THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

MICHAEL SZWARC

Transcript of an Interview
Conducted by

James J. Bohning

in

Solana Beach, California

on

11 September 1986

Michael Szwarc 3/18/96 JH

BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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MICHAEL SZWARC

1909	Born in Bedzin, Poland on 9 June

Education

1932	Degree in Chemical Engineering, Warsaw Polytechnic Institute
1942	Ph.D., organic chemistry, Hebrew University of Jerusalem
1947	Ph.D., physical chemistry, University of Manchester
1949	D.Sc., University of Manchester

Professional Experience

1934-1942	Assistant, Hebrew University
1947-1952	Lecturer in Chemistry, University of Manchester
	State University of New York at Syracuse
1952-1956	Professor of Physical and Polymer Chemistry
1956-1964	Research Professor
1964-1980	Distinguished Professor
1967-1980	Director, Polymer Research Center
1980-	Emeritus Professor

Honors

1963-1964	Royal Society Visiting Professor, University of Liverpool
1966	Fellow of the Royal Society
1969-1970	Nobel Guest Professor, Uppsala University
1969	Award in Polymer Chemistry, American Chemical Society
1972	International Award, Plastics Science and Engineering
1972	Baker Lecturer, Cornell University
1974	Visiting Professor, Louvain University, Belgium
1974	Hon. D.Sc., University of Louvain
1975	Hon. D.Sc., University of Uppsala
1976	Lemieux Lecturer, University of Ottawa
1978	Hon. D.Sc., Louis Pasteur University, Strasbourg, France
1978-1979	Visiting Professor, University of California, San Diego
1990	Polymer Division Award, American Chemical Society

ABSTRACT

In this interview, Michael Szwarc begins with his early interest in science whilst growing up in Poland, leading to his studies at the Warsaw Polytechnic Institute. Szwarc next describes his experiences from 1935, when he emigrated to Israel, until his move to the University of Manchester in 1945. At Manchester, he worked in Michael Polanyi's physical chemistry group and first embarked on his studies of polymerization. A visit to the USA in 1950 involving many lecture trips is described as are the circumstances leading to his acceptance of a professorship at SUNY, Syracuse. Research on the methyl affinities of aromatic compounds led Szwarc to work with the naphthalene radical anion and, hence, to the development of the living polymers. The interview ends with Szwarc reviewing his later studies and his reflections on co-workers and associates.

INTERVIEWER

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

TABLE OF CONTENTS

INTERVIEWEE: Michael Szwarc

INTERVIEWER: James J. Bohning

PLACE: Solana Beach, California

DATE: 11 September 1986

BOHNING: Perhaps we can start from 9 June 1909. You were born in Bedzin, if I pronounce it correctly.

SZWARC: Bedzin, yes. It was a small town but when I was only three years old my parents moved to Warsaw.

BOHNING: Could you tell me something about your father and mother?

SZWARC: My father was a businessman representing a factory in the Silesian region. My mother was a housewife. I loved both of them very much. He died at the age of 56 as a result of an unnecessary operation. It was a mistake of the doctors. They performed it in a wrong way and left an infected wound. He lived ten years until he died, but he suffered all the time. My mother was killed by the Nazis. I don't know exactly when, or how. It was apparently around 1942 because I got a letter from her in 1942 through the Red Cross.

BOHNING: What year did your father die?

SZWARC: In 1936.

BOHNING: Do you have any brothers or sisters?

SZWARC: Two sisters.

BOHNING: Are they living in this country?

SZWARC: No. One again was killed by the Nazis.

BOHNING: You have said that you moved to Warsaw when you were about three years old. Did you get your early schooling there?

SZWARC: Yes.

BOHNING: I'm not familiar with the system. Could you describe the schooling that you had?

SZWARC: Well, primary schools were essentially like day school here in the USA. You were given toys to play with and gradually they began teaching you elementary subjects. At the age of nine I entered what is called the Gymnasium in Poland. It is generally equivalent to the USA high school. One graduates at the age of about eighteen by taking the final examination which is known as the Maturum. You supposedly mature according to this term. When one passes this last examination, then one can go in principle to a university or college of technology, and continue one's education. This is what I did.

BOHNING: Were there any teachers or anyone who had a particular influence in directing you toward science in those early days?

SZWARC: Well, it is difficult to say because I was interested in science from the age of nine. At this age while I was in the first class of the Gymnasium, I was very much interested in all kinds of sciences: astronomy, chemistry, and physics. Later, when I was in the higher classes, at about the age of fourteen, I got the formal courses in physics as well as in chemistry. I had an excellent teacher in physics, Leopold Infeld, who later became Professor in the University of Lvov. Eventually he went to Canada. He was a co-worker of Einstein, a highly valued man (1).

Shortly after the war, 1946, 1947, I don't remember, he became a communist, as did many intellectuals of those days, and eventually returned to communist Poland. His departure created a problem which reached the Canadian Parliament: how could he be allowed to return, having worked with the Atomic Energy Commission? Was he putting in jeopardy the secrets of the Atomic Energy Commission? Anyway, the man was very gifted and an excellent lecturer, and wrote books which popularized science. I certainly enjoyed his courses very much. They were a bit too sophisticated for me at that time. It was the beginning of quantum mechanics when people talked about Schrödinger and his equations. It is difficult to say that Infeld induced me to science, because I was interested in science and mathematics all my life, but he certainly encouraged these interests.

BOHNING: Were you reading science at an early age?

SZWARC: Oh, yes.

BOHNING: What were your sources? Where were you getting the material you were reading?

SZWARC: There were a number of popular books about science and about chemistry. I also had a small lab at home. I nearly poisoned my mother once. I tried to mix all kinds of things. If there was an explosion I was particularly happy because that was the type of chemistry for me at that time. I was about nine or ten years old. Indeed, I could have done quite a lot of damage, but fortunately I didn't.

BOHNING: Had someone given you a chemistry set or had you acquired that on your own?

SZWARC: I don't remember. Maybe my father bought it.

BOHNING: Did your parents encourage you in that kind of thing?

SZWARC: Oh, yes. In fact, I was always absent-minded, and my father said, "No question. You are going to be a professor!" And that turned out to be true.

BOHNING: What was it like growing up in Warsaw at that time?

SZWARC: In many ways I enjoyed it. As you know, I am a Jew and there was quite a lot of anti-semitic activities in Poland, which of course I did not enjoy. But on the whole, life was quite pleasant. When I think about it and recollect, it was pleasant. People in this country frequently have a distorted idea how life was in those days. You see, I left Poland in 1935, when the situation was tense. Very tense. After that it became impossible. I was on friendly terms with my colleagues, who were non-Jews. But the year after I left there was a big change. There was no talking one to each other. I was still talking to my non-Jewish colleagues. In earlier years there were of course frictions, but nothing to compare to when Hitler came to power.

BOHNING: What about the time of the first World War. Were you affected by that in any way?

SZWARC: Well, during that time, 1914 to 1918, I was five to nine years old. I remember when Warsaw was bombed by the Germans, because in those days Warsaw was under Russian occupation. A bomb hit the house across the street from our own house. That incident had such a dramatic effect on me that I still remember it. The German army was enthusiastically welcomed in Poland in those days. From a historical point of view, it was an interesting period, because as you know Poland was divided in 1815 during the Vienna Congress between three powers, Russia, Prussia (at that time there was no Germany proper), and Austria, with Russia getting the biggest part. From some points of view, the Russians were the most anti-Polish. The Austrians were the best in comparison. Austria was liberal in those days, and remained liberal to the very end. Bismarck united all the small dukedoms and princedoms into proper Germany. The Habsburgs kept Austria, Hungary, and Czechoslovakia as their empire. When the war started, there was confusion in Poland; on one side the so-called central powers, Germany and Austria, as well as Turkey, and on the other side England, France, and Russia. So it was difficult for the Poles to decide whether to join the alliance, which meant, France, although, with hindsight, I would say France was ready to sell Poland at any time. But nevertheless, there was a feeling that the French and France were the base for democracy, liberty and so forth, because with the French Revolution, and even with Napoleon, the general feeling was that they fought for freedom, in contrast to the other countries like Russia and Austria which were absolute monarchies. So the choice in the First World War was to side with France, and therefore with Russia. The alternative was to join Germany against Russia. In fact, Poles were divided and created two armies, one which fought with France on the Western front and another which was developed in Germany and fought against Russia on the Eastern front.

