoral period at the University of Berlin, Mark was invited by Haber to join the Kaiser Wilhelm Institute at Dahlem. There Mark collaborated with Polanyi and other colleagues in using x-ray diffraction to establish the crystal structures of small organic molecules and metals. This work was extended to naturally-occurring organic materials such as cellulose and silk; as a consequence Mark was able to play an important role at the critical 1926 meeting in Düsseldorf which brought together Staudinger and the opponents of the macromolecular hypothesis.

Mark's next move was to I.G. Farben where he established a polymer laboratory and first collaborated with Kurt Meyer, with whom he published the pioneering x-ray crystallographic structure of cellulose. Mark describes the laboratories, research directions and colleagues during his stay at Ludwigshafen. The worsening political climate in Germany prompted Mark to accept a chair at his alma mater; back in Vienna he set up the first comprehensive polymer research and teaching institute. Mark concludes this interview by describing the circumstances of an approach from the Canadian International paper Company and his decision to leave Austria.

The second interview details his experiences in the Canadian paper industry and his early ventures into publishing with the first of the Polymer Monograph series. Mark explains how he was able to resume an academic career by starting the polymer program at Brooklyn Polytechnic Institute, which soon became world-renowned. The war-time years brought new projects and young faculty to Brooklyn. Mark briefly describes this period before going on to the immediate post-war era and the later expansion of the Polymer Research Institute, which forms the introductory section of the final interview. In this interview Mark tells of his part in the formation of the literature of polymer science and technology; journals, monographs, reference books and encyclopedias. Mark's many international collaborations are outlined, spanning a pre-war expedition to a Caucasian glacier to a demonstration of the nylon rope trick to Emperor Hirohito. Finally, Mark refers to his more recent research interests and describes the changes in research funding that have taken place during the past four decades.

INTERVIEWERS

Jeffrey L. Sturchio received an A.B. in history from Princeton University and a Ph.D. in the history and sociology of science from the University of Pennsylvania. He was Associate

Director of the Beckman Center for the History of Chemistry from 1984 to 1988, and has held teaching appointments at the New Jersey Institute of Technology, Rutgers University, and the University of Pennsylvania as well as a fellowship at the Smithsonian National Museum of American History. After a sojourn on the senior staff of the AT&T Archives, Dr, Sturchio joined Merck & Co., Inc. as Corporate Archivist in June 1989.

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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INTERVIEWERS: James J. Bohning and Jeffrey L. Sturchio

LOCATION: Polytechnic University, Brooklyn, New York

DATE: 3 February 1986

BOHNING: Professor Mark, I'd like to begin with your Ph.D. work in Vienna. You have stated that your preferred subject was physical chemistry, and yet you did a thesis in synthetic organic chemistry. Can you tell me how you selected Schlenk and the topic?

MARK: When I enlisted as a student in Vienna in 1917 during the war I was on a so-called study furlough. There were several professors: one was the professor of organic chemistry, Wilhelm Schlenk; one was physical chemistry, Rudolf Wegscheider and the third was analytical chemistry, Adolf Franke. They were the three masters there. The question, of course, for a young man, is personality; who impresses you. I went to maybe courses of ten lectures, ten hours from Schlenk, from Wegscheider and from Franke. There was no question that I would go to Schlenk, organic chemistry, physical, whatever: it was his personality.

So when I returned for good from Italy, where I was a prisoner of war, I enrolled with Schlenk in 1919.

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

MARK: First of all he was a most attractive teacher, a most attractive human being. And he did very exciting research. A lot of the mode of teaching which I have practiced all my life, I learned from him. To be very simple; make experiments, as many as you can; address the people more or less visually and personally; and then always tell a few little interesting stories in between. Don't make it too dry. His research was attractive because it was in a new field. He worked on what we today call free radicals, although the name hadn't been invented then: species with carbon atoms where one valency was still free. And that, of course, was extremely interesting.

After choosing this somewhat extravagant field, Schlenk had to develop the technology to work in it. He was the first man in chemistry to work with the complete exclusion of oxygen and moisture. Organic chemistry, complete exclusion of oxygen, as complete as possible; at that time it was unheard of. It was part physical chemistry, because you had to purify your nitrogen and purify your oxygen to mix them in a certain proportion. In part, inorganic or physical chemistry was necessary to get the materials for your work, and that was the second extremely attractive point. And then he was a very nice man, you know: he loved the arts, he was a good singer himself; he was a good musician. So, these things very soon convinced me that I should go and work with him.

BOHNING: When you finished that work, you followed Schlenk to Berlin in 1921. Had you considered any other possibilities?

MARK: Well, I got my Ph.D. in July of 1921 with a thesis this subject [pentaphenylethyl] (1). My family was in Vienna, where I was engaged to a girl whom I later married. She was Viennese, her family was in Vienna. The Institute of Physics had offered me a kind of a job as a temporary teaching assistant, the same job which Schlenk offered me in Berlin. Vienna in 1921 was a decrepit relic from the breaking up of the Hapsburg monarchy. What had been the capital of a 65 million empire was now the capital of seven, or six and a half million, people. And it wasn't even certain whether Austria would remain independent, or what would happen politically with Austria. On the other hand, Germany, even though they had lost the war, was very strong because they hadn't lost their industry, and they hadn't lost any territory. Well, they had lost Alsace, which was just a small mountainous area, only interesting because of the good wine that grows there. Nobody knows anything about Alsace except those people who like to drink wine. [laughter]

But they hadn't lost anything; Berlin at that time was a blooming city. In fact, you know, in the arts it was the center of the world. They had two operas; at one was [Wilhelm] Furtwängler and at the other was Bruno Walter. There was Max Reinhardt, the producer. Then, of course science; there was [Albert] Einstein and [Max] Planck, and [Max von] Laue; and [Walther] Nernst and [Fritz] Haber, so there was no question, really. So in August or September I moved to Berlin.

BOHNING: You continued essentially the same work you had done with Schlenk.

MARK: Yes. And there we established ourselves; I say "we" because three came with me, so there were four altogether.

BOHNING: Do you remember who those three others were?

MARK: Oh, sure. One was Dr. Hans Ender, and the other was Dr. Max Wolf, and the third was a girl, a Dr. Bertha Benedikt. You know, usually if a professor changes his position, he says, "Well, I need a few people to continue my work." And then everybody says, "Sure, of course." And so we were four. That's why I always say we.

BOHNING: You didn't do any lecturing or teaching, but just research?

MARK: No, I was one of these four assistants and we were supposed to take care of what was called the private laboratory of the professor, which were four or five rooms, very well equipped and where there were still a few people from his predecessor. They worked and finished their theses there. We had nothing to do with them, because they knew better what to do. But Schlenk wanted to build up his own research and that we had to do; we had to install several good machines to purify nitrogen, purify oxygen, and to mix them in certain proportions in order to carry out the experiments on the trivalent carbon, as it was called at that time. Occasionally we helped him, or we were supposed to help him, in his lectures. He did the main lectures in chemistry, experimental lecture where the whole long table was full of experiments or demonstrations. There were quite a few times when we had to put these things on when the time was coming and to take them away after they had been used; kind of lecture assistants. But my main job was to continue the research work; he and I published a second paper on the same subject (2).

BOHNING: When did you first meet Haber?

MARK: I think it was in November, 1921. One day Schlenk called me and said, "Could you come to my office this afternoon at four?" He had his villa right next to the Institute; he had an office at the Institute, but he also had one at home. So I said, "Should I come to your home or should I come here?" He said, "Come to my home." When I went over there at four and opened the door of his room I thought that a fire had broken out. [laughter] The room was full of smoke and the smoke came from two gentlemen. One was Schlenk and the other one was Haber; a portly, elderly gentleman; both smoked cigars. Schlenk told me, "This is Geheimrat Haber" and introduced me as "Dr. Mark who has just come from Vienna with me". He told me that Professor Haber wanted to talk to me. Haber said, "Our German textile industry has had difficulties since the end of the war. We have no first choice on cotton, we have no first choice on wool; we can get those fibers, but only after the British and the French and whoever else has taken the best. Cotton in Liverpool now, and wool in Leeds. We have a little wool in Germany now but nothing to speak of. We started, as you know, some twenty years ago, in 1903, to make the synthetic fiber from cellulose that we now call rayon. But it turns out that the qualities of this fiber are not such that our textile industry can compete with the British, French, Italian, and so on with rayon alone, only when it is blended [with other fibers]. Therefore the Kaiser Wilhelm Institute (which is now the Max Planck Gesellschaft) has decided to establish a research institute on fiber chemistry". They already had ten or twelve research institutes in other disciplines. "We have the money and we have started to build a new institute. Such an institute, of course, needs physicists and physical chemists; but all fibers are organic chemical substances so we need organic chemists. Professor Schlenk said that you might be ready to move. We know that you are interested in physical chemistry but you are a full-blooded organic chemist; you know how to make something; you know how to analyze something. We need such a person". Schlenk, as the gentleman that he always was, said, "Look here, I make the following proposal. Why don't you go out there to Dahlem and stay there for a while. If you like it, you stay; if you don't like it, you come back." Probably he had already talked to Haber about this possibility. Well, of course, that was wonderful: a new horizon and a big new institute with a lot of money, although Schlenk's institute also had a lot of money so there was never a shortage of equipment. So in January or February of 1922 I moved to Dahlem, which is a suburb of Berlin.

STURCHIO: Had you thought, if you can put yourself back before this conversation with Haber, what you had seen as the likely course of your career? Did you think that you would go and become an organic professor somewhere?

MARK: I thought I would start in this field and develop a certain amount of experience and maybe a reputation, and then, as every man when he reaches twenty-eight or thirty, would have to carve out my own area of research; after I had established the fact that I could do it, under the supervision of my teacher. That's the same everywhere, no? At some point, the pupil breaks loose. That's what you have to do, and for me that was the point where I broke loose. However, I broke loose in a different direction.

STURCHIO: Had you had any exposure to x-ray techniques or to polymers at that time or was...

MARK: I knew whatever was known about polymers at that time. Of course I knew what x-rays were and how they can be handled in principle, but I had never worked with them. I had to start from scratch. But I had the great advantage that organic chemistry means really careful experimentation with your own hands, often with very small quantities. You have to pay attention to minute details and that, of course, helps in every discipline; and it helped particularly in the x-ray field because we had to build the x-ray tubes ourselves as they were not commercially available then. It meant a very careful tightening, preserving a high vacuum and such things. It was just miniature experimentation, which I had learned very well in Vienna.

BOHNING: Did you have a prototype x-ray tube to follow or just diagrams from a journal article?

MARK: At that time there were a three or four existing experimental x-ray tubes. X-ray tubes which you had to build yourself, of course with your mechanic; you had to tell him what he should do and then you got it done. One design was a tube developed here in United States by the famous [William D.] Coolidge of General Electric, and another one was a tube that was developed in Sweden by Professor [Assar] Hadding. There were other tubes, but we had the best descriptions from the literature of the Coolidge tube and of the Hadding tube. So we decided that we would look at the literature and then make something which is as similar as possible to both types. By the way, Hadding once visited us; he came from Sweden so that wasn't very far. Thus we had the help of his personal counsel. But Coolidge; Coolidge never came.

BOHNING: Did you have any contact with Coolidge?

MARK: No. Not that I know of; maybe other people had. You see there was a big Kaiser Wilhelm Institute for physics, as well as a large Institute for physical chemistry. So maybe they had; but we had not. We really only copied his tube, which took two or three months before the tubes were actually running. Our director, Professor R. O. Herzog said, "Well, if we want to make progress in the properties of rayon, we ought to know its molecular structure. You boys get hold of cotton and other cellulosic natural materials, and several packs of rayon and try to elucidate their molecular structure with x-rays." He had already started to do that before I came, on the basis of work done by [Michael] Polanyi, who was not an experimentalist and who only got a few diagrams from another collaborator of Herzog, Willie Jancke. It's all in the literature. He had made a few diagrams of cotton and of other cellulosic fibers and on the basis of very scanty information, Polanyi already had made the first step in the elucidation of the structure of cellulose. We started to work with rayon to find out if it had the same structure as cotton. It turned out that it doesn't have the same structure. What happens if one draws the fiber?; the molecules orient: what is the consequence?; the tensile strength increases. And so on and so on. From then on we really used x-rays as a testing method to find out what happens with rayon when it undergoes certain treatments during spinning and after spinning, drying, wetting out and so on. Polanyi was the group leader. The group included a mathematician; that was Karl Weissenberg, another physical chemist, Rudolf Brill, and then me. Polanyi himself was a physical chemist.

We realized that we needed some first-hand information about x-ray results from simple molecules. You don't start climbing Mount Everest before you have climbed a large number of other peaks. So we said, "Well, the simplest substances are metals." So we worked on tin, on zinc, and so on with x-rays. We elucidated their structures. I don't know whether you ever saw the list of the publications? You will see one of the first papers was on the crystal structure of zinc and of tin (3).

[END OF TAPE, SIDE 1A]

MARK: We also proceeded with simple organic molecules like urea and other simple organic molecules which crystallize very well. Here the structure was known so we didn't have to invent it. We had to find out whether we could confirm what others had done with other methods, in order to find out whether the x-ray method is any good at all. Around 1922 to 1924 we continued by working a good deal on cellulose, on silk and on rubber; polymeric materials, but at the same time also using our equipment to become experienced by using it on metals and on simple organic molecules. It was around 1924 when we got the first interesting results on silk, cellulose and rubber and when the laboratory started to get a certain reputation. We'd publish, of course; gave lectures, so it was getting known that there was a group at the Kaiser Wilhelm Institute for Fiber Research in Berlin-Dahlem who have good equipment and know what to do with it and do reasonable, reliable work. It takes a number of years until you establish a high reputation, if ever.

STURCHIO: You're telling us about the series of compounds working up to to the polymeric materials; that implies an awful lot of very detailed and difficult work in the laboratory. And I trust the members of the group were teaching each other, but who were you talking to from other places also using x-ray diffraction techniques? One of the interesting things about a new technique is that it is hard to learn it from the literature.

MARK: Well, of course, right next to our Institute, in the same street, was Haber's Institute for Physical Chemistry. There was Dr. J. Böhm, and Dr. Hans Kautski; and another, [Hans] Zocher. They were all collaborators of Professor Herbert Freundlich, a department head in Haber's institute. Haber was the head and then there were several departments; Freundlich was the department head for colloid chemistry. He was at that time one of the leading colloid chemists, and he had published a big book on Kapillarchemie, as it was called (4). Of course, high polymers and colloids kind of intermingled; he also used x-rays and his x-ray specialist was Dr. Böhm. We always worked together; we told him what he should do and he told us what we should do; two very friendly institutes next to each other. They also worked on the particle size of emulsions and things of this type. So we had another x-ray man next door.

Then of course, we had many visitors and we went to all the conferences. Whenever there was a conference on x-rays, Polanyi, Weissenberg or I went; one of us was there in any event. Now in those days by far the leading laboratories on the use of x-rays for structure determination were in England. The Braggs: William Bragg in London, Lawrence Bragg in Cambridge; and [William T.] Astbury in Leeds: those were the three leading x-ray laboratories. We visited these labs whenever the occasion arose.

I went to Dahlem in 1921 and I left in 1926, so I was there five years and I was in England at least ten times during this period, sometimes just lecturing, sometimes visiting conferences such as the Faraday Society Discussions. So there were very close contacts. Of course we sent them our papers, they sent us their papers; more than that we had personal contact. Bragg came over two or three times and Astbury came over quite a few times. Then in France there was the important x-ray laboratory of Maurice de Broglie. He was the elder brother of Louis; Louis was the wave mechanics man and Maurice a physicist. In fact, he was President of the Academy [of Sciences]. And there were a number of people who worked with him; one of them was Jean-Jacques Trillat, another was very famous, Pierre Auger; and several others. They worked really not so much on crystal structure, more on the nature of x-rays. We used the x-rays to investigate crystals; they used crystals to investigate x-rays.

STURCHIO: Were there any American visitors to your laboratory during that five-year period?

MARK: [Arthur H.] Compton was there once. I'm sure there were, maybe not in our laboratory but certainly somewhere else. And maybe there were some when I wasn't there.

STURCHIO: I was just thinking that the Caltech school by the mid-twenties was using x-ray diffraction and people like Pauling had been over.

MARK: Yes. [Roscoe G.] Dickenson; Pauling came a little bit later. Dickenson, of course, was working with x-rays at the same time. Now, Dahlem and California are a little far apart. We had no planes at that time, so when you wanted to get across the ocean you had to take a steamer. For us it was practically impossible to go to the United States. It was not for the scientists here, but nevertheless, it was a long, long way to go. We had very many visitors from the Soviet Union because Leningrad and Berlin were very close; by train an overnight trip. There was work in the same field, and at that time Germany and the Soviet Union were very close politically. I remember [Abram F.] Joffé visited us as well as [Z. A.] Rogovin and [N. N.] Semenov. Many; I could tell you a dozen names, later on, if you want to have more.

STURCHIO: Of course.

MARK: Rogovin and others. Well they didn't really work much with x-rays but they were very much interested in fibers. They had an x-ray lab but for one reason or another it didn't amount to very much in terms of its output. So that was what happened in those years in Dahlem. Now then there was a sideline. I don't know whether you would be interested in this. Professor [Gerald J.] Horton at Harvard is very much interested because it has to do with the Compton effect and the interest of Einstein in our laboratory. Now if you are interested in that I can send you my letter to him, where I describe this whole thing (5).

BOHNING: Oh, yes.

MARK: You see the story is the following. In 1923 Compton published his famous experiment which was the direct and incontrovertible proof for Einstein's light quantum theory, for which he just had got the Nobel Prize. He was a little bit jittery that someone would come along and say that it was all wrong. Very soon after this publication a famous physicist in Harvard, [William] Duane, tried, unsuccessfully, to repeat the Compton experiment; tried and tried and got all kinds of scattering, but not the Compton scatter. Finally he published a note in the proceedings of the National Academy that he had tried to repeat Compton's experiment but he was unable to and he thought that the effect doesn't exist. Now, here is Harvard, here is St. Louis; you know Compton was in St. Louis. So, there was great excitement; maybe the Compton Effect is not correct. A man like Duane says so; in any event something has to be done. So Einstein came, he knew that we were working with x-rays, and asked, "Could you try to make the experiment work? Can you confirm it or not?" So we set out to do it and finally confirmed it (6). That was the first clear cut confirmation of the Compton Effect. Now, Compton, of course, never doubted; he had repeated it several times, had always obtained the same results, but it's always nice if something found in one laboratory is confirmed in a laboratory which is seven thousand miles away. Einstein was very happy. That was really a kind of a sideline, but it was Einstein. Nobody cared much then because Einstein was not yet a very great man. He had just got the Nobel Prize and he had published the theory of relativity which nobody understood. Nobody understood either the light quantum theory or the theory of relativity; all physicists were highly doubtful whether it was all a hoax. A Jewish hoax as they said, but anyway, later on, of course, things changed.

STURCHIO: That anecdote gets us into other events that were going on in Berlin at the time, I mean, not only was Einstein working on relativity theory, but a little bit later there was [Erwin] Schrödinger's work on wave theory. This was not all going on in Berlin, but I wondered what relation you had with the development of the new quantum theory in the mid-twenties.

MARK: Well, there was this one experiment; the light quantum theory and the Schrödinger theory are the same. It all means that particles can be particles, but they can also be waves; and waves can be waves but they can also be particles. At the same time, completely independent of our work, there was this tremendous development, this move over from classical physics to quantum physics which took place under our eyes; we didn't contribute anything to it, but we were all the time enormously excited. We visited the many symposia and seminars which were held in Berlin or in Göttingen because that was the real important change in physics, which took place from 1900 to 1930. And of course Louis de Broglie was a very important part of it, Schrödinger was another very important part of it, and [Werner] Heisenberg and [Niels] Bohr. I think, Bohr was the man who really pulled the wires of the whole thing. When Heisenberg [announced his uncertainty principle] he just told him, "My dear Heisenberg, it's time that you made an important contribution." [laughter] So he knew how to handle these youngsters.

He was one; [Arnold] Sommerfeld in Munich was the other one; that all took place during this time. It greatly enhanced the reputation of physics altogether, even to the public. Although, you see, the public didn't care very much. Well, the public cared nothing about the theory of relativity and whether an electron can also act like a wave or a wave can also act like an electron, nobody could care less. The public in those days were extremely interested in a new electric lamp which Seimens or Coolidge made. In the improvement of the telephone, improvement of the diesel motor, improvement of all those things. But fundamental physics became really generally attractive only after atomic energy came in. Then of course it was obvious that all this goes really down to our bones; it's sometimes nice, sometimes not so nice.

STURCHIO: We noticed in your list of publications that you published a couple of articles on atomic structure and quantum theory (7); does this come out of your general interest in this topic?

MARK: No, that all came from these experiments on the Compton Effect. Dr. [Hartmut] Kallmann and I did this work. Dr. Leo Szilard also worked in our area; he was always very interested in everything; among other things he was always interested in x-rays. I published two articles with him (8).

STURCHIO: How did that collaboration come about? Did he come to you, or...

MARK: Well, Polanyi was from Hungary. And he brought with him, in his wake... Well, Hungary at that time was in a very destitute situation; a man called Bela Kun who was a communist was in charge. For a while Hungary was a communist country so everybody tried to get out; everybody who had the possibility or the means came out. Szilard was one of them, Eugene Wigner was another one of them; Edward Teller was another one. There were many others; John von Neumann, Theodore von Karman. So we had a very good influx of those Hungarian boys, and they all visited the Kaiser Wilhelm Institute, because it was very international and Polanyi played a big role there. One day he came with a young man and introduced Szilard who immediately got very friendly; he was always a very active fellow. "What are you doing here?"; and we explained it to him. "Oh, well you shouldn't do it that way, you should do it the other way." [laughter] We never took him seriously, you see. But it was a very nice cooperation. My wife liked him very much because he was such a free, outgoing guy. We published two articles together and one with Wigner also (9). Later they both got their Ph.D.s. One, I think Szilard, got it with Planck at the University, or with Laue. Wigner at Charlottenburg with Professor [M.] Volmer. Von Karman was already in Aachen at that time. John von Neumann was never at the Kaiser Wilhelm Institute. He was a mathematician and we didn't have much contact with mathematics.

STURCHIO: You also published an article or two with Polanyi in those years.

MARK: Yes, on x-rays (10); he had it all started in 1912. He was the first man who had the idea that x-rays would be scattered by crystals and worked the theory out quantitatively, which was a tremendous job.

BOHNING: Did you have any interaction with Nernst when you were in Berlin?

MARK: Not directly. Of course, he was high up, he was one of the Geheimrats; it was very interesting. To young, innocent people like me, he was a very pleasant man; he was a sugar daddy. But to his colleagues, he was a devil. [laughter] We often visited his Institute, because the seminars were held there and when we met him in the corridor or somewhere, he would say, "Well, are you doing nice work out there in Dahlem? Can you tell me a little bit about it?" He would be very fatherly as it were, very nice. But apparently that was only to people who couldn't hurt him.

BOHNING: And Haber of course was at the top. Did you feel his imprint anywhere as you were working?

MARK: Haber was at the top, he was a very busy man. He was on so many committees and subcommittees and of course he was one of the vice presidents of the Kaiser Wilhelm Gesellschaft, he was a president of the German Chemical Society, and what not. He came to the Institute but not very frequently. Now, he always was nice, he was nice with everybody.

STURCHIO: Obviously it was a very lively and exciting time scientifically. Did you spend all of your time in the laboratory?

MARK: Well, most of it. But you must not forget that I married in 1922. Then there were all the theaters and all the movies and all the concerts. Oh, at least every other day we were in the city; twenty minutes on the subway. Either to a concert or a show. That was fantastic. All the later operettas of Franz Lehar. Lehar moved from Vienna to Berlin immediately after the war for the same reason that I did. He saw the greener pastures there and produced these various operettas, I don't know what the names are; each one was a big sensation and beautifully produced and performed with all the famous singers and conductors there at that time. In other words, the life, arts, sciences, but also museums and expositions, sports. Berlin in the twenties, let us say from 1922 to 1930, was really tops.

STURCHIO: I remember in one of your articles you mentioned that research at the institute wasn't very much affected by the hyperinflation of 1923 because there were grants from Japan and the U.S. that helped to pay for things in yen and dollars. How did that affect your personal life?

MARK: Well, it was like this. There are really two questions. Number one, how about politics? Have you been ever interested in politics? After all, there were tremendous political changes and upheavals in Germany during this period. My answer was and is, that, at least in our little, quiet scientific enclave, we had so little confidence in whoever happened to be the Chancellor or the Vice-Chancellor, I don't know what the names were, President, that we couldn't possibly care because, in fact, after a few weeks he was either murdered or he resigned. It was a state of affairs of such irregularity that you couldn't make any choices; because if you said, "OK, I'm very much interested in the program of this party," a few weeks later the party didn't exist anymore. We deliberately kept from getting involved. One knew about it, because you read it in the papers everyday. Of course, there was no radio, there was no television, so the newspapers were still the main or the only source of information.

About inflation, it was a big problem at home. Because of these funds which came from outside, it wasn't so much of a problem for our research. At home; well at that time, our salary would be given to us every week. So then my wife immediately ran out with a pocket full of money and came back with a packet, a very small packet, full of food for the week. That was a problem. Yes.

STURCHIO: Where did the research funds from the U.S. come? Was that Rockefeller money, or Guggenheim?

MARK: The U.S. money, I think, at that time was essentially from Rockefeller for physics and chemistry. I think Guggenheim was for the fine arts and maybe for mathematics, I don't know. Of course, there was the Hoover plan at that time, which poured a lot of money into Germany.

