CENTER FOR HISTORY OF CHEMISTRY

IZAAK M. KOLTHOFF

Transcript of an Interview
Conducted by

George D. Tselos

at the

University of Minnesota

on

15 March 1984

Isaak M. Kolthoff JH 3/15/96

r History of Chemistry Oral History Project

I have been interviewed on tape on March 15, 1984 by Dr. George Tselos of the Center for History of Chemistry. It is my understanding that this tape recording will be transcribed within two months and that I will have the opportunity to review and correct the resulting transcript before it is made available for research by the Center. At that time I will also have the opportunity to request restrictions on access to the interview, if I so desire.

However, if I should die or become incapacitated before I have reviewed and returned the transcript, I agree that the contents of the interview and the resulting transcript will become the property of the Center for History of Chemistry to be used for educational and scholarly purposes.

Interviewee Signed release form is on file at the Science History Institute

Date 03/16/1984

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

IZAAK M. KOLTHOFF

1894 Born in Almelo, Netherlands, 11 February

Education

1915	Diploma, Pharmaceutical Institute, State University of Utrecht
1918	Ph.D., Chemistry, State University of Utrecht

Professional Experience

1917-1927	Conservator, Pharmaceutical Institute, State University of Utrecht
1924-1927	Lecturer in Applied Electrochemistry, State University of Utrecht
1927-1962	Professor and Head of the Division of Analytical Chemistry, University of Minnesota
1962-	Emeritus Professor of Analytical Chemistry, University of Minnesota

Honors

1949	Nichols Medal, American Chemical Society
1950	Fisher Award
1960	Minnesota Award, American Chemical Society
1964	Charles Medal, Charles University, Prague
1964	Willard Gibbs Medal, American Chemical Society
1964	Polarographic Medal, British Polarographic Society
1967	Kolthoff Gold Medal, Academy of Pharmaceutical Science
1981	Olin-Palladium Medal, Electrochemical Society

ABSTRACT: In this interview Professor Izaak Kolthoff begins with his early life in Holland, and discusses his family and education. Kolthoff continues with the factors influencing his decision to become an analytical chemist, and describes the early state of analytical chemistry compared to other branches of chemistry. The interview then focuses on accusations of Communist sympathies, and the effects of the McCarthy era on Kolthoff's career. Kolthoff concludes with a brief discussion of his work on crystal surfaces, the relocation of European scientists during the 1930s, and his participation in synthetic rubber research during World War II.

INTERVIEWER: George D. Tselos holds a B.A. in biology from Carleton College, and an M.A. and Ph.D. in history from the University of Minnesota. As a specialist in archival administration, he held a position with the Archives of Labor and Urban Affairs at Wayne State University before joining the Center for History of Chemistry as Assistant Director for Archives.

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INTERVIEWER: George D. Tselos

ALSO PRESENT: Edward J. Meehan (Kolthoff's colleague)

PLACE: University of Minnesota Campus Club

DATE: 15 March 1984

TSELOS: This is George Tselos at the University of Minnesota Campus Club interviewing Dr. Izaak M. Kolthoff on March 15, 1984. We're going to talk today with Dr. Kolthoff about a number of aspects of his career both as a scientist and as a person involved in public affairs. I'd like to start, Dr. Kolthoff, with some background about your family and your early upbringing. Who were your parents and what did they do? What sort of an early family life did you have?

KOLTHOFF: There is relatively little to say about my parents. In one sense though, there is quite a bit to say about my dad, who was a businessman. I came from a part of Holland that you might call the center of the textile industry. My father was an agent for several British companies. He was a middleman. His main personal interest was Jewish affairs. He was probably the most orthodox Jew that could be found in Europe or any other part of the world. As a matter of fact, by the time that I had grown up, he didn't carry a handkerchief any more on the Sabbath day. He put it around his middle. This is one thing that I've always remembered. My mother, on the other hand, was not the slightest bit interested in this sort of thing. True, she kept very kosher; she would not cheat in that matter. Yet, she was completely free of any religious feelings. To please my dad she might go to synagogue on Saturdays.

TSELOS: Did you have any brothers or sisters?

KOLTHOFF: One brother and one sister.

TSELOS: Were they older or younger?

KOLTHOFF: I was the youngest. One brother died before I was born, so I never knew him.

TSELOS: What were your parents' attitudes towards your education? Did they want you to go into a particular line of work from an early age, or did they let you follow your own inclinations?

KOLTHOFF: No, I did what all the other kids did. First there was kindergarten. There's where I got my name, Pietje. A friend of mine from those days would always say, "Hey, Pietje, tell me what is going on," or something like that. None of my other friends knew me by the nickname Piet. Most of my family accepted

its usage. My dad refused to use it, however. The name became known around here the first year that I came here because of Willie Lindsay. You know him. He was professor in the School of Music, and knew German quite well. He was not very shy as far as reading other people's mail was concerned. In the old Campus Club he would look at the mail and say, "There's a card for you and your mother writes 'Liebig Piet'." That's how the name Piet was introduced here.