The man who became the first leader (but not president) of independent Poland after World War I was pro-German. He was fighting against Russia. His name was Pilsudski. I don't know if you heard about him or not. He was the first leader of Poland, a very liberal man, actually a Socialist who considered Russia to be the biggest danger. He was very successful, because in 1920, two years after the Versailles Treaty, he was fighting Russia and managed to conquer Kiev, the capital of the Ukraine. Well, as it happens very often during wars, he extended himself too far, and the Red Army was able to reach the Vistula river which separates Warsaw from its suburbs. Fortunately, they also extended themselves too far and in the decisive battle that followed they were defeated. Then eventually, peace was made and the border was of course not in Kiev.

You see, Kiev was part of Poland in the eleventh century. In the tenth century, Poland became a kingdom by the command of the Pope and so the prince of Poland became a King. His son managed to extend the borders of Poland up to the Elbe River, which is now the border between East and West Germany, and south to the Black Sea. Never in its history was Poland bigger. From the Elbe to the Black Sea. All the Ukraine was Polish. Well, after that, things changed again. To return to what you implied in your question when we were talking about the first World War, you see the people who were associated with Pilsudski were pro-German. And so the German army was enthusiastically accepted. I do remember the Germans arriving in Warsaw. The army was marching and the people in the streets were throwing flowers at them with great enthusiasm. Fortunately, both Russia in some way, as well as Germany and Austria, were defeated. At the Treaty of Versailles, Poland became independent of these powers after 123 years of occupation. So you see, being educated in Poland, I know the history of the country. Most of the Polish literature of those days was basically anti-Russian, less anti-German. Much less. The Germans tried to Germanize this part of Poland which they occupied, with reasonable success. Anyway, this is a brief history of Poland.

BOHNING: I am curious about your reading. Was that all in Polish, or were you reading things from other countries?

SZWARC: In Polish.

BOHNING: So you didn't have much exposure to something from England or Germany?

SZWARC: We were reading many English authors in Polish translation. Jack London, for example.

BOHNING: Really.

SZWARC: Well, all the novels of Jack London were translated and I know them all from Polish translations. The translation of the names was slightly different than they are in English but Call of the Wild, and Martin Eden, for example were available in Polish translation. Of course, there were many others, like Shakespeare's works.

BOHNING: Did your physics professor in the Gymnasium also teach chemistry?

SZWARC: No. He was a theoretical physicist. He was trying to explain to us, for example, the theory of relativity. He did it quite successfully.

BOHNING: What did you do in chemistry? Did you have laboratory classes?

SZWARC: Yes, we did. We had a laboratory, and somebody had to clean the laboratory after the class and I managed to get the pleasure of doing it, which means that after hours I could sit there and experiment.

BOHNING: What about mathematics? What was your mathematics background at that time?

SZWARC: Well, I always loved mathematics. It was one of my beloved subjects. In fact when I was going to enter the University, mathematics was one of the possibilities I considered. But in those days, during the middle 1920s, a student of mathematics could only become a teacher. There was not much future in mathematics. By the way, there was not much future in physics either. Chemistry looked like the most attractive choice from the practical point of view. So eventually, being opportunistic, I decided to go for chemistry.

BOHNING: You said that you had a choice between the Warsaw Polytechnic College and the University. Were you allowed to make that choice?

SZWARC: Yes. The choice was mine to make. The Polytechnic was a better choice because all the chemistry courses which were given at the University were also given at the Polytechnic, plus the engineering courses such as electrical engineering, construction of steam boilers, etc. When we had those courses in electrical engineering (not electronics, because in those days, electronics didn't exist) we were taught about transformers, generators, electric motors, etc. The professor told us that he did not expect us to be electrical engineers, but he wanted us to learn enough to be able to tell electrical engineers what we needed, and also to understand their language. That I thought was a very good idea because you should have a broad education which will allow you to communicate with other people. I think that's very important. You should not expect to be a specialist in other subjects but you should have enough knowledge to be able to understand, appreciate, and communicate with others. This was the type of courses which we got.

By the way, when I was in the last year of the Gymnasium, I was very interested in two things. One was the freshman lectures in physics which were given at the university. The levels were very different from American levels and I was going to these lectures with a colleague, pretending that we were sick and couldn't go to school. They were marvelous lectures in experimental physics. The second thing, since you asked about mathematics, was a book by [Waclaw] Sierpinski, published in Polish. It was not a translation, but a Polish work. This book dealt with some basic problems in mathematics (2). Sierpinski was one of the founders of what is called the theory of sets. If you open today any book dealing with problems like percolation and things like that, you will find terms like Sierpinski gasket, Sierpinski carpet, etc. These kinds of problems were conceived by Sierpinski in those days. It was very interesting to study the basic foundations of mathematics. That's what he tried to do. That's what some other people started to do in Europe and the United States, and called it modern mathematics. You know, it was the idea of dealing with operations, of which multiplication, addition, etc., are examples. The idea of having not geometry but topology, and topological problems instead of geometrical problems, was developed in those days. It became a specialty of mathematics. Much later his ideas were adopted by other people in physics and in chemistry and began to be appreciated. Have you heard about Boolean algebra?

BOHNING: Yes.

SZWARC: Well, at the time when Boole came out with his algebra, nobody considered it as something of value. But, you cannot have computers without Boolean algebra. The ideas which were developed at that time were abstract but found many applications later. No telephone exchange or computer can be built without Boolean algebra. So this book of Sierpinski was a kind of revelation for me.

I studied Sierpinski with the same friend. We planned a trip to the moon, and we realized that we have to learn quite a lot to be able to get to the moon.

BOHNING: How much chemistry did you study there? Did you have the typical courses, such as organic chemistry?

SZWARC: Well, we were overloaded with analytical chemistry. Quantitative and qualitative analysis, which is practically of no use today, made up a large portion of the courses and the lab. Of course there was organic chemistry with organic synthesis and things like that. We had courses in physical chemistry and the professor of physical chemistry in the college of technology in Warsaw was one of the famous founders of thermochemistry. I can tell you the name, but you will be unable to repeat it: [W.] Swietoslawski. He developed very precise calorimetric measurements. After that [F. D.] Rossini, found a much better way to handle many of these types of measurements, but Swietoslawski did develop what is called the adiabatic calorimeter which could measure very small amounts of heat. During the war he was at the Mellon Institute in Pittsburgh. He was interested in these high precision measurements. For me that was the most boring thing. You know, so what if you get precision of 0.1 calorie or better. I didn't think much about it then.

Well, one of the things which became important during the last war was the problem of calculating the heat of formation of isomeric hydrocarbons. The importance from the practical point of view was that the knocking in the car engine depends very much on the octane number of the fuel one is using. High octane number hydrocarbons are more branched than lower octane number hydrocarbons. Consequently the problem was, could we isomerize the low octane number hydrocarbons into high octane number hydrocarbons. If one tries to do it, one has to ask yourself, "Can that be done from the thermodynamic point of view?" In other words, are we going down in free energy or are we going up? Are we producing something more stable or something less stable? So the heat of formation of these octanes and other hydrocarbons became very important. Rossini devoted himself to accurate determination of the heat of combustion and therefore, heat of formation of the various hydrocarbons. What one measures is not the heat of formation but the heat of combustion by putting the hydrocarbon in a calorimeter, igniting it, and then measuring the amount of heat which is formed. Now, a typical heat of combustion is of the order of two to three thousand kilocalories per mole. A typical heat of formation is of the order of twenty kilocalories per mole. So if one makes a one percent error in determining the heat of combustion, one makes an error equal to the heat of formation. So one needs extremely accurate measurements. When I was a student, nobody was pointing that out to me. So, I thought that it matters very little if you have a difference of half a kilocalorie or even a tenth of a kilocalorie per mole.

The great advances which Rossini made resulted from the way he carried out the analysis. Say one introduces hundred grams of hydrocarbons and burns them in a calorimeter. The heat which is measured will be divided by a hundred and multiplied by the molecular weight to convert it into molar units. Now, it is not easy to avoid 1% or 0.1% of water. Well, water is unique in that it is found in every place and to get a hydrocarbon free of water is not so easy. Now to make the calculations: the heat of combustion of water is zero because water is the product of combustion. If one has 0.1% water, one reduces the heat of combustion by 0.1% which is about twenty kilocalories on twenty thousand kilocalories. So, one has to be sure that one doesn't have traces of water, which particularly in 1940, was not very easy to remove. Now, what Rossini did was to measure not the amount of hydrocarbons he put in, but to measure the amount of carbon dioxide which is formed. Of course, if there was trace of water, that will not turn into carbon dioxide. That method was responsible for the precision of Rossini's work.