[END OF TAPE, SIDE 2A]

BOHNING: There was a meeting in Düsseldorf in 1926 (11). Is that the first time you met Staudinger? How did your participation in that meeting come about?

MARK: Sometime in 1926, probably in the spring; it was a time when we were working on x-ray applications and when we had published a dozen articles on cellulose, rubber, silk and on other materials, starch and such things. Haber called me into his office and asked, "Do you know the work of Staudinger?" I said, "Of course." "And do you also know that there is considerable opposition to it?" "Of course." I could say that because the seminars at the Haber Institute covered many topics other than physics. Actually [Hermann] Staudinger was there to give a lecture in 1925, or thereabouts. Colloid chemists like Freundlich and Herzog did not agree with his theory of very long macromolecules, but thought that the phenomena could be explained more easily with colloidal chemistry concepts. There was a discussion as after every seminar. So I was aware; perhaps I wasn't very well aware, but I was aware of the fact.

Then he said, "This is evidently an important thing. But, very well-known chemists like [Paul] Karrer, [Hans] Pringsheim, and [Max] Bergmann oppose Staudinger's views. So the German Chemical Society has decided to organize a symposium at their meeting in Düsseldorf in September 1926. Staudinger will tell his story and the others will tell their story. They are all organic chemists, and I am afraid that this controversy goes beyond simple classical organic chemistry. Since you have worked on the structure of these materials by x-rays, which is physics or physical chemistry, I suggest that you participate. You'll get an invitation from the chairman, Professor [Richard] Willstätter, and I suggest that you accept". A few days later I got a letter from Willstätter, and of course, I accepted the invitation. "It will be a great pleasure and honor for me and so on."

Then I prepared myself for the lecture by reading a little more about Staudinger's articles and by reading a little more about the articles of the opposition. The situation was appalling. There were three types of opposing views. One was a kind of a subjective view of bad feeling. The classical organic chemists like [Heinrich] Wieland or Willstätter had worked all their lives with molecules having molecular weights between three hundred and five hundred; and they had much experience with these little things. They just couldn't swallow it that somebody should come and say, "I'm working with molecules which have a molecular weights of five hundred thousand." So that was a more or less personal view. The next group were the colloid chemists, Freundlich, Herzog and others who said, "Well, the phenomena are there; there's no question about that; high viscosities and gel formation. But we feel that they can be explained on the basis of known phenomena in colloid science and one doesn't need the extravagant hypothesis of a molecule with a molecular weight of five hundred thousand." In other words, they were nearer to reality. They didn't say, "We don't like it,"; they said,"We don't need it." There was a third group, the crystallographers. Their argument was this: it has been established, mainly in our laboratory by Dr. Rudolf Brill, Dr. [Johann R.] Katz and myself, that the elementary cells of rubber, cellulose, and silk are small; so small that only a molecule of a molecular weight of about five hundred could be accommodated inside it. Then this group said that crystallography shows that the molecules can't be larger than the elementary cell: therefore, the molecules must be small. Thus it can't be. The first was, "We don't like it," the second was, "It's not necessary" and the third was, "It's impossible."

Well, of course, Haber had sensed that the crystallographers making this statement had never worked with these materials. They were purists; what was true for rock salt, for diamond or for sulfur must be true for all crystals. Now I had worked with both classes of these materials and had the firm conviction that this tenet was not correct. That it was possible for a chain or planar molecule to be larger than the elementary cell because the periodicity which the crystal demands is within the molecule. In other words, if a certain periodicity is in the molecule, it can be larger than the elementary cell because periodicity is exactly what crystallography is about. So there was the meeting in Düsseldorf, with Willstätter as the chairman and I think what happened has been described in detail. But if you want me to tell it to you again, I'll be glad.

BOHNING: Well, I was interested in your interaction with Staudinger. Who spoke first, did he speak first?

MARK: No. Willstätter opened the meeting and said that he was very happy that everybody invited was here. "You all know that Mr. Staudinger has a very interesting hypotheses of the existence of macromolecules, and now we have here a number of people who have worked with these things with their own hands for years and years and who have their own experience and their own opinions. So maybe we should start with Professor Bergmann, who has worked on proteins, then we will ask Professor Pringsheim who has worked on polysaccharides and then Professor [Ernst] Waldschmidt-Leitz, who has also worked on some classes of these materials". I think there were three or four speakers. "After we have heard, so to say, the classical point of view, then we will ask Professor Staudinger to speak." So that was what happened.

Then I was called in to explain what the recent work on x-rays had to do with it. My essential contribution was that the presently existing experimental evidence in the x-ray field cannot prove that macromolecules exist, but it also cannot prove that they cannot exist. It was a kind of a soft position, but it was true and it took away the edge of the third group which said, "It can't be," by saying, "It can be." Other features at that time could not be elucidated because we didn't have such good x-ray diagrams as we had a few years later when the whole position was completely clarified. So that was my position; that the present state of x-ray analysis of organic molecules including cellulose, rubber and silk, cannot draw the consequence that there must be such long chains, but it can draw the consequence that there could be such long chains.

At the end Willstätter got up and said, "Thank you, thank you." And then he said, "Well, as a classical organic chemist, I don't like these big molecules very much myself. But if Professor Staudinger brings additional experimental proof, we will probably have to accommodate ourselves to them." In other words, he pointed out that in his opinion, at that time, the experimental proof was not yet sufficient. But as an open-minded scientist, he expressed his opinion that if additional proof would be established, the way would be open. Now, it was very unfortunate that neither the discussion nor his final words were printed in the Journal (12). The present formulation of his final words is only in my memory because Staudinger never said anything about it because he was disappointed, although they didn't say, "You're a damned rat." The opposing groups didn't say anything because of the same reason. So I was the only one; I didn't care very much, you know, but I was the only one who took it down. But I well remember that he said, "I don't like it personally. But if additional proof would be presented, as a scientist, I'll accept it." That was essentially what he said, as everybody would expect a Nobel Prize winner to say.

STURCHIO: What were your own views at that time. In the papers you came down squarely in the middle, saying that it was...

MARK: I came down to the view that we cannot prove it but that x-rays don't disprove it either.

STURCHIO: Can you remember when it was that you began to think that Staudinger was right? That the proof was...

MARK: Two years later, as soon as we had worked out the x-ray diagrams quantitatively in detail. Dr. [Kurt H.] Meyer and I (13).

STURCHIO: And that was after you'd gone to I.G. Farben?

MARK: Yes.

STURCHIO: Had you ever been enthusiastic about the micellar theory and the colloid view?

MARK: I must say at that point it was for me an interesting controversy. I couldn't care less. I wasn't an organic chemist. If somebody would have asked me, "Do you think that molecules with a molecular weight of five hundred thousand are possible?" I would say, "Why not?" And if somebody would have asked me, "These causative phenomena, these dilation phenomena; do you think they could be explained by the assumption of small particles which are aggregated?" I would say, "Yes, why not." So I was really in the middle. I didn't want to be but I didn't have any legitimate experimental evidence for either one or for the other. So I had to sit on the fence; not because I wanted to sit there, but there was no other place for me to sit.

STURCHIO: Well, this might be a good time, then, to talk about your move from the Kaiser Wilhelm Institute to I.G. Farben.

MARK: Yes. Well, so that was then September, 1926. Earlier, already in June or July, Haber had called me to his office. You know, there were three private discussions which shaped my life. One was Haber and Schlenk, one was Haber and Meyer, and one was with Gaus. Those were the three turning points, you see. We are at the second one now. Haber said, "This is my friend Professor K. H. Meyer. He is on the board of directors of I.G. Farben." And Meyer said the following, "Look here, you have worked now for five or six years in an atmosphere of highly scientific activities, with new methods, and in a field which has great importance for industry, namely, fiber-forming polymers. We, I.G. Farben, are actually making these fibers; we make cellulose acetate and we make rayon. And we are just exactly in the position which Geheimrat Haber described several years ago. We can't compete with the natural fibers because we are not good enough. Our wet strength is not good enough, our abrasion resistance is not good enough. We have other advantages, the luster is beautiful, dyeing is very easy, but abrasion resistance and wet strength are inferior. Now you have worked fundamentally on those things; therefore we have decided to establish a fundamental research laboratory on fibers. On these fibers. On all fibers." At that time we did not yet use the word polymers, because nobody was sure if that was correct. "Until now you have laid the ground work for an important industrial activity. We want you to come; I'll give you a laboratory, you'll have no trouble in getting people, we'll hire those you need, you'll get all the necessary equipment, space and so on. Your salary will be satisfactory, so think it over."

So, I looked at Haber, and Haber said the same thing as Schlenk had said five or six years ago, "Why don't you go there, and if you like it, stay, and if you don't like it, come back." So on January 1, 1927, I moved to Ludwigshafen. I took a number of people from Berlin with me since there was no x-ray work there at that time; so we had to set it up. Machinery, space, laboratories, everything. That took half a year or so, until results were coming along. Of course, we got a lot of samples from the plant, which was producing at a rate of maybe fifty tons a day. There was an induction period when we couldn't possibly produce any results until all of us had worked ourselves into the proper frame of mind. At this time, Dr. Otto Schmidt, who had worked on synthetic rubber, was at the plant. The same situation which existed for fibers also existed for rubbers. Germany had no source for rubber and would need it, war or no war; particularly for electrical and automobile applications. We didn't think about the war at that time.

Dr. Schmidt and I worked closely together; he told me what he knew, and I told him what I knew. He had worked on natural rubber. We felt that the synthetic and natural rubber were very close. Of course, there were no synthetic fibers at that time. But synthetic rubbers existed, early types, but it was obvious that, eventually, once one understood the structure of the natural materials, one could make them synthetically. Therefore, I soon suggested to Professor Meyer that we use our fundamental methods not only on the materials which I.G. Farben produces now, but also on materials which the company might produce in the future. Materials which are also high molecular or colloidal, which belong to the same class. We don't yet know which; at that time Meyer and I were already leaning pretty much to the Staudinger point of view. I suggested to him, "Let's not call it the Laboratory for Fundamental Fiber Research, but the Laboratory for Fundamental Polymer Research." It wasn't called that then, but it was only a year or two later. But that didn't matter. We started immediately to grab up whatever there was in the company in terms of polystyrene, for instance, polyvinyl acetate, or polyvinyl chloride. They already all existed but nobody knew what to do with them. So we took them into in our investigation.

In 1928 the macromolecular hypothesis was then quite clear, because we had worked out the chain structure of cellulose, which incidentally was independently established by a botanist in California who published it a few weeks before us (14). His name was [Olenus L.] Sponsler, a botanist at the University in Berkeley. He was interested in cellulose from the botanical side and since they had x-ray equipment in Pasadena he asked Dickenson to make x-ray diagrams. Dickenson made very nice x-ray diagrams and Sponsler interpreted them, and he and we arrived at the same result. In other words, we didn't only have the benefit of our own conclusions, but also of completely independent work from far away. I had never heard of Sponsler before; he had never heard of Meyer and Mark. So the cellulose structure can't be a coincidence; it must be true.

From then on, Meyer and I with all our group firmly went to the Staudinger view of macromolecules, of molecules which had molecular weights way up; maybe ten thousand, maybe a hundred thousand. At that time, one couldn't yet measure how large they were. However, even though in principle Meyer and I agreed with Staudinger as far as the existence of very large molecules went, we disagreed on certain properties. There were essentially two points of disagreement. One was that Staudinger visualized these macromolecules as rigid rods. In fact, he gave many lectures; actually this is a sample which Staudinger gave me. [Mark displays a cylindrical model.] "This is cellulose, those are rigid rods." Meyer and I, of course, took the attitude that long chain molecules with interchain single bonds cannot be rigid, but must be flexible. We said, "Well, we agree that these are very large molecules, but we don't agree that they are rigid." This is our model from the same time. [Mark displays a coil model.] Staudinger threw it in a corner. [laughter]

His other argument was, "These materials have a very high viscosity in solution. That can only be understandable if they are really rigid rods. Very difficult to turn around, offering very much opposition to the flow, which is the cause of viscosity." He published that several times, and actually he also developed an equation to derive the molecular weight from the viscosity, assuming rigid rods. We said, "No. This is not so. These are flexible molecules, they form coils, and these coils swirl. And they also produce opposition to flow." Dr. [Werner] Kuhn in Switzerland said the same thing; well I think it was Kuhn who said the same thing at the same time. Oh yes, also Dr. [Eugene] Guth in Vienna. So there were three of us, we were physical chemists of course, but we said, "It is impossible that a long polyethylene molecule can have a rigid structure". Therefore, Dr. [Roelof] Houwink and I, Houwink was from Holland, developed another equation to derive molecular weight from viscosity measurements. That was the substance of our disagreement.

I didn't see how anyone could get so excited about it but, quite unfortunately, Staudinger became rather excited; became rather impolite and wrote nasty letters, so that finally we didn't talk with him anymore. Unfortunately, because we certainly joined his point of view about large molecules. There can be no doubt about that. We did not agree with him about their specific intrinsic properties. Well, now everything is resolved: the large molecules exist; they are not rigid rods but they are flexible. The man who finally brought that all out was Paul Flory.

STURCHIO: From my perspective, at least, the statistical mechanical view of rotation around single bonds, and the notion that was at the heart of Kuhn's work, your early work, and Flory's, makes a lot of sense. How could an organic chemist of Staudinger's stature have really believed that they could have been rigid bonds?

[END OF TAPE, SIDE 3A]

MARK: I talked repeatedly with him about this. He was stubborn.

STURCHIO: It was just a case of somebody seeing things through certain spectacles and not being able to change.

MARK: Once I gave a lecture in Freiburg about these things and I talked about the flexible molecules and then he gave me that rod model. When he gave it to me he said, "Well if you will make enough additional experiments, you will see that I'm right." So I made additional experiments. Well, you know, like many great people, he was a little bit touchy. At that time, you know we are talking about 1928, 1929, he was irascible because of the continued opposition, whereas, on the basis of our x-ray data, we, Meyer and I, joined him. His true old friends the organic chemists did not. They couldn't care less about x-ray data, they just didn't like those things. I think that made him sensitive.

BOHNING: When did Willstätter agree that your point of view was the correct one?

MARK: Well, at the end of the meeting in Düsseldorf, he had said that if additional evidence shows Staudinger to be correct, I will accept. Then he considered that the Meyer and Mark cellulose structure was the additional evidence; which it was.

STURCHIO: I wonder if we could go back for a few minutes to when you went to I.G. Farben and talk about the people you brought with you, and about the structure of the lab, and that sort of thing.

MARK: We had three labs, each of which probably of the size of this whole floor. Some twenty individual rooms. One was essentially for the synthesis of whatever we needed, because we did not only work on polymers, but also on detergents, on surfactants, on dyestuffs; in other words, they were organic chemical laboratories equipped for the synthesis of new materials. Another was for characterization; it was a little bit smaller, maybe half of this floor; x-rays, electron diffraction, viscosity measurements, osmotic measurements. We had an ultracentrifuge and electrophoresis. In other words, it was a well-equipped colloid chemistry laboratory, plus the x-ray and electrons. The third was a Technion, as we called it; a sturdier building with larger halls where we could carry out experiments on a somewhat larger scale. Let us say, when we first made a filament from polyvinyl chloride. You know, if you have a spinning machine, you need at least ten kilos, twenty pounds, in order to get it running at all. After that, during the first hour, you make chewing gum, because you know nothing: you don't know the temperatures, you don't know the speed, nothing. Only when it starts coming out as a fiber can you start collecting it. You have to have fifty pounds of a material. So this was a Technion where we could make fifty pound lots of Buna S and Buna N -- those were the rubbers -- polystyrene, polyvinyl chloride, polyvinyl acetate, and so on, in order to be able to investigate them.

STURCHIO: Could you say a little bit about who ran each of those laboratories.

MARK: Yes. The laboratory for organic synthesis was run by Dr. Heinrich Hopff who later became director at the Laboratory at Ludwigshafen and finally was a professor in Zurich. The laboratory for characterization was under Dr. Karl Wolf, also later director. And the Technion was under the direction of Dr. Manfred Dunkel. Those were the group leaders, so to speak.

STURCHIO: What was the next level of management above that?

MARK: Well, there was another research laboratory under Otto Schmidt. He specialized on polyhydrocarbons, on synthetic rubbers. We all worked on polymers. He worked on a larger scale because he was already a step further ahead in development and made, I don't know how much, but several hundred pounds a day. Those were the two research laboratories. Above us was Professor Kurt H. Meyer. He was a member of the board, he represented us on the board. There was another board member for inorganic chemistry, another for high pressure chemistry, for sales, and so on; six members of the board. At the top of was the chairman of the board; that was Dr. Gaus. That was the organization.

STURCHIO: You said that, in the article that you published in J. Chem. Ed. [Journal of Chemical Education] (15), by the time you left I.G. Farben in the thirties...

MARK: 1932.

STURCHIO: ...you had about fifty people working for you. Could you just talk about the evolution of your organization?

MARK: We started probably with, on the average, ten in every of these three sections. What grew mainly was the Technion, as we had to produce larger and larger quantities. There must have been about fifty, somewhere in the neighborhood of fifty.

STURCHIO: So your group was responsible not just for the basic research as it were, the fundamental research in the material, but also for developing the actual fibers?

MARK: Well, no. Our spinning, casting, molding equipment was only for producing samples; samples to establish properties. Then comes an entirely different step, namely an engineering step, to scale up to a quantity, per day, let us say a ton, which will give you not only the technical characteristics, but the commercial feasibility of the whole thing. Will you have enough customers; what can you charge; what will they pay; how much does your business cost; what will your profit be? This was a special department.

STURCHIO: The Technion would establish that a fiber had characteristics that looked promising.

MARK: The Technion was really only there to produce samples for characterization.

STURCHIO: Was there much interaction with the group that worked on commercial development?

MARK: Yes, yes, permanently. The next step was supervised by Dr. Biedenkopf, whose son Kurt is now a famous politician in Germany and you may have read about him. Biedenkopf was a full-blooded engineer; and he had a pilot unit building, which was much larger, of course. High halls with pumps and stills, and pressure kettles and stirring kettles and so on. He would come to our Technion and look at a process and then change it into what he thought was necessary for commercial production.

STURCHIO: Was your laboratory ever called in for trouble-shooting, if there was a problem in the factory with spinning a particular kind of fiber.

MARK: Whenever there was a difficulty. Yes. Usually together with somebody who knew about spinning; we would take samples and the spinning unit was run under somewhat different conditions, different speed, different temperature, different pressure, and we would take samples from those different conditions. Back in Ludwigshafen we would investigate them, by x-rays and also their other properties, and then go back, or go to the phone and describe what we found and what we suggested to improve the situation. Intensive studies of rubber, polystyrene, rayon, and cellulose acetate. First only with rayon and cellulose acetate, and then these two other things.

STURCHIO: Sounds that it might have taken up a fair amount of time doing that kind of work.

MARK: Well, I was there five years. Six, almost six, five and a half years. Of course, I wasn't alone because I had a lot of very good people who did a lot of it.

STURCHIO: Could you talk a little bit about the difference between an industrial laboratory like I.G. Farben and the more or less academic environment that you had come out of in Berlin. Did you find there were real differences in the way that the work was organized and carried out?

MARK: Well, of course, it was a kind of a different approach, but not much different. Actually, if you were to look at the laboratory, you couldn't say that this is a fundamental laboratory whereas this is a laboratory which is also interested in applications. The instruments were the same and the equipment was the same; the substances were the same. In Ludwigshafen we made many more measurements, concerning for instance, yield. A chemist who is interested in making a new substance, never before known on earth, is happy just to get it; if he gets ten percent yield or twelve percent yield or so on. When we had a reaction with ten percent yield, we said, "Oh, we must change something. We must get at least eighty percent yield." Otherwise we can throw it away immediately. So there were different aspects.

Or, for example, resistance against moisture. In Berlin, if we spun, or if we investigated a cellulosic fiber which was very sensitive against moisture, we didn't care; we just wanted it for the structure. If we got a fiber at Ludwigshafen which was very moisture sensitive we threw it away, because it wouldn't be of any use. Maybe we would make an x-ray diagram of it, just in order to see whether this sensitivity could be explained by the structure. It was just a different approach.

STURCHIO: You described earlier the way you were following in the program of research for the x-ray work in the early twenties, working out the relation between x-ray pattern and structure. How was the work planned at I.G. Farben? Was there direction from above or did you still pretty much follow the problems as you saw them?

MARK: Yes, there was some, but in the more classical domains, because our organic laboratory under Dr. Hopff also took care of dyestuffs, detergents and plasticizers, small molecules. A production unit existed for each of these substances. We had a big plant which made detergents, and a very big plant which made dyestuffs; several. Also we had a very big plant which made plasticizers or lubricants. All the research sections had very close contacts with the production units who were told, or it was suggested to them, that they should do this and that; and this they did, of course, very well. But in the new field of polymers there was no production unit yet. So, we were more or less left alone.

STURCHIO: Well, you were building new lines of business.

MARK: Yes, we were building up a new business.

STURCHIO: Presumably you maintained your contacts with your academic colleagues in Germany and in Austria.

MARK: Yes. The company liked the idea that their fundamental group would be considered to be really high level academically. So [Raimund] Wierl, and [Karl] Wolf, and Hopff, and Dunkel, and [G.] von Susich, and [Emerich] Valko, and many others maintained their contacts with x-rays, with electrons, with colloid chemistry, and so on; went to conferences and lectures. We published quite a bit; you will see from the list that quite a few publications came out from the laboratory during these years (16).

BOHNING: What about patents?

MARK: Patents, yes. Many; I don't know how many. Maybe eighty or one hundred. Yes, a lot. I don't know whether they are mentioned in this publication list. Maybe they are not.

STURCHIO: 1927, when you went to Ludwigshafen, was also the year when Du Pont was setting up a fundamental research group in Wilmington and hired Carothers.

MARK: I think even a little bit earlier.

STURCHIO: Well, it was the end of 1926 that they began to discuss this.

MARK: Well, about the same time.

STURCHIO: Where you aware of the work at Du Pont?

MARK: [Elmer K.] Bolton. He was the man; he had the same feeling as Meyer.

STURCHIO: Was there any contact between...

MARK: Later. At the beginning there was none; the first publications of [Wallace H.] Carothers on polyesters appeared in 1929 or 1930, and we read them immediately. We immediately saw their importance and I sent to him whatever reprints we had, and he sent us his reprints. We had a very good rapport with him.

STURCHIO: Were there any visits from Wilmington while you were at I.G. Farben?

MARK: No. I think that at that time Carothers had not been to Europe. Later we met in England, and he visited me in Vienna, but that was 1936. This is a very nice historic book made when the first kilo of polystyrene was sold. [Mark displays a commemorative volume.] Many of the people mentioned there actually contributed to that development.

STURCHIO: It would be nice if we could borrow that or get a reference to it. Besides Carother's work at Du Pont, were there other industrial companies whose work your group was following?

MARK: Well; there were many other industrial companies engaged in rubber technology and in cellulosic and fiber technology, but along classic lines. It was wonderful for us, because when I came to Berlin, even when I came to Ludwigshafen, that there were fiber chemists, cellulose chemists, a Society for Cellulose Research, books on cellulose, journals on cellulose. The same thing for silk, the same thing for wool, for rubber and for starch. So there were five different disciplines, strongly represented by people, by literature, books, societies, and so on. But for us it was all the same; all were long molecules carrying different groups. Therefore, one was a fiber, one was a rubber, and one was a fiber with such properties, and one was a plastic. You know, it was as with the astronomers. For centuries, Jupiter, Saturn, and Uranus; each was a special world. After Copernicus, it was all the same. I mean, that such a general principle simplifies things tremendously. Quantum mechanics is essentially that, isn't it? Quantum mechanics is essentially a tremendous simplification of the complexity of classical physics. But it takes a long time to realize that and to use it.

STURCHIO: Over the course of the time that you were at I.G. Farben, over that five-year period, did you see evidence that the management view of the polymer work was changing?

MARK: Improving, yes. You know, these directors of classical organic chemistry had made tremendous amounts of money with ammonia, and with fertilizers, and with fibers, and with dyestuffs and such things; they were very critical and very, very cautious people. They said, "Fine, fine, fine. You come up with something new. If you can show us that the whole development is good, we'll step in. But we're not going to do so before that." They were very reasonable, critical, but they never hyper-critical.

STURCHIO: Maybe it would make sense to talk a little bit about synthetic rubbers, because that was something that I.G. Farben was also very much involved in. A little bit later, but there was still work going on.

MARK: No, not later. It started at that time. We had also worked in Dahlem on rubber, of course, because of Dr. [J. R.] Katz, who came as a visitor to us. He was the discoverer of the crystallizability of rubber; and he needed x-rays, so we worked with him. Germany industry took the attitude, as in the fiber field, since we have no access to natural rubber we ought to do something along synthetic lines. Leverkusen, Bayer took the lead. Already, I think in 1915, they started work on the polymerization of isoprene and butadiene. Dr. Fritz Hoffman and a whole group of people. Do you know the book of Herbert Morawetz (17)? There he describes the rubber development quite nicely. But these rubbers were not very good. So there was Leverkusen with classical organic chemistry. They polymerized something; well, it was a material but it didn't have much abrasion resistance. We were here at Ludwigshafen and we believed the polymer theory, so we told them, "Your material has a low molecular weight. Give it a higher molecular weight and it will be better." Then they said, "Well, first you do it and then show it to us." That was the reason why some of the rubber work was done in Ludwigshafen. It became a big field and there were several types; one was a copolymer of butadiene and styrene, the other butadiene and acrylonitrile, and others. This was done in Leverkusen and in Ludwigshafen.

