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RUDOLF SIGNER

Transcript of an Interview
Conducted by

Tonja Koeppel

at

Berne, Switzerland

on

30 September 1986

Rudolf Signer
DH
3/15/96

KMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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(Date) 6. December 1989

(Revised 20 February 1989)

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

RUDOLF SIGNER

1903 Born in Herisau, Switzerland on 17 March

Education

1928 Ph.D., chemistry, Swiss Federal Institute of Technology, Zürich

Professional Experience

1926-1935 Teaching Assistant and Research Fellow, University of Freiburg, Germany
University of Berne
1935-1937 Associate Professor of Organic Chemistry
1937-1972 Professor of Organic Chemistry

Honors

1933 Rockefeller Fellowship, Uppsala and Manchester
1948 Rockefeller Special Fellowship, USA
1949 Lavoisier Medal, La Société Chimique de France

ABSTRACT

Rudolf Signer starts this interview by talking of his family background in Herisau. The Kantonschule at St. Gallen emphasized mathematics and the sciences and here Signer's youthful interests in astronomy and philosophy were reinforced. Study of chemistry at ETH followed and Signer recalls some of his professors there. Graduate research on polyoxymethylenes with Staudinger introduces Signer to the young field of polymer chemistry and he remembers the controversy about Staudinger's macromolecular hypothesis. Moving to Freiburg with Staudinger, Signer set up equipment to measure streaming birefringence, which proved a powerful technique of the solution characterization of polymers. A Rockefeller Fellowship enabled Signer to work with Svedberg at Uppsala and to apply ultracentrifugal sedimentation to synthetic polymers in organic solvents. The rest of that postdoctoral year was spent at Manchester with Bragg, where Rudolf Signer used X-rays for structural investigations. Signer tells of his decision to leave Freiburg and of his acceptance of a chair at the University of Berne. The interview includes mention of much research made at Berne, including the isolation and characterization of nucleic acids, water-protein interactions, molecular separation techniques and the thermodynamics of polymer solutions. The interview concludes with recollections of a post-war tour of the United States and of Signer's memories of Staudinger.

INTERVIEWER

Dr. Tonja A. Koeppel received a master's degree in chemistry from the Swiss Federal Institute of Technology in 1944. Since then she has written about chemistry, done research, and taught college chemistry. Dr. Koeppel is also a historian of chemistry. In 1973 she earned a Ph.D. degree in the history and sociology of science from the University of Pennsylvania. She is especially interested in the development of organic chemistry in the nineteenth and early twentieth centuries.

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INTERVIEWER: Tonja Koeppel

ALSO PRESENT: Peter Signer

LOCATION: Berne, Switzerland

DATE: 30 September 1986

KOEPPEL: Professor Signer I'd like to start the interview with a few questions about your early childhood. You were born in 1903, in Herisau, Switzerland. Could you tell me something about your parents, your father?

SIGNER: For many generations the family has owned a textile firm. And now my oldest brother has his sons running the firm, so there have been six or seven generations of Signers, working in the field of dyeing and bleaching textiles.

KOEPPEL: Oh, that's very interesting. Did that have an influence on your studying chemistry?

SIGNER: Yes. I was very interested in two things in my youth: astronomy and philosophy. My father told me that when I have my Maturität I should not study philosophy, because I would not earn a living with this theoretical field, and that I should take either mathematics, physics or chemistry. And so I took chemistry.

KOEPPEL: You took his advice?

SIGNER: Yes, and that lies in the line of the family. My father was a chemist, educated at Winterthur. There is a Technikum with a section of chemistry, where he studied. He had many friends, chemists too, and he saw them each year, and they all were very happy to come together.

KOEPPEL: What was your father's first name?

SIGNER: Jakob, Jack.

KOEPPEL: And your mother, was your mother related in any way with science or was she a housewife?

SIGNER: No. My father married one of many sisters of a man in St. Gallen who built windows, a "glaser". But she died after the birth of the fourth child. And then he was left with his four children, ranging from ages seven to three days old. Soon, he married the sister of his dead wife, and she was my mother.

KOEPPEL: He had more children with his second wife?

SIGNER: I am the only child of the second marriage.

KOEPPEL: So you are the youngest of the family. Did any of your brothers and sisters take up chemistry or anything scientific?

SIGNER: The oldest was a student of chemistry also in Winterthur. And then he was in Krefeld, where a special school for textile studies was available.

KOEPPEL: So he became a textile chemist?

SIGNER: A textile chemist.

KOEPPEL: And then he was active in the company. I see.

SIGNER: Since he died, three or four years ago, his two sons have been running the firm.

KOEPPEL: What was his first name?

SIGNER: Jakob, too.

KOEPPEL: They also called him Jack?

SIGNER: Yes.

KOEPPEL: So you grew up in a lively family, and you must have many memories from your childhood. Your elementary schooling was in Herisau.

SIGNER: That is correct, Herisau. The family played a big role, for each of my brothers had a hobby, and they all were very nice to the youngest. And I saw how they worked in their hobbies. The oldest was building electrical machines, motors and electrical circuits. The next brother who is now still living at eighty-nine, was a gardener, he planted fruits and vegetables and sold them to his mother. Later on, he went into commerce and was many years in India where he built up a big business.

KOEPPEL: But not connected with the textile firm, he did not build up a textile firm.

SIGNER: No. Then the great disaster came in 1929. He lost all his money, and my father had to pay a good part of his own money to help him.

KOEPPEL: What was the great disaster?

SIGNER: The great Depression.

KOEPPEL: The Depression in 1929.

SIGNER: Yes. The next son, my next brother was interested in collecting stamps. And so I saw how each one pursued very intensively his hobby.

KOEPPEL: It was a very stimulating environment, I would say, for you.

SIGNER: Yes, I saw so many different things, and all, in my eyes, very agreeable and pleasant.

KOEPPEL: So you must have been a good student, in school, which was of course easy in elementary school.

SIGNER: At the gymnasium.

KOEPPEL: That means we're going now to the secondary school. Where was that?

SIGNER: St. Gallen. Even the Kanton Appenzell had at that time, not this sort of school. So if one was to be prepared for studies at the university, you had go to St. Gallen from Herisau.

And in our class there were two people who would be the first, one was myself, and the other was the father of the Bundesrätin [Federal Councilor] Elisabeth Kopp, Max Iklé. We always tried to be at the top of the class.

KOEPPEL: You were competitors. That was healthy. Very good. And so you both went to St. Gallen. Did you enter into the first grade at the gymnasium or later, after secondary school?

SIGNER: I was six years in Herisau, and then there was a secondary school, where I was two years. And from there I entered the first class of the technical division of the Kantonsschule.

KOEPPEL: Oh I see, not the gymnasium.

SIGNER: You see the technical division of the Kantonsschule had the same classes as the gymnasium, but no ancient languages. Instead of the old languages we had a lot of mathematics, and physics, chemistry and the principal natural sciences, botany and geology, and so on. So I had, already in St. Gallen, the possibility to develop my mind in the direction of natural sciences. And that was passed on to my son and to his son. They don't like languages. There are so many irregular verbs.

KOEPPEL: Yes, and so you must have had chemistry. You liked it at St. Gallen?

SIGNER: Yes. There was a new professor for chemistry, just coming from the ninth division of the Swiss Federal School at Zürich. That's the division of the teachers in natural sciences. At that time it was the tenth division, and this teacher in St. Gallen told me not to go to the division of chemistry because there were so many students that they didn't get good contact with the professors. He said, "Go to the tenth, the Natural Science Division, where there are two or three students [in chemistry], and they each have a very good contact with the professors." And that was what it was like. I had very fine professors.

KOEPPEL: That was good advice. What was the name of your professor in St. Gallen?

SIGNER: Enz. He died two or three years ago.

KOEPPEL: So you went on to the Swiss Federal Institute of

Technology, and you entered the Division of Natural Sciences. That was in what year?

SIGNER: That was in 1921.

KOEPPEL: In between, we had a world war, did that affect you in any way?