After kindergarten I went through the regular school where at about the age of ten I was taught French--the universal language in those days. I took three years of French before I went to high school. There I took five years of French . This is the only language that I flunked when in high school. The quality of teaching in the high schools of Holland was subpar. I think that it is even worse in the United States today.

You probably want to ask me about how I got interested in science in general and in physics and chemistry in particular. I don't know why I got interested in chemistry. It's awfully hard to say why you do get interested in something. Nevertheless, I did get interested when I was fourteen, I guess, and took three years of chemistry. Anyway, my chemistry teacher was a character and would have been better as a lecturer or professor at a university. He had his Ph.D. in chemistry and he really didn't care whether his students cheated or not. He always assigned problems and then he went through the class to get answers. During my last two years in high school he taught quite a few of the more advanced scientific topics. None of my teachers needed to take education courses; they knew how to teach their subjects.

TSELOS: Were you also attending a Jewish school while you were in elementary and high school?

KOLTHOFF: No. I do recall, however, that before school started I would go to the synagogue at 7:00 in the morning, and then rush home at 7:30 because school started at 8:00. In those days I was as orthodox as my dad. Later, when I was a student, it occurred to me that I really didn't know what I was praying about. Even though I could translate the prayers, which were all said in Hebrew, and even though I said them correctly and my voice was even better than a cantor's, I started to think, during my student days, that the prayers didn't make any sense to me. I was not conservative or religious, or whatever you want to call it, and so I changed. Doing this hurt my father immensely because my brother and sister, who were older, had already pulled free from the Jewish religion. And so had my mother. It was very clear that she did things only to please my dad.

TSELOS: So you did not remain actively Orthodox after that point?

KOLTHOFF: No. No. It was one or the other. I still remember with pleasure the jealousy of my friends on Jewish holidays when I went to high school. They were all terribly jealous that I didn't have to go to school.

I bring this matter up because I rarely experienced anti-Semitism. Of course, it exists also in Holland as well as elsewhere. But in my own personal life, it's hardly affected me at all. I still recall when Willie Lindsay dragged me into social life here. I visited a leading society lady, Mrs. Pillsbury, for the first time. She and I happened to be alone for a while. It was not a particularly nice occurrence. She must have been told that I was Jewish because she told me that she was an anti-Semite. You don't forget these things, you know. The first thing that I said was, "If you think I feel extremely flattered to be invited by Mrs. Pillsbury, I have to disappoint you." I also said, "Maybe when we know each other better we may like each other." We later became very good friends, you know. I got a very lovely letter from her on my ninetieth birthday. I said that what interests me is why you are an anti-Semite. She answered, "Because Jews sell things under the market price." I told her that I can't argue with you on that because it's not my field at all. (I don't know whether you want me to tell you things of this nature; it's all very personal.)

TSELOS: Well, I think that you've treated one topic about which I have an interest, the development of your religious views. I was wondering whether you were looking toward a scientific career when you left high school. Had you decided that you would pursue a scientific career at the time you went on to university?

KOLTHOFF: Well, I don't think I had given any thought to the future. I was interested in chemistry. I wanted to study chemistry, but the law in Holland made it a difficult thing to do. The kind of high school I went to concentrated on modern languages. We had to take a final exam in French, German, and English. We had to submit a list of twenty books we had read in each of these languages. The Gymnasium stressed a classical education. We spent much less time on the sciences. By the way, I think that the system in place now that allows students to take this subject and not take that subject has made their education much less sound than ours was.

In my young days, you could get a diploma from the high school at the end of the third year. At that time, about sixty or seventy percent of the students left school and obtained a job. We started the first year of high school with between fifty and sixty students and ended up the last year with twelve or thirteen students. It was really quite an education.

As you undoubtedly have read or heard, the law was changed in 1918--the year that I was able to get a Ph.D. Otherwise I wouldn't have gotten a Ph.D. The new law recognized that the old way was outdated. It was the same as in Germany. I don't know how it was in England; I know very little about the situation in England. The American university setup is completely different from that in Europe, which is classical. What impressed me very much when I first came here and learned a little more about American universities was that education towards a bachelor's degree really was intended to educate young people to become useful citizens. That qualification didn't play any role at all

in the classical universities where undergraduate education was simply the bridge you had to cross in order to go on to advanced studies. After attending college for two or three years, you took examinations, and if you passed those exams, you got something comparable to a bachelor's degree here. The next stage was not called graduate school, but graduate education was and is comparable to graduate school.