STURCHIO: Could we talk a little bit more about the state of the work in 1933, and especially the Mark-Wulff process?

MARK: Well, that was just styrene.

STURCHIO: Could you just talk a little bit more about the details of that?

MARK: We became convinced sometime in 1928 or 1929 that polystyrene is a good material: well you know, if you go on an airplane all the cups are polystyrene. Our customers liked our samples. Then we said, "Well, we can't start a building on the fifth floor. We have to go all the way down and make the monomer. Right now the monomer is a laboratory curiosity." It was made by splitting off HCl from chloroethyl benzene in xylene. "So let's try to make it by the direct reaction of benzene and ethylene." And we first did it in the laboratory and it worked all right with good yields. Wulff and [Eugene] Dorrer and Dunkel were the three who worked on that (18). It was clear that the reaction as such was possible but it wasn't an easy reaction. It was a reaction between two very combustible gases at seven hundred degrees centigrade. Benzene and ethylene under pressure with a catalyst. Well, we had a large number of small autoclaves where we could do that easily. I don't know what the pressure was, maybe two hundred atmospheres or something like that. But the question was how to do it on a larger scale?

[END OF TAPE, SIDE 4A]

MARK: Biedenkopf helped us to build up the first pilot plant for styrene monomer. Wulff, Dorrer, Biedenkopf, Dunkel and I cooperated, labored and sweated there for many, many weeks. You know, when you do something like that on a larger scale, it is safety, safety, safety. Fire, oh, God.

STURCHIO: How large a scale were you working on?

MARK: Eventually, our first pilot plant would make about a hundred kilo a day, enough to go on and study whether the product is any good. Those who run such a pilot plant, maybe four or five chemists or engineers, learn a great deal, because everyday something goes wrong or nearly so. So after three months, things are under control. Then more is needed by the laboratory. They then would say, "Okay. Now you can have more, unlike three months ago." During those three months the pilot plant delivered a hundred and fifty kilos, or maybe two hundred kilos of monomer which we polymerized and could establish the properties of the polymer fairly well. The next step was a larger scale polymerization unit, which was easy because the reaction takes place at a hundred degrees centigrade at normal pressure, for which we had the details fairly well in hand from our laboratory experiments. Initially, that was really no problem.

BOHNING: You had mentioned previously about concerns of laboratory safety and about safety in the plant at that time.

MARK: There was a lot. You see in 1923, there had been a catastrophic explosion of the plant in Oppau. That was a fertilizer plant where they made calcium and ammonium nitrates in large quantities which blew up. The silo blew up one night; killed three hundred people. The whole hamlet, you know; Oppau was just gone. In terms of numbers for a single accident it was one of the largest catastrophes in the German chemical industry. Therefore I.G. Farben were very conscious of safety.

STURCHIO: During this time you also began to write the first edition of your book in collaboration with Meyer.

MARK: Yes. Well, in 1928 we published four or five articles and we felt that we had enough material to write a comprehensive book which would demonstrate to the chemical community that macromolecules actually exist, without using Staudinger's arguments but using our own. The book really was a classic. When it came out in 1930 it was the first book in the field (19).

STURCHIO: Can you recall some of the reactions or some of the reviews?

MARK: Well, the reviews were very, very good. Only Staudinger didn't like it. [laughter] Well, I'll tell you. The real reason Staudinger didn't like it was that we anticipated his book. He was working on a larger, bigger book, but because it was larger he was a little bit slower. It came out only in 1932 (20). I think if our book had come out in 1934, he would have said, "Beautiful book. Just a copy of mine." But we came out first. I can understand. It disappointed him; somebody pulled the carpet out from under him.

BOHNING: By this time you had written a number of papers and I'm curious as to how much time you devoted to writing? Your publication list already was growing very rapidly.

MARK: Well, I would say that in a normal day I would be in the lab at half past eight or so. First of all quickly look through the mail, with the secretary next to me and tell her the things she could do without me. Then I would go through the lab, talking with the individual people, asking what they were doing and just sitting around and chew the rag until about noon. Then we would head to lunch; quick, short. After lunch I would read the literature, whatever came in, and then I would start writing until maybe six or so. I would say, a third handling daily mail and the literature as it came in, a third going through the laboratory, talking, seeing to it that the work was progressing and a third writing for publication. Sometimes in the evening there was additional time for reading galley proofs and such things.

STURCHIO: Did you keep to that distribution through most of your career afterwards?

MARK: Pretty much, yes. Of course there comes a time when there are emergencies. An emergency in the lab or an emergency in reaching a manuscript deadline.

STURCHIO: We noticed from you resume that you were an associate professor at the Technische Hochschüle at Karlsruhe at that time. How did that come about?

MARK: It had a little to do with Haber's words that if you don't like it, come back. I told Professor Meyer that I didn't want to cut completely my academic contacts. He agreed with that. In Berlin, of course, I had been an assistant professor at the University. Down there [Ludwigshafen] there were really three universities; Heidelberg, Darmstadt and Karlsruhe. Karlsruhe and Darmstadt were Technische Hochschüle, what we would call technical universities. It turned out that there was a very good friend of Haber's in Karlsruhe, Professor [Georg] Bredig, so that was the obvious way to take. Haber wrote him a letter and Bredig invited me to come and I gave a lecture there. They first took me on as a predocent, as is usual. And after two years, they promoted me to ausserordentlicher professor, essentially an associate professor.

STURCHIO: Meyer had no problem with you going there? It hadn't been usual, until very recently, for professors to also work in industrial labs and vice versa; for people at industrial labs to also be professors at the same time.

MARK: All our group leaders had the title professor. [Walter] Reppe, and Hopff and Otto Schmidt. It was a title.

STURCHIO: But would they give lectures from time to time at the university?

MARK: Yes. They would from time to time. They would either give a lecture or give money which was equivalent. [laughter] Sometimes probably the money was more welcome than the lecture.

STURCHIO: Well, that's interesting though because at that time, that suggests that the relation between I.G. Farben as a major chemical producer in Germany and academe was much closer than the relations between universities and industry in the United States at that time.

MARK: Probably it was. In Germany I don't know whether all industry was that liberal. But here for instance, General Electric was always very close to the schools. Well, they founded Rensselaer, and Rensselaer for years and years got all its money from General Electric.

And Kodak; "Why does MIT exist? Because of Mr. Eastman." He came and said, "What do you want?" And so on. But you are right. Some industries and private universities were very close here, whereas industry and state universities here were not. Whereas in Germany, it didn't matter whether it was a private... actually, there were no private universities; all were somehow connected with the state. But industry didn't care.

STURCHIO: Here the companies would be a little bit leery about letting their researchers teach at universities because they might be worried about proprietary interests. But that wasn't a problem in Germany?

MARK: No, it wasn't a problem. The companies would only let people talk at meetings or seminars who knew what subjects they could discuss.

STURCHIO: Did you have academics working as consultants at I.G. Farben?

MARK: Staudinger was a consultant. Many others were; [Karl] Ziegler was a consultant, he was in Heidelberg at that time. I presume we had maybe a dozen consultants. They came once in a while, but not very often. I saw most of them. I saw Ziegler there; Ziegler came several times.

STURCHIO: Did you find them very helpful for your work?

MARK: Ziegler would help with our work and we helped him a great deal. You know, he worked on the addition of lithium to butadiene, and of course, to prepare butadiene in a university laboratory is an ugly job. We sent him cylinders of butadiene; it is a toxic gas at room temperature. Thus, we assisted him with materials and eventually also with instruments when he needed them.

STURCHIO: Were your laboratories better equipped with instruments than the Kaiser Wilhelm Institute and other academic, standard university laboratories?

MARK: At that time it was the Kaiser Wilhelm Institute and there the answer is no; the Kaiser Wilhelm Institute was as well equipped as we were, except that they didn't go so far into production. For instance, they didn't have high pressure equipment, except for small autoclaves.

STURCHIO: How did Vienna compare, when you got there, in terms of instrumentation?

MARK: In those days? Very poorly. Austria was just struggling along at that time, and there were permanent near-revolutions, or certainly internal upheavals: later even worse when Dolfuss was murdered [in 1934]. As soon as Hitler had his eyes on Austria, everything there was in great disorder.

STURCHIO: It would be good to talk about Vienna. Perhaps you should tell us about the third turning point in your career first.

MARK: The third turning point was a conversation with Dr. [W. K. Friedrich] Gaus. One day Dr. Gaus called me and said, "Why don't you come over to my office?", so I came over. He was a super gentleman you know, a real gentleman; all the time, not only on this occasion. It was in May, 1932, that he said,"Dr. Mark, I'm afraid Hitler will take over the government. And if he does, there will be stringent conditions on employment. You're an alien, you're half-Jewish, probably we won't have to fire you," because he didn't know what happened eventually, "but one thing is sure. We could not promote you. Therefore, I think it would be the best for you if you tried to get a university position. We have given you much freedom. We have left you alone. You have a good reputation in academic circles; you have published. You have learned a great deal here and you have had additional university activities, so why don't you?"

I said, "Thank you, thank you," went out and immediately started to write letters. And I got two calls maybe within two months. One was to Breslau and one was to Hamburg, but neither would have helped me because they were both in Germany. Of course I couldn't reply that I had to get out of Germany. And on top of that, maybe Dr. Gaus was wrong; maybe Hitler would not come to power. But then came a call to Vienna, and there, of course, I said that I would be glad to come and discuss the conditions. The situation in Vienna was this: when there was a vacant professorship, three names would be put up. Primo Loco, Secundo Loco, Tertio Loco. In this case Primo Loco was Professor [Karl F.] Bonhöffer, who was a physical chemist in Leipzig at that time. I was number two. And number three was a certain Professor Thiele who was at that time in Münster. So I had to wait for Bonhöffer. It was pretty clear that he wouldn't accept, because he had a tremendous institute with enormous influence on all physical chemistry in Germany. In fact after two months, Bonhöffer declined the offer and so then they asked me. I said, "Well, I'll come to Vienna and we can talk about it."

So I came to Vienna in September, when the ministries were working after the summer break, and we talked about it. My salary was kind of meager and the facilities which they could give me for the institute were not very good. But Dr. Gaus had told me, "We will help you." In fact he had intimated they would continue paying me my full salary for five years. For I.G. Farben that was peanuts. [laughter] For me it was a life saver, because there was a great difference between the schilling and the mark at that time. So with this surplus I could easily ensure a very good life for myself and take care of three or four assistants. Dr. Gaus had also said that they would supply materials to me if I needed them. So I agreed, went back to Ludwigshafen and resigned my post. I got my appointment in Vienna and I started my activities there on the 20th of October, 1932 with my Anfangslesung, my opening lecture. This was very interesting for me. First of all it was a very lucky situation; but it was very interesting, because, as in every discipline, there are these big areas: research, application, and teaching. I had done research in Dahlem, I had done application in Ludwigshafen, I would have to be teaching in Vienna. It was necessary to establish the teaching method for a new discipline. This was evidently a very attractive problem. I was able to do it, since I had unusual means to bring it about.

STURCHIO: Had you given any thought to leaving I.G. Farben before you had your conversation with Gaus?

MARK: No thought whatsoever.

BOHNING: This conversation with Gaus came as a complete surprise to you?

MARK: Three decisive conversations. Schlenk and Haber. Haber and Meyer. And Gaus and Mark. There was nobody else there.

STURCHIO: Do you think you would have just continued to move up the industrial ladder?

MARK: Sure. I would have been successor to Professor Meyer, maybe after five or six years, when he retired or did something else. No, I wouldn't have left.

STURCHIO: Did you bring people with you from Ludwigshafen when you went to Vienna?

MARK: Not right away, but later, yes. [A. Reis] Weickert and [H.] Suess came to Vienna. [Robert] Simha came to Vienna. There were enough excellent young men available anyhow. There was no real reason to bring anyone from I.G. Farben.

BOHNING: Did Gaus make the same offer to anybody else, or were you the only one that he helped in that way?

MARK: I think, I don't want to say he made the same offer, but he talked with Professor Meyer. The reason that he didn't make that offer was that Meyer was on his own level, they were colleagues on the board. But he certainly said something similar to Professor Meyer and Meyer did exactly the same thing that I did and eventually got a professorship in Geneva. He also was an alien, being Austrian, and he also was half-Jewish.

BOHNING: During the four years you were in Vienna then, did you anticipate...

MARK: I was really there from 1932 through 1938, six years.

BOHNING: I'm sorry. Yes.

STURCHIO: Before we get into that, how did your wife feel about this move? You had two small children by then.

MARK: Maybe I should talk a little bit about my family. We really didn't dare to start raising a family in Berlin because we felt that our conditions were not yet sufficiently stabilized. We would have started if I had an appointment as a [full] professor at the university. But when we got to Ludwigshafen we said immediately, "Now this is the time." Hans was born in 1929 and Peter was born in 1932. Of course, we had a very good life. We were rich in Mannheim; we had a large car. But for Mimi to come back to Vienna was to come home, to be back together with her family again. So she was happy, she didn't object at all. She liked Germany even though she was a Viennese. She liked Berlin also particularly as it was so international. She liked Mannheim and Heidelberg, they were not so international, but the country is so nice there.

BOHNING: Did she accompany you on any of your trips?

MARK: On the business trips? No, but she always came with me to meetings. For the Düsseldorf discussion, for instance; she came to Düsseldorf, but she didn't go to the meeting, she went to the shopping center. [laughter]

STURCHIO: Sounds like my wife.

MARK: Usually with considerable success. [laughter] And never alone, you know. There was a group of girls who did that.

STURCHIO: How did you feel about going to Vienna. Well, there are two things, personally and professionally.

MARK: I had no choice. But, even if I had not got a call to Vienna but let us say to Graz or to another smaller university, I would have taken it. Because sooner or later I would have been diffusing up in the upper echelons. But Vienna was a lucky strike. The lucky strike was that Bonhöffer... The lucky strike really was that the Viennese were idiots, because they thought they could get Bonhöffer. It's as if our institute here said, "Next year we will have three Nobel prize winners." You see? Maybe they did it to protect me, so that eventually I would get it. But when they called Bonhöffer, they didn't even know yet whether I wanted to come. Anyway, luck was good to me. And then, as I said, there was a very attractive goal in Vienna. Namely, how to organize an institute for polymer research and teaching? None existed in the world at that time.

BOHNING: You were instrumental in starting courses?

MARK: Sure, sure. I sat down with my people and, again, I had a large number of very able people. [Philip] Gross, [Franz] Patat, [Anton] Wacek and [Otto] Kratky and others. And we sat down and said, "Well, what do we need? First, we need a general, introductory lecture; what are polymers? Maybe one semester. Then we need a course on the synthesis of polymeric materials, synthesis of monomers, synthesis of polymers, in two semesters. Then we need a course on how to characterize them?; how do you know what you have synthesized? So the first thing was organic chemistry, then came physical chemistry. And then, what are their properties? So there was the physics." It was really an interdisciplinary activity, as it is now. Now you start with organic chemistry, you go into physical chemistry, you end up with physics. These courses had to be organized, people had to be instructed on what to do and how to give these courses. I had very nice help from the physicists. But all that had to be done. On top of that, all the courses already in the catalog had to be given anyway, because I was a professor of physical chemistry, not of polymer chemistry. There was one on electrochemistry, one on photo-chemistry, one on reaction kinetics, which had to be given anyway. The others were given additionally. How to organize the time, the classrooms, and so on?. Took three years until that was really... 1932 was very short, 1933, 1934, 1935. I think by the beginning of 1936 I could say we had a polymer research institute, with all the courses necessary to produce a polymer chemist in place.

BOHNING: How many students did you attract at the beginning? Was that any difficulty?

MARK: No, not at all. Not at all.

BOHNING: What about research? Your research was certainly going to be different now than it had been at I.G. Farben.

MARK: Well, not too much. As far as research went, I turned down synthesis a little bit, because I knew that industry was doing so much anyhow. But I accelerated the mechanism of polymerization. In Ludwigshafen we didn't care too much how the molecules reacted with each other as long as they reacted. But of course, you can't go on that way, so I started to emphasize kinetics and the mechanism of polymerization reactions.

[END OF TAPE, SIDE 5A]

MARK: For characterization, of course, we had viscosity measurements, osmotic measurements, centrifuge measurements, diffusion measurements. Those all came more or less from colloid chemistry, and there I needed people. There was [Frederick R.]

Eirich and there was [Max] Bunzl and there was [Herbert] Margaretha. Eirich and Margaretha and Patat and Gross and Guth and Simha and Wacek and so on. A dozen. The third part, namely the properties, was more or less a matter of physics. There I had great help from our physics department from Professor [Hans] Thirring. Simha was very good at that, and a man called [H.] Dostal also. I gave the introductory course myself, and for each of the other courses I had three candidates. First they were assistants and eventually they would become professors. All this time I was very fortunately supported, not only by I.G. Farben but by all the industries.

BOHNING: Were you,in a sense, consulting with these other industries at the time?

MARK: Not so much consulting. Because I gave lectures which were published it became known after a year that we in Vienna were introducing a new discipline in chemistry, polymer chemistry, and that we were developing a teaching and educational schedule in this field. Industry liked that because they wanted our graduates. So they sent us many students, including many from Russia.

BOHNING: Were there other teaching institutions in Europe?

MARK: Well, Staudinger could have been but he was so bullish and so egoistical that he never did that, he never really established a teaching program. He gave his own lecture; his attitude was that if I give a lecture nobody else can go beyond that and that's the end of it.

BOHNING: Did most of your, the people who went through your program stay in... Where did they go when they finished?

MARK: Practically all of them either went directly to the I.G. or they went to other companies. In Switzerland to Ciba or to a rubber company, such as Kontinental in Germany or Michelin in France. They were sold out right from the beginning. They were a new breed, you see. Suddenly there was a valuable new language on the market, and so they were gone immediately.

STURCHIO: By the late thirties, [Carl S.] Marvel must have been turning out a few students at Illinois. So there was at least one other small center for polymer chemistry.

MARK: Yes. Of course, if we come over here now, you see, Speed and his excellent laboratory, turned out a large number of very high level synthetic polymer chemists, but they knew not much about characterization and they knew only little about properties. He only did what he did so well, and did actually better than anybody else, but he had no desire to form a new science.

STURCHIO: Wasn't [Harry W.] Melville training students in England?

MARK: Well, Melville also; in Cambridge. He had a number of excellent students, but they only worked on characterization. They didn't make new polymers. See? They tore into pieces all the existing polymers. They characterized them to the last detail. But they only took a piece of it. My idea was to do the whole job. If you want to create a new discipline you have to do the whole job.

STURCHIO: Who were some of the students who went through your institute in the thirties who later went on to set up institutes of their own or head industrial labs? Are there any names, a few names who really stand out?

MARK: Well, I mean in Germany, well who... Well, Eirich was one and Herbert Margaretha was one, and A. Wacek was one, and [E.] Suess was one. Simha, Guth, [Hans] Motz, but then when Hitler came they all left.

BOHNING: Any Americans came over to study?

MARK: At that time only one, Donovan [J.] Salley. He was there for two years, and when he came back he became director of American Cyanamid here in Stamford within a few years. Until a few years ago, when he retired, he was on the board of American Cyanamid. Donovan Salley.

BOHNING: What were you anticipating for the future as the political climate began to change in the late thirties?

MARK: Well, let's say a few more words about Vienna. First of all, of course, we kept very close relations with foreign countries, particularly with England and France. And in England, the Faraday Society meetings of course were particularly attractive and there was an important one at Cambridge in 1935, because it was a replica of Düsseldorf, but an advanced replica. At this meeting the existence of macromolecules was accepted: the problem was, how to make them, how to characterize them, how to use them? You know, that's exactly what we did in Vienna, isn't it? Most of the people who worked in the field, including Carothers, were there. Of course, we knew in Vienna that he was working in the same field and we knew that he was making fantastic progress in the synthetic rubber and synthetic fiber fields. He was very much more advanced than we were, in the final phase of industrial application. That was the first time I met him. And as he then visited the Continent I invited him to come to the Institute. He came to Vienna, we walked around and I showed him the Institute and we discussed various things. Then, unfortunately he committed suicide in 1937. I only met him in Europe. He didn't bring any of his people with him, such as [Julian] Hill or others who worked with him.

But our contact with England became very close. English industry. ICI decided that they should have such an institute. Actually they started to do so in Welwyn Garden City, near London and Courtaulds built up a research center in Maidenhead. The first was more product-oriented, more fundamental. The Maidenhead Institute under [Clement H.] Bamford was more teaching or educationally oriented; they didn't teach any classes but they placed emphasis on education [Ph.D. research projects; ed.]. The ICI and Courtaulds plants were somewhere else, but these two institutes were very close to what we had in Ludwigshafen, and later in Vienna. In France there were two companies which also did similar things. I don't want to say the same, but similar. One was Rhône-Poulenc and the other was Michelin. They were interested in polymers from manufacture to the application. Other companies either made the monomer or made a certain resin, then sold the resin but didn't care much what would happen with it. On top of what I told you already, quite a considerable strengthening of international relations in Vienna. With America, unfortunately only with Carothers.

Publications went on, of course. The question then came to write another book, because my book with Meyer was more in defense of the macromolecular concept, whereas now "here they are". After the Faraday Society Discussion everybody accepted their existence (21), so why don't we write a book? Start with the existence of macromolecules and go on from there; for instance, reaction mechanisms, how are monomers polymerized, the different polymerizations systems, how are polymers characterized. I published one alone (22), I published one together with Meyer (23), and later I published one with [R.] Raff (24). Those were three books which followed up on the original book of Meyer and Mark. Follow-ups in the sense that we are not any more arguing about their existence but the study of them.

Otherwise, I think the boys grew up and we all had a very happy time in Vienna, because we were well off financially. We had our family, Mimi's and my families were nearby. In 1937, it must have been in May, I got a letter from a certain Dr. C. B. Thorne. He was the managing director of the Canadian International Paper Company in Montreal, a very large pulp and paper company. They had four mills, big mills; it was an enormous company with their headquarters in Montreal. He wrote, "People who come back from Europe say that you have worked a great deal on cellulose, as I also learn from the literature. In fact, I understand that you have helped to clarify the structure of cellulose and that you have written books on cellulose. Our lifeblood is cellulose. We have a research laboratory in Hawkesbury, Ontario, but I have the feeling our research laboratory is obsolete. We are old-time cellulose chemists; we use the alpha value and such purely empirical measurements to characterize our material. This is very dangerous for us because both our customers and our competitors are getting more and more sophisticated. Someday we will no longer be up to date in characterizing our material, of which we make fifteen hundred tons a day, which we deliver to the United States and other places. I would like to talk with you about an opportunity for you come over, reorganize our laboratory and become our research director." So I wrote him, "Thank you, thank you, thank you. Wonderful, wonderful, wonderful. Certainly I would be delighted to talk to you."

He always came to Europe in the summer because the Canadian summer is much hotter than here; he was a Norwegian. From then on he cabled, he didn't like to write letters. So, sometime in the summer, he cabled me that he would be in Dresden in a few days time and asked whether I could come to Dresden. I agreed and met him in Dresden with his wife. Very cultured people. He visited Europe mainly because of the museums and because of the expositions, because he liked statues. Visited Rome every year to see the temples and so on. His wife, of course, she liked the fashions. She wanted to know what was happening in Paris and so on. He repeated his offer. I told him, "Dr. Thorne, I have a big institute on my back and it's just now running reasonably well; I have this load and I have obligations. I certainly just couldn't go away and not come back. Maybe," and then I remembered Schlenk and Haber, "we could make an arrangement for me to come over for a half a year or a year, whatever is needed in order to get your laboratory up to date and to teach your people how to use these new methods. Eventually, I can come over again after two or three years as a kind of a consultant." Well, he didn't like that idea very much. Evidently he had other people in mind whom he wanted on a permanent basis. Well, as I said, he didn't like it very much but he said, "Well, that's not a bad idea. Let me see what else I can do." I know he had two other candidates. I learned later, I didn't know then. So that was it.

When Hitler came to Austria in March of 1938, I immediately sent him a cable inquiring whether, under the changed conditions, it would be an appropriate time for me to come over and maybe even be inclined to stay there for a longer period. Of course, he sensed it all, and cabled back, "Yes, why don't you come over and take over this job." Being a very intelligent man, he sent an official letter of employment to their office in London; they had a big office in London. Well, by hook and by crook I got out of Austria after I had been imprisoned for a while. It was quite difficult to get out of Austria in those days because they took away my passport. I had to get another passport, and had to get a visa. I had to get a Swiss visa and the Swiss didn't give visas to anybody whom they suspected would stay in Switzerland. So I told them, "Look here. I have an offer." I told the Swiss consul in Vienna, "I have an offer to go to Canada so that I wouldn't stay in Switzerland." And he said, "Well, how can you prove this?" I said, "Well, why don't you send a wire to the Canadian International Paper Company in Hawkesbury?", which he did. And of course they replied. Then I got a Swiss visa, and I got a French visa, I got an English visa. In other words, the employment letter of a Canadian company: Canada was heaven. Not only because it was in America, but because it had such a good reputation as a free country and as a beautiful country. From then on everything was easy.

BOHNING: What did you find when you got there to the laboratory?