SIGNER: Yes, I was very impressed by this big war. This impression goes on up to now, and oftentimes I ask myself why it is not possible that human beings live friendly and peacefully on this earth, which grows smaller and smaller.

KOEPPEL: Well that was really a consequence of your witnessing this war at a very early age. So now let's go to the ETH [Eidgenössische Technische Hochschule], the Swiss Federal Institute of Technology.

SIGNER: I was very lucky to have wonderful teachers. [Hermann] Staudinger, we will turn back to him many times this afternoon. The physicist Paul Scherrer.

KOEPPEL: Paul Scherrer. Oh, he was there already?

SIGNER: Yes, he was a young professor at that time, and was wonderful in the great number of experiments he made in each lesson. That was a firework.

KOEPPEL: Yeah, he was an exciting teacher wasn't he?

SIGNER: And then there was the old botanist [Carl] Schröter, very famous for his knowledge of plants of the Alps. He wrote a thick book, Die Pflanzen der Alpen(1), and he made excursions and I photographed. I was his so-called Hof photograph [court photographer]. If he saw a very nice plant he would call, "Signer, come and take a picture."

KOEPPEL: Did you develop your own pictures?

SIGNER: Yes, I had made them on glass, diapositives, and so had a nice collection. I have it no more, because now, at my age, I must see to it that not too many things which have only a relation to me are still there when I close my eyes. So it's not more than half a year that I let go of these fine pictures.

It was hard for my heart because I said, "Now a new generation is coming and they have their own interests."

KOEPPEL: Now, at the ETH, you had inorganic chemistry, do you remember who taught it?

SIGNER: Yes, the son of the very famous Treadwell [Frederick P.]. Treadwell was one of the masters known all over the world for analytical chemistry.

KOEPPEL: He was English? [American; ed.]

SIGNER: Yes. At the time when I was there it was his son, the young [William D.] Treadwell, who was very good in analytical chemistry, and he instructed in organic chemistry. Staudinger gave the first lecture of inorganic chemistry, not only in organic chemistry. In one term it was inorganic and the next term organic chemistry, so you see Staudinger had a broad view on chemistry as a whole.

KOEPPEL: Yes, he was very versatile.

SIGNER: But there was not a very good relation between the scientific division of the Swiss Federal Institute of Technology and the technological division, each division seemed to feel more important than the other.

KOEPPEL: Was the technology section chemical engineering?

SIGNER: Chemical Engineering.

KOEPPEL: And who was there, was it Fierz?

SIGNER: Eduard Fierz [Hans E. Fierz-David].

KOEPPEL: And August Guyer, was he there?

SIGNER: Guyer came later.

KOEPPEL: Fierz was quite famous for his dye chemistry.

SIGNER: He was a very strong personality. As well as Staudinger.

KOEPPEL: But you had no interest in going into dye chemistry, although you would have had some relationship to it through your textile background.

SIGNER: I had no intention to go back to join the firm after my studies because there was my older brother, who was especially educated to run the firm. But there was a competition for good students between the good professors. [Paul] Niggli asked me whether I would join his department as an assistant, and take mineralogy. But then it was chemistry which had the higher attraction for me.

KOEPPEL: I wanted to ask you who taught mineralogy, but you have answered my question: it was Niggli.

SIGNER: Paul Niggli, yes, and his son is now professor of mineralogy at Berne.

KOEPPEL: From your work I have seen that you have quite an interest in mineralogy. So you must have concentrated quite a bit on this aspect during your studies.

SIGNER: The lectures of Niggli were fascinating. And I was fascinated by these Raumgroups [spatial groups], by this wonderful geometry and symmetry, the mathematics of crystals. So when I got the Rockefeller fellowship I chose to go to Manchester for six months, where the young [William L.] Bragg was the professor of physics. He got the Nobel Prize at twenty-five together with his father [William H.]. Both got, if I am right in my memory, the Nobel Prize for applying the X-ray method for determining the crystal structure of organic materials [in 1915: ed].

KOEPPEL: So you worked with the younger Bragg. But we're jumping ahead a little bit. We are still in 1921 to 1925, at the Swiss Federal Institute of Technology, and I would like to hear a little more about your relationship with Staudinger. Your Ph.D. work was done with Staudinger.

SIGNER: I started my thesis on polyformaldehyde at Zürich and shortly afterwards Staudinger was asked to be the main professor of chemistry in Freiburg. And then, having made half the thesis, I went with Staudinger to Freiburg and I finished the thesis there, and had my examination at Zürich, by Professor Emil Ott.

KOEPPEL: Oh, I see, you got your degree from Zürich but you did your doctoral work in Freiburg.

SIGNER: The examiners were Richard Kuhn, the successor of Staudinger, and co-referent Fierz. They took my oral examination.

KOEPPEL: Was that a difficult situation for you?

SIGNER: Staudinger said I should not prepare anything, as they will only question me in relation to my work, and so it was. So it was very easy. It was more a formality than an examination.

KOEPPEL: Do you know why Staudinger preferred to go to Freiburg, instead of staying at The Swiss Federal Institute of Technology?

SIGNER: Yes. The reason was that the two divisions, the industrial and the scientific had a very poor relationship.

KOEPPEL: It was a personality conflict?

SIGNER: Yes, and in Freiburg he was the chief of the whole chemistry. So that was in accordance with his intentions. He was a man with enormous energy.

KOEPPEL: Did you like him?

SIGNER: Oh yes. I admired him. He did very much for me. He was very content with me. My wife was not so happy with him. He was very busy running the institute, all morning.

KOEPPEL: Are we talking about Freiburg now?

SIGNER: Yes. Then between twelve and half past one he finished that work, and then he came to my small place at the Institute and said, "Oh, Herr Signer, jetzt haben wir Zeit. Jetzt wollen wir etwas uber Wissenschaft diskutieren." [Oh Mr. Signer, now we have the time. Now we want to discuss a little science.] And that went on until two or three, whilst at home my wife was waiting for me and keeping the meal warm. I had very many fine discussions with Staudinger.

KOEPPEL: Did you talk a lot about polymer chemistry, I guess

that was his main concern in those days?

SIGNER: Now and then. From time to time he remembered the nice times we had in Switzerland. And then there was the great change in Germany, when Hitler took power. And then one had to walk -- all the Institute, Staudinger and the assistants and the men who cleaned -- all together had to walk to a big arena, where thousands of people had to attend the speech of the Fuehrer. Then Staudinger said to me, "Herr Signer, wir nehmen das als Sonnenbad."

KOEPPEL: "We take this as a sun bath," very good, yes. But that was only in the early thirties, and Hitler was already so strong, his influence was so strong then?

SIGNER: I left in 1935. I had to leave Freiburg because my son Peter, and the other boy who was born at that time, would already stand by the street, and when people walked along they lifted an arm and said, "Heil Hitler." At that time my wife and I decided that we would have to leave.

KOEPPEL: So you were married at that time. When did you get married?

SIGNER: That was in 1928.

KOEPPEL: I see, and that was still in Zürich?

SIGNER: That was in Freiburg. I changed from Zürich to Freiburg in 1927.

KOEPPEL: Was your wife German?

SIGNER: No, I knew her during my studies in Zürich. We were in the same dance class and then, we made a life together, and we are now 58 years married.

KOEPPEL: And what was her maiden name?

SIGNER: Margarethe Meiers.

KOEPPEL: From Zürich?

SIGNER: From Rueschlikon on the Zürichsee [the lake of Zürich].

KOEPPEL: I would like to know a little more about Staudinger at the ETH. Who else was there? Was Leopold Ruzicka there?

SIGNER: Ruzicka was there, but then he had not an important position in the Institute. He was a hard-working man. He always wore a green Gärtnerschürze [gardener's apron], and carried a very big Scheidetrichter [separatory funnel]. In his very small room, where he worked with two or three doctorands [doctoral candidates], he was very active in performing experiments himself, in contrast to Staudinger who had no time to do experiments himself. But Staudinger had a great number of students working on their thesis and that was fine with me. Because later on, when I came up with the idea of streaming birefringence, I had so many different high polymer substances that I was in a very fine position, with so many materials, to get a good insight into the new method. [Tadeus] Reichstein was no longer in Zürich at that time. He had done an enormous amount of work together with Staudinger. Staudinger tried to extract the products which make up the aroma of coffee. And he, Reichstein and many students worked in this field for years. But then it was shown that by roasting coffee at a high temperature the molecules break down, and what makes the taste is a number of ten to twenty different things. So it's absolutely impossible to make synthetic coffee.