TSELOS: I recall that your first degree was in pharmacy.

KOLTHOFF: Yes, it was. Pharmacy and medicine were taught in the classical university because there was no other place where it could be done. They certainly needed the pharmacists and the M.D.'s. They did not take what was called the classical examination, however. It was the same examination, but it was a state and not a University examination. This was kind of silly. I finished in 1915, I think, but I had been doing quite a bit of scientific research in the meanwhile. I had published by then several papers. I didn't work for the Ph.D. at that time because I couldn't get that degree. We knew in 1917 that the law was going to be changed in 1918. So, I got my degree in 1918 when it was changed. I had an advantage because my teacher had a background similar to mine but had also taken the classical examination and had then gotten his education with van't Hoff, the famous chemist from Amsterdam.

I'm changing the subject completely now, but my teacher, [Nicholaas] Schoorl, noticed my interest in what I was doing. He never suspected that I might be impatient, for example, when washing my filter before I made a gravimetric determination. It was the first thing the student had to do and I recall that I got a high result. We could cheat because we knew what we were supposed to get. I never did that; rather I reported whatever I found. But I also remember when I washed the filter that I never waited until all the water was gone. So, when I got a high result Schoorl referred me to a paper in the Journal of the American Chemical Society of 1912 where they described the word coprecipitation (1). In 1936 [Ernest] Sandell and I published a book and we referred to that same paper (2). When I learned about coprecipitation I got interested in it.

Very soon after we did those gravimetric experiments, we started with volumetric analysis. We used a burette and made titrations. There were only a few indicators in those days, and I didn't understand that you used an indicator, like methyl orange, for the titration of a strong acid, but for a weak acid you use phenolphthalein. I recall that my teacher referred me to a paper he had written in 1904 in the Dutch Chemical Weekly (3). I didn't understand a word of it, so I had to go back to the very beginning.

In those days my teacher would encourage me by making available whatever instruments he could afford to get. That was not always possible until about 1915. In 1915 he got a beautiful new laboratory that was connected through a corridor with the chemistry lab called the van't Hoff Laboratory.

Ernst Cohen was a favorite pupil of van't Hoff and he headed the chemistry lab. Cohen wrote the most outstanding book on

van't Hoff in German; he wrote perfectly flawless in German as well as in Dutch (4). But he looked down terribly on people who studied pharmacy; he considered them as laymen in chemistry. So, it happened, I think it was in 1917, that I published several papers, one of which enabled one to make a fairly good guess about what the dissolved solids would be through the electrical conductance (5). In order to write an acceptable paper I had to do quite a few experiments by mixing things up and seeing what the conductance was.

In connection with my remark about Ernst Cohen. We were connected through a corridor with the chemistry lab. I could walk into that lab at any time. At that time, the man who had the same position as I had in those days in pharmacy was very helpful to me in several ways. He gave me advice and so on. Anyhow, after I published that paper on electrical conductance, Ernst Cohen told his students not to read it. He said that it's all nonsense and there is nothing any good in it. Immediately after the lecture was over, a whole gang of students came and told me what he had said.

I was furious, of course, and went over to speak with him, but very politely. "Professor," I said, "I understand that you disagree with me." Well, he had to admit finally that he hadn't read my paper very carefully. Yet, he was convinced that it couldn't be any good. I told him that at the least I expected that he would correct himself at his next lecture and tell his students the truth. I forget now what else I said. I was pretty mad. Cohen went to my boss, Schoorl, and said that I had been pretty rude to him.

Schoorl called me into his office because at the time he was acting as my professor again. I still recall that I said to Cohen, "Cohen, you're verrecken." That is a phrase that you can't translate, but it's similar in meaning to the phrase the Hitlerites used about the Jews, "Juden verrecke". It is a mean thing to say. But in Dutch it didn't have the same connotation as it had in German. Anyhow, Schoorl was mad at me again, "You can't talk about a professor that way. You go and apologize." I said, "No, I'm not going to apologize." He said, "Well, he can do you more harm than good." He said that because in 1924 the faculty had to approve my becoming a lecturer in electrometrics. At the time a lecturer was called a "privatdocent." You didn't get a penny's salary, being a privatdocent, although you could require the student who wanted to take the course to pay a fee. I refused to charge a fee because I was sure I wouldn't get a single student that way.

I'm jumping now a little from one topic to another because I'm going to speak about how I developed an interest in visiting the United States in 1924. In 1923 [Charles] Foulk, a professor at Ohio State University who had been working with the famous Wilhelm Ostwald, visited me and said, "Why don't you come and make a lecture tour in the States?" That's where the idea began. I don't want to go into a more personal matter concerning a relationship with a woman that made me quite upset. My old boss Schoorl said, "Why don't you get away for a few months and make that lecture tour and get over your feelings." I never visited Minnesota during that trip.