MARK: My predecessor was also German. Professor Emil Heuser, who had come there during the war. I think he left Germany because of the first World War. He had equipped the laboratory according to the state of the art of cellulose chemistry of the early twenties. They had measurements of viscosity, measurements of alpha, measurements of tensile strengths, and Elmendorf tear strength, and all kinds of things. All empirical. Never any word in the company about cellulose molecules. [laughter] How can cellulose have a molecule? Water has a molecule. That was all fair and good and there were a number of people who were pretty good and very ready to accept any education. It took about a year and a half or two years until three more people were added and until we had all the equipment which we needed, all the x-ray equipment and all the infrared equipment. From then on everything went according to what I had hoped. Du Pont had been somewhat disappointed with the activities of the Canadian International Paper Company in comparison with the Rayoneer company on the west coast. Rayoneer was under the guidance of Professor [Harold] Hibbert who was professor at Montreal, a cellulose chemist; we were good friends, because he had visited my laboratory earlier in Ludwigshafen. Du Pont evidently had told Dr. Thorne to get his lab up to date and see to it that it was on a level with modern cellulose chemistry.

When I came there, since I had met Carothers and then because the collaborators of Carothers knew me from my publications, Du Pont were relieved. They liked the idea that there would be a new wind up there. Once when I visited Du Pont on a complaint, they said, "For God's sake, see to it that this lab gets in order." And they gave me all possible help, where to get equipment, where to get people, and so on. Of course, Dr. Thorne was very comfortable when he sensed that I had close relations to Du Pont, his best customer. He would see to it that I wouldn't do any kicking. Du Pont was a good capitalist, you see. In 1940, two years later, Dr. [William] Zimmerli, who was on the board of Du Pont, was at the same time on the board of this institute [Brooklyn Polytechnic]. He felt that it would be a good idea to get me out of industry to repeat what I had done in Vienna, namely build up a polymer research institute. Well, Dr. Thorne was furious when I went and told him. "Oh, you're a rascal. You're a rascal. I've helped you get out of Vienna. I've helped you get out of the hands of Hitler and now you run away!" So I said nothing. But I called Zimmerli and asked him, "Couldn't you appease Dr. Thorne?" He appeased Dr. Thorne by saying, "He has fixed up your lab. Keep him as a consultant, but let him go to Brooklyn, because Du Pont is interested." And that settled it although I didn't know all this at the time.

STURCHIO: You said that Thorne had two other people in mind when he came to talk to you in Vienna. Do you remember who they were?

MARK: Thorne had in mind a student, a disciple of Staudinger, Dr. Hans Kressig. And he had also in mind a cellulose chemist of the company Mannheim Waldhof by the name of Deutsch. Now, apparently both either said no or it didn't proceed further. But when I left and the laboratory needed another director, I recommended Kressig. And Thorne took him, so Kressig was my successor.

STURCHIO: And you said you met Hibbert in Ludwigshafen?

MARK: Hibbert had visited us; and I don't altogether remember but I think it might have been in Dahlem. He was a world famous organic cellulose chemist who was very much interested in the whole Staudinger controversy. But I think it was Ludwigshafen where he visited us. We had corresponded.

STURCHIO: I think, if he's the same Hibbert I'm thinking of, he worked for Du Pont before World War I.

MARK: Yes, Harold Hibbert. He was an Englishman who came over early and worked in industry and then got a professorship at McGill.

STURCHIO: What was he like? I presume you saw a fair amount of him when you were at Hawkesbury.

MARK: Oh, a delightful fellow. Delightful, both he and his wife. He was a real sportsman, you know. He was a fisherman. I think whenever he went consulting to the Rayoneer Company on the West Coast, he did nothing but fish. Didn't give a damn about cellulose! [laughter]

[END OF TAPE, SIDE 6A]

MARK: But he was also a Canadian, you know. He had combined the British past with a Canadian future. Very nice fellow.

STURCHIO: Had he started to build up a polymer program at McGill or was it strictly cellulose chemistry?

MARK: Only cellulose. Only cellulose.

STURCHIO: But he brought you in to teach polymer chemistry?

MARK: Oh yes. He asked me to give a few courses at McGill. But he stuck to cellulose chemistry.

STURCHIO: We've been going for three hours now. I wonder if it might be time to round this session up?

MARK: Sure. Well, I presume there will be little holes. I have to look for this polystyrene book and for a few other things maybe. I would be delighted to have another session anytime you want.

BOHNING: Can I ask just one more question about Canada before we break up.

MARK: Sure.

BOHNING: The company had plants scattered around Canada. Did you have to do much traveling or did you stay in Hawkesbury?

MARK: Not much traveling. I visited all the plants, maybe once a month. One was far up north on the Temiskanino River. Two were on the Ottawa River, and one was north of Quebec. Each pair of them operated on different wood; the wood influences the properties of the final pulp and paper a great deal.

BOHNING: So then it was mostly quality control laboratories at the different plants.

MARK: Yes. They had only quality control laboratories. And for a while, you know, the company delivered maybe thirty percent of the paper for the New York Times. Big job. There were very critical factors, Suppose you have a thousand tons of pulp, ten boxcars. Big loads of paper, newsprint. And you have a list of specifications. Elongation, wet strength, wet elongation, and so on. How many samples do you have to take from these ten boxcars so that this shipment meets the specifications? Ten? Well, obviously, if you tell the New York Times that you use ten samples, they throw you out. [laughter] Well, a thousand? Ten thousand? A hundred thousand? Well, you can't take a hundred thousand because you can't handle so many. That was the permanent question. Do we characterize our product properly, even if we know the method exactly? There were endless conversations with the people from the printers. Of course they also came up and said, "We would like to increase the speed of our printing machines by five percent. It would be money in our pocket. Gold in our pocket. Do you think we can do it?" And there were our questions: "How fast do the machines run? What is the moisture content and how is it controlled? What kind of ink do you use?" They would come up and I had to go down to New York to talk that over. In the end, we would make a test. We would say,"Okay. We will send you one boxcar which has a little higher elongation so that you can run it faster." But just one. And then another one and after a year more. You see, that is commercial progress. That is commercial progress. And since that time, the speed had been doubled.

STURCHIO: Well, perhaps what we can do is bring things to a close today and thank you very much for taking time and talking with us.

MARK: I thank you very much for coming and it's certainly a great privilege and pleasure for me to have an opportunity to talk.

STURCHIO: Well, we've enjoyed it.

[END OF TAPE, SIDE 7A]

INTERVIEWEE: Herman Mark

INTERVIEWERS: James J. Bohning and Jeffrey L. Sturchio

LOCATION: Polytechnic University, Brooklyn, New York

DATE: 17 March 1986

BOHNING: Today's discussion is a continuation of a session that began on 3 February. Professor Mark, last time we finished with a brief discussion about your experiences at the Canadian International Paper Company. You arrived in Ontario in September of 1938.

One question I have is about a meeting that you had in the following January with [Eric S.] Proskauer in Montreal. Could you tell us a little bit about that meeting, what your relationship was with Proskauer, and how did that meeting come about?

MARK: Yes. My relationship with Dr. Proskauer goes back to the early thirties. When he was in Germany, he was one of the editors in the publishing house of the Akademische Verlags-gesellschaft, particularly charged with chemistry and chemical engineering. And already at that time, 1932, we were cooperating in the field of what was known at that time as Hochmolekulare Organische Naturstoffe because Professor Meyer and I published a book in 1930 with them which used this expression in its title (25), and which really was the first book which propagated Staudinger's ideas, not only on the basis of his own evidence, but also on the basis of additional evidence which we had supplied. So we were in rather close contact at that time. Some time in 1937 Proskauer emigrated. He lived through all these difficult and trying periods in Germany; three, four, five, six, seven years, and went to the United States together with Dr. Mauritz Dekker, and he did what Dekker did. He was more or less sent from Elsevier in Holland to establish an independent company in the United States because there was good reason to believe that someday Holland would be invaded by the Nazis. Elsevier with Dekker and Proskauer as their agents created and organized an American company, Interscience Publishing Company. Later, after the war, Interscience again started to cooperate very closely with Elsevier.

So we knew each other. He had published our ideas about macromolecules earlier in 1930, and so it was obvious that when he and I were on this side of the Atlantic, that someday we would get together and discuss, plan and find out what we could or should do together over here. First of all we telephoned several times and after clarification of the whole situation, we eventually met in Montreal.

BOHNING: And what was the result of that meeting in Montreal?

MARK: Well, the result of that meeting was that I indicated to him that very probably I would not stay with International Paper Company in Hawkesbury for very long. Originally the idea was expressed by Dr. Thorne that he wanted me to come to get their laboratory modernized and eventually become director of the laboratory. I had told Dr. Thorne earlier, and I presume this is probably in our earlier narration, that if conditions allowed, I would be delighted to stay there, as I would have a top salary and a really very pleasant position, but if other things intervened, I might prefer to go back to an academic career. Dr. Thorne wasn't particularly happy about it, but we'll talk about that a little later. When Dr. Proskauer and I met in Montreal we assumed that I would go to the United States to a university, although I didn't know which one it would be. Then, of course, the time would have come to resume polymer science publication. We sat together and said, "Well, how would it be best to start something like that?" It appeared it would be best to lean on something already existing.

But nothing substantial in the polymer field existed in academia. It was in Du Pont, in the work of Wallace H. Carothers, on fibers and on rubber. So we thought it might be a good idea to start a series of monographs with the collected papers of Carothers. It would be a bow to tradition, it would be a bow to the Du Pont Company and it would be justified because it was the only coherent work which was done in this field at that time. As a result Proskauer said, "Okay. Why don't you start working on that?" Then after a little thought we both felt that, since we were both immigrants, it might be better to lean on somebody who was already an established authority here. Since I was a fiber chemist rather than a rubber chemist, I would be able to take care of Carothers' fiber-oriented work, but we might like to try somebody who could take care of the rubber-oriented part. Of course, that was George Whitby. I had cooperated with him closely in England. We contacted him in Akron where he was at that time, and asked whether he and I together would edit these collected papers of Carothers (26). He agreed. Thus we started to develop this first volume of the endless series on high polymeric materials and related substances. That was really what we did in 1939 when we met in Montreal.

By the way, Proskauer came to Hawkesbury and stayed with us for a week or so which really marked the beginning of this series, and also was the beginning of another, more general tendency. Namely; you have to take care of the literature if you want to start something new in the scientific field. Not to be satisfied with the existing literature, but to generate new literature. So that was what happened at this meeting.

Of course everything was very interesting and satisfactory and pleasant in Hawkesbury. My main problem there was that the rayon which Du Pont prepared from the wood pulp delivered from the Canadian International Paper Company was of two types. One was a textile-type rayon; the big business, originally. Their original material made from this pulp, large quantities, 300 tons a day, was rayon. Textile rayon has only a moderate strength; the premier qualities are its smoothness, resilience and elongability. It has to be a nice, soft and luxurious textile fiber, and it is. Suddenly in 1939 Du Pont discovered that one can make a rayon which is much stronger, harder, more durable and therefore not so good for textile applications but good for tire cord. They called it Cordura. In fact it was much better than cotton so Cordura became a very important development for Du Pont. It turned out that, whereas our pulp, the so-called Novacel, was a very good material for textile rayon, it wasn't any good for Cordura. Why? Well, we made molecular weight determinations, we made viscosity determinations, we moved them back and forth, a little bit higher viscosity, a little bit lower viscosity, and within a certain range the textile rayons were all very, very good. But apparently Cordura was outside of this range.

We became interested not in the average molecular weight, but in the molecular weight distribution, because of the earlier work with Simha in Vienna. Was it this way, was it that way; was it narrow, was it broad? Every macromolecular material is a mixture of many individual species and it's not only the average which counts, but the exact distribution function. So we determined the distribution function of our rayon, and found that there was a substantial amount of low molecular weight material in the textile rayon which was good as a plasticizer, for extensibility and such properties, but which was bad for tire cord tensile strength. We removed it and made a number of samples from which we spun Cordura; it was fine. That set the problem for us. How to make a pulp in the mill which contains a minimum of these low molecular weight constituents?

We worked on that in 1939 and within the year concocted a cooking, purifying, bleaching and oxidizing, whatever steps are needed in order to go from the raw pulp to the final pulp, some seven steps. You have to maneuver with these steps to get a narrow molecular distribution, and within a year or so we had a practical process. That was really my main contribution to the manufacturing of this new material at the Canadian International Paper Company. If you have a new process where a lot of improvement and refinement is possible, you have to increase the yield and you have to save chemicals. Engineering problems. When you have fundamental progress with engineering problems, they haunt you. And they did. After this was more or less done, and after we had produced, I don't know, maybe a few hundred tons of a Cordura Novacel, then I... I don't want to say I got restless, but I didn't really see any particularly attractive and exciting problems in front of me, because what was necessary now was to scale it up and gear it up and to make this Cordura

Novacel as cheaply and as pure as possible. It was just engineering.

So one day I went to Dr. Thorne and told him that I would really like to come back to our discussion that I didn't want to stay here for ever, but rather return somehow to an academic position. And he didn't like it at all. He was furious. He said, "Look here, I helped you to get out of Europe; I helped you to escape the concentration camps and now you tell me that you don't want to stay." I was really in a bad situation because it was, to a certain extent, a breach of confidence. He was such a nice fellow and he really had helped me substantially to get out of that mess in Europe. I talked it over with [Sigmund] Wang, the plant manager, and Wang said, "All you need to appease the boss is the word 'Du Pont'. If you can convince him that Du Pont would look favorably on your move to the United States to a university or to a school which is somehow in contact with Du Pont, then he would say 'sure, sure, sure,' because the whole damn mill lives from Du Pont." [laughter]

Wang did it. He went to Thorne one day when Thorne again raged and said, "This Mark. He's a real German. He's a real German. He promises something to me and now he lets me down." Then Wang said, "Look here. Du Pont. They want him as a consultant. He will be their consultant and he will see to it that our pulp will be well liked." So he smoothed it all out. I stayed in Hawkesbury the whole year of 1939, when we made a lot of additions and improvements. But the essential thing was the recognition that for special performance the average values of molecular weight are not sufficient; you have to have a profound knowledge of the molecular weight distribution, which has to be adjusted to the ultimate use of the material.

BOHNING: I think last time we just asked briefly whether you did much traveling throughout Canada? There were a number of mills.

MARK: Yes.

BOHNING: You did then. Did you travel to the western part?

MARK: Yes. I profited very much from this. I learned how large scale production works. Even in Ludwigshafen I had never been in large scale production. We had made polystyrene, synthetic rubbers in, I don't know what, maybe a ton a day. Now we were making Novacel at three hundred tons a day; thirty boxcars, you know. So that was a different story. I talked with engineers and learned a great deal about doing that. We had another interesting problem to discuss at that time. Okay, so now there are your thirty boxcars to be delivered. You had specifications from your customer. How many samples do you have to take from three hundred tons, in order to be sure that you are meeting the specifications? One, two, three thousand, three hundred thousand? So that was an important problem, and we had to contact statisticians at McGill University, as well as engineers who had a certain feeling for this field. Finally we settled down to three hundred individual measurements. Now if you make three hundred individual measurements a day, the measurements have to be absolutely automated. Really, it was the beginning of the automation of analytical chemistry; it had already been done in other instances. So there were a lot of interesting problems to be solved; not absolutely fundamental problems, but additional problems.

BOHNING: Who was the first person that you contacted at Du Pont when you were working on the Novacel problem.

MARK: It went as follows. Routinely, Mr. Wang, our plant manager, went to Wilmington or New York in order to meet with a number of Du Pont technicians. Usually he went alone. He was also the sales manager. When this shaky problem occurred about [whether the new Novacel] will be good enough or not, he felt that he needed technical assistance while he talks to these people who actually knew the details. So he took me with him. The first time we went to the Du Pont hotel in Wilmington and met Dr. Ernest B. Benger, and Dr. Rollin F. Conaway. Benger was the director and Conaway was the plant manager of the Cordura plant. They were the first Du Pont people I met.

BOHNING: When was that?

MARK: Sometime in the fall of 1939. Conaway and I were immediately close because we had the same ideas. Of course Benger and Wang argued about the price. From then on, probably once a month, a Du Pont team of two or three people would come up to Hawkesbury and report additional complaints, on things which didn't go exactly as we had hoped, make recommendations, whilst we told them what we had done in the meantime.

STURCHIO: Would it be Benger who would come up to Hawkesbury, or chemists?

MARK: Neither Benger or Conaway. Quite a number of chemists. I mean that each time might be different. From the end of 1939 to April or May of 1940, we had very close contacts and I went down several times. I think the contact between International Paper and Du Pont was very sound from then on. You know, it used not to be. When I was first there, much to my disappointment, it was a seller-customer position. "You have to give us $167 per ton." "Oh, we can't give you $167 per ton. That's much too expensive for us; if we give you so much we won't make any money." You know what happened then; up and down, up and down. Like Gorbachev and Reagan. [laughter]

[END OF TAPE, SIDE 1B]

MARK: I asked the Du Pont boys, "What should we do to increase our yield, in your opinion?" It was a sound technical contact and as a result, of course, the prices came up more or less automatically. Then by, I think in April 1940, the news from Europe became very alarming. If you remember in April or May, 1940, May I think, France collapsed. The German offensive started on May 10 and by the end of June France didn't exist anymore. So all the gears had to be accelerated then; more tires [were needed] and industry became more active in this pre-Pearl Harbor atmosphere. Then we packed our car and moved down to New York.

STURCHIO: In the documents that we've seen there are a couple of different versions of how you came to Brooklyn. One says that Benger helped to arrange it directly. Another one talks about William Zimmerli, who was on the Board of the Polytechnic Institute of Brooklyn.

MARK: I think what really happened was this. Zimmerli, who was on the Board, felt that the Institute should embark on the polymer field and that I would be the right man to start the development here. He contacted Benger who told him, "Look here. I know him because we had a lot to do with him in connection with our Cordura production." They agreed that this should be done. I do not know whether they ever contacted Dr. Thorne personally or whether Mr. Wang was doing all that. I don't know. But, anyway, it was done so that in the end there were no hard feelings anymore.

STURCHIO: Had Zimmerli contacted Kirk here, or how did this end work out?

MARK: Yes. Zimmerli had not only contacted [Raymond E.] Kirk, but also [Harry S.] Rogers, and more or less suggested to them, or persuaded them, to give me an adjunct professorship position to see whether this could all work out. Du Pont would pick up the tab. It wasn't much, as I got two hundred and fifty dollars a month, which at that time was quite a good salary. Well, I didn't need too much because I had [earned] a lot of money from International Paper. Not a lot, but enough.

STURCHIO: I'm curious to know whether you ever thought of going anywhere else when you left International Paper? Did you have any other offers at that time?

MARK: Yes. In 1939 I visited several conferences here in the United States, ACS meetings and such, and gave lectures. Of course, it eventually became known that I would be interested in leaving Hawkesbury. One opportunity was from Dr. Emil Ott who was the research director of Hercules Powder, also a cellulose company. He was on the board of Rutgers University, and did essentially the same as Zimmerli did with Rogers. Ott talked with the president of Rutgers and said, "Look here, there is a fellow who wants first to come the United States and second to go back into the academic world. Why don't you have a look at him?" I went to New Brunswick and gave a lecture there; it was a very nice place. I don't know today whether it would have been better to go to Rutgers, but I didn't. There was another opportunity at the University of Chicago on one of the various visits. I also visited Chicago, giving three lectures there. There was a very well-known organic chemist who knew of my work from Professor Schlenk. He said, "Well, if you really want to come to the United States, maybe we can do something here at the University of Chicago." But these two things were just more or less tentative.

STURCHIO: Was that [Morris S.] Kharasch in Chicago?

MARK: Kharasch, yes; he was Russian. He had visited me while I was at the Kaiser Wilhelm Institute in Berlin. He had visited us there and he knew about our work. Today I don't know what would have been better but anyway it turned out that Brooklyn was the best, or was the closest at least. When we were ready to move I got a phone call from [Irving] Langmuir from General Electric, who said, "I understand that you intend to move to the United States." Apparently either Benger or Zimmerli or Rogers had already talked about that. "Why don't you come to Schenectady for a week and give us a set of lectures on your x-ray investigation of high molecular weight substances." So we first drove to New York with all our belongings and stuffed them all in a small apartment somewhere on 96th Street. Then we drove up to Schenectady and spent a week there, a very nice and interesting week in Schenectady, giving lectures to their staff and having day-long discussions. For me, very educational, because you know there were people like Langmuir and [Charles P.] Steinmetz and [William D.] Coolidge. In other words, it was a very high level atmosphere. They had a very active polymer chemist who mainly worked on phenol-formaldehyde resins. Italian by birth, but by that time had been in the United States for some years. Oh, what was his name? [Gaetano F.] D'Alelio. He went to Notre Dame University when he left General Electric. So it was a very interesting time.

STURCHIO: Was Rochow there?

MARK: Sure. Eugene Rochow. Rochow was there. D'Alelio was there. They were chemists. Langmuir, Coolidge, and Steinmetz, they were physicists.

BOHNING: Was there ever an attempt to get you to go to GE?

MARK: No. Really not. They probably knew about the deal which had been made. And they had Rochow and D'Alelio. I never had a written contract with GE as a consultant, but occasionally they rang me up and said, "Why don't you come up a day or two. We would like to talk a few things over with you." Which, by the way, was the kind of consultantship I preferred all my life. I had only one written consulting contract, and that was a good one. You can't have two: not if one is with Du Pont. I mean if you have one with a small company, you can have a lot of them with other small companies. But if you have a binding consulting contract with Du Pont, then all you can do is talk occasionally about problems of minor importance with another company. But not on a retainer basis. So really, really that was the transfer [from Hawkesbury to Brooklyn].

BOHNING: When you came here you were associated with a shellac bureau.

MARK: Yes. We moved in May and then I went to Schenectady and eventually we came back to New York in June. Then of course, we had to get an apartment; then the stuff from Hawkesbury arrived, furniture, the books and everything. All had to be installed, which took about a month or so. Maybe two months, in order to get installed in Brooklyn. 325 Ocean Avenue was the famous place, in Flatbush.

STURCHIO: How did your family like Brooklyn?

MARK: Well, my wife liked it very, very much. Flatbush Avenue at that time was a very nice shopping center with a lot of very nice stores. Of course Manhattan was close and she had a lot of friends here; immigrants, earlier immigrants. The boys were at first very adamant, but I bought them bicycles when we were out there in Flatbush. Then they found out that they could be at Sheepshead Bay in twenty minutes on their bikes. At the ocean - Hawkesbury was forgotten. [laughter] Also there was the Miramar Yacht Club which from then on was their normal after-school activity. There they painted boats and they learned how to rig up a sail and all; right from the bottom. So they were very..., well you know how kids are. In August we still had a whole month before the school started, so we decided to leisurely and slowly drive down to Florida, spending six weeks on the way there and back, which we did. We drove all the way down to the Keys and

Key West and so on. There was the water and the sea and swimming and everything, fishing and whatnot. We came back early in September, then the boys started school, I don't know when, early in September. And our school started also at that time.

STURCHIO: Was that your first experience with the American south, that trip down to Florida?

MARK: Yes.

STURCHIO: How did that strike you? I mean, you'd come from Vienna and Hawkesbury.

MARK: Oh, wonderful. From Austria; the spy came in from the cold, you see. [laughter] We were longing for sun and for warmth. Then I went to the Institute and told Dean Kirk, "Here I am. So what do you want me to do?" And he said, "I suggest the following. We have here at Poly a small laboratory which we call the Shellac Bureau. It is supported by the American shellac industry, the shellac importing industry, by the Montrose Corporation in Brooklyn. They import shellac, raw shellac from India essentially and bleach it. Of course, there are specifications. Two ways; first, what they get delivered has to be analyzed before they pay. Then, when they have bleached it and delivered it to their customers, certain specifications have to be fulfilled before they get paid. It is an analytical job." The director of this Shellac Bureau was Professor William Howlett Gardner, a very distinguished gentleman. A first-class organic chemist who had four or five collaborators there in a nice analytical laboratory, all set up in a room two or three times as large as this one. The Montrose Mill itself was out at Gowanis Canal, you know, right here in Brooklyn. You know where Gowanis Bridge is, and Gowanis Canal? The ships from India came in there and were unloaded right at the mill. They carried out the bleaching operation and the washing and purification and then eventually delivered the purified shellac. Shellac conversion was a very substantial business. The preservation of all furniture and all furniture decoration was shellac. It was a relatively expensive resin, two dollars a pound, not like polyethylene.

It was a very good business and the company was doing very well. They maintained a laboratory, they paid Dr. Gardner's salary, they paid my salary, they paid the salary of all the boys. They had a chemist at the mill, Dr. Walter P. Hohenstein, who was supervising this bleaching operation. He was an organic chemist, also Austrian. He got his degree from in Vienna in 1933. I had recommended him to Montrose and he was first-class man. He still is. So then; I told Dr. Kirk, "Well, fine. I'd be delighted to do that. But this is not really why you wanted me to come down. You want me to introduce something new; namely synthetic polymers, not bleaching shellac." And he said, "Yes, yes, sure. So let's talk about it." Then I said, "Well, in order to be of real use for your school here, I think the first two things which we have to visualize is a comprehensive teaching program for polymer science, or polymer chemistry, together with a few attractive research projects. So let's set that up."

What would we need for the teaching? Well, polymer chemistry was not really known. With the exception of the papers of Carothers and a few other chemists, there was no literature. Fortunately, there was a lot of German literature which had it all in hand. Kirk and I sat together and I said, "First we need a basic course on general polymer chemistry. I'll give that. Four hours a week, maybe six hours a week, one semester. Then we need a course on the organic chemistry of high polymers. I suggest that Dr. Hohenstein give this course because he speaks German and can read all the German literature. He will be very close to the fountain of this information as he is an organic chemist, so let's assign him to this course. Then we will need a course on physical chemistry of polymeric materials; the mechanism of polymerization reactions, determination of molecular weights, determination of molecular weight distribution; in other words physical chemistry. And there I recommend that we employ Dr. Robert Simha as an adjunct professor." Simha was also one of my pupils who had also obtained his degree in Vienna, and as he also spoke German, it wouldn't be any difficulty for him to pick up whatever was needed.