KOEPPEL: Synthetic coffee aroma, yes.

SIGNER: That was the great work which Reichstein did under Staudinger.

KOEPPEL: Now you went to Freiburg with Staudinger, and you discussed his theories, and you must have witnessed the controversy around Staudinger. His theories were attacked, and there must have been great concern on his side. Can you tell me a little about this?

SIGNER: Yes, that was very interesting to see how intensely Staudinger was attacked when he came out with his idea of macromolecules. Now, every student or pupil with secondary schooling knows about nucleic acids and it's easy to understand that there are big molecules. I told you that I was interested in philosophy, and I read, when I was sixteen or seventeen, the works of Kant and Schopenhauer. And in the introduction to his main work, Die Welt als Wille und Vorstellung, Schopenhauer says "Eine grosse Wahrheit kann nur ein kurzes Siegesfest feiern zwischen den langen Zeiträumen in der sie als absurd verlacht und als trivial geringgeschätzt wird."(2)

[END OF TAPE, SIDE 1]

KOEPPEL: So you think that Schopenhauer's ideas relate to Staudinger's situation.

SIGNER: It's only one thought, one idea, of Schopenhauer, it's not his principal idea. He says it with respect to his own work. Nobody was reading his main work "Die Welt als Wille und Vorstellung". And the publisher could do nothing better than to "einstampfen, die ganze Auflage." [Scrap the whole edition.]

KOEPPEL: Oh I see, he had to can it [take it off the market] because he couldn't sell it. I can see that your philosophical background, your interest in philosophy, comes through in your approach to science, which is something very valuable. There's a lot of depth.

SIGNER: When I heard that you were coming, I read all my works, out of a long series, and decided that they are perhaps of some value. I had the idea that, [in] each work, [it] was very well founded. All I did was sedimentation of macromolecules, flow birefringence, but it's very complete and very detailed in publication. And that's perhaps what I gained from philosophy.

KOEPPEL: Now we're gradually getting to your work. The influence of Staudinger is obviously very strong in the beginning. Could you relate the start of your professional work, with what you did for Staudinger? Your first publication was on polyoxymethylenes (3).

SIGNER: That was a very fine work for me. There was the idea of all other scientists, that these are particles with a great surface, and there is something absorbed on the surface, so that the content of formaldehyde is not one-hundred percent, but less. And the smaller the particles, the greater the surface, and the greater the difference between the content of formaldehyde and one hundred percent. And Staudinger had the idea that these are long chains of $-CH_2-O-CH_2-$ so on and as end groups, H and OH, the parts of water. Then he said if that is right, one should break these long chains into small pieces of different length. And he had the idea that with acetic acid anhydride he could break this long chain. And the shorter the parts were, the fewer molecules of formaldehyde were between the two parts of acetic anhydride. He told me to take a big quantity of the formaldehyde polymer, heat it with acetic anhydride in Carius tubes to two hundred degrees and then it breaks, then what's coming out is like a slurry.

KOEPPEL: What kind of tubes, excuse me, what did you call them?

SIGNER: The tubes which you can heat in the oven to a high temperature after having them sealed. And then what comes out is something which is not solid and not liquid but is all together. Because it has dozens of different kinds of molecules, and then my problem was to separate this complex mixture, to real unique molecules. And I did it as a "Pedant" with great enthusiasm and really I could isolate pure sharp melting substances, with one, next two, and so on, until twenty-two groups of CH_2, between the end groups of acetic anhydride. And then the wonderful thing was at the same time he worked together with a mineralogist, and he made x-ray pictures and could see the lengths of the molecule because they, like the paraffins, form sheet lattices. You could measure the lengths of all types of molecules. And that was in complete accordance with the number of CH_2 groups, I got out by chemical analysis. So I'm very proud to have given one of the most impressive points to Staudinger's idea by the separation and this breaking down of the particles of the polymer formaldehyde.

KOEPPEL: So he must have been very pleased.

SIGNER: He was very pleased.

KOEPPEL: You published this work in two different chapters (3,4), and it was one of your earliest publications with Staudinger, and then you also habilitated [habilitate: qualify for a teaching position at a German University; ed.] at the University, you started teaching. What was your position then?

SIGNER: Staudinger was very pleased that I hit on the idea of flow birefringence, and his teacher [D.] Vorländer was the physical chemist, with whom Staudinger studied. He had studied the flow birefringence of low molecular weight materials, such as stearic acid or palmitic acid in solution, or molten, and he saw that the longer the molecule, the stronger the flow birefringence. Then I said to Staudinger that if he were right, all the polystyrene and cellulose derivatives should all show streaming birefringence. Then I built an apparatus to measure it, and it was a second new proof of Staudinger's idea. And I was so lucky, to have so many different materials for this investigation of flow birefringence because Staudinger had ten or fifteen students, each working on another sort of polymer. I could get material, to study in this streaming birefringence apparatus and, in a very short time, showed a wonderful accordance with Staudinger's work. And then if other young people had the idea to habilitate, he said that they must do like Signer, make something new with a bottle of liquid. So he was very pleased about my work, and support of his theories. For that he obtained the Rockefeller Fellowship for me.

KOEPPEL: I see, that's how you got it.

SIGNER: Yes, because the fight was in 1932 or 1933. That was already in the period where the idea of macromolecules had a good root and all people not too closed in their minds saw then that Staudinger was right. And in this connection I will tell you about a discussion after a great lecture which Staudinger gave in Düsseldorf, before the Gesellschaft Deutscher Naturforscher und Ärzte (5). They meet once each year, and one of the leading scientists is called on to give the main lecture. So Staudinger very enthusiastically told of his idea of the macromolecules. Old [Richard M.] Willstätter was present and said, "I am not absolutely convinced now that this idea of Staudinger is right, but if he is right, he opens to chemistry a field which is much bigger than all organic chemistry."

KOEPPEL: That's great; Willstätter said that?

SIGNER: Yes, at a time when most of the chemists went against this idea of macromolecules.

KOEPPEL: That was a lot of support for his ideas. At least he opened the door to the acceptance of Staudinger's theories. Do you remember what year that was approximately?

SIGNER: I cannot say directly, but it was when I was not very long in Freiburg.

KOEPPEL: Because in 1933, you went to Uppsala on your Rockefeller Fellowship, and wasn't Staudinger reluctant to let you go?

SIGNER: No, he knew that it was only for a short time. And he told me that he would like me to come back to Freiburg after this period.

KOEPPEL: Which you did.

SIGNER: Which I did, yes.

KOEPPEL: Could you tell us a little bit about your work with The Svedberg, which was of great influence on your own development.

SIGNER: It was a wonderful time, there existed only one ultracentrifuge in the world, that was Svedberg's in Uppsala. There you met people from all the world coming with special proteins to determine the molecular weight by sedimentation in

the ultracentrifuge. People from the United States, from London, from everywhere. This ultracentrifuge was used so intensively that at the beginning of the week one of his assistants made a plan, Monday between midnight and four in the morning that's for MacFarlane, from 4 to eight that centrifuge is for Signer. And so all through the week.

KOEPPEL: You had to take turns. Did Linus Pauling come over with some problems?

SIGNER: No, his time came later. Svedberg's ultracentrifuge was perhaps a little time before, yes.

KOEPPEL: He got the Nobel Prize in 1926, so he already was very famous, when you studied with him.

SIGNER: He got the Nobel Prize as a young man, I don't exactly know how old he was, but I don't think he was thirty. [forty two; ed.]

KOEPPEL: Now, much of your work really centers on determining molecular weights of high polymers and this is where you used the ultracentrifuge.