I'm telling you all this because Swietoslawski was boring me to death with his demand for precision in thermochemistry. What should we use as a standard? Should we use sugar, or benzoic acid, etc. That was very boring for me. My first truly scientific work was when I joined [Michael] Polanyi in Manchester. Polanyi suggested that I look into the problem of bond dissociation energies. I made my reputation in the field of bond dissociation energies. It turned out that this work demands measurements of the heats of formation. Then I started to appreciate Swietoslawski and I realized that the problem was important for the success of my work. If somebody had told me that I would be a thermochemist and measure heats of combustion, I would have said, "Better kill me."

BOHNING: How many years were you at the Polytechnic College?

SZWARC: It was a five year course.

BOHNING: And what had you thought about doing when you finished your work there?

SZWARC: I wanted to get a job and to have a bit of money! My father was ill and we didn't have much money. Fortunately I got a stipend, which in those days was not common. So the first thing which I wanted was to get a job and to start to work.

BOHNING: I believe that was in 1932.

SZWARC: 1932 or 1933, something like that.

BOHNING: What job did you get when you graduated?

SZWARC: It was horrible. I worked for a year in something which was supposed to be a factory. It was a very small enterprise in which my job was to prepare all kinds of pure salts for pharmaceutical purposes. The pharmacopoeia defined the amount of impurities allowed in chemicals used for making drugs and we had to meet the standards of the pharmacopoeia. I was particularly proud of the barium sulfate I prepared. Barium sulfate is used as a contrast when there are problems with the gastric system. So when one feeds someone with barium sulfate, one has to ascertain that the barium sulfate does not contain any soluble barium which is a very strong poison. In addition, one demands that barium sulfate should be suspended and settle down slowly when shaken with water. The sedimentation must be very slow and uniform. The rate of sedimentation is tested. The best known product in the market was made by Merck, in Germany, a well known manufacturer of pure chemicals, and of pharmaceutical chemicals. I succeeded in making it better than Merck! The sedimentation was slower and very uniform.

By the way, you know I feel myself relaxed sitting with you in my garden and talking about all kinds of stories, old and not so old. So let me tell you a story about barium sulfate. I was once invited to give a series of lectures for two weeks in

Florence, Italy. On this occasion I got acquainted with the Tuscan Chemical Society. It was something like the American Chemical Society Section in Pittsburgh, for example. I was invited by them to give a lecture, and I asked the chairman of the section, what his interest was, and what kind of research he was doing. He told me that he was studying the crystal structure of precipitated barium sulfate as a function of the phase of the moon. He mixed barium chloride and sodium sulfate, got barium sulfate and, depending on the phase of the moon, the morphology of the precipitate was different and there was a correlation with the phase of the moon. I thought, well, there are so many crazy people in this world, there is one more. I don't need to worry about him.

Two years later, I was in the Weizmann Institute in Israel where they were very much interested in solar energy and desalination. The one way to combine these two is to use the sun's heat to evaporate water, condense it, and get water free of salt. Now in this type of operation, there is a kind of a boiler, and a heat exchanger, and the problem is that when brackish water is evaporated a scale forms, as in every kettle, and it precipitates on the walls of the heat exchangers. Because it reduces heat conductivity the scale should be removed mechanically with some kind of scraping device. I was talking to a girl whose task was to find a way to deal with the precipitation of salts like calcium carbonate and magnesium carbonate which contribute to the formation of scale, and to find ways by which the precipitate formed won't be hard, but fluffy and could then be washed out instead of scraped. Obviously, it was an important technological problem. She quite correctly thought to start by looking into the problem of nucleation. Any precipitate forms around a nucleus, as in the case of crystallization. She found in the literature that nucleation often arises from the tiny microcrystals of salt which are suspended in the air, and these tiny crystals are coming from the foam of the sea when the waves are breaking. Then I realized that there is a connection between the phase of the moon and the growth of the crystals because at full moon the waves are higher, and more of this type of microcrystals are formed. Obviously, this affects crystallization and the crystal shape and form of the precipitate. So I realized that this man, or at least his observations, were probably right. Of course, his reasoning was wrong. He was thinking about the gravitational action of the moon on the crystals and things like that. But it is again interesting to notice that people do something and they might get good results, valid results, with very bad reasoning and a very bad approach to the problem.

BOHNING: What was the name of the company you were doing this work for?

SZWARC: Well, that I don't remember. It was a very small outfit. It was not a company, but a one-man business. The whole enterprise was in the basement of a house. You can imagine how high my salary was. It's really fantastic how low it was! I worked there for a year. It was a very unpleasant job. Then I emigrated to Israel, or Palestine as it was named in those days.

BOHNING: You went there in 1935.

SZWARC: Yes, in 1935. In talking about the history of chemistry, and journalism, you are somehow connected to journalism in your activity. My first trip to the United States was in 1950 and was arranged by Hugh Taylor from Princeton. Among various places visited was Pittsburgh. For the first time in my life I was there. I had to give a talk to the American Chemical Society. The people from the American Chemical Society were waiting for me at the airport in the early morning, took me to my hotel, I registered, left my luggage, and then we went to the Mellon Institute, where I stayed for the whole day. The lecture was at eight o'clock in the evening. At six o'clock before dinner they brought me back to the hotel. As I walked into my room, the telephone rang. I picked up the telephone, and it was a young girl on the other end. She said, "My name is spelled like your name, and my father wants to know whether we are relatives." It didn't take much time to find out that we were not relatives. I was puzzled how she knew that I was in Pittsburgh. Three days later, I was in another location, and I got a letter from the Pittsburgh Section of the American Chemical Society, with a clipping from a local newspaper with my photograph and the story of my life. They wrote, "In 1935, he left his native Poland when the German army overrun his country." I thought, they are really good. They knew that I had left Poland in 1935, but they didn't know that the German army overran Poland in 1939. You can imagine how highly I appreciated this journalist!

BOHNING: Could you describe why you went to Israel at that time and how that change occurred?

SZWARC: Well, I felt that the situation of the Jews in Poland would become worse and worse and I wanted simply to get out. There was a lot of enthusiasm about building Israel, which at that time was still a British mandate. A Jewish community was developing, and a university was created. Hebrew is a very ancient language, a language which was used for four thousand years or more. This language was perpetuated by the Bible and was spoken and written for years and years, but it was not a modern language. Obviously, new words were needed to deal with modern ideas and realities. Many new words were needed to cover sciences, like chemistry, physics or mathematics. A special committee was created in Jerusalem for developing new words needed to express the modern concepts, but at the same time, these new words had to obey the rules of the language.

English is a peculiar language in the sense that it can adopt foreign words and incorporate them rather easily. You probably are not aware of the fact that, not only all kinds of French, Indian and Hebrew words are incorporated into the English language, but sometimes even the grammar. For example, the English plural of the word cherub is cherubim and not cherubs. This is the Hebrew plural. So not only the words were taken from the Bible (there are many others), but even the grammar. It is very easy for English to adopt words. In Hebrew, because of the Hebrew grammar, there are a number of conditions which have to be fulfilled in order that the words will fit into the language. So the committee had to be of scholars both in science and in linguistics in order to develop these words. In those days we started the courses in chemistry and I could lecture on chemistry in Hebrew. That was very important for political reasons as well. The British government suggested to the Hebrew University in Jerusalem that they would provide funds for the development of the university under the condition that the lectures in science would be given in English. This condition was not accepted, and so it was necessary to develop scientific terms in Hebrew. Today there are many books and journals dealing with chemistry in Hebrew. In 1942, during the war, I was in Egypt, working with the British War Supply Board. I managed to contact some professors of Giza University in Cairo. They were teaching in French or in English. They said it is impossible to teach in Arabic, because they don't have the scientific terms. I told them that in Israel we have developed a full vocabulary and we could discuss science in Hebrew. I think even today they don't have it properly developed.

BOHNING: That's interesting. When did you learn your English then?

SZWARC: I got a dictionary with phonetic pronunciation. I learned it in this way essentially by myself.

BOHNING: Let me go back for a moment to the time when you went to Jerusalem. How did you make your contacts there? How did you support yourself? Had you been saving your money to make the trip?

SZWARC: Well, at that time I was married and my father-in-law arranged the trip. He also got me the visa which was not an easy thing to get at that time. They were very, very difficult times. I had the privilege of working in the lab at the Hebrew University and to participate in the teaching of students, but they didn't pay me. So I had to do some other work. For nearly two years I was earning money as a night guard. At night I had to patrol and to guard the university. It was a duty from seven until midnight or from midnight to five o'clock. One night seven to twelve and the next from twelve to five. It was not an easy life.

BOHNING: What were you teaching?

SZWARC: Organic chemistry, which I still don't know!

BOHNING: Were you using a text or did you have to use your own notes?