STURCHIO: Where was Simha at this time?

MARK: Hohenstein and Simha were already here. Then a third lecture on properties and processing. There I hadn't anybody whom I could recommend because such a man wasn't available amongst my earlier collaborators. Dr. Kirk said, "Well, maybe I can find somebody who can do that." He was a very well-known scholar, knew very many professors, and the schools in the field. In fact, a week or two afterwards, he came and said, "I think I have the right man for you. His name is Turner Alfrey. He doesn't really speak German, but he can read it. He comes from the University in St. Louis and they have a German course there which is obligatory in the chemistry department." So Turner came and we talked it over. I helped him a little bit to get into the German lingo of the property end, mainly mechanical properties, tensile strengths and so on. So there we were, with a general course, organic chemistry, physical chemistry, and applications. And those were the four courses which were given immediately, beginning September 1940.

STURCHIO: That sounds pretty much like the curriculum that you'd established at Vienna in the thirties.

MARK: Exactly. Exactly. I just said, "Well, for God sakes, just let me do it again."

STURCHIO: How were those courses taught? Were you teaching them right from the German literature as you suggested?

MARK: Well, we had to because we didn't have very much available. The first problem was to get the teaching activated. The next was to start a few attractive research projects, because teaching alone [means that] you are a high school. In order to have an attractive standard, you have to produce new things. Novel information. In other words, research.

[END OF TAPE, SIDE 2B]

MARK: So that was then the next problem. What to do experimentally in the lab, not in the classroom. Again as I said, we had a few perfectly adequate laboratories for a beginning. Evidently what I had to do was to draw on experimental skills, the special experimental skills which we had acquired in Vienna, or earlier in Ludwigshafen. We would have to be doing something beyond what we knew, continuing what already existed.

Thus x-ray structure determination was in. That went back all the way to Berlin, to the Kaiser Wilhelm Institute, actually went back to 1922. The first thing was to get an x-ray machine and to get an x-ray physicist. So we got one, Professor Isidor Fankuchen, who had worked in England with Bragg in Cambridge for three years or so. Fortunately for us, when France fell in the summer of 1940, there was a considerable degree of nervous vexation in England, because they were afraid that Hitler would invade so they wanted to get rid of all foreigners. They didn't want anybody there who would not actually fight. So all the Americans returned to America and so on. Fankuchen was American and he came back to this country, maybe in August. He had no job so Kirk grabbed him immediately. The first real experimental laboratory which we established was an x-ray laboratory. There we analyzed Novacel, nylon; in other words, all the new interesting materials in the fiber field. And published (27).

Now the other burning problem was molecular weight distribution. In Canada I had just learned how important molecular weight distribution was for the ultimate performance of a material like Cordura. So another task would be to study molecular weight distribution. In order to do that you must be able to measure molecular weights and to fractionate the polymer. First fractionation and then measure the molecular weights of the individual fractions. Simha and [G.] Saito and others in Vienna had published on fractionation (28), so we were able to draw on existing experience.

Also we had published osmotic molecular weight determinations of fractions (29), again in Vienna, with Eirich, Saito and with others. Now, what is an osmotic cell? An osmotic cell is really two cells separated by a membrane which lets the solvent go through but not the polymer molecules; hence you get the osmotic pressure from which you determine the molecular weight. So we had to continue our membrane study and build osmometers. We built a few dozen osmometers and had them standing there and measuring molecular weights. This work would have to go on and therefore it was necessary to train new people for that because there was nobody... Well, Simha was there, but he was more a theoretician. So I myself had to train a series of young men to make membranes and to use these membranes in osmotic measurements of molecular weights. Well, it wasn't difficult for me because I had done that sort of thing for ten years now.

There was no particular difficulty because I was very lucky to get excellent people to take up and develop this new technique. One was Turner Alfrey, another one was Paul Doty; others were Bruno Zimm, Arthur Tobolsky and Bob [Robert B.] Mesrobian. Those really were the five, and all were very young then. You know, if you say 'Doty', you think of a man of sixty years of age, which he is, but then he was twenty. They had just completed their theses, in fact, some of them hadn't even made their theses yet. They were young students, but we created the first osmotic molecular weight laboratory in the United States. Right here, next door. A laboratory that was spitting out molecular weight and molecular weight distribution results. That was really new. The novel thing was the realization that averages are not enough, that you have to have the distribution.

All right. X-ray analysis is really an analytical tool. Molecular weight distribution is another analytical tool. The final analytical method which we had to develop was mechanical properties. If you deal with fibers or films or with something similar, in the end you want to know the tensile strength, the glass transition temperature, the elongation to break and such things. It turned out that the head of our chemical engineering department was Professor [John C.] Olsen, a very famous, classically-trained chemical engineer. Besides chemical engineering, he was also interested in the properties of building materials. Actually there is a company, Tinius Olsen which made, and still makes, mechanical testing instruments. I asked him for help and said, "We now have a method to measure the molecular weight and the molecular weight distribution, and we have a method to find out the crystalline structure. Now we need methods to find out the ultimate properties."

He got in touch with one of his people. Well, of course, he was a university professor; the company was a different thing. Here was the Tinius Olsen company; here was the professor at the Polytechnic. But he knew the people, and they very kindly gave us on loan half a dozen of our instruments. Tensile strength, abrasion resistance, burst strength and such instruments. Of course, we never gave them back and there was never the idea that we would give them back. They also recommended to me a man who would help us to get familiar with these techniques. What was his name? Well, it will come back to me. He taught Dr. J. Press and Turner Alfrey how to use these instruments. So we had a method to determine molecular weight distribution, a method to determine degree of crystallinity, and methods for property measurements.

If you take this publication list: let's make a test. Around 1940 you will find articles on these areas. Those were our first publications from Brooklyn. For instance, "Recent Developments in the Field of Synthetic Rubber." (30) That was x-ray. "X-ray Investigations of Carbohydrates." (31) That was x-ray. "Elasticity of Natural and Synthetic Rubber." (32) That was mechanical testing. "Composite Elasticity of Rubber." (33) That was mechanical testing. If you just go through the list you will see that these three original attempts to get an experimental foothold here for polymers resulted in twenty or thirty publications. Otherwise, if we hadn't done so the whole enterprise would have clearly collapsed. The names which you will read are exactly those which I have mentioned here.

STURCHIO: You didn't have a synthetic chemist, though, on your team.

MARK: Well, Hohenstein was originally a synthetic chemist. There was another, I just saw it here, there was another additional, I would say supportive, contribution to the mechanism of polymerization reactions. If you have a monomer, how do you get the long chain? There was David Josefowitz, who made his Ph.D. thesis with such measurements. It is also in the publication list (34).

STURCHIO: One reason I was asking about synthesis is that, from the fall of 1940, you had the leading Institute of Polymer Science in the country. I can't imagine there was any other place doing anything quite the same, with the one exception of Illinois where Speed Marvel was. And that was mainly synthetic work, wasn't it?

MARK: Yes. Let's talk about that a little bit. When I looked from Canada on the United States, the question was whether there was any work on polymers going on in academia. The answer was; scattered, not organized. Speed worked on the synthesis of new monomers and new polymers, but he never gave a lecture course on it. He gave ACS and other lectures but he didn't even give a course on it. And if he had given a course, nobody would have gone. There was North Carolina; work was done there on fiber strength, fiber elasticity. It was very good work in the textile school of the North Carolina State University, but it was isolated. National Bureau of Standards was working with rubber, but not with fibers; no synthesis, not the whole thing. Maybe there were two or three other places where isolated activities existed, but without an organized program. Our idea was that we were going to have an organized program here. It all started in September 1940, and took maybe six or eight months, before it was more or less under way. You can see from the list of publications when the first publications started to come out.

BOHNING: Were these courses at the graduate level?

MARK: All except my general course which was on the undergraduate level.

BOHNING: And how did you attract students to the program? Particularly given it was war time, or shortly before.

MARK: Well, we immediately had a full house because there were so many, mainly part-time, students who came from industry. This program was sent out to industry in New Jersey and New York, two or three hundred companies around here, and the introductory polymer course had to be given in two sections. We had eighty people.

STURCHIO: I know many people from Du Pont at Parlin, for instance, came. Who were some of the companies who sent students in that first round?

MARK: Our main customers were Allied Chemical, Union Carbide, Du Pont, Celanese, from New Jersey, they are all in New Jersey. Then American Cyanamid, Hercules, General Electric, and a large number of small companies.

STURCHIO: Did any come from Esso over in...

MARK: Esso, yes.

STURCHIO: So it was a real advantage to be in Brooklyn rather than North Carolina or Illinois.

MARK: Oh, yes. I mean it would have been much more difficult to organize courses at all, almost anywhere else. Boston might have been better. But not easy any other place. Chicago would have been good.

STURCHIO: Now, in addition to the courses you set up, there were the Saturday Symposia.

MARK: This comes a little later. The first thing was, as I said, to get the teaching under way and to get the research underway, because research takes time. If you start a research project today, you won't be able to publish anything until at least a year afterwards, or probably more than that. So, first things first; to get both things under way. By then we had assembled about ten or twelve people like Fankuchen and [Kurt G.] Stern. Stern; I didn't mention him yet. He was also a man who worked on molecular weight distribution because he had an ultracentrifuge. The ultracentrifuge is an instrument which permits the separation of the individual species of a broad molecular weight distribution. So we had Fankuchen, we had Stern, Simha, Hohenstein, Doty, Zimm, Alfrey, Mesrobian, Tobolsky, about ten or twelve people. They were all young at that time, around twenty. And very active, all of them, and a particularly efficient one was Herbert Morawetz.

So when the original research and the teaching was beginning to get under way, the question was what to do to take advantage of these activities? Well, publishing, publishing in existing journals. But when we started to send our papers to the Journal of the American Chemical Society the reviewer or the editor would say that this is a very nice paper, but we don't publish polymer papers. Evidently we should initiate our own literature. This was done in 1941, or maybe 1942, in a kind of a pilot plant run. A small publication which we called Polymer Bulletin. It appeared only for two years and then it was converted into the Journal of Polymer Science. That was our own calling card. Of course other people started to send in their papers, smelling that there was a new way of getting polymer papers published.

The next thing were seminars and symposia. That had more or less a two-fold impact. First it would draw people to the Institute and would make the Institute visible as a center. We would treat them nicely, have hors d'oevres and cocktails and such things which didn't cost a lot. Thus we started getting in amicable contact with our customers. It was an opportunity to present our own work, to present the work of others and to criticize. I don't know exactly when the first seminar or Saturday symposium was given, but it must have been late in 1940 or early in 1941. However, the spoken seminars are transient and the journals are only for small presentations so it was necessary also to consider the publication of reference books.

There should be a book for every one of our classes, of our polymer courses. A book on general polymer science (35), on organic chemistry of polymers, physical chemistry of polymers (36), and on mechanical properties (37). These were prepared and all came out within two or three years. The first, as I mentioned already, was the Carothers volume (26) and then there were four or five others and from then on a long tail of high polymeric reference books or monographs developed. Maybe fifty by now. All that came not at once, but within a reasonably time. This was essentially the housekeeping that we would have to do at home. Our homework to impress our colleagues somewhere else to cooperate with us and to consider us to be a substantial part of this new science. We also made personal contact with societies. We went to every meeting of the American Chemical Society, to the local meetings, and to the annual meetings and lectured there. One, two, three, kinds of activity. Again, visibility. One has to see to it that one of us becomes the secretary of one of the local sections and eventually the chairman of the local section. All that belongs to business and doesn't function all at once, but does so within a few years. So we were relatively deeply enmeshed in the American Chemical Society, in the Chemists' Club and other clubs or societies.

Then came international connections. As I came from Europe I knew very well how important it is to have very sound and far-reaching international contacts, particularly for the Journal, because our Journal depends on international contributions to at least fifty percent. How do you establish international contacts? First you go whenever there is a meeting somewhere. Then you invite colleagues from Europe or Japan or wherever, to come and see the Institute, give a lecture at one of the symposia, and so on. Very fortunately, Dr. Rogers was very magnanimous and we always had enough money. I would say that every year we invited at least six scholars from Europe or Canada or even from Japan. Eventually we even paid for the trip which, on the average at that time, was a matter of a thousand dollars. Now it's more, six to eight thousand dollars. Again, this doesn't get off right away, but it was soon initiated. So soon that already by 1946, one year after the war, Alfrey and Mesrobian went to Belgium. I went to Belgium. Two years later [Charles] Overberger went to Holland and then a little bit later Murray Goodman and Paul Doty went to England. Of course, my prior close relationships with foreign scientists greatly facilitated these exchanges. Not only did they visit us automatically when they came over, but also whenever I wrote...

[END OF TAPE, SIDE 3B]

MARK: The symposia took place during the school year, starting September 15 and ending on June 1. Every other week, so that for eight months, that would be 15 or 16 symposia. We had the idea for the summer that we would organize an additional, more in-depth informational meeting. What today we would call a workshop. Then we called it a clinic. I don't know why we called it a clinic. Somebody said, a clinic is where people stay for a while and then have some benefit. Anyway, we called it a clinic. What was our best weapon? Molecular weight determination. In the summer we usually had two clinics, x-ray and molecular weights. Then maybe others, I don't remember exactly anymore. The attendees had to pay tuition and we used this tuition to invite people [to the Institute]. Dr. Rogers, President of the Institute, was very magnanimous and said, "Well, whatever money you get you may use. We will give you an account to satisfy the Internal Revenue Service, you tell us what you use the income for."

So, by the fall of 1941, a year after my arrival, although we had not got very far into our program, it was all more or less initiated. Then came Pearl Harbor and the war started, and our war started here. We got a tremendous lift immediately because we were working on products and problems which became extremely important for the conduct of the war. Rubber synthesis, synthetic rubber, properties of synthetic rubber. Hohenstein and I had published on emulsion polymerization which was just exactly what was needed (38,39). Alfrey had published a thick book on the mechanical properties of polymers (37).

As I pointed out, we had worked on the permeability of membranes because of our osmotic measurements. The big problem then was the protection of soldiers against toxic gases. There was always the fear that a gas war would start. Actually it didn't but it could have. The problem of the permeability of polyvinyl chloride and other films against gases was very important. Is the film permeable enough? When the film was used as a mechanical background a very thin coating of polyacrylonitrile was put on it to make it impermeable. Permeability and impermeability of films; synthesis and characterization of synthetic rubber; and Cordura. With these three things we had very large programs from the Army and from the government; several million dollars.

We had to hire a lot of additional people. As soon as I saw that we would get into such a position, I was very anxious to take care of our complete lack of synthetic polymer chemistry, as you commented earlier. Overberger was a Marvel product, very good in synthetic chemistry, so we hired him immediately. We hired Murray Goodman, we hired Jerry [Gerald] Oster, for photochemistry of polymers, and Harry Gregor for mechanical properties. Because we had the money we added another five or six professors to our group.

STURCHIO: Did the wartime work help you to expand the post-war period? Now you had a base of equipment with new staff.

MARK: Yes. The war helped us a great deal because we had a lot of money and we could hire additional people. Some could be elevated to supervision of the work rather than to do it themselves. I mean it was silly that Doty or Zimm should work in the laboratory themselves; they were excellent supervisors. Or [George] Goldfinger work in the laboratory. He wasn't a good experimentalist but he was a very good theoretician. This was the situation of the Institute in the middle and towards the end of the war.

BOHNING: Did you maintain contact with private industry along with the government contracts?

MARK: Very little during the war. I don't know whether it was actually in our contracts as I never read them very clearly. Rogers and Kirk did it all. They did all those things, they signed everything. The idea was that while you were working for the government and while you are getting all your money from the government, you would not embark on private consulting unless it was very close to government work. For instance, Cordura. In fact, I brought Cordura to the government through my prior activity with the Canadian International Paper Company and Du Pont. I think this was more or less what happened during the war. But there were sudden certain interruptions or certain unexpected activities. One day I got a call from a Mr. C. P. Putnam; I have it all here. In fact, maybe you should take this book (40). Beginning in Brooklyn. Interlude. Weasel, DUQC and Habakkuk.

BOHNING: I have that here.

MARK: It says there: "The Weasel. In the spring of 1942 I received a call from...." and so on. You have it all. But you wanted it oral.

STURCHIO: Well, I was intrigued by the story of the Weasel and Habakkuk. From the experience that you had understanding the mechanical properties of materials under stress, you could...

MARK: You know, it was all a cinch for us because we were in the midst of it anyhow.

STURCHIO: Looking back at it now, did those projects... Well the Habakkuk project seems a little crazy, if you don't mind my saying so. The idea of building floating airfields in the Atlantic out of reinforced ice. How did it appear at the time? Perhaps, crazy is putting it too strongly. It's a little bit unusual.

MARK: It's far out. Well, I never thought it would be anything. But you know the funny thing about the Weasel? You know what the Weasel was? Actually the Weasel was used in one action in the war. One action, that was all. This action was important. The Weasel was built for the following purpose. Somewhere in the northern mountains of Norway, near Narvik, the Nazis had a radio station which was in touch with all the U-boats in the North Atlantic. The U-boats would give the station their position and the station would instruct the U-boats what to do, where the other U-boats were and so on. It was a specific type of radio message which was highly scrambled so that nobody could decipher it, but if you could unscramble the message, you would immediately see what it means. These U-boats, as you remember, did very heavy damage to the North Atlantic shipping lines and to our Russian shipping lines, so that it was necessary that this station should be demolished. It was in the middle of the mountains where there was a lot of snow. And that's the reason why the Weasel was built. After fifty Weasel's were built and tested, they were let loose and they were landed from big aircraft by parachute and assembled. Actually they attacked the station when most of them were destroyed. Whether all the people were captured I don't know. But the station wasn't destroyed. Unfortunately it wasn't damaged very much because in order to put up another mast was only a question of a few days. So I think it was all not very intelligent, but that was why the Weasel was built.

After the war the Weasel turned out to be the best business which Studebaker ever made. Because all mountain hotels and all ski resorts bought Weasels. Canada was full of Weasels, still is, you know. Now when you go skiing, you fly to Denver Airport or wherever it happens to be, Dorval Airport in Montreal, and the bus comes and takes you to the valley station where the Weasel takes you up to the hotel. So the Weasel is still produced. Of course now it's much more luxurious than it used to be. And it's a good thing. Of course all the experience and the practice with tank tracks helped to build them later. The DUQC is even a better thing. Today, if cruise ship wants to get its passengers dry on land, they have a DUQC or two. Habakkuk is used a great deal in the permanent Artic and Antartic stations; because whenever they built an igloo or anything like that, they don't use ice, but they use Pykrete.

STURCHIO: Well, it wasn't as far out as it seemed then.

MARK: Yes. Of course the DUQC was absolutely essential in the Normandy landing, so that it was a strong contribution to the war effort, whereas Habakkuk was not, and the Weasel a weak one.

STURCHIO: It sounds like you were very busy during the war. You had the Institute business, you had polymer...

MARK: Yes. By then we had about thirty people, with all those people who were hired. Many were hired only for a short time and disappeared again after three or four years. We were a big group. A big group.

STURCHIO: You established a Ph.D. program at the end of the war?

MARK: I think we had it already because Alfrey got his Ph.D. during the war.

BOHNING: Did he get it through the chemistry department?

MARK: Everything went through the chemistry department. Dr. Kirk and I felt that this young, new body should not be loaded up with a lot of administrative jobs such as a Ph.D. program. My colleagues and I were all members of the chemistry department anyway. We would say that the candidate has a good thesis, and he has passed his exams in the chemistry department. Even today we have no Ph.D. in polymer science, the degree is in chemistry.

BOHNING: You were certainly very fortunate in having extremely supportive administrators such as Kirk and Rogers who were extremely supportive.

MARK: The whole thing couldn't have happened otherwise. Certainly not without Kirk. It may have happened with somebody else in place of Rogers, because he was a little bit distant. He had a lot of other responsibilities, but whenever he was asked to do something he reacted immediately. But Kirk was our guardian angel, day in, day out.

BOHNING: When did you first return to Europe after the war?

MARK: When Alfrey came back. I went over and substituted for him. I went over in January 1947.

BOHNING: Did you return to Vienna then?

MARK: I couldn't go to Vienna because that was still occupied and Germany was still off-limits. You could go to Belgium and to Holland and to France and to England, but Germany and Austria, that was still difficult. There was no war on but it was an occupied zone. You had to have a permit to go there, and in order to get it, you would have to explain very specially why you want to go. I said that I would like to visit my family. The official grabbed the telephone and said, "Want to phone with your brother?" "Yes". And he passed me the telephone.

BOHNING: You also went to the Weizmann Institute. Was that right after the war also?

MARK: Yes. That was also an interesting situation. Dr.

Weizmann would be seventy years old in 1947. One day in 1942 or 1943, right during the middle of the war, a number of people, Mr. [Meyer] Weisgal and two and three others and I got together. Weisgal said, "It would be very nice if we could present him with the Chaim Weizmann Institute for Science on his seventieth birthday." A nucleus existed in Rehovot where there was the Daniel Sieff Institute for Organic Chemistry, a small institute. Weisgal said that he hoped to drum up enough money to build a much larger institute; the Daniel Sieff Institute was only for organic chemistry and his idea was that polymer chemistry and inorganic chemistry would be included. After a while, he said, "Well, I have enough money, perhaps not much in my pocket but I can see that by 1947 we would be able to set up a very substantial institute for Dr. Weizmann. What shall we do?" I said, "As far as preparation goes, we can do that now. We can buy equipment and we can talk with prospective section heads of this new institute." Fankuchen for physics, Hohenstein for organic chemistry, [K. G.] Stern for biochemistry and so on. Louis Fieser advised on organic chemistry. We got together a group of seven prospective, potential department heads of the new Weizmann Institute. Weisgal got some twenty million dollars. We had x-ray and infrared equipment; a whole room full of instruments by the end of the war. In 1947 this group and I traveled to Rehovot and got in touch with the architect there and laid out the plans for the institute. I was the chairman of the planning committee. The equipment was gradually shipped over and the official opening was in 1949 or 1950, I think.

BOHNING: One of the things that we haven't talked about; patents. I noticed you had a patent with [Sidney] Siggia in 1943 (41), and you did have one in Germany on styrene production, with I.G. Farben (18). Do you have a list of patents?

MARK: I think the patents are also in this publication list. [pause] No, they're not here. But I have a list of patents.

BOHNING: Would it be possible to get them?

MARK: Sure.

STURCHIO: Speaking of patents, this might be an appropriate time to go back to your consulting work for industry. Ray Boyer in an article characterized your activities as being an ambassador of polymer science by your industrial consulting (42).

MARK: Yes. Here is a very nice story in this book.

STURCHIO: Well, we look forward to reading that. In this particular article he was talking about your coming to Dow as early as 1939.

MARK: I think that in 1939 the fall meeting of the American Chemical Society was in Detroit. We stopped in Midland on the way down from Hawkesbury.

STURCHIO: So that was one example. Then he goes on to talk in that article about your subsequent visits to Dow and the sort of work you did there. We talked already about your work at Du Pont and how you began consulting for their textile fibers department. And I just wondered, for instance in 1940, when you began at Brooklyn, about how much time were you spending at Du Pont or consulting elsewhere versus in Brooklyn?

MARK: Oh, I would say that I probably was spending a day a week at Du Pont. At that time for no other company, only Du Pont.

STURCHIO: What sort of things would you do for Du Pont then?

MARK: In the early days, certainly during the war, it was mostly Cordura. During the war Cordura was a very important item. Nylon tire cord didn't exist yet; polyester tire cord didn't exist yet; Cordura was the only tire cord. All the jeeps. Four million jeeps were built and I don't know how many million other cars. So they needed tires. Permeability of films was also an important item.

STURCHIO: One thing that Boyer mentioned in this article was that you gave lectures on the fundamentals of polymer science to people at Dow. Did you do that at Du Pont as well?

MARK: Sure. Certainly I didn't give them engineering information, because I didn't have it. We were discussing molecular weight and its determination, the consequence of higher molecular weights, how is tensile strength related to molecular weight; why does this curve look this way and what is its equation. To every curve belongs an equation, you can have either the curve or the equation. Most of all, I think, the most important fundamental lectures I gave at that time both to Dow and to Du Pont were on emulsion polymerization.

STURCHIO: Bringing them the latest research findings on particular mechanisms.

MARK: That's correct.

STURCHIO: And showing how they could apply it to the problems that they faced.

MARK: Yes.

STURCHIO: What other companies did you eventually do a lot of consulting for?

MARK: Well, as I said, I only consulted for Du Pont. Elsewhere it was not really consulting in the sense that you go somewhere to discuss a certain problem. It was more that I went to, for instance, Esso, and told them what a macromolecule is, the importance of molecular weight and its distribution, end groups, and the determination of these parameters. Discuss the consequences of this kind of information. But, during the war, I visited very few companies.

[END OF TAPE, SIDE 4B]

MARK: I was quite busy in supervising all the work at Brooklyn. And then came Habakkuk and DUQC and Weasel. Once in a while, during the war, I went up to Hawkesbury because of Cordura problems. But let's talk about after the war. What were my main consulting firms? Who asked me to come and preach to them the gospel of macromolecular science? Important ones were the Shell laboratory at Emeryville, the St. Paul laboratory of Minnesota Mining [now 3M; ed.], the Springfield laboratory of Monsanto. Well, those were really the most...