SIGNER: Yes, and the work in Uppsala was very difficult, because when we came with our substances, only soluble in organic solvents, the ultracentrifuge could not be used at first, because it was constructed for aqueous solutions.

KOEPPEL: Why was it difficult?

SIGNER: The cell in which the solution was placed is closed by quartz windows on both sides. You measure the sedimentation velocity using the rotation of the rotor at a thousand times a second, by illumination with ultraviolet light through the quartz windows. But between the quartz windows, and the middle part containing the solution, was a very thin sheet of India rubber.

KOEPPEL: I see, so that was attacked by organic solvents.

SIGNER: Yes. When I came to Uppsala I needed to construct a film which was insoluble in organic solvents, but had the mechanical properties of a thin sheet of India rubber. And for that I could fall back on the experience gained from Staudinger. I made a very thin film of polyacrylic acid with a plasticizer of

glycerin. I made it in a dish over mercury by evaporating aqueous solution of these two components. At the end there was on the mercury a wonderful uniform film of this new material, polyacrylic acid, plasticized to the desired degree with glycerin. We made a small sheet which was put between the quartz windows and the central cell. It took four months before we could make the first sedimentation measurements with our substances.

KOEPPEL: Oh, and you only had six months.

SIGNER: In two months we worked day and night, the details of which may be seen now in three publications: three works on the sedimentation of macromolecules (6-8). Svedberg was really pleased and he said to me, "No one of the many people who have come to this place to use the ultracentrifuge became so quickly acquainted with all the details. I think I keep you here, come to Uppsala and work with us for all your life."

KOEPPEL: And you didn't want to?

SIGNER: I preferred to go back to Staudinger, because one knew that in Sweden it was very difficult for a non-Swedish man to get into the higher positions. They had the very good practice to look out for their own people. And Staudinger had said to me beforehand, "Come back to Freiburg." So I went back to Freiburg, and there I had the idea of the streaming birefringence, and that, Staudinger said, "That's the way our people, who should get to the venia docendi [permission to teach; habilitation], must open something new, as Signer has done."

KOEPPEL: That was really a demanding precedent. But then you went to Manchester first, before you got back to Freiburg, and you stayed six months in Manchester with Bragg.

SIGNER: Six months. There are some publications in quite another field. I had my idea what I wanted to do with Bragg, but then the professor of physical chemistry in Freiburg, [György de] Hevesy, a very well-known man and, later [1943], a Nobel Prize winner, said, "If you go to another place don't come with your ideas, go and see what is being done there and what you can learn there." So it was clear to me that when I went to Manchester I would see what Bragg would propose for me. Just at that time a very important work of one of his students had been completed on hetero poly-acids. Bragg suggested that, as there are other hetero poly-acids, I could, in this half year, see whether they have analogous structures. So, in Manchester I had the chance to work on hetero poly-acids. A student of Professor Bragg had just evaluated the constitution of these very complicated

inorganic substances (9). We could see from the Debye-Scherrer diagrams that other hetero poly-acids had just the same structure. The different hetero poly-acids gave Debye-Scherrer diagrams with almost exactly the same lines, with the same intensity. So in a very short time, only a few months, we could see that all these hetero poly-acids have analogous, if very complicated, structures.

KOEPPEL: So this had an effect on your later approach, when you also worked with X-ray analysis.

SIGNER: No, that was the only work in this field.

KOEPPEL: Oh I see, that had no bearing on the molecular structure of macromolecules.

SIGNER: No. We have been very interested in the arrangement of chain molecules in solids. That comes in another publication (10).

KOEPPEL: Yes, let's talk about Bragg. Do you remember something interesting you could tell us?

SIGNER: He was very impressive, he was a Nobel Prize man, famous for ...

KOEPPEL: Are we talking about the younger Bragg?

SIGNER: The younger Bragg, yes. When I came to his office, it was as if he would greet a very old friend. Such a friendly atmosphere. Such an important man, well known all over the world, that made a very great impression on me. And the whole time when we were there, my wife and I, we always had the impression with these English people you are like old friends.

KOEPPEL: And that's where you learned your excellent English! Was [Francis] Crick there, or wasn't he there yet?

SIGNER: No, that was later. There was once a colloquium of the Solvay Congress in Brussels, and I was invited. There Bragg greeted me and said, "Oh, the double helix was evaluated from the material you have isolated in Berne."

KOEPPEL: Oh wonderful, your thymonucleic acid.

SIGNER: It was perhaps in 1940 or 1942. Two years after I started my work in Berne, [Torbjoerr O.] Caspersson, a Swedish colleague, came and said that his master [Einar] Hammarsten had the idea that the molecular weight of nucleic acid which they had prepared could best be found by flow birefringence. In half a day Caspersson and I knew the molecular weight, and the next day we wrote the small publication which is in the...

KOEPPEL: It was published in Nature in 1938. "The Molecular Shape and Size of Thymonucleic Acid," by Signer and Caspersson (11). So you consider this your most important publication?

SIGNER: Yes, from this time on I saw that nucleic acid was one of the biologically most important polymers. This led me to undertake student dissertations including the preparation of nucleic acids. About eight years later there was a symposium in London, organized by the Ciba Company in Basel, and I talked about improvements in the preparation of nucleic acid. Not only did I talk about, I brought a bottle of this size [Signer demonstrates], of finely prepared nucleic acid, and I gave it to the director of the symposium, Dr. [John A. V.] Butler, and he distributed the nucleic acid to different institutions. One part went to Cambridge, Massachusetts to Doty, who measured the light scattering (12), and one part came to Cambridge, England where Watson and Crick evaluated the double helix with a Bernese nucleic acid (13). And then I was, as I heard later, proposed for the Nobel Prize but it was not enough.

KOEPPEL: But you were nominated for the Nobel Prize?

SIGNER: I heard that I was nominated but I don't know from which side. Staudinger, when he came to Berne, was always interested in what I was doing, and he would tell other pupils that Signer always makes new things. That's my great fault, if I would have worked one invention, it would have made more of an impression.

[END OF TAPE, SIDE 2]

SIGNER: When I did the birefringence of this thymonucleic acid prepared in Stockholm by Hammarsten and Caspersson, I saw that this nucleic acid was one of the most important biological substances and then I tried to isolate it from calf thymus, because it has a high concentration of thymonucleic acid, as native as possible without degradation. In [Adolf] Knapp's dissertation we came one step further(14).

KOEPPEL: So he was a student of yours and he did his thesis with you.

SIGNER: And another student [H.] Schwander continued the work of Knapp, and the result was this. [Signer points to fig. 2 of note 15] That's the viscosity of the material which came from Stockholm, Caspersson and Hammarsten's, from which you always knew that it was high molecular by the flow birefringence. This work of Knapp and Schwander gave the material with a much higher viscosity, that means with much more native molecules. I took to the London symposium a bottle with fifteen grams of this material. And this material was distributed by the man who arranged this colloquium, Dr. Butler, and he sent one part to Cambridge where Watson and Crick discovered the double helix with the material from Berne.

KOEPPEL: So you actually played a role, in a way, in the double helix structure.

SIGNER: Yes, there is an English botanist who came to Berne, and asked me all these details, and he wrote an article, I don't know in which journal it came out, with the title "Before The Double Helix." And in this, you'll find all that I explained just now. But in Berne it was tedious work, to prepare the nucleic acid without degrading the molecules.

KOEPPEL: You also had some interesting speculation there about the arrangement of the purines and pyrimidines in a communication in Nature, in English, a communication on the molecular shapes and size of thymonucleic acid (11).

SIGNER: In this first publication you already see the idea that the purine and pyrimidines rings must lie perpendicular to the long axis of the molecule, because it showed negative double refraction, which means the main polarizability of the arrangement of atoms lies perpendicular to the length of the molecule.

KOEPPEL: I think we should go back. We have jumped ahead a bit, because you were in Manchester, now you went back to Freiburg, and then you went to Berne. So could you tell me about that period when you were ready to leave Freiburg.

SIGNER: I told you already that it was the behavior of the sons which made the Signer family leave Germany.