SZWARC: Yes, we had a text. In my recollection it's called Getman, and that was on organic synthesis (3).

BOHNING: Could you tell me something about the university when you got there. What was the Hebrew University like?

SZWARC: Well, it was a university in a state of development. It was established in the early thirties. I got this position in 1936. Various departments were being established. The first ones were in literature, linguistics and the study of the Bible. Eventually the department of chemistry was created and the first students came in 1937. I was in charge of the students' organic synthesis laboratory which was rather primitive. All the heating was done with Bunsen burners, leading to numerous fires.

BOHNING: Were they building new buildings, or were you refurbishing older structures?

SZWARC: Well, gradually various buildings were erected. Were you ever in Israel?

BOHNING: No.

SZWARC: I recommend that you go. It's a very interesting country. There was a building, now called the main building, in which biology and biochemistry was taught. Then some space was given for inorganic and organic chemistry. Later, about 1938 or the beginning of 1939, a new building for physical chemistry was erected. We had a very good man, [Lasislas] Farkas, in physical chemistry. There were two brothers. The older was a professor who came from the Bonhöffer lab in Germany. He was very much interested in photochemistry, and catalysis. They made their reputation on the hydrogen-deuterium type of work. There's a book written by the younger Farkas [Adalbert] which is to some extent a classic. It is on hydrogen and heavy hydrogen (4). It was a classic for people who were interested in hydrogen and deuterium and those kinds of problems.

BOHNING: Who were some of the other faculty that were there at that time?

SZWARC: Well, as I said, there was Farkas' younger brother, who I think is still alive and lives in this country. Besides those two, no one else was well known. Among the people who came very early were the two Katchalsky brothers [Aaron Katchalsky and Ephraim Katchalski], who later became well known. They worked with an unpleasant and not very knowledgeable man who was interested in polymer chemistry. They were about the same age and they turned out to be very, very good. We also had a very good biochemist, but somehow he didn't develop as much as the others. But many of the younger scientists who came since then have become outstanding in a variety of fields.

BOHNING: You published your first paper in 1936 (5). That was just a year after you got there.

SZWARC: When I graduated, I wanted somehow to retain contact with the academic life. In the last year of our studies, we had to do something which you would call today a master's thesis. We had to choose a field and have somebody to work with. I wanted to work in physical chemistry, but the professor was boring me so much with thermochemistry, that I decided not to do it. So, I did my master's thesis, if you call it master's thesis, with a physicist. He was a very interesting self-taught man. He was not a first class scientist, but a very, very interesting man. I was working at that time on Raman spectra with not much success. Then after graduation, I asked him if we could continue some kind of cooperation. He agreed and allowed me to work in the lab on some pharmaceutical research. The idea was to produce gluconic acid by electrolysis. This is the subject I tried to describe in this paper. It was not outstanding, and not very profound. I wrote it when we completed the work and left it with him. Later, I got a letter telling me that he had decided to publish it. Strangely enough, he published it under my name only (5). I thought that was very nice of him and it fit very well with his character. It was published in Polish in a Polish journal dealing with pharmacology. So this was my first paper. The next one was published in 1947.

BOHNING: You eventually got a Ph.D. from Hebrew University in 1942. Did you start working on that degree when you arrived, or were you just doing some teaching for them?

SZWARC: Well, I was teaching and working at the same time. Essentially, everything was so primitive. Being there, I tried to get a Ph.D. in organic chemistry, which I eventually got, and of which I am not proud at all. I don't think I did a good job. But there was nobody with whom I could work, because the organic chemistry professor was very poor. He was the brother of Chaim Weizmann and that was all he was. So I didn't get any help from him, and I was working in an intellectual vacuum at that time. There was war and one had to do all kinds of things for the army. The Australian and British Army, who were in Palestine, couldn't get their supplies, because the Mediterranean was controlled by the German and Italian Navies. So it was necessary to manufacture all kinds of relatively simple products for the Army or for the Air Force which normally were sent from England. The problem was how to make them without the proper raw materials, equipment, and labor. The only thing which was not important was price. It didn't matter if it would cost a hundred times as much as it should. For example, the pistons in the aircraft engine have to be cleaned after a thousand hours of flying, and in this process of cleaning they should be submerged in phosphoric acid to remove some corrosive substances which are formed there. Now phosphoric acid is made from phosphorus, which is oxidized to phosphorus pentoxide, which is then dissolved in water. The army insisted in using extremely pure phosphoric acid for this purpose. I thought that a trace of sulfuric acid, for example, would not matter. But the military stuck to their rules and regulations. So to make the pure phosphoric acid, as required, I had to start with superphosphate, react it with rejected sulfuric acid obtained from an oil refinery, which was full of mercaptans and other impurities. The resulting green, impure phosphoric acid was neutralizied with sodium hydroxide to form sodium hydrogen phosphate, which crystallizes nicely. Then the recrystallized salt was reacted with pure sulfuric acid to get very pure phosphoric acid. Well, it cost a hundred times as much as it should. But eventually, I got the product conforming to the specifications which the Air Force demanded. By hook or by crook you had to do it. And consequently there was not so much time for proper research. Well, we were at least teaching the students.

BOHNING: How many students did you have had during the war?

SZWARC: The first class, which I still remember, was seventeen boys and girls. Then we increased the number to about twenty. These were undergraduate students. They cannot be compared with students of today. They were at different levels and had to meet different requirements. Some of them became very, very good. As time went by we got better and better students. I left shortly after the end of the war, because it was not really the proper academic life I wanted. It was distorted by war, and the whole situation was very fluid. So I decided to go to some well-known British or American university. I talked to Farkas about it and he suggested to go to Michael Polanyi. That was perhaps the best luck I had in my life, because Polanyi turned out to be an extremely inspiring man.

BOHNING: Had Farkas known Polanyi?

SZWARC: Oh, yes, he knew him well. Polanyi was in the Kaiser Wilhelm Institute in Berlin which today is the Max Planck Institute and spreads all over Germany. Haber was the head of the Kaiser Wilhelm Institute and Polanyi was one of the professors there. When Hitler came to power, he realized that he had to leave. And since he had an offer from Manchester, he accepted this offer and moved there. Farkas knew him from those old days. Farkas was working with [Karl F.] Bonhöffer, and this whole scientific community was in close touch with one another.

Polanyi was extremely gifted and had a most unusual career. He started as a medical doctor. During the first world war he was in the Austrian Army. He was from Budapest. There was a high school in Budapest where in one year they had in the same class, Polanyi, Teller, Szilard, and von Neumann. It's something fantastic.

BOHNING: Herman Mark told us that.

SZWARC: Oh, Herman Mark. Well, Herman Mark was a student of Polanyi's.

BOHNING: Did Polanyi pay you a stipend? Did he give you support?

SZWARC: Not for the first eight months because I went as a graduate student. That was the only way in which I could go, but since I worked hard during the war, I managed to save enough money to afford this type of trip and to stay without any salary for eight months and to leave enough money to my wife. At the end of this period he told me, "You know, you have done enough work for your Ph.D.", which I got sometime later because I could get it formally only after two years. He offered me an ICI fellowship which was a high class fellowship, given to people such as [Charles A.] Coulson. He was an ICI fellow at about that time. Later I became a lecturer.

BOHNING: Could you tell me something about what Manchester was like when you arrived there? The war had just ended.

SZWARC: The European war ended in May, and I arrived there in August. Manchester was one of these drabby industrial towns of England and the war was in some way good for Manchester. They got many desirable squares from the empty spaces created by the destruction of bombed houses. Still, it was very drab. Many houses were still without windows and without roofs. There were shortages of everything, including food. But it was a very happy time, at least for me. There was quite a lot of enthusiasm. During the war, the British were very devoted to their country. They did a really marvelous job. I think the feeling among the working class was, "Well, we won the war, now let's relax." I think that, together with the loss of the empire, ruined England completely. There were difficulties. You couldn't get this, you couldn't get that. For example, we couldn't get Pyrex glass. We had to use the old Jena glass.

BOHNING: Were there other people being attracted to Manchester?

SZWARC: Yes, it was an excellent group. For example, [H. C.] Longuet-Higgins, one of the outstanding theoretical chemists, came to Manchester. It was a very, very good group. At midnight you could get easily a cup of coffee in company of ten people. You cannot do that now.

BOHNING: What were the laboratory facilities like at that time?

SZWARC: Poor. But the work was good. There were technical difficulties. Many gadgets which you have now didn't even exist in those days. Gas chromatography, for example, not to mention NMR, or ESR which practically didn't exist in those days. But quite a lot of good work came out of this lab despite the shortages.

BOHNING: When you started your work on bond energy, did you select that problem or did Polanyi?