STURCHIO: In addition to Dow and Du Pont.

MARK: And well, Dow yes. In addition to Du Pont. I would say I went probably once a week to Du Pont and to these other laboratories maybe once every other month. Rarely more, sometimes only once a year.

STURCHIO: Wasn't Du Pont asking you to report on your European trips from time to time?

MARK: Yes. We are now in 1947. The European trips came a little bit later. By 1947 the Institute was fairly well established, the symposia were in full swing, research was in relatively full swing. Publishing was thriving and the journal was started. The Weizmann Institute was founded. By now probably eight or nine volumes of the polymer series had been issued, so that I now had time to look for other things. What were the other things? New projects, problems, fundamental problems which had nothing to do with what I had done before. Until now, if you analyze it correctly, all we did was a continuation of what I had done in Vienna and even before that, in Ludwigshafen. From now on I started to look around and to see what comes next, in terms of new problems. Of course you can't take up ten new projects. We took up two, let's say. One was copolymerization.

Evidently the art of copolymerization would tremendously widen possible ultimate properties just by using all kinds of combination of two or three monomers. What are the laws? What are the fundamental laws which tell us why two monomers go together and why two other monomers don't go together, and so on. First we published a book: Alfrey, [John J.] Bohrer and Mark 43). A collection of all the current information, both theoretical and practical. Then we published probably twenty articles on various types of copolymerization. When that had reached a certain level the next additional problem appeared. In normal copolymerization the two monomers A and B are randomly distributed or maybe more regularly, but always in short segments. What about macromolecules where we have a stretch of A and a stretch of B and a stretch of A and a stretch of B? What about block copolymers? Or copolymers with a trunk of A and branches of B, graft copolymers. Block and graft copolymerization emerged quite naturally out of the simple copolymerization studies (44). Again, I don't know how many papers were published. We hadn't done anything like that before, it was a new entry, as it were.

STURCHIO: You began that work in the late forties?

MARK: Late forties and early fifties. All you really have to do is to look through this list. You see, for instance, block and graft copolymers and their synthesis in 1955.

STURCHIO: You were also adding new techniques to the instrumental methods that you and your colleagues were using, like light scattering, the ultracentrifuge and that sort of thing.

MARK: Well, we just used the ultracentrifuge, we didn't do any development. Light scattering was really introduced by Debye but we used it a great deal and improved it. But we didn't really contribute anything original. Well, perhaps a little bit, but Debye was the man. Peter Debye and [Arthur M.] Bueche. We were into light scattering when Debye and Bueche in Cornell started their first experiments. They got certain values for the molecular weight of polystyrene, for instance, in toluene, but they didn't have an independent method for the molecular weight of the same sample. Therefore, we immediately asked Professor Debye to send us his samples and we would measure osmotic molecular weights. We had forty osmotic cells and everyday we got five molecular weights from them. So we really helped them to calibrate their method, that was our contribution. We didn't do much on the technique itself until Zimm did something later.

Well then, that was one new activity. Copolymerization was another. Then there was another strange phenomenon. You take a polymeric material, let us say a rod or ribbon of polystyrene, at a certain temperature. [Mark sketches a curve.] This is the length and this is the temperature. You measure the coefficient of thermal expansion or, if you cool it down, thermal contraction. Then, you come to a point where this coefficient suddenly changes. In other words, here it contracts faster and here it contracts slower. Ueberreiter made such tests and Alfrey and I repeated such experiments and this effect was found in many other polymers and finally it was realized that this is a general phenomenon. It's a transition point: above a certain temperature the chain segments move more freely, therefore the coefficient of expansion is larger. Below that temperature, the segments can't move so freely and therefore the coefficient of expansion is smaller. So we determined the glass transition point (45), first with polystyrene and then with polymethyl methacrylate and so on. With any such phenomenon you first measure it with many different systems, and you measure it as carefully as possible. And then you make a theory. This we did, but even today a complete understanding of the glass transition point has not yet been reached. It has something to do with the motion of the segments, but if you start to try to describe it quantitatively with rotation and twisting... [Pierre-Gilles] de Gennes in Paris probably has the best theory for the glass transition phenomenon.

So that was another one, which entailed quite a bit of experimental work and publication and so on. Again, looking around a little bit and reading other papers, another area was of special interest. If you melt certain polymeric molecules, polyesters, nylons, polyethylene, polypropylene and cool the melt down, if necessary with the addition of a few nuclei, crystallization shoots up very rapidly, and the material crystallizes within a relatively narrow temperature range. From then on, you have it crystallized, in other words, you can't undercool these things. Well, you can if you have the material so very pure that there are no nuclei at all, then you can undercool it. But then as soon as you add a nucleus--bang. This is nothing new, rock salt does the same thing. All crystalline materials, all crystalline polymers do. But we found that there are other long-chain molecules, for instance, cellulose acetate, or certain synthetic polyesters which you could easily undercool. Almost as much as you wanted to. If the undercooled material were in the form of a fiber and you stretched it, then it would crystallize immediately. This created an impression that there are two types of chains, flexible chains and rigid chains. The flexible ones immediately adjust themselves to the thermal conditions of the system, because the segments are moving very fast. The rigid ones don't. They are sluggish, and they survive. They get into a range of nonequilibrium or supersaturation. But you can very quickly release this mechanically. In other words, a close link between thermal and mechanical properties. Thermal crystallization on cooling, mechanical crystallization on stretching. Again, so many papers. At that time people said, "So what." I myself. But Alfrey didn't say, "So what." He then sketched a theory of the liquid crystal character of long-chain compounds, of rigid chains. Flory then developed this theory completely (46); Alfrey just sketched it. But it was a thermal-mechanical phenomenon involving two types of chains, very rigid ones and very flexible ones. Those were three new fields of experimentation as well as theoretical contributions none of which had anything to do with our earlier researches.

STURCHIO: We know from the list of publications and some of the things that we've read that very interesting developments led from that. For instance, from Boyer's article; that you were then bringing the new work to industry as well, in your visits to Dow and Du Pont and elsewhere.

MARK: Yes. I think so. To give you rough time limits, I would presume that our interests in copolymerization became very intense already in 1947 or 1948. Graft and block copolymerization came in the early fifties. Then it flattened out again. The second order transition point, the glass transition, was already of interest in 1944 and 1945 and then remained so for about three years. This last one is later. It may have started in the early fifties and went on until the late fifties. All of this belongs to what we are discussing now, namely scientific activities, experimental and theoretical, on new topics.

STURCHIO: There were just a couple of other things that I wanted to ask you about before we finish. We alluded briefly earlier to European trips.

MARK: Yes, oh yes. European trips; Russian trips, very important; a very important Israel trip, and a trip to Japan, very important. Well, let's see what we still have. Why don't you take it down what we have left? We still have Gordon Conferences and activities in patent suits which sometimes were very busy. Sometimes they were very unsuccessful. Then trips abroad. Then IUPAC. Then the expansion into this new building.

STURCHIO: We haven't talked about the Encyclopedia of Polymer Science and the other journals, <u>The Journal of Applied Polymer Science</u>.

MARK: Well, maybe we can add that. I mean, the Journal of

Polymer Science soon needed additional volumes. Of course in the end, there's the final crowning of literary activity. First you start publishing your own work; then you publish the work of other people in a journal; then you publish reference books and monographs. And the end is when you publish an encyclopedia. Also we should discuss the scattering of the original members of the Institute and the creation of new polymer centers. Amherst, Cleveland, Michigan, North Carolina, San Diego, all started that way. In every case we lost a very important man, and the main problem for me was to replace him. That is a story we have to talk about a little bit.

STURCHIO: Would you like to talk at all about your work on the commission dealing with fires in airplanes and construction materials?

MARK: Yes, you're right. This should be additional to those we were just discussing.

STURCHIO: Would you like to schedule another session? It sounds to me like it's warranted.

MARK: Sure. Committee activities. Well, this probably completes it more or less.

[END OF TAPE, SIDE 5B]

INTERVIEWEE: Herman Mark

INTERVIEWERS: James J. Bohning and Jeffrey L. Sturchio

LOCATION: Polytechnic University, Brooklyn, New York

DATE: 20 June 1986

BOHNING: Professor Mark, in our last session, we concluded with the state of polymer science and education in the early postwar period. I'd like to begin today, picking up at that point, with the continued expansion of the Polymer Institute. Could we discuss that expansion and growth in terms of, well, let's start with students. Did you actively recruit students or was the reputation of the Institute sufficient to attract them?

MARK: The soldiers came back after the war and there was a tremendous influx of fee-paying students. They got money for their studies as a reward for the fact that they had served in the Army, Navy or in the Air Force and we were really swamped. Not only us, I think all universities and all educational institutions. Several things were needed in order to respond to this demand. One was space, the other was people and a third was equipment. So, soon after the war ended, say, the late forties and early fifties, those were our main obligations. Well, what did we do? Since we had earned a lot of money during the war and the Institute was in very good shape financially, we rented space around the Institute, in downtown Brooklyn. Research laboratories: you can do research almost in any apartment because, with the exception of fumes and odor, the quantities of materials with which you operate are so small that there is no danger to the environment. In fact, we rented six or seven apartments in the neighborhood, on Livingston Street, Jay Street, Willoughby Street, and started research and teaching. Teaching is no problem, all you need is a little larger room. If necessary, you can make it by breaking a wall and have a seminar room, or have a lecture room, and that's what we did first.

In terms of people, we added a number of professors; adjunct professors, assistant professors, and then we called on visiting professors, professors to come for half a year or a year and help in taking care of this larger load of educational activities. [Arthur V.] Tobolsky from Princeton was one and [Charles C.] Price from Notre Dame was another. We got somebody on loan from MIT for a short while, [Walter H.] Stockmayer. We got visiting professors from Columbia, [Victor K.] LaMer was one of them. They didn't have permanent positions at the Institute, but they were here for half a year. Later on they just came to give a course every other week. I think I probably mentioned all those teachers who were already here but we got others who became firmly attached to the Institute; Murray Goodman was one, Harry Gregor was another. They became assistant professors and later they became associate professors. They made a career here until eventually they were called away.

BOHNING: How did you identify these people when recruiting them? How did you select them?

MARK: Well, some of them, the first four or five which I mentioned as visiting professors, were already working in the field. The others came in almost immediately after their Ph.D. graduation, and those we took on recommendation from Dr. Kirk and others. Since we already had a good group together, they helped us to get more of them. And then equipment, well equipment of course, we had to buy. Again, since we had enough money, we modernized ourselves with whatever was necessary. In those days it was much less costly than it is now. Then you could do a lot of good work without super-sophisticated, push-button equipment. There were no computers, nothing of that kind. We had an ultracentrifuge, we had an instrument for electrophoresis, we had x-rays, we had infrared, we had osmometry and we had very good viscometers. When Debye discovered the light scattering method, we immediately collaborated with his colleague, [Arthur M.] Bueche, who also came and gave lectures here. For a while, we had the best light scattering equipment available. So we were in reasonably good shape.

BOHNING: Did you ever manage to get local companies or other companies to contribute equipment?

MARK: Yes, many times, many times. Usually, they said they would loan it to us but it would never be returned [laughter]. At that time that was the meaning of the word "loan", I think it still is.

BOHNING: Were there any companies in particular that you had a good relationship with?

MARK: Well, of course, Perkin-Elmer; we had and still have a good relationship. Waters Associates, and those three or four major companies which make this type of scientific equipment. I don't remember all the names, at most it would be four or five. Also, you see, we had good relations with Columbia University because of LaMer. When we needed equipment up there we could go and use it. We had very good relations all the time with Bell Telephone through [Calvin S.] Fuller and [William O.] Baker because they came in and watched us make x-ray diagrams. In fact, they published on the x-ray structure of nylon and of other fibers, after they had discussed things with Professor Fankuchen, who was our x-ray man. So that we had very good contact with Bell Telephone and we could always use their equipment if we needed to.

So, the elements for expansion were there and the question was what to do? First, teaching: we felt that we had to develop for us, and later for other schools, a standard teaching schedule for polymers. So we had one general introduction. A lecture course 'General Introduction to Polymer Science' which went back to the twenties and described what had happened in the meanwhile; how the products are made. That is a short review of synthesis, a short review of characterization, a short review of structure and properties and their interrelation, and a short review of applications. So there were really four themes in a two semester course. At the beginning, I usually gave it with these four themes. Our idea was then for those who wanted to major in polymer chemistry or polymer science, there should be special lectures for each of these themes. There was a course on organic chemistry of polymers, essentially synthesis; a course on the physical chemistry of polymers, essentially characterization and structure; a course on properties and applications. So there were three special courses, sometimes there were four when there were separate courses on properties and on processing, but both had to do with applications. Each of these courses was given by one of the professors. In the main that was our idea for structuring a logical curriculum for polymer science or polymer chemistry. It was then refined, here and there, but in general, it is the structure at all those institutions, where now, not then, but now, there is a polymer department or a polymer division. Institutions where there is somebody in charge of polymer teaching, instruction and information. That was really, more or less, our theory. And then we started doing it.

Now, this is the lowest level of teaching and instruction. The next level, of course, are seminars and symposia. We started a series of symposia every other Saturday. It turned out that people liked to come to New York. They leave Friday from wherever they are and then they spend the weekend in New York, they go to a theatre. A by-product of all that was to visit us and attend our symposium. I think I already told you these things which go back to 1947 and continue to this day. We profited a great deal, our students profited a great deal, and the Institute profited a great deal as it became known. Who knew the Polytechnic Institute of Brooklyn in 1946 or 1947? Nobody. [laughter] We usually succeeded in getting very good speakers with attractive topics, so that was an important item of publicity. That was the next level.

STURCHIO: Do people routinely come up from Du Pont at Wilmington and so on?

MARK: Yes. Routinely we had people come up from Wilmington; Du Pont, Hercules, Atlas. They came over from New Jersey; Allied

Chemical, Bell Telephone, Union Carbide, Inmont. They came down from Connecticut, American Cyanamid, and the rubber companies up there. U.S. Rubber came from New Jersey. Even from Boston, we had quite a few visitors coming down from Boston. And from Washington. This was more or less the circuit.

BOHNING: What was the average attendance of one of these symposia?

MARK: Oh, I would say between 50 and 200. We had a nice hall and the attendance depended on the attraction of the subject but more on the attraction of the speaker. The weather; at Christmas time and January the weather was usually so bad that we had smaller audiences, but, in spring and fall, sometimes we had a good 200. The next step was literature. I don't know whether we talked about that already.

BOHNING: We did a little bit, but before we go to the literature, I'd like to ask you a couple more questions. When was it that you moved into this building and was your Institute responsible for the expansion into this building?

MARK: We moved in 1956. I think the final situation was that the Institute for Microelectronics and the Polymer Institute had rented so many spaces outside of the Livingston Street community that the President felt, rightly so, that this was getting a little bit too difficult to handle and to control. He had seventeen leases to pay and to sign. So, soon after the war, 1950 to 1952, they started looking for a much larger facility to move in the whole Institute and also get all these outsiders back into the main building. This building, which was a razor-blade factory, was rented already in 1953 or 1954 but it took several years to remodel it. Not only this building, but there is another one next door here. This is now Rogers Hall and the other one is Nichols Hall so that we have now two buildings, or we had at that time, two buildings close together, large enough to take care of all these rented spaces. Now, meanwhile, of course you know, that we have another campus out in Farmingdale and we have a third campus out in Westchester. That was a later period of growth. This is what was happening soon after the war. The New Yorker brought out the profile on me which you have (47). Morton was doing what you are doing with me now: Morton Hunt was his name. He did this profile in 1957, so he started working on it in 1956. That's the reason I know for sure that we were already here.

BOHNING: Tell us how large the Polymer Institute was, relative to the rest of the work that was going on at the Polytechnic Institute of Brooklyn at that time?

MARK: In chemistry? Well, we were a little more than half of the chemistry department. In the early and late fifties, we had 12 professors and 90 graduate students; a relatively large organization and we needed space for all that.

BOHNING: How many graduate students were there throughout the Institute in all subjects at that time?

MARK: Oh, you mean electrical engineering and chemical engineering and so on. I think, altogether, maybe 1500 graduates of which we had 100.

BOHNING: When did the Institute reach its maximum size of staff and students?

MARK: Now. Now we have 5500-6000 students, something like that.

BOHNING: But I mean the Polymer Research Institute.

MARK: Oh, the Polymer Research Institute. Oh, we had our peak in the late fifties and early sixties. Then many of our best professors went to other places and then we shrank. Now, for the last three or four years we're on the upswing again.

BOHNING: When those other people started to leave, did you encourage their separation from Brooklyn?

MARK: Well, it depended. You know, one day Overberger said, "I have an offer to go to the University of Miami," but I said "Don't go. You go when the offer is such that there is every reason that you can there develop what you want to develop; maybe a polymer group, maybe some other group." For Overberger, that time was when he got the offer to become head of the chemistry department at Michigan. The same thing with Murray Goodman; he also had several offers to smaller places and I always said, "No, don't go," and then, when he got an offer from the University of California at San Diego, I said "Now, you go." And the same thing with [Robert] Ullman, and with Alfrey. When I felt that their future would be endangered if they stayed with us, I said "No, get out of here." And it was to our own advantage, because what did we have then? Very good friends. At Case Western, at Michigan, at the University of California, another at Dow, at Du Pont, and so we profited. And I must say they all did very, very well, in terms of their own careers.

BOHNING: Did you participate in looking for positions for them?

MARK: Not really, no. I was asked, of course. I was asked in each case, but I didn't start it. They were lured away. Maybe we can continue the mainstream of the story by coming back to this higher level of teaching through the seminars. Then, of course, when a new science is being developed, you need the literature. Until then, articles on polymer chemistry and physics were published in the Journal of the American Chemical Society, the American Journal of Physics, in other words, scattered all over the place. Now, of course, this could not be tolerated for a very long time. In fact, Germany, already starting ahead of us, had founded a macromolecular journal. So we felt that what the Germans did, we should do over here. We got a publishing company which was ready to take the risk because one didn't know if it would work. At that time it was Interscience Publishing Company, a small company, but with a very vigorous leadership. So, in early 1948, or whenever it was, we started the Journal of Polymer Science.

At that time we asked Professor Marvel, who was already a very distinguished polymer chemist to come in as editor and he kindly agreed. He, Doty and I started the journal as a little micro-journal, and it took three or four years until it really got momentum. A journal is one thing, monographs are another. A journal represents the general spread-out activities in the field, but for every specific important segment of the field, for synthesis, for characterization, for behavior, for processing, you need monographs. So, we started a series of monographs with the same company earlier, in 1940. By now there are fifty or something like that. They were good business for the company because the field was attractive. Fortunately, we got good people to write these monographs.

Then, this is now later on, the next -- I don't want to say the last -- but the next important step was to get an encyclopedia. The volume of literature becomes larger. Journals: in the fifties and sixties there were four to six polymer journals; there were the two German journals and there were our two journals, and there was one in Japan and there were two in Britain, and there was one in France, and one in Italy. Perhaps a dozen international journals on polymers. And, of course, all the monographs. So it was felt that a necessary concentration would be as an encyclopedia.

[END OF TAPE, SIDE 1C]

MARK: It was a little later that we started to organize an encyclopedia of polymer science and engineering. Essentially those were the steps; educational, all of them; informational, all of them; and that's where we are now, more or less.

While we organized these symposia here at Poly, I was also chairman of the Management Committee of the Gordon Research

Conference. At first they were located on Gibson Island on the Chesapeake Bay, but I think in 1949 or 1950, this location became too small. They moved up to New Hampshire, and have grown to be an internationally recognized large collection of symposia, 90 or 100 every year, but the origin of them was in polymers. The first Gordon Conference was in those years, 1946 or 1947 when there was one on fibers, one on rubbers, on adhesives, on coatings, and there was a general polymer conference. Also there was another on petroleum as well as one on biochemistry. But four out of seven were on polymers. Therefore I suggested to build that up and in fact now [Mark refers to a listing of Gordon conferences] I have underlined those which have to do with polymers. There are a dozen or so of these conferences which are now essentially devoted to polymeric materials. Meanwhile the entire enterprise has grown very much into the biomedical and into the biological field, the trend which is now so evident. But, in those days, we really started to push Dr. [Neil] Gordon and Dr. [Wilber G.] Parks; Parks was really a moving element in those years. So this was an outside contact where we could make good propaganda for polymeric materials. At that time we had a polymer conference, a fiber conference, a rubber conference, and one on coatings and adhesives, so four Gordon Conferences were definitely devoted to polymers. They were also part of [polymer] education, national and international education.

STURCHIO: What impact did the those Gordon Conferences have on the polymer field at that time?

MARK: Enormous, I would say.

STURCHIO: In what ways?

MARK: Well, you see, many companies didn't recognize the existence of polymers. None of the oil companies, none of the rubber companies; the rubber companies didn't know that they were working with polymers; the textile companies didn't know that they were working with polymers. Derring-Milliken, James River Mills, J.P. Stevens and all the large textile companies didn't know that what they really did was to process polymers. Particularly, the fiber companies had a tremendous impact when they hired polymer scientists. You see, since Carothers, fiber technology is polymer technology. Well, it was largely mechanics as long as cotton, wool, and silk were the mainstays. The only chemistry in the textile industry was dyeing, and that was an art. Everything was mechanical. And then suddenly, everything became polymer chemistry. With nylon, with Dacron, with Orlon and with all these fibers. There I think the Gordon Conferences had a tremendous impact.

BOHNING: The companies would send their scientists?

MARK: Yes, and not only their scientists. Their presidents came, or vice presidents and research directors. I mean, in the Gordon textile conference, which was managed by Milton Harris for several years, the research directors of Burlington Industries, and of J.P. Stevens, and of James River Mills, and of Derring-Milliken; they all were there or one of their leading chemists.

STURCHIO: I know that Gordon Conferences are well known for their informality and the fact that people were restrained from publishing.

MARK: Yes, see that's really what they learned from us. I think this policy took over from our seminars here, from these polymer symposia. I and others told them to look here. Gordon and Parks knew about them. If somebody gave a lecture here in one of the Poly symposia, he didn't publish it. Why should it be published? He would describe the experimental results, and would explain ideas about the meaning of the results. It was preliminary information. Of course, he told the truth, but did not imply that it was ultimate truth. If somebody said something in our symposia which he shouldn't have said, because his company patent lawyer may scold him, this was not [considered as] official information. It would not preclude a patent, because a few sentences or words were said in a symposium in Brooklyn. wouldn't mean that the material was unpatentable. This informality is exactly what the Gordon Conferences took over, and still maintain.

STURCHIO: So there is real interchange between the academic side and the industrial?

MARK: Yes. And an interchange which is not, let us say, made difficult or impossible by legal considerations.

STURCHIO: Do you recall some particularly important episodes from the Gordon Conferences in those years where startlingly new results came out which people were really excited about?

MARK: Almost any important new result was first orally presented at the Gordon Conference. For instance, the theory of polyelectrolytes, the influence of ions on the conformation of macromolecules. [Raymond M.] Fuoss at Yale worked on that as did Turner Alfrey here and there was a most interesting discussion between them from which later developed the whole theory of polyelectrolytes. Copolymerization. Immediately after the war we said, "Okay, now here we have twenty polymers. How about blending them, how about adding the monomers together." That was when copolymerization evolved. First as an art, and then the time came when there were enough experimental data available that a theory on the principles of copolymerization could be formulated. Goldfinger and Alfrey worked it out here and Mayo and Lewis worked it out in New Jersey at the United States Rubber Company, almost at the same time, and almost identical in content. And that was first discussed at the Gordon Conference with a tremendous amount of interest to everybody and with a very lively discussion.

Another first was living polymerization. In the fifties, or whenever it was, Professor Michael Szwarc, who was at Syracuse at that time, demonstrated that if you have a polymeric chain growing with an anionic end you could have a cation which would protect this end from termination. So that you could, for instance, polymerize styrene up to a certain molecular weight, and then you can add another monomer and it would keep on adding to this chain. In other words, the chain was living, it wasn't growing anymore, but it was still alive. That led to the expression "living polymers." The first time that this was openly disclosed was at a Gordon Conference. And so, there were many examples.

When I came back from Germany in 1954, I gave a lecture on the Ziegler polymerization so that the first information about Ziegler polymerization was given at the Gordon Conference. And a year later, I did the same thing for the Natta work on polypropylene. Now, that was not my work, but I had heard of it. At a Gordon Conference, you know, you could talk about anything, it wasn't necessary to talk about your work. You could say, "Well, reading this and reading that, I think that there is something interesting going on in this and this field," then you could present it, and then if they didn't like it they would tear you to pieces; like tigers.

STURCHIO: Do you recall what the reaction was like when you brought back news of Ziegler's work?

MARK: Tremendous. Nobody believed it. The first thing was that nobody believed it. Can't be! In fact, the feeling was so strong that I went up to the Dow Laboratory, Alfrey was there already, and showed it to them. They gave me titanium tetrachloride and triethylaluminum and so on. Then they saw it. They saw.

BOHNING: Did they believe it then?

MARK: Then they believed it. Yes. Well, everybody who saw it believed it. Two companies took advantage of their direct contact. One was Montecatini and the other was Hercules. Montecatini had two people there who actually saw it and Hercules had Dr. [George E.] Hulse there and he immediately said, "My God, get it, get it, get it." That was the reason that Hercules had such a leading position.

STURCHIO: When you talked about it at the Gordon Conference, did people try to construct theories why it couldn't work, why they didn't believe it?