KOEPPEL: Now you were married, and you had how many children?

SIGNER: Peter and the next younger, Dieter. Then we decided to get out of Germany and I went to the director of Ciba, Dr. G.

Engi, and asked whether they had a position for me. And he said, "Certainly." I had already a contract with Ciba, when we found a house in Riehen, and then we moved with our possessions to Riehen. Then came a telephone call from Berne, from the professor of inorganic chemistry, [Volkmar] Kohlschütter, he was a friend of Staudinger, and Kohlschütter told me, "You must come to Berne, you are proposed as the professor of organic chemistry, and the people in the government would like to make your acquaintance." I came to Berne and after a short time it was decided that I could take this position. Then I had to go to Ciba at Basel, and tell them that I could not come. Then Staudinger said that was very nice. If you want to ride in a car, you should go to Basel, but if it doesn't trouble you to go by foot, then go to Berne. In other words, I could be a wealthy man in Basel, but must be content with a small salary as a professor [in Berne]. My wife said that there was no doubt, I was not the man for big industry. I took the good advice from my wife. I was very lucky.

KOEPPEL: You have not regretted it, have you?

SIGNER: Never, I very enthusiastically introduced all students of medicine into organic chemistry. They had, in the first two terms, inorganic chemistry, organic chemistry, botany, and zoology. And very often, when I came to any place, maybe Chur, with one of my injured sons at eleven or twelve at night, when the doctor arrived he said, "Oh, I know you. You were my teacher in organic chemistry."

KOEPPEL: Well, that's wonderful. And [in Berne] you then started as an associate professor, and very soon became a full professor.

SIGNER: First in charge of the whole institution, inorganic and organic together, and with time, inorganic developed, and we decided that it should be two institutions, each with a director.

KOEPPEL: So you succeeded Fritz Ephraim as professor of organic chemistry, in 1937. You started in 1935. In 1933 you were in England. You went back to Freiburg, then in 1935 to Berne where and in 1937 you became a full professor, and in 1939 you became the director of the chemical institute, and chairman of organic chemistry. And the institute was then split, you say, into organic and inorganic, during the war. And how did the war affect the institute or your work, did it have any bearing or any effect?

SIGNER: I was a soldier of the Swiss Army. And I had to spend two months as a soldier, and I had Urlaub [leave] for one month

and then I came to the institute and looked what problems had piled up. After several periods of military service I got a heart attack, because it was too much to do the military service and then one month go to the institute and see what happens there. Our doctor said, "Now you have a nice house and garden. For a half year you don't go to the institute and you will cure your heart." And I had no more difficulties since then.

KOEPPEL: And you had no more military service, I'm sure you were discharged.

SIGNER: Yes, the only effect of World War II was that it meant too much work.

KOEPPEL: No other problems, like shortage of materials or anything that would have had a direct bearing on your work?

SIGNER: No, things went on in the institute normally. There were very few students, because most of them had to do military service. So there was not that much work.

KOEPPEL: It was a very disruptive period because the students also had to interrupt their work. It's been very difficult.

SIGNER: Yes.

KOEPPEL: Well I think we could talk a little more about your work. The compounds, you worked on were polystyrenes, cellulose derivatives, proteins and later again polyoxymethylenes, so what can we discuss here?

SIGNER: I worked out a method of separating molecules by different diffusion coefficients, and that was intensive work, for ten years, and the man who could best see what work was done was Harold Urey, from the States. He came to Switzerland, visited different universities, and I showed him my apparatus for separating organic molecules by different diffusion coefficients. He was very interested, and told me about his work. He developed the method of separating the uranium hexafluorides for the Atomic Bomb. And he said this was the greatest industrial complex which ever existed in the United States, at the time, because it needs an enormous number of membranes to separate these uranium hexafluorides where the uranium atoms differed less than one percent in the molecular weight. He was extremely interested to see that a man in a small university in the small country of Switzerland took up the same problem of separating the molecules of different molecular weights by diffusion.

KOEPPEL: You have been quite seminal in many fields.

SIGNER: Staudinger always said, "Signer macht immer wieder etwas Neues." [Signer is always doing something new.]

KOEPPEL: To go back to flow birefringence, you developed an apparatus?

SIGNER: Yes, that was nothing new, because Vorländer had done this thing in 1925, for low molecular weight materials, as a paraffins or acids like palmitic and stearic acid. And when I saw this publication then I said to myself, and to Staudinger, your chain molecules must give a very intensive flow birefringence. Then we measured these different substances with different molecular weights, and developed very nicely all the rules, which govern the influence of concentration of the dissolved material: of the constitution of the chain molecules, of molecular weight, and all that is in the first publication to be found (16).

KOEPPEL: That was the one with Signer and Gross (17). Actually there are a great number of papers on high polymers. It's a whole series -- this was the eighty-third paper on macromolecules, as far as I can see. But that was back in 1933, that's still from your Freiburg days.

SIGNER: It was in the numeration of Staudinger.

KOEPPEL: This relates to the Staudinger papers. And you published with Gross. You published in Zeitschrift für Physikalische Chemie, eighty-third, and then we have the eighty-seventh paper, that was on sedimentation rate, that was in Helvetica (6), but that was a paper originating from your Uppsala work. So we're talking about a whole series of papers. Then we have a ninety-first paper that deals again with the ultra-centrifuge (7). And then you started concentrating on thymonucleic acids in 1938. And then from 1938 to 1948 you worked mainly on thymonucleic acids.

SIGNER: With these two collaborators, Knapp and Schwander. And this was the material for the colloquium in London, where I brought the material which gave the double helix by x-ray.

KOEPPEL: And then your interest shifted somewhat to proteins and mainly casein. We're jumping ahead a little bit, now that's in 1960, when you published on proteins and casein (18).

SIGNER: Yes that is a big field. The influence of electrolytes like sodium chloride on the water sorption of proteins. The basic idea was that eighty-five or ninety percent of oneself is water. So the relation of water to all my molecules must be very important. If Gandhi started a hunger strike, he drank each day normally his tea, that means even if one will die from a hunger strike, and no longer eats, one drinks a very long time, showing that of all effects in the body the solvent is very important. And then I said let us investigate the influence of different things, of the water sorption of proteins, and then I was lucky again. A young man from Hungary, Stefan Gal, who fled Hungary for political reasons, emigrated from Hungary, to Berne, and was employed by a firm in Gümligen. The chief of this firm was a student colleague of mine, Gottlieb Lüscher, some years older than me and he's also a great friend of Reichstein. When I was in Berne, he came to me and said, "I have an idea that it is very useful for an industrial factory to have good contact with the university, therefore I work together with Reichstein, and I would like to work together with you, and the way we do it is, that you can do what you like, and what it costs will be paid by us. If there is something that brings money by a patent, then this firm of which I am the director will take over the patent, and what is coming out." And so I had no difficulty with money.

KOEPPEL: So you always got the funding from them.

SIGNER: I always could say I'd like to investigate this, that, and I could do what I like.

KOEPPEL: And did anything come out of it, in a practical way, on polymers?

SIGNER: No. This Hungarian man is now one of the directors of the firm, and he worked in this field: influence of electrolytes on proteins, on the water sorption, and the drying of different materials, coffee and cabbage, whatever, that's one of the problems of this firm. So the whole thing I could do for the firm was to work with this young Hungarian man. He saw many things which were useful for the firm, and he's now one of the directors.

KOEPPEL: I see. But then you did work on casein, that was one of your main fields. Were you ever interested in fibers, for commercial purposes?

SIGNER: Yes, from this direction, from this company came the question, would you not try to make a protein fiber? And then I said why not. And we developed a dry spinning procedure. The normal preparation of viscose is that the solution of cellulose

derivatives is put through a fine hole into a non-solvent and in Italy there was the Lanital which was also a wet spinning method. We tried to develop a dry spinning method, where a very concentrated casein-water mixture was extruded in a long channel and where warm air was streaming against it, and then at the end you could wind up a dry spun fiber. That came to production of twenty to thirty kilo per year, several people had already this dry-spun fiber clothing.