TSELOS: Did Cohen ever retract his criticism?

KOLTHOFF: Several years later, when I went to say goodbye to him because I was leaving for the States, he told me, "Kolthoff, you may remember the Dutch have a very good name in the United States and you can easily spoil it." I've never forgotten that. I've told this to some of the people who know Ernst Cohen. I saw him every summer before the war, when I used to visit Holland. I always thought of my position here as a temporary one. I came for a year. Even though the position was made a permanent one after one year, I almost went back to Amsterdam to stay in 1931.

TSELOS: Well, from what you've said it would seem that your interest in analytical chemistry really developed from the fact that there was so little explanation for the analytical techniques that were being used at the time and that it was really an undeveloped field in many ways. Would that be correct?

KOLTHOFF: Yes, it was purely empirical. We used Autenrieth's book, I believe (6). But even the old Treadwell book, best known for analytical chemistry, gave only the procedures (7). You do this and you do that. You weigh, you dissolve, etc. It did not explain. As I mentioned before, when I showed interest in the explanations of things, my old boss would refer me to the literature.

He was a first class scientist, but he published very little after he became professor because he had a strong social concern. Consider that all the people who became directors of food and drug laboratories in Holland had the same education that I had in analytical chemistry.

It is difficult to say how I became interested in chemistry. I had enough interest during the first year that I took chemistry, or had to take chemistry in high school. I made myself a laboratory under the sink in the kitchen of our home. And, of course, I was particularly attracted to making things that had a nice color. It was really more like playing than having a great scientific interest. I happened to be interested in the chemistry that we got in high school and did something for which the students here, for example, would get expelled. That happened many years ago when we were being taught how to make gunpowder. I got myself a good eczema by showing that the gunpowder really worked. I don't think that they teach students how to make gunpowder anymore because the temptation for them to play with it is too great.

TSELOS: You mentioned that your professor in Holland had a strong social concern--that many of his students went to work for the government checking for adulterated food and this sort of thing. Did he talk much about the social responsibility of using scientific knowledge?

KOLTHOFF: No, I don't think it ever came up in any of his lectures. Quite simply, the courses were given and we were strongly advised to take those courses. He became the chairman of the

faculty committee that dealt with food and drugs.

He was a very hard worker, but at the same time he was a real sportsman. We went horseback riding together, swimming together, and soon rowing together. He even had a sailboat. I would go sailing with him on the Zuider Zee, which is now a lake. Of course, I learned then to know him much better as a person. He was always very modest. When he was to receive a high decoration from the Dutch Queen he told me, "I'm not going to accept it." I advised him differently. I said, "You'll make matters much worse if you don't accept it because then <u>everyone</u> will know that you have been offered it. So, you might as well take it."

TSELOS: Did you have any contact yourself with Ostwald or any of the people that we had inquired about?

KOLTHOFF: No, I never met Wilhelm Ostwald, although he is responsible for my interest in analytical chemistry. In 1912 I went to a book sale and bought ten books for fifty cents. One of the books, which Herb Laitinen now has, was by Ostwald, "The Scientific Foundations of Analytical Chemistry" (8). Ostwald wrote at the beginning of that book that analytical chemists are the maidservants of other chemists. This made quite an impression upon me because I didn't want to become a maidservant. Ostwald's book greatly affected my interest in the scientific aspects of analytical chemistry.

I would later criticize Ostwald's book pretty severely because, strangely enough, Ostwald didn't mention any of Nernst's work. [Walther] Nernst was a famous electrochemist, who did important electrochemical work, a completely classical piece of work, right in Ostwald's lab (9). Whether Ostwald was jealous or not, I don't know.

Ostwald has written quite a number of books on the history of chemistry and the history of inorganic chemistry. He was quite an outstanding man on the whole. He even won the Nobel Prize [in 1909]. You may ask me what did he do, what was his own contribution to chemistry? Well, writing all of those books is quite a contribution in itself.

I guess that the second person responsible for my interest in analytical chemistry was Schoorl, my teacher. I want to talk to you about him because I can say many interesting things about him. In 1915, for example, during the first World War, the temperature in the lab was not permitted to get higher than fifteen degrees centigrade. The authorities would be furious if the temperature got higher. That was pretty chilly, less than sixty degrees Fahrenheit.

TSELOS: That would be cold to work in!

KOLTHOFF: Well, it was necessary because there was not enough fuel in the country. As a matter of fact, there was not enough fuel because they sold too much to the Germans. There was a great shortage in Germany and the Dutch profited greatly from it.