SZWARC: Polanyi suggested it to me. He was interested in bond energies, and said, "Perhaps you can look into the problem of determining the bond dissociation energies of organic iodides." So I worked for a little while on benzyl iodides. Then I came to the conclusion that that is not a system which will lead to what we want to find. The idea was to break the bond and somehow measure the resulting kinetics. During this work I came up with the idea that it would be better to determine the dissociation energy of the C-H bond in toluene. Polanyi said, "Well, that's an interesting idea. I don't think that it will work, but try it." When it started to work he came every evening, and made suggestions. The next morning he was down asking for results. I used to tell my students that it's very nice when the professor comes in the evening, tells you what to do and in the morning comes and asks for the results.

Polanyi had a wide range of interests. He started as a medical doctor, moved to physical chemistry, and contributed enormously to the solution of fundamental problems. For example, the famous work in which Mark participated was the determination of the structure of cellulose by x-ray crystallography. The idea was that cellulose, being crystalline, must have unit cells which determine the structure of the crystal. How could a polymer fit into the unit cell? It was a novel idea that only a segment of the polymer fits in a unit cell and not the whole of the polymer molecule. That was a great innovation by Polanyi and Mark. He got interested in the problems of adsorption and was nearly destroyed by Langmuir, who came with a different type of picture for the adsorption, the unimolecular single layer instead of the multilayer which Polanyi developed, and which is now important. He was interested in catalysis. Amongst others, he was interested in bond dissociation energies. Well, his famous work is on the so-called sodium flame. In the book by Hugh Taylor, there is quite a lot about the sodium flame reactions (6). Of course, sodium flame reactions led to problems of bond dissociation energies.

During the war he was unable to work because the whole university was asked in some way to help out in the war effort. And so he got interested in economic problems. He published a book on the thermodynamics and kinetics of money, treating economic problems in terms of equilibria and kinetics involving money (7). Well, I don't know how this book was accepted by economists. It seems to me that it was a bit over their head. But anyway, that was his interest at the time when I came to Manchester. It was economics and not chemistry. Three years later, the University, in recognition of his contribution to economics, allowed him to resign from his chair of physical chemistry and appointed him to the professorship in social studies. So he became professor of economics and for me it was a blow that I lost him. Then he got interested in philosophy and all kinds of questions connected with the theory of probabilities. Eventually, he finished with theology at the end of his life. He was a very strange person. If you are interested in history you may read a book published after his death with contribution of various people, amongst them Melvin Calvin, and Henry Eyring. Polanyi and Henry Eyring developed this famous H plus H_2 reaction. You can find quite a lot about his activities in this book (8). It is a book edited by his son John Polanyi. He was an extremely interesting man.

BOHNING: What was his group like when you joined it? How many other people were there in the group?

SZWARC: There were probably fifteen in the group. The system was that a professor had a few lecturers, and each lecturer had two or three people. H. S. Skinner, A. G. Evans, and others, whose names I cannot remember now, were lecturers at that time. When Polanyi resigned M. G. Evans became the professor. He was a very different type of man, a close associate of Polanyi, and had become a professor at Leeds. And then from Leeds he came to Manchester. So my last few years in Manchester were with M. G.

Evans.

BOHNING: I see. You published your first paper on bond energies quite early, in 1947 (9). I'd also like to ask how quickly did you get to the para-xylene -- when you found the first polymers?

SZWARC: Two weeks.

BOHNING: Two weeks after you started?

SZWARC: Two weeks.

BOHNING: Could you tell me something about that?

SZWARC: Yes. As I told you, I was working on benzyl iodide. And the idea was that the benzyl radical is very stable due to its resonance energy. Therefore I expected that C-H bonds in toluene would be very weak, and should break and produce a benzyl radical and a hydrogen atom followed by reactions of hydrogen atoms with toluene leading either to formation of H_2, or formation of methane plus another radical. Obviously the next question was to examine how substituent groups are going to affect the reaction. And so I studied pyrolysis of various aromatic hydrocarbons and particularly the xylenes in anticipation that I would again obtain the same kind of product. In each case the formation of substituted dibenzyls confirmed the anticipated pattern of decomposition initiated by the rupture of the relevant C-H bond. When I carried out the pyrolysis of para-xylene I got an unexpected result. A film had formed on the glass of the trap. I realized that it was a crosslinked polymer. That was in 1946 shortly after I arrived when I was working alone before I had students. Within two weeks, I proved that I'd produced a quinonoid hydrocarbon. A very simple experiment, simply by letting the pyrolysis gas into a cold trap with iodine and isolating the product. Eventually I showed that the final product was a polymer of p-xylylene. It took me only two weeks, without any assistant or sophisticated equipment. Later we developed a whole class of polymers with this method, using substituted para-xylenes, the 1,4-dimethyl naphthalene, 2,5-dimethyl pyrazine and so forth. But the original work was done in two weeks. Keep in mind, however, that a week had seven days, not five, and a day was twenty hours long.

BOHNING: Had you had much exposure to polymers before that?

SZWARC: None.

BOHNING: That's interesting.

SZWARC: Herman Mark and Charlie Price came a year later (in 1947) for a meeting. They came to see this polymer film and Herman Mark said, "Well, you see! You are a polymer chemist!" Some results of this work were published in the Journal of Polymer Science (9).

BOHNING: But that paper was some years later, in 1951.

SZWARC: The original results were published in 1947. Thereafter, I evaluated a series of compounds and these results were published in 1951 (10). My intention was to calculate the dissociation energies and not to produce polymers. After that, I deliberately tried to produce polymers. Some were successful, some were not.

BOHNING: You must have attracted a lot of attention with that early announcement.

SZWARC: Yes, it was interesting to many people. At that time they were looking for thermally stable polymers and it turned out that poly-p-xylylene was a thermally stable polymer. Today, of course, there are many others with better properties.

BOHNING: You said you had made your first trip to this country in 1950. Was that work in part responsible for the invitation? You also had a large number of publications on bond energies too.

SZWARC: Hugh Taylor came to Manchester where he talked to me and was very much interested in some bond dissociation energies. He said, "Why don't you come to the U.S. for a tour?", and I accepted. So he organized the tour and I came in March 1950 for four months. I enjoyed it very much. Taylor arranged everything, got all the invitations, and set Princeton University as my base. He did everything well with disregard only of geography. I had to give, let us say, two lectures in Yale, following by something in Chicago, then at NYU, and something in Boston, and then again somewhere else. And during this four months, I covered seventeen thousand miles. It was exciting. One of the funniest arrangements was to give two lectures in Purdue followed by another two at the University of Illinois in Urbana. They didn't have highways then. The only way to get from Purdue to Urbana was to go by train to Chicago and then back to Urbana; along the two long sides of a triangle rather than along the shortest side. But there were no roads. My second lecture in Purdue was in the morning and my first lecture in Urbana was on the same day in the afternoon. We were sitting during teatime trying to find out how to go from Purdue University in Lafayette to Urbana and be on time. One of the members of the faculty there, [Elliot] Ritchie Alexander, suggested to fly me in his plane from the lawn of Purdue to the lawn of Urbana. I had to pay for the gas and he would take me. And that's how we went. From one lawn to the other in three-quarters of an hour. The people of Urbana were simply bewildered! They also had my itinerary and they wondered how I could make it. Alexander died later in an air accident. He was with his wife and they crashed.

BOHNING: He wrote an excellent book that I have a copy of (11).

SZWARC: It was on ionic reactions in organic chemistry. Yes, I remember that. I was sorry to hear that he and his wife were killed in that crash.

BOHNING: Was that shortly after he flew you, or was it later?

SZWARC: I think two years later. I don't remember the date, but relatively shortly after.

Well, to come back to my tour of the US, Dow Chemical was interested in the polymers, and Ray Boyer invited me to visit Midland. When I arrived, Ray was waiting for me at the station, and took me for dinner at his home. As we were driving to his home, we stopped at the supermarket because he had to buy something. I went with him into the supermarket, and I was overwhelmed by the quantities of meat and food. At that time we had rations in England and we were getting a tiny amount of meat per week.

BOHNING: How long did you stay at Dow?

SZWARC: For a week. We did some experiments. They built a furnace for me and we repeated some of the experiments I did in Manchester. I taught them how to do all that, and then they continued the work.

BOHNING: Did you meet anyone else at Dow while you were there?

SZWARC: Well, I met a number of people, but Ray was the one whom I had first contacted and was my closest contact.

BOHNING: Did you or anyone else get a patent on that?