KOEPPEL: Well, I heard that you came to your lectures with a suit or some pants or something spun of that casein, and your students really got a great kick out of that.

SIGNER: Yes, and I had a very nice contact with a man from the United States who was the most important man in that field; Dr.[Hale] Charch of the Du Pont Company. And I gave a lecture on the body comfort of fibers.

KOEPPEL: In America?

SIGNER: That was somewhere in Europe, I think it was Strasbourg. And I did a small publication about water transport in fiber packings (19), and there I could show that silk and wool and our casein fiber, had a very low transport of humidity through the packing of fibers. And all materials which you feel cold, had a much quicker transport of water. And then Dr. Charch said, "What we do in the United States about body comfort of fiber with very very complicated apparatus, you do in the test tube."

KOEPPEL: Wonderful, and the casein fiber, was it like wool?

SIGNER: Yes, in handle, but if it was wet then the tensile strength was very low.

KOEPPEL: I see. So it never developed commercially. It was not successful.

SIGNER: We hoped by dry spinning that the packing of the molecules would be denser than by wet spinning, and we combined it in the hope that the strength could be higher. The only positive thing was that in keeping warm, it was like wool or silk, and much better than cotton, or artificial viscose. And there are several patents in this field how to prepare the casein to be able to make the dry spinning process (20).

KOEPPEL: You have a number of patents.

SIGNER: These separators for separating molecules by diffusion, that gave a patent too (21). But then I saw that the diffusion coefficient goes with the third root of the molecular weight. So you need enormous differences in molecular weights to have enough difference in diffusion. And from that moment I looked for other methods of separating materials -- and Staudinger once more said, "Signer is always making something new," -- an apparatus for separating molecules by counter-current distribution (22). And there is another project I'd like to show you, because it is continued by the professor of chemistry in Basel who is the successor of Reichstein.

KOEPPEL: What's his name?

SIGNER: Max Brenner. And he works now with this principle which I just would like to explain to you.

[END OF TAPE SIDE 3]

KOEPPEL: Now we're talking about the lecture that you gave before the French Chemical Society in 1949 (10).

SIGNER: "L'arrangement des macromolecules dans les solides." The Arrangement of Macromolecules in Solids.

KOEPPEL: It was published in the Bulletin de la Société Chimique de France.

SIGNER: We were very interested in using the electron microscope, and then at the same time I was asked by a Swiss firm working in textiles whether one could see something about the structure of fibers by the electron microscope. And we specialized in reproduction of the surface of solids, especially fibers, and then what came out looks like this. That's cellulose with a string of macromolecules, and that's wool with this very interesting surface. [Signer refers to fig. 4 and 12 of note 10]

KOEPPEL: So there's a difference here between the cellulose and the wool.

SIGNER: Yes.

KOEPPEL: And what causes this difference? Is it in the crosslinks?

SIGNER: If the cellulose is clear there is a motion of the

material inside the cell in which the macromolecules are dissolved. And there is a spiral-like arrangement, which can be seen.

KOEPPEL: You're talking about these regions of crystallized material which are called crystallites and that's why you can actually use the material.

SIGNER: Yes. There was an old observation that acetylated cellulose, different products, gave a flow birefringence like this [Signer sketches a curve]. That means at low flow gradients one had already a very big birefringence which increased only very little with increasing flow velocity.

KOEPPEL: Yes, we are talking about about fig. 2 here in this publication (10).

SIGNER: My idea was that to produce such a big birefringence by small flow rates, there must be big particles, micelles of the cellulose, which are not dissolved into molecules. Other acetyl-celluloses gave this normal increase of the streaming birefringence with the gradient. Then by electron microscopy one saw that this material gave a picture like this, [fig. 4, note 10] and this one where the molecules are dissolved gave in the same concentration in the same manner of preparation, such a film without these big particles [fig. 3, note 10]. So it was very good proof that our idea from birefringence, that there must be big molecules which orientate easily, was right. That's what I would like to say about electron microscopy, which took years until we had developed the method which could be used.

KOEPPEL: So, in other words, the cellulose is organized into domains, you call them domains, and therefore you can see the structure.

SIGNER: Yes. And now the work of the counter-current distribution.

KOEPPEL: Is that the Dechema publication, the laboratory apparatus which you developed (23)?

SIGNER: This apparatus is now applied by Dr. Brenner. He was earlier interested in the synthesis of peptides. He told me just two months ago when he visited me that he works now in this field, which I created, with high intensity. They come together each year, once in Europe and once in the States, all the people interested in peptides and proteins. He works now on this

apparatus, where two currents flow one against the other, you see that it comes in here and goes out there and that's the lighter fluid which goes in this direction and you can introduce by this tube [Signer refers to fig. 2 of note 23] the mixture at any place of the whole apparatus. He says now his apparatus according to this principle is able to separate the mixture of many proteins because that part of the protein more soluble in one solvent goes in one direction and the other in the other direction and you can choose the velocities of the two solvents so that the wanted protein stays in the middle. It spreads very slowly over the apparatus and all other proteins are already out of that mixture.

KOEPPEL: You can actually separate one out of a mixture of many.

SIGNER: Not an enormous quantity, only the quantity which at the end fills the whole apparatus. But he thinks that this is a very important method to separate one protein which only differs a little bit from the others.

KOEPPEL: Do they call it the Signer Apparatus?

SIGNER: I asked him and he said no, but he will do it.

KOEPPEL: You will call it the Signer Apparatus! That publication was in 1956, and maybe you now want to talk about esterification of cellulose, but that was really in 1950, in "Monatshefte für Chemie." (24)

SIGNER: Yes, that's what I showed you. In the electron microscope where one has in one preparation only the molecules and the other the molecules with the smaller quantity of bigger particles [fig. 6 of note 24].

KOEPPEL: I see. This is an application of the principle of electron microscopy and streaming birefringence.

SIGNER: And then one of the newest is... Here is another. Staudinger was right, I could always do new things, polystyrenes can dissolve in some solvents, with a high viscosity, and with other solvents with a low viscosity, the same material, and the idea is that in the solvents in which the viscosity is high, that solvent penetrates into these clouds of molecules very intensively. And in the other solvents it's just able to dissolve, but the molecules are clustered and then I had an idea that these two types of solvent should have quite different thermodynamical interactions with the small fragments of the

molecules. We took, as an example, ethylbenzene which is the smallest repeat unit of polystyrene, and a good solvent which dissolves the polystyrene with a high viscosity, is toluene which is chemically related with to ethylbenzene. And a bad solvent which makes for low viscosity is methyl ethyl ketone. And then I gave my colleague Arm the idea that he should do an exact thermodynamic investigation of mixtures of ethylbenzene with toluene as a good solvent, methyl ethyl ketone as a bad solvent. And that is what he measured, mixing heats and vapor pressures over the whole range of composition, and the result is here.

KOEPPEL: Now we're talking about a publication on "Thermodynamic Measurements on Mixtures of Ethylbenzene and Toluene and Methyl Ethyl Ketone," published in Helvetica Chimica Acta in 1957 (25). It's dedicated to Professor Reichstein on his sixtieth birthday.

SIGNER: That's possible.

KOEPPEL: That's what it says. Do you want to talk about this last phase diagram [fig. 1 of note 25]?

SIGNER: The mixtures of ethyl benzene and methyl ethyl ketone from zero to one hundred percent give very extreme effects in free energy [delta G], and in entropy [T delta S]. The same diagram for the other combination, of ethylbenzene with toluene, would give all three quantities very near the zero line. So you see that ethylbenzene shows very different behavior with toluene and with methyl ethyl ketone, so that the difference in the viscosity of polystyrene, in these two solvents goes, for the part, back to the thermodynamic interactions of the small fragments with the two different solvents.

KOEPPEL: I see. Well, that's very interesting.

SIGNER: One of my colleagues, Arm, now goes on in this direction.

KOEPPEL: So actually we have now reached the point where we can talk about your retirement from the University, in 1972 and...