Sørensen, a Dane with Carlsberg Laboratories near Copenhagen, had written a long paper in 1909 in which he

introduced the concept of pH (10). He had written it first in Danish, I guess, but then published the paper in the Biochemische Zeitschrift in German. He prepared the first set of what we call buffer solutions, that is solutions of known pH. They were used for years and years until an American biochemist, Beverly Clarke, made a new set. Quite generally, they are still being used today. Sørenson also wrote in detail about how you could determine the pH with indicators and also electrometrically. Sørenson got particularly interested in the things that I wrote about indicators and I recall that he came to Holland in 1917. I didn't see much of him, because my boss felt pretty good about having a famous man like Sørenson visiting. My boss had a nice office, beautiful chairs and everything. I saw Sørenson for only a few minutes--well, maybe we talked a little bit longer.

After the first World War, there was a great deal of hatred toward Germany in countries like Belgium, Britain, and so on, which had suffered. This hatred was also reflected in the scientific organizations. So, because the Dutch had not been in the First World War, they organized the first postwar chemical conference in 1921 in Utrecht, where my alma mater is. I think that is where a meeting of the IUPAC (International Union of Pure and Applied Chemistry) was held.

I met several people there including Niels Bjerrum, whom I got to know better after I came to this country. I was invited once to give a talk to a group of Scandinavian chemists. In those days, when you gave a lecture here in America you couldn't just start; first you had to tell a funny story. I made mistakes on that score, but anyhow, when I talked to that group, Niels Bjerrum was chairman and he introduced me. After I had finished my lecture he said, "Well, you people all found out Kolthoff is Americanized now because he starts by telling stories." He had also learned about this introductory habit by giving lectures here. It really was expected that you tell a funny story. That was Bjerrum.

I'd like to mention Joel Hildebrand. In 1949 when I got the Nichols Medal, I mentioned quite a few people from whom I had learned so much. I referred to Joel Hildebrand as one of my early teachers. In 1912 or 1913, my boss, Schoorl, referred me to a paper written by Joel Hildebrand, who had gotten his Ph.D. in Europe like most of the outstanding American chemists in those days (11). I learned from that paper. Analytical Chemistry mentioned in an article, however, that I had referred to Hildebrand as I did because he wrote me a letter in 1949 that stated, "You found gold where others have only found dust" (12). Anyway, Hildebrand's paper, the pH paper, was a classic. All of this leads me back again to Nernst.

I never met Nernst personally, but he was Herr Geheimrat Nernst. In Germany, this is the highest title that one can get. He felt that way. I recall a meeting of the Deutsche Chemische Gesellschaft in 1924. The chairman introduced Nernst and quite a number of other speakers. The meeting started at 8:00 o'clock and every speaker had twenty minutes at most. Nernst, however spoke on and on and on, and nobody dared to interrupt him. The next speaker was [Kasimir] Fajans, who in the Hitler period came over from Munich to Michigan. You wanted to talk about that

anyhow. The chairman said to Fajans, "Will you please think of your time, you have only so much time." Fajans replied, "I will not speak as long as the Herr Geheimrat has done!" Funny the dumb things that stick with you in your life!

At that time I met Hückel of the Debye-Hückel theory. Hückel was a student of Debye, who was Dutch. Debye was ten years older than I. He won the Nobel Prize, but not because he was rightfully famous. Peter Debye was a very faithful Catholic. In 1912 he taught for a year in Holland, but the older professors made things difficult for him. He had a good sense for teaching. I recall that the students who took his lectures were very excited. He had never had a billiard cue in his hand, but he taught them all of the laws of collision. There was tremendous interest then in knowing how to play billiards. Anyhow, because he couldn't get along with the older professors he went to Germany. He came to America just before the start of the Second World War and went to Cornell University.

TSELOS: I was surprised that he was able to leave Germany easily in 1940. That was my impression, that he left in 1940.

KOLTHOFF: Yes. He left in 1940. I still have his letter. It was written in Dutch, of course. What should he do about his citizenship? He had remained a Dutch subject while he was in Germany. He had used it for quite a number of years. What should he do with it? You don't have to do anything except take out your first papers. You have to wait five years before you can take out your second papers.

This might interest you. Ed Meehan can tell you what kind of teacher Debye was. When Ed and I ran the show for the Office of Rubber Reserve, Debye had developed an optical method to determine particle size. We wanted to learn that method so we decided to visit Debye. Because Ed's field included optics, he went to visit Debye one day before I did. I thought that since it's his field he would pick it up better than I. When I arrived at Debye's place Debye said to Ed, "You explain the method to Kolthoff. I explained it to you yesterday." Debye had to admit (reluctantly) that Ed had explained it very well. He had nothing to add to it! No kidding! Funny how those things stick with you. Debye was very impressed.

TSELOS: One of the things that I wanted to discuss with you was whether you had been at all involved in consulting work or any kind of chemical work that had a direct application to industrial concerns while you were still in Europe.