SZWARC: On poly-p-xylylene? Yes. I got a patent in England (12). I couldn't afford to go through all the patenting procedures and expenses. I managed to contact someone in the petrochemical industry. They offered me a consultantship and they patented it. Did you ever come across Butch Hanford?

BOHNING: No, I don't think so.

SZWARC: He was an interesting man. Butch [William E.] Hanford was a typical American businessman. He was then vice president in charge of research of the M. W. Kellogg Company in New Jersey. He eventually resigned from there and became vice president in charge of research at Olin Mathieson in Connecticut. He tried to be elected President of the American Chemical Society, but he failed. I think because of that he started to drink. Anyway, he was the true American, you know, with the crewcut of that period. He came to Manchester, got interested in the polymer and bought the patent. So the patent was sold to M. W. Kellogg Company. M. W. Kellogg Company was and is today an engineering company which builds refineries, crackers, and petrochemical installations. Butch was trying to expand its activities into chemicals.

Actually during the war, he developed something which turned out to be very successful, the so-called Kel-F, polytrichloro-fluoroethylene, an extremely useful compound needed in the manufacture of uranium hexafluoride. He tried to make a chemical company out of M.W. Kellogg and buying my patent was one aspect of that policy. He also acquired from Phillips Petroleum the patents for polyethylene and as a result of that, the M. W. Kellogg Company, which was sold then to Pullman, got into trouble with its customers. The oil companies insisted that if they were going to become a chemical company they were not going to give them any orders. So the Pullman people had to decide whether they wanted to continue as an engineering company or whether they would like to become a chemical company. They eventually decided to remain an engineering company. As a result of that, Butch Hanford resigned. But I must say that he did something very nice. He arranged that 3M would buy all the chemical assets with the understanding that the personnel which were involved in this type of work would have the option to join 3M with the same seniority. Some did, some did not. One day I was in my office and I got a telephone call from Butch. He said, "Mike, (and I hate it when somebody calls me Mike) we sold your patent to 3M. Can we also sell you?" I said, "All right, do that." So I became a consultant to 3M. It was typical for him to put it in this way.

Eventually Union Carbide developed para-xylylene on a commercial scale. I was working on the pyrolysis of para-xylene. One of the products of this pyrolysis is a cyclic dimer, known as paracyclophane. This has two benzenes linked by two CH_2-CH_2 bonds, and accordingly is an exceptionally strained material, which, not surprisingly, decomposes back into the quinonoid hydrocarbon at much lower temperatures than needed for pyrolysis of para-xylene. As a result of that, instead of working at 900°C one can work at 600°C. So the formation of by-products which lead to crosslinking and made the polymer difficult to handle are avoided. By using paracyclophane as starting material, non-branched and non-cross-linked materials are obtained and these can be easily processed (13). I was consulting at Carbide for a number of years in this connection. Actually, Union Carbide is not selling the product, but the procedure. Coatings are made directly by depositing the polymer from the gas phase. This is the novelty in my process. The gaseous monomer is converted directly to the polymer without going through the liquid phase.

BOHNING: Were there any other places you visited on this 1950 trip?

SZWARC: You may ask the other way around. Which are the places I did not visit? I was in forty-odd places during this four months. I started from the East and finished in the West at UCLA Berkeley and Canada. I had a marvelous tour.

BOHNING: Is this what led to your eventually coming to the United States?

SZWARC: Well, the following year I was invited to the seventy-fifth anniversary of the American Chemical Society. That was in 1951. I was offered a position in Syracuse which I accepted. A year later I moved to the U.S.A.

BOHNING: It has always struck me as being sort of unusual to have you going from a place like Manchester to a College of Forestry.

SZWARC: Yes, that was very, very unusual. I agree with you. Well, as I told you, in 1951, Polanyi left and M. G. Evans succeeded him. Soon after that an opening materialized at the College of Technology in Manchester [now UMIST] following the retirement of the professor there. I asked M. G. Evans whether he would like to recommend me so that I would get my own place. He said that although I was a very good research man, he didn't know if I would be able to create a department. I was annoyed with this answer. So when I came to the United States shortly after that and was offered a position at Syracuse I thought that this might be an opportunity to prove that I could create a department. And so I accepted the offer. I remember that when they wanted to show me what they had at Syracuse, I told them that I was not interested in what was there, but in what they were going to have in the future. There was no polymer department at Syracuse. Then it was just a College of Forestry.

But we very rapidly developed a very active group. I made the decision to prove to myself, and to others, that I could do it.

BOHNING: How did they make that contact with you?

SZWARC: Through Mark. Ed [Edwin C.] Jahn had a strong influence in what was happening in Syracuse. Again he was one of the men whom I appreciate very much. He got his degree in Montreal. He was originally a graduate of the College of Forestry. Then he moved to Montreal and studied in McGill University. He got his degree in cellulose chemistry and returned to Syracuse. His ambition was to start research and teaching in polymer chemistry with the idea that the paper industry (and he was very well acquainted with the paper industry), was going to require a wide variety of polymeric materials. So it would be good to have a group of polymer chemists. He asked Mark to find somebody who could do it. Mark suggested me and I was offered the position. I made it clear at that time that I would never take any interest in cellulose and paper. And Ed replied, "That's all right. We have people who are interested in cellulose and paper."

BOHNING: Did you go up to Syracuse when you were in the States for the ACS meeting?

SZWARC: Yes. That's when I first went to Syracuse. I told them that I could not come immediately because I had seven or eight people working with me, but within a year I would be able to complete my work in Manchester. Actually I brought some of my students from Manchester to Syracuse. Unfortunately, one of them died about three months ago. Maybe you have heard his name -- Alan Rembaum?

BOHNING: No, I don't think so.

SZWARC: Alan got his Ph.D. with me. He then worked for two years with [Arthur V.] Tobolsky in Princeton, and eventually moved to the Jet Propulsion Lab. He contributed enormously. He got two gold medals from the Jet Propulsion Lab in Pasadena. Unfortunately he developed cancer and died in June. He was incapacitated for more than a year. It is sad, you know. During the last fifteen months I lost three of my former students. Alan Rembaum is the last one. You probably didn't hear about Ralph Milkovich, either. He produced some new polymers. Apparently that is what you have to expect if you live too long. So when you will be ninety, you probably will have a long list of people who passed away.

BOHNING: I wanted to ask you about some of the people that you have worked with and co-authored papers with in Manchester. I notice that J. S. Roberts is a name that showed up quite a bit.

SZWARC: Yes, he was my second student. Allen Shaw was first, Roberts was second. Why you are interested in Roberts?

BOHNING: Only in the fact that he did a lot of work with you (14).

SZWARC: Yes, that was on bond dissociation energies.

BOHNING: What did he do after he left Manchester?

SZWARC: Roberts eventually went to ICI.

BOHNING: Okay.

SZWARC: Another one was Alec Sehon. Alec Sehon is director of the Immunology Institute in Winnepeg, Manitoba. He became one of the leading men there.

BOHNING: Shortly after you arrived in the United States, well within that next year or so, you published a number of papers. You began that work on methyl affinities very quickly.

SZWARC: There's a very simple connection with the problem of pyrolysis. I postulated that a methyl radical is formed by reacting hydrogen with toluene thus producing benzene plus methyl radical. "Atomic cracking", that's how it is known now. [William A.] Waters at Oxford was one of the leaders in free radical chemistry in those days. He claimed that methyl radicals are substituting hydrogens in aromatic hydrocarbons. That is, benzene will eventually be transformed to toluene and toluene to xylene. That didn't fit into my values of bond dissociation energies, because a weaker bond was produced by breaking stronger bonds. I thought that there is something fishy in this whole story and so decided to check it. The idea was to produce methyl radicals, react them with benzene and check whether they form toluene. This work led to the study of methyl affinities. We found they do react, but not by removing hydrogen, as Waters said, but by addition, producing eventually a radical which in turn loses a hydrogen. So this is the continuity with the previous work. As usual, when one starts a new subject of research one develops it. Thus we carried out a series of studies of affinities of trifluoromethyl radicals, ethyl radicals and so forth. That is a normal type of expanding the field and, by the way, it led to my work on the living polymers.

BOHNING: Do you want to tell me how that followed.

SZWARC: Well, that was again some kind of strange story. I was working with one of my former students at Brookhaven. The general idea was to study bond dissociation energies again by pyrolysis but using compounds that had been labeled with radioactive carbon in order to establish mechanisms of reaction. The idea was to pyrolize ethane, produce methyl radicals, and to trace them by having this ethane labeled with radioactive carbon. I was going every month to Brookhaven to supervise the work and discuss the results with him. One summer when I arrived, I met Sam [Samuel I.] Weissman, whom I consider an outstanding physical chemist.