SIGNER: There is only one more thing. I would like to talk about this colleague coming from Hungary, who is now director of the Haco Gesellschaft, his name is Stefan Gal.

KOEPPEL: Did we talk about this earlier in the casein story?

SIGNER: Yes, and here you have the graph [fig. 2 of note 18]. That's the water vapor sorption of pure casein and of casein with mixtures of sodium chloride. And then you see [fig. 1 of note 18], up to a certain quantity of sodium chloride, the water vapor sorption increases very strongly, and then there is no further increase. That's very simple to explain. The casein, like a liquid, is able to dissolve a certain quantity of sodium chloride, and when this quantity is reached and you have a mixture with more sodium chloride, then the sodium chloride is crystalline in the material and has no water-sorption power.

KOEPPEL: I see. Could we talk a little bit about your retirement now, after we have talked extensively about your work. We're going on to 1972, when you gave up your position in Berne, at what age? You were older than 65.

SIGNER: At that time a professor could retire at his will, between sixty-five and seventy. Now there is a new law in Berne, that one has to retire at 65. And the faculty had to replace in the last weeks, all professors between 65 and 70.

KOEPPEL: All at once. That was not a very good idea.

SIGNER: That was an enormous amount of work. I was sixty-nine-and-a-half when I gave the last lecture, and I did it over thirty seven years, each year with the same enthusiasm.

KOEPPEL: And your students must have given you a great round of applause, after your last lecture. Was it difficult to retire?

SIGNER: Not at all, because I had long before my big garden, and I never had the time to do the most urgent work. When I retired I thought I would do some chemical work, in one or the other direction, but then I had so much to see, and to do and enjoy in the garden, that I really left the institution at once.

KOEPPEL: That's good, you had your big hobby, gardening.

SIGNER: That's it, yes. And then I also thought that when I came to Berne nobody gave me direction on how I should instruct the students. And so I think that all my younger colleagues are happy when they can do things the way they want without seeing the old man, who has different ideas. They changed the curriculum very much and I think it was good to change the curriculum, but in my time, I still think it was done not so badly.

KOEPPEL: For your time, and of course when you were there polymer chemistry dominated organic chemistry. I read that after you left it was synthetic organic chemistry that was favored.

SIGNER: Yes, I did not follow during my time all this development of reaction mechanisms, and I think my students got nothing of these ideas, but they got some ideas in the field of macromolecules.

KOEPPEL: Yes, of course.

SIGNER: It is very good that younger men should now teach what young and new organic chemists should know.

KOEPPEL: And I think that's a very wise insight on your part. You have also had some honors. You got the Lavoisier Medal. What year was that, do you remember?

SIGNER: I could perhaps quickly find the medal and you could find the year, I think it was the year when I gave this lecture to the French Chemical Society. And then the Rockefeller Fellowship, something which I can be proud of. Then I got the second Rockefeller Fellowship in 1948, a special fellowship which was to go to the United States to discuss my research with those people who did related work. I had a list of perhaps six professors whom I visited.

KOEPPEL: Could you tell me who you visited, do you remember some, at least.

SIGNER: Yes, Debye was one. The great physicist. I had already a very nice time with him. His collaborator, Harold Scheraga, greeted me and said, "Ah, you are the father of streaming birefringence." And then Debye took me -- let me give you the place where he took me for lunch. There was a part of the university where women learned cooking and household. He took me there and we were served by these people and he said that it is part of the school. About Debye I must tell you something. When he was in Germany, he had many collaborators and then Hitler and his men tried to fire several of them. And Debye said, I think to Hitler himself, "In diesem Haus bin ich der Führer" [In this house I am the Führer].

KOEPPEL: Were they Jewish, that he wanted to fire them?

SIGNER: Yes, Staudinger had several assistants who had a grandmother who was Jewish. That was enough.

KOEPPEL: Oh, it was awful. In addition to Debye, who else did you see in the United States?

SIGNER: In Wisconsin, there was a colleague of Svedberg, John W. Williams who helped to develop the first ultracentrifuge. I had a very nice time with him. And he told me that not far away was a Swiss Colony, New Glarus.

KOEPPEL: Must have been in Madison, University of Wisconsin.

SIGNER: He took me to these Glarus people and I spoke with them in my dialect. And then to Cornell to visit J. B. Sumner, the man who crystallized the first protein.

KOEPPEL: Well it doesn't matter. Did you meet Linus Pauling?

SIGNER: No, Pauling I saw later on.

KOEPPEL: In California or in Europe?

SIGNER: In Europe.

KOEPPEL: And what about Flory or some other big polymer chemist?

SIGNER: Oh, with Flory I had a really nice time. And he showed me the journal, the National Geographical Magazine, and said that it was very good and when I came home I had a subscription! I was several days with Flory.

KOEPPEL: Is there anything especially that you remember about Flory? Did you visit him at home or at work?

SIGNER: At home, and we had many discussions about macro-molecules.

KOEPPEL: Do you remember any other people, even though it was a long time ago!

SIGNER: I got very good advice from the Rockefeller Institute. If you go now from one place to the other, keep first two or three quiet days in the new place before you ring up anybody. The moment you ring them up, they take you out.

KOEPPEL: You're swamped with invitations, and so it happened.

SIGNER: And I came back very happy, and in good health.

KOEPPEL: And it was a great experience!

SIGNER: One more interesting thing. I asked what could I do for the Rockefeller Foundation. They said look for some young men you think are good.

KOEPPEL: At Berne.

SIGNER: And give us, when you are back, two or three names, and that I did.

KOEPPEL: And you did send them some prominent young men?

SIGNER: Yes. And they were very thankful. I had the first contact with the Rockefeller Foundation in Uppsala. The foundation gave The Svedberg a lot of money to develop the ultracentrifuge. During the time I was there it was first a half year, and then I went two or three times later with new substances which I wanted to be investigated in the ultracentrifuge. So I saw there Mr. Teasdale, he was one of the officers of the Rockefeller Foundation who visited Europe from time to time.

KOEPPEL: Did you lecture in the USA?

SIGNER: No, I gave one lecture to the man who worked with viruses, [Max A.] Lauffer, and we invited him to come to Berne, to lecture for half a year. Once he invited me for a lecture about streaming double refraction in his institute, a special part of streaming birefringence. Seeing the polydispersity, that there are molecules of different molecular weight and that you saw here in my different publications, was one of the things I saw with streaming birefringence. And he was very impressed to see how this streaming birefringence shows polydispersity. And he invited me to come to work for his place at the University of Pittsburgh.

KOEPPEL: Well you certainly have had a very rich and varied scientific life, you have over two hundred and fifty publications in at least three languages, in German, French, English -- even in a Finnish journal -- which sounded very interesting.

[END OF TAPE, SIDE 4]

SIGNER: That is to emulate the sedimentation velocity to find out the thickness of the chain molecules.

KOEPPEL: Now, let me maybe mention that you also have a great number of patents on manufactures, procedures, and apparatus, especially on the distribution of mixtures of substances of non-miscible liquids. We have talked about this. So, before we conclude the interview I'd like to ask you another question. You have talked about your versatility, that you do a great number of things as Staudinger said, and I was wondering how you see yourself, do you see yourself as a disciple of Staudinger or a pioneer in physical organic chemistry or what is your idea about yourself.

SIGNER: A man who likes science, and sees different things it would be interesting to see closely. I think it's very difficult to say in which field of chemistry my "Schwerpunkt", my center of gravity, lies. I think that it comes perhaps from my youth, where I was interested in philosophy. If something interested me, I took it and...

KOEPPEL: You went after it, like a true scientist or almost like a renaissance man who is interested in many fields. But it seems that you did everything very well, and in depth.

SIGNER: Your visit brought me back to the study of all these publications, and the reading of each brought me pleasure, because I saw that even now I could not do it better than I did it back then. But I was a lucky man and I had a wonderful chief, Staudinger. We worked optimally together, he with his great ideas, and I as "Pedant", did different things in all details. I had the luck to be able to avail myself of a great number of different substances which were produced in his great number of dissertations.