KOLTHOFF: No. I never was, at least not until the rubber situation--but that was toward the end of the Second World War. We developed a recipe for rubber that could be made at a much lower temperature than previously. Paul Flory, the man who got the Nobel Prize and who is now at Stanford, was at Esso in those days, wasn't he?

MEEHAN: He was at Esso and at Cornell.

KOLTHOFF: I know that at one of the meetings we were asked whether we could get better quality rubber. Flory said that looking for different chemicals wouldn't help at all, but he made it clear that the temperature used during production was a major factor. So we developed a kind of process for which Phillips later got the patent. You see anything that has been financed by the government can be used by anyone within the country. Otherwise, we might have patented it.

At that time I was a kind of consultant. That was the only time when I had been a consultant to a company. I was more than a consultant to a book publishing company, Interscience, which sold out to John Wiley. Two people, a Dutchman and a German, started it in this country [Maurits Dekker and Eric Proskauer]. They got me in as an advisor but they didn't have a penny to pay me. So, I think that they made me one of the directors when the business improved somewhat.

TSELOS: Prior to World War II and your work on the rubber project, then, you really were not involved in industrial consulting to any extent?

KOLTHOFF: No. At Phillips I was consulting less than one day a month for only a couple of years. It may have been four or something like it.

TSELOS: I was wondering how you and Professor N. Howell Furman got together in the mid-twenties to do the work that you did on potentiometric titrations.

KOLTHOFF: I had been doing considerable work on potentiometric titrations. I recall that in 1924 I had a Czech working for me by the name of Tomiček. He later became a professor in Prague. Through Tomiček I got to know [Jaroslav] Heyrovsky, the man who developed polarography, receiving a Nobel Prize in 1959.

TSELOS: How did you get together with Furman?

KOLTHOFF: Well, we were interested in that kind of work because in those days, analytical chemistry was looked down upon as the maidservant of the other fields. That was still true even in this country when I came here, though the situation improved here very much earlier than in Europe. Not until the mid-1950s would professors of analytical chemistry be appointed to positions at bona fide universities. The British were the slowest to do so. I know that in 1953 I gave a talk at Oxford and I forget what comparison I made, but anyhow, I told them that they were awfully slow at picking things up, etc.

Furman was going to work with me for a year in Utrecht. He and his wife and daughter actually came over to Utrecht, but before I really started to work with him I received a cable asking me to come to Minnesota. Furman therefore went to work with [William] Treadwell, a man whose name I mentioned at the very beginning of the interview. Treadwell's father had written a classical book for analytical chemistry (7). Incidentally, Furman's daughter lives in the Twin Cities. She's married to a

biochemist and still calls me Uncle Piet.

The situation in analytical chemistry was very unfavorable in this country when I came here. I asked colleagues in physical chemistry to help. They did. People would officially major in physical chemistry and satisfy the requirements of a minor in my field, but I would be their major advisor. For example, Herb Laitinen got his major in physical chemistry. This procedure changed considerably at the end of the 1930s.

In those days, a fellow with a Ph.D. in analytical chemistry was used as an analyst, but not as a scientific analyst in industry. He was not called upon to solve the real problems; he simply made analyses. That changed considerably.

Treadwell told me that analytical chemistry was viewed by the (international) IUPAC as a second-rate kind of activity. Analytical chemists then published papers, but in the old-fashioned way.

I wrote a letter about 1950 to the professor of organic chemistry at Columbia University who happened to be president of the IUPAC [Marston T. Bogert]. I told him that it is scandalous to have analytical chemistry treated as it is by the International Union of Pure and Applied Chemistry (IUPAC). I was asked to take the place of a young chemist who was sick and couldn't attend a meeting of the IUPAC. A Dutchman was chairman and he asked me kindly to leave because they didn't consider me to be an analytical chemist. I said, "Gentlemen, I am sorry, but you need first to talk to Albert Noyes who is the chairman of the group in Washington and Linus Pauling who is here." I said that I will stay at the meeting, but if it were agreeable to them I would not return to their meetings. I couldn't get very much cooperation from either Albert Noyes or Linus Pauling. There was still that feeling that analytical chemistry was not a real science.

For years Albert Noyes was editor of The Journal of the American Chemical Society, and I was an associate editor. I often mentioned to him that the table of contents always contained organic chemistry, physical chemistry, and inorganic chemistry, but no analytical chemistry. Finally, after I distributed a note about this matter, they put analytical chemistry in the table of contents.

TSELOS: Let's talk now about your being accused of being a Communist sympathizer in the late 1940s and early 1950s.