MARK: Well, I think there were several arguments. One was that triethylaluminum is a dangerous material which catches fire on exposure to air. Even though the reaction works in a small glass container, you could not develop a big industry involving such a super-dangerous material. That was the real reason. They didn't deny that polyethylene was made. In fact, you know it's very interesting that Speed Marvel, although by then it wasn't realized, had made polyethylene in 1930 with the aid of butyl lithium. It is in a paper by Marvel and Friedrich (48). Strangely enough, Du Pont at that time wasn't interested in polyethylene; they had polystyrene, nylon, polyester and such things so they didn't follow up. The fact that polyethylene can be obtained under mild conditions as well as at very high pressures was known. It wasn't known to most people but it had been established. The reason why there was a great deal of reluctance concerning the importance of this discovery was that people said it was such a dangerous process for large-scale operation. However I said, "Look at the Grignard reaction." The Grignard reaction also uses a reagent, methylmagnesium bromide, which starts to burn if it exposed to air. It's not as vigorous or as violent as triethylaluminum but the Grignard reaction is being used industrially. Their argument against me was that the Grignard reaction was used in the preparation of pharmaceutical products on a relatively small scale. It's a reaction where you put a methyl group in some compound to improve or modify an known complicated molecule. But here, you want to make a hundred thousand pounds of polyethylene and that's a different story. So, we battled it out, you see.

STURCHIO: It would be interesting to have a list of who was at that meeting to find out who was taking part in these discussions because of its interest. Are there records?

MARK: I think there are attendance records.

BOHNING: We might try to get a hold of them.

MARK: You should look at the Polymer Gordon Conferences for 1954 to 1956.

BOHNING: We could write to [Alexander M.] Cruickshank.

MARK: Yes, Cruickshank, he's the man. He's a wonderful fellow. He handles this tremendous job without any fuss. You know, if something of this order of magnitude had to be handled in the government, there would be two hundred employees. [laughter] You know? There would be a director, there would be two assistant directors, and there would be 150 secretaries. And he does it all himself, with three or four secretaries. Fantastic.

STURCHIO: That says something for free enterprise, and also for entrepreneurial ventures.

MARK: A man who runs it all, who spends all his time on it, is knowledgeable, and doesn't irritate people. If you talk with him, he will always listen. Whether he'll do anything is another question.

BOHNING: At least you get the impression he will. One thing, when you were setting up the original Polymer Gordon conferences, was anyone else involved in defining the nature of these conferences?

MARK: Well, I think at the first conference, Carothers spoke, I think the first was in 1936, he died in 1937. But, then, of course, there was the war and there wasn't much going on then during the war. And, then, Milton Harris was in the fiber field. Paul Flory was in the polymer field. Gus Egloff was in the oil field. Whitby was in the rubber field. Every field had somebody who was putting in suggestions and proposals. [Carl S.] Fuller at that time, also in fibers from Bell Telephone.

BOHNING: Let me go back for a moment to the encyclopedia. How did you determine the organization of that encyclopedia?

MARK: Well, the original classical encyclopedia is a German work; Ullmann. A certain professor [Fritz] Ullmann in the years before the first World War felt that chemistry was now important enough to have an encyclopedia. And it was Die Enzyklopadie der Technischen Chemie by Ullmann, very famous. Still exists as a famous traditional work. I knew of it from my activities in Germany and we all used it. It was in the library and whenever I wanted to know something about an unfamiliar field I went to Ullmann. Vacuum; how do you make a vacuum? How do you make very high temperatures? And so on. This was our model. Quite early after I came over, maybe 1941 or 1942, Dr. Proskauer, who was with Interscience and who also knew Ullmann, and I together realized that there was a vacuum in the United States with respect of such a work. He had a lot of good connections in the publishing field, [but inquiry showed that] there was nothing.

When we approached Dr. Kirk and told him he thought it a wonderful idea. That we should try to make an American Ullmann. So there were discussions for and against, and at one of them he said, "Well this is all right, I am a chemist. I could take care of the chemistry, but who could take care of the engineering?" And then a minute later he said, "Maybe I should ask Donald Othmer, who has his office next to mine. Maybe he's interested." From then on, there was a group of Kirk, Othmer, Proskauer, Dekker, and I. In the planning stage of the Kirk-Othmer, we met every other Wednesday, I remember, in our Faculty Club and started developing ideas. I don't know when the first edition of Kirk-Othmer started, but I think it started soon after that. So then there was the Ullmann. Well, once we hit that as an example, it wasn't difficult to set up the same thing for the polymer field. That was really the origin of the idea we ought to do the same thing for the polymer field as had already been done for chemistry in general. And there is now, you know, these red books on the left side; [Mark points] this is now the second edition of the Encyclopedia of Polymer Science and Technology. Kirk-Othmer now is in its third edition.

STURCHIO: The first and second editions of Kirk-Othmer are still useful in some ways; they aren't entirely superseded, I think.

MARK: No, no. Particularly, there is always a certain change in the authors. First of all, some of them die, some of them don't want to do it anymore, some recommend someone else. So that the style changes.

[END OF TAPE, SIDE 2C]

STURCHIO: A venture like this; the Kirk-Othmer Encyclopedia or the Encyclopedia of Polymer Science and Technology has to be very expensive to the publisher. I wonder if you could talk a little bit more about Proskauer's vision as the publisher involved in polymer science, because the two of you, with Dekker as well, really were a team in building up this literature. It must have been a very risky venture, as you said earlier, for the publisher. How did they find the resources to do this?

MARK: Yes, there was great hesitation, particularly on the part of Dekker, who eventually would have to take over the financial responsibility. I mean Proskauer was an editor, and I was a scientist, Kirk was a scientist, and Othmer was an engineer. We didn't care too much about whether it would make money, but Dekker did. They were very careful and of course they knew all about Ullmann. They knew that Ullmann was one of the best businesses in the publishing field. So they calculated how many people would take it, what would be the probability of success and so on. Right now, both encyclopedias are the best businesses in Wiley, in terms of return on investment.

STURCHIO: So, it was the right choice.

MARK: Yes. You see, the authors get a handshake. Most authors are satisfied with that because they like to have an article in an encyclopedia. It's propaganda for them personally, it's propaganda for the company. If you appear as a witness in a court case you can say that I have published this article in an encyclopedia. That would make a big impression on the judge, because, "encyclopedia" for a judge; that's wisdom and there is nothing beyond that and everything contradictory is either wrong or at least doubtful. So, in other words, the authors get nothing or very little. The editors, the same thing. What are we; three editors or something like that. The publishing itself; each encyclopedia in terms of the entire publishing volume of Wiley is not even one percent. They publish 162 journals, or something like that; they publish so and so many books every year. So this is a droplet. In other words, it comes with the standard costs. They have no specific investment for the encyclopedia. Well, they have a little staff, that's all. They have Mr. Grayson, they have Jackie Kroschwitz. Five maybe, so the costs are very small. And you know, every volume costs six or seven hundred dollars, and they have almost two thousand subscribers.

BOHNING: So every library that does any chemical or engineering...

MARK: Yes. And it spreads out. Let us say a volume costs $700 and two volumes a year are issued. In the end, when the librarian has to defend expenses of a research character, that would be $1,400 for the encyclopedia. Forget it, no? That's the reason why it comes out slowly.

STURCHIO: Speaking about how the encyclopedia did so well for Wiley and for Interscience; what were some of the other books in the Polymer Monograph Series that sold very well?

MARK: Well, the Carothers volume sold very, very well. The Alfrey volume sold very, very well. The Ott cellulose volume sold very, very well. Later, the volumes on polyethylene and polyolefins sold very well, polyurethanes sold very well, polycarbonates. Three volumes on the analytical chemistry of polymers. Those were best sellers.

BOHNING: Many of those came when the series was very well established. Can you just talk a little bit about the relation between the Journal of Polymer Science and the monograph series. Dekker must have been convinced that this was going to be a commercial venture. He had to worry about cash. Was it difficult in the beginning? Were you worried about its long term viability?

MARK: Oh, yes. The circulation of the first three years of the Journal were small.

BOHNING: How many subscribers did you have in the first year?

MARK: Well, I think 500 to 600; something of this order of magnitude, and then it gradually built up to 2000 for each of the journals. We were lucky in the series because the Carothers volume was a big hit. And the Alfrey volume was a big hit because it was the first and only publication on mechanical properties of high polymers. The Ott volume was a big hit because it was cellulose. In those years, rayon and cellophane were very important items. I think these three helped the whole thing to get going.

STURCHIO: Carothers was the first of the series and Ott was an early volume.

MARK: Ott was volume five and Alfrey was volume six. In between, there were other volumes. Meyer and Mark was not bad; that was an overall presentation of the polymer field. I think that was also a good one.

STURCHIO: Were you using those books in the teaching program?

MARK: Sure, sure. Not only us, but eventually, everywhere.

BOHNING: And one other question I had was about the decision to form the Journal of Applied Polymer Science.

MARK: Well, the Journal of Polymer Science was swelling the spectrum of information. Certain articles really dealt with theoretical and very fundamental aspects, like most of the papers of Flory. A normal polymer chemist couldn't care less, he wouldn't know what to do with it. On the other hand, there were processing papers. For example, how to cast a double layer film, or a five layer film. A professor of polymer chemistry couldn't care less. We felt at some time that a split should be made. The birds should be here and the snakes should be there.

[laughter] I don't know when it was, sometime in the sixties. Each of them kept on swelling. We even added another split into Polymer Chemistry and Polymer Physics [sections of the Journal of Polymer Science]. There were proposals that we also should split the applied journal into characterization and application. Or into properties and processing. We haven't done it yet. Maybe someday, we'll do it.

BOHNING: Does that mean that polymer work is becoming very segmented? Production and such?

MARK: The customers become segmented. You see, polymer science to a certain extent, a much lesser extent, has followed the development of medicine. Two hundred years ago the doctor knew everything about medicine. He knew everything about the body and he knew everything about all the diseases. And what is it now? Fifty different disciplines. This is a different doctor, and this is a different disease; this tooth and this ear. Now there are so many more diseases. Well, chemistry was the same thing? There was organic chemistry, and there was physical chemistry, and there was analytical chemistry, and there was biochemistry. Every science, as it grows, splits up. And, of course, it's a good thing if you can anticipate that.

STURCHIO: Who were your closest collaborators in editing the journal in the early years? You've mentioned a couple of them to me.

MARK: Well, of course, Marvel, Whitby, Doty, then Overberger; very prominent, also Alfrey. Ott helped us a great deal. Milton Harris helped us a great deal. And then came in many, many others. And now the series is more or less automatic, works more or less automatically. And then the journals; both of the journals have a symposium edition and these symposium volumes more or less correspond to a frozen-in Gordon Conference. Do you have a list of those? So there was a feeling, and I think it's a correct feeling, that if there is a good symposium, not a Gordon Conference but a more formal symposium, where people want to have their stuff published, with all the consequences of patentability and so on, we should give them the opportunity. I think there have been 50 or 60 but since these symposia volumes are being issued as part of the journal, the necessity for the monographs became less important. So the monographs have now slowed down somewhat except when a particularly exciting new field opens up. What is now mainly thriving are the journals and the symposium volumes.

STURCHIO: Well, through your efforts and the efforts of your collaborators over the years, you've really created a library of polymer science and engineering. It's a remarkable collection.

MARK: Yeah, unfortunately, unfortunately. [laughter]

STURCHIO: But it's very different than it was 45 years ago.

MARK: There really wasn't anything, no.

BOHNING: Are there similar publishing ventures in other countries?

MARK: Yes, sure. In Germany.

BOHNING: But to the extent and the variety that you have here?

MARK: Yes. They have three polymer journals in Germany. They had the first before us, and then the others started after we did. They have a large number of monographs. Springer, and VCH, you know these very active German publishing companies. I don't think they have yet a real polymer encyclopedia. I talk with them, of course, occasionally, when I go over or when they are here, they always come in. And they say, well, you know, encyclopedias are something so international that there isn't another like yours. It's hard to imagine that one in the German language would sell very much. Our own encyclopedia sells 60% abroad. Unless a German encyclopedia also gets 60% abroad. Well they will never get that with a German encyclopedia.

STURCHIO: Although isn't Ullmann now publishing an English edition?

MARK: Yes. They started to publish quite a while ago.

STURCHIO: Does that have much of an impact on Kirk-Othmer?

MARK: It's difficult to say. My personal opinion is no, because I presume those libraries which need it will take both. They are sufficiently different in approach if not in content.

STURCHIO: Speaking of German polymer publications, I wonder if you might say a few things about the changes in the polymer literature over the past fifty years. Fifty years ago, if one wanted to find out about polymers in the U.S., with the exception of Carothers and Marvel's work, one really was out of luck; one had to read the German literature, the work of Staudinger or the work that you and your colleagues were publishing in Europe. Now, presumably, things have changed. I think it's fair to say that most European polymer scientists have to pay much closer attention to American work than one had to do fifty years ago.

MARK: Yes. The volume is very much larger here. It's all in one country, it's all in one language. If you take Western Europe all together: I think there are 350 million people in Western Europe, the Germans and the French and the English and the Spanish and the Italians, and the Swedes and so on. There is a special literature in each of these countries, in French, in Spanish, in Italian, in Swedish, all scattered in the wind. Whereas here it is all uniform, it's all English, it's all made in certain places. The center of gravity has moved over to this country. There is no question. But, of course, there is now a second center of gravity in Japan.

STURCHIO: When would you say it was clear to the international polymer community that the center of gravity had come to the U.S.?

MARK: Oh, I would think it gradually developed, but you see, after the war, Europe was completely obliterated and for five years practically nothing was done. We had five years, from 1945 to 1950, to get in gear and afterwards there was never a question anymore that it would be maintained, not only maintained, but it might even be accelerated.

STURCHIO: And, of course, those were the five years that you were building the Institute.

MARK: Yeah, that was the five years, exactly. [laughter] I was lucky, you know? I had this opportunity.

STURCHIO: You mentioned Japan. When would you say it became clear that there was another center?

MARK: Well, it became clear, not in polymers, in fact, not in chemistry. It became clear in photography, in microelectronics, and even in building automobiles. I think the first thing which made it clear that the Japanese can do things which were not easy to do anywhere else were the big ships. You know, when they were the only ones who could launch a ship of three hundred thousand tons, or something of that sort of magnitude. And then came physics, essentially. Microelectronics, optics and these things. The center of gravity is still in this area. There, apparently, they are clearly ahead of us. In chemistry, including polymer chemistry, one cannot say that. Yet.

STURCHIO: But are they strong competitors now?

MARK: They are certainly strong competitors and welcome competitors because they are very good people. Even though, Japan has not yet produced a Ziegler catalyst, a living polymer, or a completely new theory of polymer configuration like Flory's. In other words, in the polymer field, the firsts have yet to come from Japan.

STURCHIO: Have you trained many Japanese polymer scientists here?

MARK: Yes, yes. Oh, in the course of the years, we have maybe 200 students, and at least forty or fifty have important positions in academia or in industry. One of them, [Yasunori] Nishijima, is the President of Kyoto University, another, [Naoya] Ogata, is the president of Sophia University. [Kazuyuki] Horie is the President of Tokyo University. [laughter] So, we have a good record. And then, quite a few have important positions in industry.

BOHNING: Well, we've started to talk about international relations and powers, maybe this is a good time to talk about IUPAC and the long term international collaborations.

MARK: Yes. Reviewing my specific activities on the international scale: it started in 1934 when I was a professor at the University of Vienna and it had nothing to do with polymers. It had to do with heavy water, with the existence of deuterium. It had been established that the melting point of D2O is 4oC and of course, the melting point of H2O is zero. That was one way in which these materials had been separated in the laboratory. The density, of course, was different, that's why it`s called heavy water. The discoverer of heavy water himself had established that and had separated them by repeated crystallization and melting. Urey discovered heavy water in 1932 and got the Nobel Prize almost immediately, in 1934.   He had used a number of methods. Well, from this difference in melting points and from his results, I concluded that with a large mass of ice exposed to temperature fluctuations, the heavy water would melt off later, so its concentration would increase. There would be a separation in situ, not experimental. First we went to the Alps and investigated the ice from under a glacier, and compared it with the water on top of the glacier, but we didn't find any difference in density. Our conclusion was that the glaciers in the Alps might not be old enough or large enough. Maybe an effect could be established if we go to a much larger glacier.

Now, the closest glacier which is much longer and much older, is in the Caucasus, the Bezingi glacier. So I published an article in the Austrian Academy of Sciences on this possibility (49), and they got in touch with Soviet Academy of Sciences, and the two academies sponsored a joint scientific expedition to the Caucasus. A group of four went there, the Russians had another four, and the group of eight and went into the Caucasus, climbed mountains. The highest mountain there is almost six thousand meters, which would be 18,000 feet or something like that; Mt. Elbrus. We made measurements at the Bezingi glacier. Professor Vavilov was the leader of our Russian companions and the results were published by my collaborators E. Baroni and A. Fink (50). There was a noticeable and measurable difference in the water and ice at the top of the glacier from that at the bottom. It showed that the laboratory observations actually took place in nature.

That was the first time that I was in the Soviet Union, I was there three months. The next trip abroad was to Israel, to the Weizmann Institute, in 1946. I think we spoke about that already.

[END OF TAPE, SIDE 3C]

MARK: In 1947, I was invited to the University of Liège in Belgium as a visiting professor to introduce courses on polymer chemistry. At this time, there was a general IUPAC conference -- IUPAC is the International Union of Pure and Applied Chemistry. There I proposed that there should be a special division of polymer chemistry. There were already divisions of organic chemistry, inorganic chemistry, analytical chemistry, and biochemistry. The proposal for a special division of polymer chemistry was accepted in principle and a committee was formed, of which I was the chairman, in order to organize future polymer symposia within the framework of IUPAC. This committee still exists. I think that the next chairman was Harry Melville. I think the chair goes around internationally every three years. The Russian was B. N. Kabanov, Nishijima from Japan was chairman, Aaron Katchalsky from Israel and so on. So it is now a big thing which grew like the Gordon Conferences, because it was effective and because of the importance of the subject.

Then, a few years later, in 1949, I was approached by a Dr. Egon Glesinger, who was the chief chemist --- I don`t know what his title was actually-- in the FAO, the Food and Agriculture Organization of the United Nations. You know the United Nations has a number of subsidiary organizations: UNESCO, the World Health Organization, and UNIDO. FAO had a branch on forestry, and Dr. Glesinger was the head. Forestry then and all during its development was an agricultural activity. It had to do with growing trees and cutting trees, and utilizing trees, making money with trees and so on. They didn't have the slightest idea that chemistry was involved. Since I was a cellulose chemist at that time and had written a book on cellulose chemistry, I was appointed to organize a chemical committee in this forestry division of FAO and somehow to introduce some chemistry into the forestry branch. Well, that was when I urged Emil Ott to write his book on cellulose chemistry. He did, and his book was important in instilling more chemistry into FAO activities.

BOHNING: What sorts of applications did they find for chemistry?

MARK: Well, of course, the wood has to be cooked in order to separate the lignin from the cellulose. The entire activity is chemistry. In the old days, they called it cooking because they didn't know it was chemistry. They wanted to dissolve lignin to leave cellulose. A tremendous amount of chemistry is involved, and the growth of paper chemistry, rayon chemistry, and cellophane chemistry from after the war until now was essentially caused by the realization that the separation of lignin and cellulose is a purely chemical problem; different ingredients, different temperatures, different pressures. In other words, it was a chemical engineering problem. It is considered that now but it wasn't in the forties. Then it was considered an art where the lumberjacks get in touch with somebody who cooks, who runs a digester, and in the end you make paper. But not rayon, and not cellophane, or nothing that was any good.

I was the chairman of this wood chemistry committee in FAO for fifteen years. We had yearly symposia on an international basis, of course, like all United Nations activities, and a lot of publications came out. You'll find them in my publication list (51,52). Finally, of course, I made way for someone else but the committee still exists, gives awards and everything. In fact, it is more important than it used to be because of the environmental situation. The old paper industry was very destructive environmentally. Pulp and paper mills produce a lot of SO_2 and other noxious gases which pollute the atmosphere and the water is contaminated. In my days, that was not yet so important; all these environmental worries started sometime in the sixties, but not in the forties. We weren't worried about nature in the forties, but this committee now has a great deal to do with environmental improvements.

Then, a short but also interesting international contact in 1956 took place in Japan. I was invited to give a lecture, in fact, to chair a symposium on polymer science in Tokyo, and later one in Osaka, so I was to be in Japan for two or three weeks. Before I left, I got a telegram from Professor Mizushima who suggested I should demonstrate nylon polymerization to Emperor Hirohito. There is a little experiment to make nylon in a test tube by the reaction of a diacid chloride with a diamine: I demonstrated it in Europe and in several other places. It was originally invented and shown for the first time by a Du Pont chemist [Paul W. Morgan: ed.], but then the Du Pont lawyers said, "Don't show it any more." They didn't want the public to know how easy it is to make nylon [laughter]. Anyway, I was known to have shown it to a number of people, and this Professor Mizushima, who was an advisor of the Crown Prince, his tutor in fact, felt that it would be nice to show it to the Emperor. I was invited to the palace with my wife and there we demonstrated the rope trick; a rope of nylon is pulled out of a mixture of liquids. That was a short but very glamorous contact. Since then I get a lot of letters from the Emperor; you know, he's a world renowned marine biologist. He still works in a laboratory in his palace. So that was a little bit like the coat hangers made of platinum wire.

BOHNING: What was his reaction when you did that?

MARK: Oh, it was very interesting. He didn't speak English with me although he speaks English very well. He understood every word; I explained to him the chemistry of the reaction but he never answered in English. Of course I didn't understand Japanese, but an interpreter amongst the several people around explained to me what the Emperor meant. When the Emperor had left, I asked the interpreter, " Why didn't he answer in English? He speaks English fluently" And he said, "You see, the Emperor is not supposed to make any mistakes. Not even the smallest. He speaks English fluently, but he makes some mistakes, and we didn't want him to be exposed to this danger. Therefore, he spoke Japanese and I interpreted. If I make a mistake, it doesn't matter, but if the Emperor makes a mistake ..." [laughter] While I demonstrated the experiment to His Majesty, my wife and the Empress sat in another room and talked whilst they had something to drink. When I asked Mimi what they talked about she replied, "Only about fashion." [laughter] Not a word about anything else; only fashion.

About ten years later, there was another contact with the United Nations. You know, the United Nations have their ministries, so to speak, not in New York. FAO is in Rome, UNESCO in Paris, World Health Organization in Geneva, and UNIDO, the United Nations Industrial Development Organization, is in Vienna. Dr. Rothblum, who had studied in Brooklyn was there in a high position. He wasn't a polymer chemist, but a chemical engineer. He knew the Institute and we were acquainted. So, at one of the visits I made at that time, I suggested to him, when we contacted each other, to use the Austrian polymer laboratory to organize a course in Vienna for visitors from the third world countries and to do the same in the fiber field. The Vienna laboratory may not be a leading laboratory for fiber technology, but it's a good technological laboratory. It's a good informational laboratory and a good teaching laboratory. The same thing in plastics. There is good teaching in plastics, there is good teaching in fibers so why not invite people to attend seminars? And that was done. There were two directors, Dr. [Hubert] Tschamler, the director of the plastics institute and Prof. Hertzog, at the fiber institute. These two-week symposia ran with twenty people from various countries, usually from Africa, India or South America. UNIDO picked up 50% of the check and 50% by the countries sending the attendees. It has become a very successful teaching activity. In fact these activities have increased since UNIDO moved into a very much larger and very beautiful location. They are all quite satisfied; perhaps it is really not technology transfer, because you cannot transfer technology in a lecture room, but it is transferring information and knowledge, which you can do and have to do in a lecture room. I think that knowledge and information transfer to the third world in the field of fibers and plastics is being taken care of rather well by this UNIDO organization. UNESCO also has programs and each has its own program in transferring information, and of course, in transferring technology, which is not so easy.

After some time of successful information transfer to the third world it was decided to try technology transfer at a single place in one instance. Mr. Shroff, the director of Sasmira, an Indian silk textile mill, contacted UNIDO because he wanted to know how polyester and nylon is spun, not in the laboratory, but in a pilot plant; certain nylon and polyesters, blends, mixed fabrics and so on. A rather large project was developed for India by UNIDO and Sasmira. Over the next few years, a pilot plant unit was constructed and established for Sasmira in Bombay, and started to spin nylon and polyester. Then they continued by weaving the synthetics with silk, and with cotton, and with other fibers. This was an early case, in 1965 or 1966, of a successful technology transfer on the pilot plant scale. We started it all, our Institute being the polymer part, and UNIDO giving the money. We did the chemistry, they got a German engineer, Dr. Herlinger, to do the technology, and the project started to work in a few years. It was the cradle of a number of larger scale nylon and polyester plants which grew up, partly in India and elsewhere. Technology transfer is one of the important activities of UNIDO. I spent several months in India, not in one stretch, but over three or four years and was there many times. We still have rather good contacts with India.

Then the last exciting international connection was in 1972, immediately after the Nixon visit, when I was one of the first American scientists to visit China, together with Sheldon Atlas. We toured a number of laboratories and, of course, started talking about polymers. Since then a great deal has been done. Many of our people, Eirich and Eli Pearce amongst others, have been to China. China is now gradually building up a polymer industry, in the rubbers, in plastics and in the fibers.

STURCHIO: What was the state of understanding of polymers?

MARK: Zero. I mean, if you used the word polymer, they didn't understand it. You had to use the word plastic. They knew what a plastic is, a fiber is and what a rubber is, but they didn't know that they were all the same thing; a polymer.

STURCHIO: That was like the state of things in the early 1920s?

MARK: Yes, that was about it; the state of things, exactly.