KOEPPEL: You think you were able to go into details. Pedant: that means you were very thorough with what he lacked the time to do. You more or less did a lot of things for him that he could not pursue, because he did not have the time.

SIGNER: He got his "Professur" at Zürich for the work he did

with ketenes. These are O double bound to C and two valences onto another C [C=C=O]. And he worked on ketenes very intensively, as a chemist, but when he came to Zürich he was only a teacher, he had so much to do with teaching and organizing that he did not have the time to work in the laboratory. People like me enjoyed very much to work at the laboratory bench so that gave a very good...

KOEPPEL: You complemented each other very well. So you would say that he was the biggest influence in your scientific life, Staudinger.

SIGNER: Yes. And I was fascinated by the fight against him, which I could judge from a philosophical point of view. Here was something new and so many people, opponents, were competing for it.

KOEPPEL: Was that in your opinion only scientific or was it against his personality?

SIGNER: No, it was purely scientific. Did I tell you that when he gave the last lecture in Zürich before he went to Freiburg, the famous mineralogist Paul Niggli said in the discussion, "Herr Staudinger, wenn Sie Ihr Polystyrol genügend reinigen würden, würde es crystallisieren wie Naphtalin oder Benzoesäure."

KOEPPEL: Meaning, "Professor Staudinger, if you would purify your polystyrene enough it would crystallize like naphthalene or benzoic acid."

SIGNER: And when he gave his first general lecture in Freiburg his famous colleague with the Nobel Prize, Wieland, afterwards clapped him on the shoulder and said, "Lieber Staudinger, organische Moleküle mit mehr als fünfzig C-Atomen gibt es nicht!"

KOEPPEL: Wieland said, "Dear Staudinger, organic molecules with more than fifty carbon atoms do not exist."

SIGNER: You see that.

KOEPPEL: Well that was his problem, it really was scientific. Well I think this brings us to the conclusion of our interview. Professor Signer I would like to thank you very much on behalf of the Center for History of Chemistry, for your time and your patience. It was most stimulating and interesting to review the story of your scientific life and I appreciate that you have

agreed to this interview.

SIGNER: I thank you very much, your visit gave me the motivation to re-read my papers. Thank you very much.

[END OF INTERVIEW]

NOTES

1. C. Schröter, Das Pflanzenleben der Alpen, eine Schilderung der Hochgebirgsflora (Zürich, Raustein, 1908).

2. "The weightiest knowledge of the truth, to which only a brief triumph is allocated between the two long periods in which it is condemned as paradoxical or disparged as trival." A. Schopenhauer, The World as Will and Idea, translated R. B. Haldane and J. Kemp (Garden City: Doubleday, 1961).

   The original text is: "...in der wichtigsten allerzeit der Wahrheit zuteil ward, der nur ein kurzes Siegesfest beschieden ist zwischen den beiden langen Zeiträumen, wo sie als paradox verdammt und als trivial geringgeschätzt wird."

3. H. Staudinger, H. Johner, R. Signer, G. Mie and J. Hengstenberg, "Der polymere Formaldehyd, ein Model der Cellulose," Zeitschrift für Physikalische Chemie, 126 (1927): 425-448.

4. G. Mie, J. Hengstenbeg, H. Staudinger, H. Johner, M. Lüthy and R. Signer, "Die polymere Formaldehyd, ein Modell der Cellulose," Naturwissenschaften, 15 (1927): 379-380.

5. H. Staudinger, "Die Chemie der hochmolekularen Organischen Stoffe in Sinne der Kekuléschen Strukturlehre," Berichte, 59B (1926): 3019-3043.

6. R. Signer and H. Gross, "Über das Verhalten von Polystyrolen in der Svedberg'schen Sedimentationsgeschwindigkeits-Zentrifuge," Helvetica Chemica Acta 17 (1934): 59-77.

7. R. Signer and H. Gross, "Ultrazentrifugale Molekulargewicht-bestimmungen an synthetischen hochpolymeren Stoffen," Helvetica Chemica Acta, 17 (1934): 335-351.

8. R. Signer and H. Gross, "Ultrazentrifugale Polydispersitäts-bestimmungen an hochpolymeren Stoffen," Helvetica Chemica Acta, 17 (1934): 726-735.

9. W. L. Bragg and J. West, "The Structure of Certain Silicates," Proceedings of the Royal Society, A114 (1927): 450-473.

10. R. Signer, "L'Arrangement des Macro-Molecules dans les Solides," Bulletin de la Société Chimique de France, 5th series, 16 (1949):663-667.

11. R. Signer, T. Caspersson and E. Hammarsten, "Molecular Shape and Size of Thymonucleic Acid," Nature, 141 (1938): 122.

12. M. E. Reichmann, R. Varin and P. M. Doty, "The Molecular Weight and Shape of Desoxypentose Nucleic Acid," Journal of

the American Chemical Society, 74 (1952): 3203-3204.

13. F. H. C. Crick and J. D. Watson, "The Complementary Structure of Deoxyribonucleic Acid," Proceedings of the Royal Society, A223 (1954): 80-96.

14. R. Signer and A. Knapp, "Über den Aufbau der Nukleinsäure bei der Isolierung aus Kalbsthymus," Die Makromolekulare Chemie, 1 (1947): 89-

15. R. Signer and H. Schwander, "Isolierung hochmolekularer Nucleinsäure aus Kalbsthymus," Helvetica Chemica Acta, 32 (1949): 853-859.

16. R. Signer, "Über die Strömungdoppelbrechung der Molekülkolloide," Zeitschrifte für Physikalische Chemie, 150A (1930): 257-284.

17. R. Signer and H. Gross, "Über die Strömungsdoppelbrechung verdünnter Lösung der Molekükolloide," Zeitschrift für Physikalische Chemie, 165A (1933): 161-

18. R. Signer and S. Gal, "Untersuchungen der Bindung von Natrium- und Chlorionen durch Casein mittels Wasserdampf-Sorptionsmessungen," Die Makromolekular Chemie, 44-46 (1960): 259-268.

19. R. Signer and M. Roth, "Ein Einfaches Verfahren zur Messung der Wasserverdunstung durch Schichten verschiedener Textilfasern," Die Makromolekularen Chemie, 3 (1949): 281-285.

20. Rudolf Signer, "Improving the Dry Spinning Qulaities of Aqueous Casein Solutions," British Patent 571,468, issued 27 August 1945. idem., "Spinning Protein," British Patent 571,518, issued 28 August 1945. idem., "Increasing the Spinnability of Aqueous Albumin Solutions," Belgian Patent 448,749, issued February 1943. idem., "Dry Spinning of Aqueous Albumin Solution," Belgian Patent 448,765, issued February 1943. idem., "A Process for Rendering Insoluble and for Hardening Threads, Ribbons, Films and other Filaments Obtained from Aqueous Solutions of Protein," British Patent 573,423, issued 21 November 1945. idem., "Dry Spinning Albuminous Masses," British Patent 573,888, issued 11 December 1945. idem., "Hardening of Dry Spun Protein Fibres," British Patent 579,007, issued 14 March 1946.

21. Rudolf Signer, "Separation of Substances by Dialysis," Swiss Patent 244,043, issued 31 August 1946.

22. Rudolf Signer, "Separating Mixtures by Counter-Current Extraction," U.S. Patent 2,765,298, issued 2 October 1956.

23. R. Signer, K. Allemann, E. Köhli, W. Lehmann, H. Meyer and W. Ritschard, "Eine Laboratoriumsapparatur zur

multiplikativen Verteilung von Substanzen zwischem zwei nicht mischbaren Flüssigkeiten," Dechema Monograph, 27 (1956): 32-44.

24. R. Signer, A. Aeby, F. Opderbeck and H. Studer, "Aufteilung der Cellulose in Moleküle und Grössere Teilchen bei der Veresterung," Monatshefte für Chemie 81 (1950): 232-238.

25. H. Arm, F. Hügli and R. Signer, "Thermodynamische Messungen an Gemischen von Äthylbenzol mit Toluol und Methyl-äthylketon," Helvetica Chimica Acta, 40 (1957): 1200-1205.

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