KOLTHOFF: I remember that John Cowles, who was the big man of Minneapolis newspapers, sent me a copy of a letter written by Senator Child to James Morrill. The letter included me in its discussion of "Reds" at the University of Minnesota. I called John Cowles and said, "John, this is funny. I have just been talking to the Pentagon and here I get this letter." He asked, "Can I use it?" I said, "No, I don't want anyone to make excuses for the things I do. You know me." We usually talked together about twice a year in a general way.

TSELOS: How did you get to know Cowles?

KOLTHOFF: Well, as I told you, I had gotten to know Mrs. Pillsbury. Once you get acquainted with one person, you get to know the whole group of people with whom she associates. I used to ride horseback regularly with Mrs. Peck. Her husband was President of Northern States Power Company.

TSELOS: Right. I remember.

KOLTHOFF: Once this matter of the letter got started, I called Gideon Seymour, a leading man in the newspaper. Cowles was the man with the money, but Seymour really made a good paper out of the morning paper. I spoke to him about this matter and he said, "Yes, we'll ask him. I have never refused to have lunch with anyone and I am tempted to write an editorial about old Senator Child and ask him when he stopped sucking his thumb?" Funny, I can remember this thing very well because it pleased me no end to hear it. Morrill wrote a letter that I didn't like and then sent me the copy. He said that I had an international reputation, but that that had nothing to do with whether I was a Communist or not.

I went to Russia in 1945 with a group of scientists. While there, I met [Frederic] Joliot-Curie who impressed me very much. He was not in my field at all; he was a physicist. You may recall him. He married one of Marie Curie's daughters. Well, both he and his wife interested me very much because they were world citizens. You would not expect this kind of person to be an arch Communist. But Joliot-Curie became the leader of the French Communists.

He later wrote to me asking if I would be willing to be the sponsor of a meeting? I think that this thing really caused all of the difficulties. The meeting would be an international one and be held in Oslo or one of the Scandinavian capitals. I would be a sponsor of it. This occurred after the bomb had fallen on Japan and scientists were talking about their responsibilities as human beings. I wrote back saying that I had no objection to being a sponsor. This then appeared in a Communist paper in New York. My name was mentioned and the paper went into details.

TSELOS: Would it have been the Daily Worker? The Daily Worker was the main Communist paper.

KOLTHOFF: I think so. Anyhow, when I learned of that, I wrote to Joliot-Curie saying that I didn't know that the meeting was Communist-dominated, that I wouldn't lend my name to it, and that I wanted to talk as a world citizen, not as a Communist. He apologized in a letter after that meeting had been held. It didn't matter because I didn't go to the meeting. I had nothing to do with it whatsoever. My name had been mentioned, however, and that has caused me continuing difficulty. Joliot-Curie got sick and died relatively soon after.

My reputation was further damaged in the McCarthy days when a man (whose name I now forget) made a mistake and wrote an article with the title, "Reds in American Universities," for a conservative monthly magazine (13). He singled out two of us in Minnesota. One was the president of a college in St. Paul, I

forget...

TSELOS: Macalester or Hamline?

KOLTHOFF: I think it was Charles [Charles J. Turck, President of Macalester College in St. Paul] ...I knew him superficially and he and I were singled out. Well, anyhow, when this happened, when that paper was published, I called Charles and said, "My condolences to you." He said, "You don't know how funny this is. A week ago, I was made President of the Presbyterian Churches in the United States." That finished that whole story, you know. Nobody took that article seriously any longer.

TSELOS: As long as we've gotten onto this topic of politics, why don't we continue with it. I think that I'd like to return to the rubber project and a couple of other things later. One of the things that you had mentioned in one of your letters was that there was something that you thought should be mentioned about the House Un-American Activities Committee.

KOLTHOFF: Well, I got a call from the paper after midnight. I was told that I had been cited to belong to thirty-one subversive organizations and I was asked for my response. I said, "Well, write in the paper that it is now one o'clock in the morning and that I have to be up and working for the Air Force by nine o'clock tomorrow morning." I also told them to write in the paper that I had received grants for scientific work. It had become quite clear during the war that scientific research was going to play a leading role in the policies of the country. They did indeed publish the story and they mentioned Judy Holliday in it (14). I think that she was a movie star?

MEEHAN: She was a movie star, in "Born Yesterday."

KOLTHOFF: Well, they wrote about her and then at the very end of the article singled me out in Minnesota.

Now, in those days I had called Linus Pauling the day that Khruschev had the nuclear bomb tested and told him that this was against the international agreement. After the article appeared I called Linus and said, "I think you should do something about the article because your name was mentioned." He said, "I have already sent a cable to protest against it." Pauling was exposed and had real trouble with the thing. I knew Pauling at the time, though not intimately. We did talk a little more to each other when we were both accused of being Communists. He never had been a Communist. That is the end of that story.