BOHNING: Isn't he in St. Louis?

SZWARC: Washington University, St. Louis, Missouri. We had published a paper on methyl affinities of aromatic hydrocarbons, benzene, naphthalene, anthracene and so forth (15). Sam said, "Well, I read your paper with great interest, because we are studying electron affinities of aromatic hydrocarbons. And our series follow yours." I had no idea how he measured electron affinities. I asked him, "How do you do it?" He said that essentially they were studying equilibria involving radical anions, for example,

$(\text{naphthalene})^{-}$ + phenanthrene = naphthalene + $(\text{phenanthrene})^{-}$

The equilibrium, as expected, is governed by the difference of electron affinities. As he was talking I thought, well, if we transfer an electron to something like styrene, then maybe we will form a carbon anion on one end and a radical on the other. Since both can start polymerization, we may simultaneously initiate radical polymerization in one direction, and ionic in the other. I asked Sam, "Did you try to transfer an electron to styrene?" I was tickled with his answer. He said, "No use. It polymerizes." I said, "Would you mind if I would look into the problem?" He said, "By all means." And so we started.

Very quickly we found that first of all we deal with dimerization of radical-ions and what has turned out to be so important, we avoid termination. So once more in a very short period of time, less than a year, we proved that we had a system in which termination and transfer are prevented. I reported it as <u>living polymerization</u> and eventually the whole idea was further developed and expanded.

The last work which I did at Syracuse dealt with problems of electron affinities, electron transfer, disproportionation, using a completely different approach, namely through flash photolysis. During the last few years we were involved in studies of electron transfer, disproportionation of radical ions into dianions, etc.

BOHNING: When you started that work on styrene, did you realize that both ends could polymerize?

SZWARC: Yes.

BOHNING: Did you need to do anything special with styrene?

SZWARC: Oh, yes. You have to purify it very carefully. The problem of purification took most of our time. Traces of water had to be removed and work carried out under vacuum.

BOHNING: Had you anticipated the fact that the ends wouldn't terminate or was that unexpected.

SZWARC: At the beginning I didn't think of that because we didn't know whether polymerization would occur. Usually, I don't worry how to cross a bridge until I come to it. So, the first thing was to initiate the reaction. But when that was done, what was so striking was that the color, the characteristic color of the benzyl ion, persisted. Therefore, I thought that maybe the reaction was not terminated, that the polymer is still capable of growing. Once this hypothesis is put forward, then experiments should be designed to prove it. And again, very simple experiments were designed to prove the point. It turned out that that was really the case.

BOHNING: So it was the color that gave you the clue.

SZWARC: The color suggested that there is something strange. Normally polystyrene is not colored. So it was a tentative suggestion that apparently it is capable of continuing to grow.

BOHNING: What year was this?

SZWARC: We started and ended in 1955. The first paper was published in 1956 (16). I had a very good coworker.

BOHNING: Who was that?

SZWARC: Moshe Levy, who is now director of the polymer section in the Weizmann Institute.

BOHNING: Those first four years were very busy for you in terms of the research you were doing. But at the same time, you were building up the department at Syracuse. You had to bring in students, get support, and do a number of other things as well as doing all of this research work.

SZWARC: Yes. Well, it worked. Did you ever meet Walter Kirner? Walter Kirner, I think, was professor in one of the New Hampshire colleges. He was the first Chairman of the Chemistry Division of the National Science Foundation. He was an extremely nice person. We became very, very friendly. The National Science Foundation was not the first but certainly the most important supporting agency. The first was actually the Research Corporation. But the National Science Foundation supported me to the very end. And of course, at that time, you could get support from the Navy and from the Air Force and from the Quartermaster Corps etc. So we got the support.

I remember that when I brought students from England in those days, (my former students and so forth,) every time I had to get the dean's signature for their student visa. I remember that when I gave him the fourth or fifth name to sign the application he said, "Don't we have good American boys?" I said, "Sure, you have excellent American boys, but they will not come to Syracuse. They will go to Harvard and similar colleges. You have nothing to offer them. But to a British boy, I have something to offer. I offer them a chance to see and live in America." And that convinced him! So yes, there were problems like that.

Returning to Kirner, when we first met, and he was as I said in charge of the Chemistry Division, they were on some corner house, I think on Sixteenth Street in Washington. The chemistry division was in one room and the files were all around. That was in 1953. Eventually they got the building of the Atomic Energy Commission, when it moved into a new building.

The chemistry department of Syracuse University was completely dilapidated. There were very old people, deadwood. Once on visiting Washington I asked Kirner, "When will you come to see us again?" "Oh," he said, "It might be soon, because we have a program, rejuvenating old chemistry departments, and the Chemistry Department of Syracuse University sent us a proposal. They have a good case." They helped them enormously.

BOHNING: Did you start bringing in American graduate students as well.

SZWARC: I am now talking about Syracuse University, not about the College of Forestry. You see, Syracuse University is a private university. The College of Forestry is part of SUNY. So administratively we are completely different institutions, although we were on the same campus. NSF really helped the chemistry department of Syracuse University very much by this rejuvenation grant. They now have a number of good people.

BOHNING: In the middle 1950s you published a number of papers on the permeability of polymer films by gases (17). How does that tie in with the rest of your work?

SZWARC: Well, that was a side-line. The man who suggested that we go into this field and who developed it to a great extent was Vivian Stannett. We got involved in that area while looking for financial support. We were talking with people from the Quartermaster Corps in Natick, Massachusetts. They wanted to provide some support, if we were willing to look into the problem of permeabilities of various polymer films. We developed the technique of how to do it, and we then pursued it further. Stannett eventually moved to the University of North Carolina at Raleigh and he made his reputation on this problem of permeability. Interestingly, the man who replaced me in Syracuse in the College of Forestry is a man who is also interested in permeability.

BOHNING: Oh, really?

SZWARC: Yes. Israel Cabasso. Our work on permeability started in the most prosaic way. The Quartermaster Corps told us that they were interested in such and such films, such and such gases, and we had to determine the permeabilities. It was a service type work. But as is always the case, one starts to ask questions. Should we investigate the effect of this, the effect of that? And while looking into these permeability curves, I noticed that there were normal situations when the permeation increases with pressure in a linear fashion, and there are cases which are strange e.g., permeation goes up exponentially on increase of pressure.

It is not difficult to understand. For example, water vapor permeating through nylon. Nylon is a very tough polymer. The hydrogen bonding between the NH and the carbonyls of the amide groups on adjacent chains is responsible for the strength of nylon. When one adds water, it replaces this hydrogen bonding, and opens the structure. The more water, the more open the structure. This leads to this dramatic, exponential increase in permeation. While we were doing this, I thought it would be interesting to make a laminate from nylon and, let us say, polyethylene, which behaves normally. Such a laminated film behaves like a valve. If water is on the side of polyethylene, and vacuum on the side of nylon, the nylon will find itself in the low gradient of pressure and therefore will behave as a very good barrier. But in the other way, nylon is in a high pressure region and behaves as a very poor barrier. Thus, the permeability will be different whether water is on this side or on that side. The film is anisotropic. It was not difficult to make the necessary calculations. Then a graduate student checked it. He's now a professor in Cleveland. A plastic can be a good barrier in one direction, but a poor barrier in another. Like a valve! This student, Charlie [Charles E.] Rogers, continued with this work and became involved in research leading to artificial lungs, kidneys, etc. For me that was perhaps the most interesting work in this field, and Rogers did quite a lot of other things after he left.

BOHNING: When did you work on cage reactions?

SZWARC: That's again related to the work on methyl affinities. A typical situation of working on a subject which generates all kinds of other interesting problems.

BOHNING: How many students did you have at Syracuse during the 1950s and 1960s?

SZWARC: Well, the total number of people, that is graduate and postdocs, was over a hundred and fifty.

BOHNING: When did you begin your ESR work?

SZWARC: That was a result of the work on electron transfer.

BOHNING: Weissman was doing ESR work quite early.

SZWARC: Weissman developed the whole field. The understanding that aromatic hydrocarbons can be converted into radical ions was something new which Weissman developed. Working with a radical generating device, in one or another way, I got into the problem of ESR.

BOHNING: In the 1960s you continued to work on all of this but there's a new term that appears -- the <u>dormant polymers</u>.