BOHNING: Did you visit academic institutions?

MARK: We visited four or five academic institutions, universities. Then, oil refineries -- there were no other industries. Well, we visited a silk plant. Beautiful, but they didn't realize that silk was a polymer. There were no synthetic polymers there at that time although they exist now. We visited several refineries.

The last thing I have here on my list is that in the mid seventies, chemistry and particularly, plastic materials, became criticized because of fire hazards, because of contamination of water and the atmosphere. There were X committees of the American Chemical Society considering what to do. Of course FAO was also concerned. At one of these committee meetings, it was agreed to take something of particular urgency, something particularly in the public mind and analyze it in depth. This was, of course, the fire hazard. A committee was originated of about fifteen members, half from industry and the rest from government agencies and universities, to study the fire hazard of plastic materials. I was the chairman of this committee and we had many meetings and finally we published ten volumes (53), one on the textile industry and one on the packaging industry and so on. All aspects: the reasons for the fire hazards of plastic materials, how can they be diminished, and so on. This more or less created an official platform, because it was under the auspices of the National Academy of Sciences, for the additional improvements which are now being made. In fact, today, one doesn't talk so much about the fire hazards of plastic materials. It's accepted that hazards remain but we know why and what can be done about it. Well, those were on the list and I felt I should tell you about these special international or national connections.

STURCHIO: Well, those were all interesting and important, I'm glad that you did tell us about them. About the last one on fire hazards of plastics. You had been doing work on the heat stability of polymers for some years?

MARK: Yes, yes.

STURCHIO: As I recall, there was a connection with space capsules also. Maybe you could tell us a little bit about that background.

MARK: Well, I think the origin of our interest in high performance plastics was in the late sixties or early seventies when we realized that the big commodity plastics and the big fibers, vinyls, polystyrene, polyesters, polypropylene, and the rubbers are the domain of industry. What should the universities do? Our answer was that we could follow two big mainstreams. One is medicine, it's the last goal; and the other is to synthesize and process materials which will ultimately compete with metals and with ceramics. The end goals were medical science and replacing ceramics and metals. Of course, we did both, but our medical branch has kind of dried up since Professor [L. Guy] Donaruma has left. He is a biopolymer man and was taking care of that here at Poly.

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MARK: We followed the other line. The question was what to do with a molecule to make it harder and get less heat-sensitive? For this you have to use stiffer chains. When heat increases molecular motion in very flexible chains like rubber or polyethylene, or nylon, there will be a break of the chain with subsequent deterioration. The chain can't bend in a rigid molecule. Heat energy goes in waves, like it does in a crystal so it takes a long time until the wave fluctuations become so large that the chain breaks. Rigid chains are a safeguard against heat degradation. Flory had shown that theoretically, and many people have shown it practically. We started to work on rigid chain polymers, including fibers. The first rigid chain fibers were the carbon fibers. We worked on carbon fibers twenty years ago before anyone else felt that it was worthwhile. Rigid chain plastics are materials which have a large proportion of aromatic components. Since then, we have worked on polyimides and on polybenzimidazoles and on a whole variety of polymeric materials essentially consisting of aromatic ingredients. They can be used alone as heat resistant polymers or they can be used together with fibers in composites. Professor Pearce is working in this field together with Professor [Chan D.] Han, Professor [Jovan S.] Mijovic and Professor Atlas and maybe one more. We now have five professors who are particularly pushing ahead in the field of high performance plastics and high performance composites. Of course, you know the great demand for these products is in the aircraft industry, in the aerospace industry and in every industry where one wants to replace metals with something cheaper. Not cheaper, but lighter. Lighter. Well, you know, in the packaging industry, glass bottles are gradually disappearing, and cans will slowly disappear. Why should they be metal? They could be just as well plastic. That is the reason why we embarked on this area, and there are lot of publications available which have been made in this field. We had a number of symposia here and when you look at this list of symposium volumes, you will see that, beginning in the 1970s, almost every other symposium had to do with high performance materials, either plastics or fibers or rubbers.

STURCHIO: When did that research begin here in Brooklyn?

MARK: Well, it started maybe in the early seventies. Gradually, gradually. I was the first to publish a few articles and to make a few experiments. And then Eli Pearce came in and he took it up on a larger scale. He had a number of associates and a number of graduate students. Then Han came, followed by Mijovic so it gradually grew up, and now I would say that we have five or six professors with some forty or fifty graduate students, all working in the domain of high performance, either polymeric materials themselves, or composites.

STURCHIO: Hadn't you done some work for NASA earlier that your son had gotten you involved in?

MARK: No, it was the other way around. When we started to work in this area, we went to NASA, to Ames. Hans was the director of Ames at that time and we asked him for money. We submitted not only to Ames, we submitted to NASA at Lewis and at Langley, to all three NASA laboratories. Project proposals as to what we could do on a small scale to make a new molecules or modify existing molecules. We had, oh, a dozen joint projects, $16,000 a year, $20,000 a year, something of this order of magnitude.

STURCHIO: That raises another issue. Here you've mentioned the connection with aerospace and with NASA as a patron of research in the high performance area. Maybe you could talk a little bit about how support for research has changed here in Brooklyn over the past forty years. When you began the Polymer Research Institute, as I recall from the New Yorker profile and some of the things you told us, things began on a relatively small scale. You used the income from the summer courses to buy equipment and to do certain things, and then as it grew, it began to snowball. But, presumably, you also had to find other sources of outside support for research projects.

MARK: In earlier days, after the war and maybe in the fifties, our best support was when a company would sponsor a graduate student; fellowships, as they were called. We had Du Pont fellowships, we had Monsanto fellowships, Dow fellowships. On the average, I would guess, maybe ten a year. First of all it paid tuition, and there was always a little over for equipment. For several years, five, six, seven years, the fellowships were our main support from industry. Then projects gradually developed into something larger. The fellowship really was something for a person. The fellowship was an educational expense for the company. The company wants good people in the future, and to get them they sponsor fellowships. But then there was a change. It was not the man anymore who was the target, it was the material. Out there in a helicopter is a problem, something breaks too soon. "Here it is, analyze it. Find out why it breaks, and then think about making it better. If you have a good idea, come to us, and let's talk it over." So, we went to Ames, to Langley, to Lewis, to Wright-Patterson and so on. We explained what we intended to do, and they would give us a project when they felt that it was reasonable. Now, projects were, of course, on a larger scale. A project was $50,000 a year and was given to a professor, or to the department. Eventually he would hire a number of people and buy whatever equipment was necessary, and give an annual report. The sponsors would come in and look at what he had done. In the sixties this was a very popular way of milking the industry.

And there was the government, the National Institutes of Health was very good, but not for us because we didn't do medical work, but the Army was very good; Army Research Laboratory, Navy Research Laboratory, Air Force, NASA. Well, those were the government agencies, as well as the National Bureau of Standards; five or six government agencies which could be approached. If you presented a reasonable proposal to them, they would go along with you. I would guess that sometime in the mid-sixties, our income for the polymer research from such projects was between two and three million dollars a year. Overberger was a very successful man, and Alfrey, of course, Murray Goodman and Harry Gregor. Herbert Morawetz; Morawetz was particularly effective.

STURCHIO: That's quite substantial change from the early days to the scale of the research.

MARK: Well, then as I said before, we had some twelve professors and a hundred graduate students.

STURCHIO: How has that changed in the last twenty years, with the decline in federal funds for basic research? What decline from the level of the sixties?

MARK: Well, of course, then the people left and we didn't have so much appeal to the agencies. I don't know, but I presume sometime in the mid-seventies, we may have had $500,000 instead of having two million.

STURCHIO: Do you and the other polymer scientists here look for money from the same sort of constellation of federal agencies or are there now more interesting ties with industry?

MARK: Well, there are interesting ties with industry, but still, you know, the cooperation with a university is always difficult because of secrecy. We are a public institution and every thesis which is done here has to be published. When a man gets a degree, the work, the basis on which he gets the degree, must be published. For industry that may, in certain cases, be unacceptable. Then a lawyer may suggest that the student work in two areas; one he will publish and the other he will not publish. But then, you get in a gray area where the man is not supposed to know what he did in the morning, so that was not very acceptable. What several universities, including us, did was to set aside a certain research foundation where money for the Institute could come in and everything could be published, but no degrees would be awarded; for postdocs.

What is the situation now? Well, we have now maybe eight or nine professors in the polymer field. Some are in the chemistry department, some are in the chemical engineering department, with some eighty or ninety graduate students. Where do we get the money? Well, again, maybe from the Army, maybe NASA, some SDI -- SDI is now a very good one, no? This new agency the Strategic Defense Initiative. And, I would again say a smaller quantity from industry as fellowships.

Individual professors consult for industry, but that really has nothing to do with the Institute. This is a personal relationship of the professor with, let us say, the Du Pont company, where they invite him to visit and they ask him questions, and he gives them the best answers he can. But no work is done at the Institute in the context of these consulting activities. I'm still a consultant for Amoco, but there is no experimental work done here for Amoco. If they want to do something, they do it there, they call me in and they discuss the way they did it and what the results are and whether they could have been better and such things. This consultancy work is essentially a pepping up of the salaries of the professors. But the Institute doesn't profit. The Institute as an institution doesn't profit.

STURCHIO: Speaking of consulting, if we can go back to the late forties and fifties when you were building the group here, we spoke before about your own consulting activities in those years, and you've always been very active. Were Overberger and Alfrey and the others also active?

MARK: All of them. All of them. I was a consultant for Du Pont, Overberger was a consultant for Du Pont, Morawetz was a consultant for Dow, Eirich for Pittsburgh Plate Glass, and Alfrey for Monsanto. Everyone was a consultant for some company.

BOHNING: Although I understand what you were saying about the formal distinction between consulting activity and activity of the Institute, but the fact that everybody, in fact, was a consultant must have had some kind of effect on the atmosphere here in terms of ideas that it may have suggested and the work that may have been pursued here.

MARK: Yes, there were several instances. First of all, as soon as one of our boys graduated, he immediately had a job. [laughter] So that was a really desirable influence. And then, of course, as long as something is in the domain of fluctuating ideas, say I went to Du Pont or wherever and talked about something and we had some good ideas and I grabbed up one of these ideas and started to improve it. Well, what if I had? As long as something is in this area, there is little danger of overlapping. I mean, overlapping starts at the stage of reduction to practice. That's the essence of a patent. You can't get a patent on an idea. I mean, you can write one down, but it doesn't mean anything [laughter].

STURCHIO: So, those were the two main influences through the very desirable market for graduates provided [by industrial contacts].

MARK: Yes, yes, and the men, of course, saw a lot of things, you learned a lot of things. It was an exchange of information on a high level.

STURCHIO: And, of course, that had benefits for both parties.

MARK: Yes, sure. You know, there was a rush, when we were the first here to have a really good light scattering instrument. Debye had developed the whole theory, and Bueche, of course, worked on the instrument. But they didn't have any polymeric materials available to run in the instrument. We had in every room another polymer. As soon as we had the instrument developed, we could then measure exactly the molecular weight of almost everything and that was very exciting for us.

STURCHIO: So, they were interested in learning the methods?

MARK: New methods, new experimental and fundamental methods. In the early days, you know, with Fankuchen in the forties, they all came to use our x-ray equipment. No industry at that time had an x-ray equipment, not even Du Pont, because they didn't know what to do with it.

STURCHIO: With new instruments, with new fundamental techniques, it is important to watch somebody do it. You can't just read about it.

MARK: Well, you can read about it. The more you read about it, the more you want to go and see how it is being done. Then, how are the results analyzed and interpreted? Particularly if there is some kind of little barrier which has to be overcome in order to make the results valid. Like in x-rays, monochromatic radiation or, as in light scattering, complete purification of the solution for as long as there is a little bit dust or anything of that kind your values are completely erratic.

STURCHIO: Those are the sorts of things where it's important to have personal contact.

MARK: That's quite correct because he shows you how to do it.

STURCHIO: That seems to me a very important element over the long term of relations between university researchers and industry. You mentioned before how in the forties it was easy to get a research program going without a vast investment in equipment and things have really changed now.

MARK: Yes.

STURCHIO: People tend to overlook the mundane but fundamental...

MARK: How to run it. Well, of course, those things have also changed now. The instrument companies go out and show industry exactly what to do and themselves publish important novelties and so on. In this sense, since the instrument companies now have very powerful advertising, the universities are no longer so necessary for the instrument-using industry. You know, some companies, like the Du Pont company build instruments themselves. They feel the necessity to be in front. Right now, they have the best light scattering instrument. So much so that, at a Du Pont board meeting, one of the board members stood up and said, "Damn it, what do we have to do with light scattering? Where do we scatter light?"

STURCHIO: Well, it looks like we've covered almost our entire agenda. Jim, do you have anything?

MARK: Well, why don't we do the following. You can now reread it and think it over, and if you feel that you want another two hours, I'm always very, very pleased to have you here and talk with you. There may be gaps to be filled in here or there and it is not good to do it on the phone, but you are always welcome.

BOHNING: I do have one area I'd like to ask you questions in. You actually began your trips to the Soviet Union back in the thirties.

MARK: Yes.

BOHNING: And in 1958, you wrote about the state of Soviet polymer science based on your trip there. A couple questions. What change did you see from the thirties to the fifties in the Soviet Union?

MARK: Well, immediately after World War I, there was nothing in the Soviet Union. Then they sent out a large number of people, several to Vienna with me, and it took ten years or so; they stayed abroad two or three years and it took maybe five or six years in order to build up five or six centers of polymer research and maybe even engineering, but mainly research. Then it took another five years until these centers started to produce significant and interesting novel results. There was [A. V.] Topchiev and there was [Valentin A.] Kargin, Kabanov, [Kh. U.] Usmanov, [Vasily V.] Korshak, [A. F.] Plate and so on. Fifteen or sixteen - maybe more. Some of them came to Germany, some of them went to France, to Austria, all over the place. They created these centers: an important one is in Leningrad, three important ones are in Moscow, a very important one in Kiev, a very important one in Novosibirsk. An excellent one is in Tashkent; then a big one, mainly responding to the oil industries, is in Baku. There is a big one in Yerevan. Maybe a dozen. Of course everything there is government sponsored, but they are doing excellent work and of course they are publishing. They are publishing more on polymers in the Soviet Union than we do in this country; they have four or five journals.

BOHNING: What kind of a relationship did you develop with your colleagues in the Soviet Union?

MARK: Well, they came over and we had several of them here for several months; they visited the Gordon Conferences very frequently. We went over, not for several months, but for several weeks. Many of the people here, Eirich and [Otto] Vogl and Morawetz have all been to the Soviet Union. Overberger, of course, and Murray Goodman. Those were extremely pleasant and nice human relationships. And then, in the late seventies, it all cooled off, and now, there's almost nothing. They don't come to anybody. They don't go anywhere. They are invited; they even send in abstracts of lectures, but they don't arrive.

BOHNING: Is the government preventing them from coming to the West? That brings us to the Tashkent meeting, the boycott that you participated in. Could you describe your participation?

MARK: Well, it was just what I said. No? What is the sense of scientific reciprocity when we come from all countries, people go to Tashkent and talk there. I have been in Tashkent several times before and there would be an exchange at a high level without any restrictions. When we invite, on the same level and with the same words, our Russian colleagues to come to a meeting in Brooklyn or in Stanford or anywhere, or for that matter in Paris, for it's the same thing in other countries, then they say they will come, they send an abstract, but they don't come. So, I won't go to Tashkent.

And, of course, on top of that, you know, there was always Sakharov, and there are always human rights violations and so on. I think Flory, Stockmayer, and I signed it and I don't know who else, but several signed. The main purpose and our main reason was that we did not like the idea that they invite and we come. They have all the advantage of a large number of international scientists coming there, talking with them, giving them all kinds of ideas, and information, but when we invite them, they don't come! If at the outset, they would say, "I am awfully sorry, I can't come. I have something else to do." Fine. But when they say, "Yes, we'll come," and send an abstract or even the whole paper, and then they don't come; that's not polite.

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MARK: I have the impression now that things are loosening up again, because of my correspondence with several of them. For years they didn't answer letters but now, they have started to reply. So, maybe, depending upon the political development -- maybe in a few years we will be together again. One has to be patient.

BOHNING: Well, that about covers it.

STURCHIO: We'd like to thank you again for being so generous with your time and for sitting down and telling us this fascinating story.

MARK: It's all yours. It's a great pleasure for me to have the opportunity to talk on these things with you.

BOHNING: Thank you.

STURCHIO: Thank you.

NOTES

1. H. Mark, "The Synthesis of Pentaphenylethyl," Ph.D. Thesis, University of Vienna, 1921.

2. W. Schlenk and H. Mark, "Nature of the Chemical Union. Free Pentaphenylethyl," Berichte, 55B (1922): 2285-2299. idem, "Analogs of Pentaphenylethyl," ibid, 55B (1922): 2299-2302.

3. H. Mark, M. Polanyi and E. Schmid, "Processes at the Stretching of Zinc Crystals," Zeitschrift für Physik, 12 (1923): 58-72, 78-110, 111-116. idem, "Investigations of Uni-Crystalline Wires of Tin," Naturwissenschaften, 11 (1923): 256.

4. H. Freundlich, Kapillarchemie, (Leipzig: Akademische Verlagsgesellschaft, 1922).

5. BCHOC Oral History file #0030.

6. H. Kallmann and H. Mark, "Some Properties of Compton Radiation," Naturwissenschaften, 13 (1925): 1012-1015. idem, "Some Properties of Compton Radiation," Zeitschrift für Physik, 36 (1926): 120-143.

7. H. Mark, "Atomic Structure and Quantum Theory. I," Zeitschrift für Angewandete Chemie, 40 (1927): 16-20. idem, "Atomic Structure and Quantum Theory. II," ibid, 40 (1927): 645-649. idem, "Atomic Structure and Quantum Theory. III," ibid, 40 (1927): 1497-1500.

8. H. Mark and L. Szilard, "A Simple Attempt to Find a Selective Effect in the Scattering of Roentgen Rays," Zeitschrift für Physik, 33 (1925): 688-691. idem, "The Polarization of Roentgen Rays by Reflection from Crystals," ibid, 35 (1926): 743-747.

9. H. Mark and E. Wigner, "Space Lattice of Rhombic Sulfur," Zeitschrift für Physikalische Chemie, 111 (1924): 398-414.

10. H. Mark and M. Polanyi, "The Space Lattice, Gliding Directions, and Gliding Planes in White Tin," Zeitschrift für Physik, 18 (1923): 75-96. idem, "The Lattice Structure of White Tin," ibid, 22 (1924): 200.

11. Special Meeting, Gesellschaft Deutscher Naturforscher und Ärtze, 23 September 1926.

12. See Berichte, 59 (1926): 2973, 2982, 3000, 3008, 3019.

13. K. H. Meyer and H. Mark, "The Structure of the Crystallized Components of Cellulose," Berichte, 61B (1928): 593-614.

14. O. L. Sponsler and W. H. Dore, "The Structure of Ramie Cellulose as Derived from X-Ray Data," Fourth Colloid Symposium Monograph, (1926): 179-202.

15. H. Mark, "Polymer Chemistry in Europe and America; How it All Began," Journal of Chemical Education, 58 (1981): 527-534.

16. For example: K. H. Meyer, H. Hopff and H. Mark, "The Constitution of Starch," Berichte, 62B (1929): 1103-1112. H. Mark and G. v. Susich, "The Orderly Micellar Structure of Rubber," Kolloid-Zeitschrift, 46 (1928): 11-21. H. Mark and K. Wolf, "Polarization of Characteristic X-Radiation," Zeitschrift für Physik, 52 (1928): 1-7. H. Mark and R. Wierl, "Intensity in the Hydrogen Stark Effect," ibid, 55 (1929): 156-163.

17. H. Morawetz, Polymers. The Origins and Growth of a Science (New York: John Wiley and Sons, 1985).

18. Herman Mark and Carl Wulff, "Styrene and its Homologues," German Patent550,055, issued 9 August 1929. Carl Wulff and Eugene Dorrer, "Continuous System for Polymerizing Styrene, Indene, Vinyl Esters and Like Unsaturated Compounds," German Patent 634,278, issued 22 August 1936.

19. K. H. Meyer and H. Mark, Aufbau der Hochpolymeren Substanzen (Berlin: Hirschwaldsthe Buchhandlung, 1930).

20. H. Staudinger, Die Hochmolekularen Organischen Verbindungen, Kautschuk und Cellulose (Berlin: Springer, 1932).

21. "Phenomena of Polymerization and Polycondensation", Faraday Society Discussion, Cambridge, 1935.

22. H. Mark, Die Chemie als Verbereiterin des Fortschrittes (Leipzig: Hoelder-Pichler-Tempsky, 1938).

23. K. H. Meyer and H. Mark, Hochpolymere Chemie (Leipzig: Akademische Verlagsgesellschaft, 1937).

24. H. Mark and R. Raff, High Polymers. III. High Polymeric Reactions, Their Theory and Practice (New York: Interscience Publishers, Inc., 1940.

25. K. H. Meyer and H. Mark, Der Aufbau der Hochpolymeren Organischen Naturstoffe (Leipzig: Akademische Verlagsgesellschaft, 1930).

26. H. Mark and G. S. Whitby, Collected Papers of W. H. Carothers on Polymerization (New York: Interscience, 1940).

27. I. Fankuchen and H. Mark, "Improved X-Ray Technique for the Study Of Natural and Synthetic Fibers," Record of Chemical Progress, 4 (1943): 54-57. idem, "X-Ray Study of

Chain Polymers," Journal of Applied Physics, 15 (1944): 364-370.

28. H. Mark and G. Saito, "Fractionation of Highly Polymerized Compounds by Chromatographic Adsorption Analysis," Monatshefte für Chemie., 68 (1936): 237-243.

29. F. Eirich and H. Mark, "Substances of High Molecular Weight in Solution," Ergebnisse Exact. Naturwissenschaften, 15 (1936): 1-35.

30. H. Mark, "Recent Developments in the Field of Synthetic Rubber," Chemistry and Industry, (1940): 89-90.

31. H. Mark, "X-Ray Investigations of Carbohydrates," Chemical Reviews, 26 (1940): 169-186.

32. H. Mark, "Elasticity of Natural and Synthetic Rubber," Trans. Inst. Rubber Ind., 15 (1940): 271-297.

33. H. Mark, "Composite Elasticity of Rubber," India Rubber World, 102 (1940): (3) 41-45, (5) 45-49.

34. G. Goldfinger, D. Josefowitz and H. Mark, "Heat of Polymerization of some Vinyl Compounds," Journal of the American Chemical Society, 65 (1943): 1432-1433.

35. H. Mark, The General Chemistry of High Polymeric Substances (Amsterdam: Elsevier, 1940).

36. H. Mark, Physical Chemistry of High Polymeric Systems (New York: Interscience, 1940).

37. T. Alfrey, Mechanical Behavior of High Polymers (New York: Interscience,1948).

38. W. P. Hohenstein, S. Siggia and H. Mark, "The Formation of Vinyl Polymers in Emulsions and Suspensions. II. Some Experiments on the Polymerization of Styrene in Emulsion." India Rubber World, 111 (1944): 173-177.

39. W. P. Hohenstein and H. Mark, "Polymerization of Olefins and Diolefins in Suspension and Emulsion," Journal of Polymer Science, 1 (1946): 549-580.

40. R. F. Boyer and H. Mark, Selected Papers of Turner Alfrey (New York: M. Dekker Inc., 1986), pp. 6-9.

41. H. Mark and S. Siggia, "Esters of Carboxymethylcellulose," U.S. Patent 2,379,917, issued 10 July 1945 (application filed 11 August 1942).

42. R. F. Boyer, "Herman Mark and the Plastics Industry," Journal of Polymer Science, Part C, 12 (1966): 111-118.

43. T. Alfrey, J. J. Bohrer and H. Mark, Copolymerizatio (New York: Interscience, 1952).

44. H. Mark, "Preparation and Properties of some Block and Graft Copolymers," Angewandte Chemie, 67 (1955): 53-56.

45. T. Alfrey, G. Goldfinger and H. Mark, "Apparent Second-Order Transition Point of Polystyrene," Journal of Applied Physics, 14 (1942): 700-705.

46. P. J. Flory, "Statistical Thermodynamics of Semi-Flexible Chain Molecules," Proceedings of the Royal Society, A234 (1956): 60-73. idem, "Phase Equilibriums in Solutions of Rod-Like Particles," ibid, A234 (1956): 73-89.

47. Morton M. Hunt, "Profile of Herman Mark," New Yorker, 34 (1958): 48-50 (Sept. 13), 46-79 (Sept. 20).

48. M. E. P. Friedrich and C. S. Marvel, "The Reaction between Alkali Metal Alkyls and Quaternary Arsonium Compounds," Journal of the American Chemical Society, 52 (1930): 376-384.

49. H. Mark, Das Schwere Wasser, (Leipzig, F. Deuticke: 1934).

50. E. Baroni and A. Fink, "Investigation of the Concentration of Deuterium Oxide in Natural Ice," II. Monatshefte, 67 (1936): 131-136. IV. ibid. 71 (1937): 128-130.

51. H. Mark, "Submicroscopic Structure of Wood Constituents," TAPPI, 32 (1949): 108-109.

52. E. H. Immergut, B. G. Ranby and H. Mark, "Recent Work on the Molecular Weight of Cellulose," Industrial and Engineering Chemistry, 45 (1953): 2483-2490.

53. H. Mark, Editor/Chairman, Fire Safety Aspects of Polymeric Materials, 10 vols. (Washington, D.C., National Academy of Sciences: 1977).

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