SZWARC: Yes. This is a general problem important in ionic polymerization. Namely, in the radical initiated polymerization, a process elucidated over the 1940s and 1950s, a radical grows;

period. In ionic polymerization a growing species must be an ion, a cation or anion, and there must be also a counter-ion. Obviously they may combine together to form an ion pair. The concept of different ion pairs appeared, if my recollection is right, in 1954 or 1955 in two papers. One was by Saul Winstein, and the other by Raymond M. Fuoss (18,19). Winstein called them contact and solvent-separated ion pairs. Winstein derived this idea that ions in ion pairs can be in contact or separated by solvent molecules from his study of solvolysis. Fuoss came to this conclusion through the study of conductance in solution. So both came with the idea that ions were either in contact or solvent-separated i.e., there are two different types of species. Both of them published their results in the same year, in the same journal, JACS [Journal of the American Chemical Society]. I was a friend of Saul Winstein, and I was a close friend of Fuoss. So I can assure you, because I talked to both of them, Fuoss didn't know about the work of Winstein, and Saul didn't know about that of Fuoss. They published in the same year in the same journal, but they didn't know about the other's work for quite a time.

Obviously, intuition led to the feeling that the free ions should be much more aggressive in polymerization reactions than the ion pair. A contact ion pair should be less aggressive than a separated pair. So several species are propagating at the same time. It's not difficult to imagine that some of them are propagating so slowly that they seem not to grow at all. Nevertheless they do grow because they are always in a dynamic equilibrium with the growing ones. The free ions combine into a pair. The pair dissociates. So although for a while a particular polymer doesn't grow and is dormant, later it will grow. And one which grows collapses into a dormant state. So this idea of distinguishing between the various types of species led to the definition of dormant and living species. The dormant contributes to polymerization because the dynamic equilibrium converts it into a growing one, and vice versa. But you have to consider that they lead to different types of problems, and they have effects, not only on the kinetics of polymerization but also on molecular weight distribution.

The first specific example I had occured in polymerization performed in the presence of anthracene. When anthracene was added, the polymer became dormant. But the addition is reversible, as with ions and ion pairs and eventually the polymer will grow. At that time I had a French coworker whose name was Gerard Spak, a very nice and very intelligent man. When I suggested that we would have to call them dormant polymers, he said, "Why not sleeping?"

BOHNING: That's excellent. You spent a year in Liverpool in 1963-64.

SZWARC: As a visiting research professor of the Royal Society.

BOHNING: Anything special during that year?

SZWARC: Yes. I had a Chinese student as a coworker. He was a postdoc from Red China, because in that year the Royal Society signed an agreement with the Chinese Academy for an exchange of scholars. Since I was a research professor of the Royal Society I got this student to work for me. Being his host or boss or whatever you call it, I was trying to be polite -- to invite him for dinner at home or to a restaurant. But he never accepted. On the last day when he was about to leave I said, "Look, we have spent a year here together, let me at least invite you for lunch." That was the only time when he accepted the invitation, and we went for lunch. I lost track of him, and after about twenty years, we got in contact again. By that time, he became the president of Zhejiang University in Hangzhou, and invited me to China. When I met him, the first thing I said was, "Can I invite you for dinner?" And he said, "NO! Now I invite."

One thing which I did while in Liverpool was to write a long review on the so-called NCA polymerization (20). These are the N-carboxy anhydrides, I don't know if you know these compounds. At that time [Clement H.] Bamford was in Liverpool and he was very much interested in this type of work. So I decided to write a review in order to learn the subject. This is the only review I wrote on a subject on which I didn't do any work on myself. The review turned out to be useful to many people; Elkan Blout told me that he was using it in his class. During the time I was in Liverpool I went back to the United States two or three times and I spent two weeks in Israel and two weeks in Italy. When I was leaving Liverpool we had a formal farewell dinner and I told them that during this year I had never left Liverpool, but for me Liverpool is the greater Liverpool with a radius of three thousand miles. And they agreed.

One thing which I initiated there was the cationic polymerization of tetrahydrofuran leading to a living polymer. There was a postdoctorate researcher there whose wife was a chemist. She came to me and asked if I could suggest some project on which she could work. I suggested she look into the polymerization of tetrahydrofuran cation, explaining what kind of experiments I wanted to do, which they did and it worked. In the paper they published, it is acknowledged that I suggested it (21). They both were from Akron or Cleveland. It was a pleasure for me to visit them in their home and to learn that they continued this work for a number of years. They were an interesting couple. Their name is Peter and Pat Dreyfuss. Whenever she sent me a letter, she signed P^2 Dreyfuss.

BOHNING: They're not at MMI [Michigan Molecular Institute], in Midland, are they?

SZWARC: Exactly! They moved to MMI quite recently, about two or three years ago.

BOHNING: I thought so. I recognized the name.

SZWARC: Peter and Patricia Dreyfuss. She was the driving force, not he. She is really an ambitious and driving woman. I think she is still the driving force in this couple.

BOHNING: Have you visited MMI?

SZWARC: Yes.

BOHNING: I'm not clear when you left Syracuse.

SZWARC: I'm not surprised, because I did it gradually. I accepted the visiting professorship here when I was officially still at Syracuse.

BOHNING: I see.

SZWARC: And I resigned formally, when I had my seventieth birthday. According to regulations, at least the regulations of those days, you had to retire officially at the age of seventy. And I am a very old man as you know. You can quickly calculate my age! So, officially it was in 1979 when I left. But actually, I came here in 1978.

BOHNING: Are you still doing work here in UCSD?

SZWARC: I still have some research work going but of a different type. And I'm trying to write books.

BOHNING: I talked to Bruno Zimm on Tuesday, and he told me that the two of you were working on a book.

SZWARC: Yes. He's a charming man. Not only charming, but a man who's ready to help everybody.

BOHNING: Yes. I've heard that, and I've heard that from others.

SZWARC: He is an extraordinary nice man, and a fantastically good scientist. Extraordinarily good scientist. He grasps things very rapidly, and can see the important points.

BOHNING: As I told him, I'm looking forward to seeing when that book is produced.

SZWARC: It is a much bigger task than I have expected. So you'll have to wait and I have to live. I hope that I will live. The field is developing so much in so many directions. It's not easy when you have to decide what to put in a book and what not to put in. It is not an easy job. And I have to learn many things.

BOHNING: He told me that you were doing the writing and he was doing the reading.

SZWARC: Yes, he is correct. I have more time than he does. He makes suggestions and he is very critical. He is very good.

BOHNING: You were at Uppsala for a year. Didn't you work with...

SZWARC: With Stig Claesson. I was talking to Bengt Ränby today. Do you know him?

BOHNING: No.

SZWARC: He's a professor at the Royal Institute of Technology in Stockholm. He just retired. We were talking about Stig Claesson, who is in an asylum and irreversibly ill.

BOHNING: That's a shame.

SZWARC: Anyway, I was working in Uppsala for more than four years. I was there for two weeks every three months, and I stayed once for a full year in Uppsala. The work on flash photolysis was developed there. I also took some of my students there. Stig provided the money which was very convenient. When I finished I moved the equipment to Syracuse. I'm very fond of this work in Uppsala. It was really interesting and novel. I enjoyed it.

BOHNING: Wasn't flash photolysis recently developed at that time?

SZWARC: No. Flash photolysis was developed by [Roland G. W.] Norrish and George Porter. Stig Claesson was an interesting kind of man. He was very precise in his work, essentially developing gadgets. When he developed the gadgets, he didn't know what to do with them. So we complimented each other because I don't know how to build gadgets and as you can gather from some remarks I have made, I'm not very precise, so this cooperation was very fruitful for both of us. I'm sorry that he is in this state. It's quite irreversible. If you didn't know this man, then you cannot appreciate what it does mean. Well, he's not aware of it, you know. He doesn't know what is going on. But for people who knew him....

BOHNING: It must be very difficult.

SZWARC: Well, it is tragic. I don't mind that I will die. I have to die. So does everybody. But I don't want to lose my mental capacities. He's not an old man. He's much younger than I am.

BOHNING: Really? I wasn't sure how old he was.

SZWARC: I would say he's about eight or nine years younger.

BOHNING: Are there any recollections of him when you were there during that year?

SZWARC: Many. He was a very dictatorial type of man. But as I say he had this capacity to develop gadgets. He was very astute and critical. When we were writing papers and I was giving him the draft, he was always able to spot the weak points immediately, and to see how it should be done from his point of view. He gained on this ability to be precise. He got into trouble with women. He divorced his wife and got a witch. And this witch, I don't know what she did. Maybe it was organic, probably it was. But she really changed him.

BOHNING: Well, we've been going for over three hours. I just wanted to ask if there's anything else you thought we can cover at this point?

SZWARC: I don't know. I was talkative, perhaps gossipy, and told you all kinds of things, simply thinking about various people, various days.

BOHNING: Well, I'd like to thank you very much, then, for a very enjoyable afternoon and for spending the time with me.

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INDEX

N

O

P

Q

R