TSELOS: I've read the newspaper article about your ninetieth birthday. It appeared in February in the Minneapolis papers (15). It mentioned that in the 1950s, well, I think it was the 50s, maybe it was later, that you and former governor Elmer Benson had become sponsors of the effort to get Morton Sobell released. I was wondering whether this was something on which you worked actively with former governor Benson or was it simply that the two of you had signed a petition?

KOLTHOFF: I don't know anything about that. All I know is that one day in Chicago I attended a luncheon and...Who was the man you mentioned, a man's name with whom I would have done something, Sobell?

TSELOS: Sobell, right. He was a friend of the Rosenbergs. He was sentenced to thirty years.

KOLTHOFF: Well, I know that at that luncheon I heard the man with whom Sobell had worked. I gave money and afterwards I read about it. The man said, "Well he is a little bit Communist, just as I am." He said, "I supervised his work and I know him quite well and this thing is exaggerated. Even the Rosenberg situation..." During my horseback riding days, when I used to go to Arizona I met a Supreme Court Justice, a friend of the people who ran the place. I forgot his name. It's terrible forgetting names. Anyway, we went riding together. He complained that the people...

TSELOS: Was that Pierce Butler of Minnesota?

KOLTHOFF: No, I never knew him. I was good friends with the Pierce Butler who was the son of the Justice. He always told me, "Young fellow, young fellow, I'll go with you to Washington." I would say, "I'll get myself in trouble if I'm being called before McCarthy and told not to do things. I'll tell him a couple of things that I probably will regret later." He said, "Young fellow, I will go with you." I said, "I want you to know as much as possible about exactly what I have been doing so that you know ways that they can attack me, if they want to attack me now."

The Justice in Arizona was another Justice with whom I did quite a bit of riding. He was somewhat unhappy because people always introduced him as Supreme Court Justice so and so. He complained to me about it. We had some drinks together, so that we could talk a little more personally. I brought up the Rosenbergs' case. He said he could interpret it only in the following way. "The Communists had given the case to a lawyer who was a Communist because they wanted to make victims out of the Rosenbergs. To me, this was an injustice. It still is. I headed a national group to get the verdict reversed, or at least to get the people to know that it was a murder and that injustice had been done." Again by doing such a thing, one can be labeled a Communist simply because you send some money, that's all. I don't know whether the Rosenbergs were guilty or not guilty.

TSELOS: We jumped ahead to the 1950s. I'd like to go back and talk about some events in the '30s and '40s. One of the things that I was interested in was your early work at Minnesota with precipitates of various kinds. One of the things we were interested in was how you came to the idea of incorporating radioisotopes in the solutions.

KOLTHOFF: Well, we were interested in determining what we call the specific surface of crystals because that played quite a role in the impurities which you find within the crystals. Otto Hahn, who was the man who split the atom, was one of the people who had done work with radioactive things. It happened that Dr. Lind, the man who got me here, had quite a reputation as a nuclear chemist, as did one of his students, Charlie Rosenblum.

MEEHAN: Yes, I know the name, but I never knew him.

KOLTHOFF: That was before your time, I guess. When did you come in?

MEEHAN: 1939.

KOLTHOFF: It must have been awfully close to that time. Oh no, it is earlier because it was around 1933 that Otto Hahn and Heyrovsky were both here. It was quite an event for the students to have those two people come here. No, he started much earlier than I was thinking of. That was when I started to use radioactive matter to determine the surface and we did this examination which became quite widely known. We added together at room temperature an inactive lead solution and a sulfate solution. Various times after the mixing a radioactive lead solution was added to the suspension of inactive lead sulfate and the mixture was well shaken. When the lead sulfate suspension was well aged after various recrystallizations and the radioactive isotope was then added to the suspension the exchange remained limited to the surface of the aged crystals. The method could be and has been applied in our laboratory to a study of the extent of perfections of various crystallized precipitates after various methods of precipitation.

These studies accounted for the empirical methods of "aging" after the precipitation in the empirical procedures in the outdated cookbook methods of gravimetric analysis. Quite a few of the students in those days were working with different kinds of precipitates and using a radioactive method.

Otto Hahn was at Cornell for three months. They still have that position I believe--"foreign lecturer". Coming back again to the nonchemical part, I'll tell you, they wrote to me from Cornell, "Don't touch any politics with him because he really explodes." Since I was more or less his host I picked him up at the railroad station and decided to keep my mouth shut about politics. We hadn't gone one block when he showed me the cable he got from Max Planck in 1933. "Come back and save whatever can be saved." He never would have shown me this thing except that he had written me from Cornell. He was violently anti-Hitler. He saw the harm that Hitler was doing to the country by causing all of the Jewish workers as well as non-Jewish non-Hitlerites to emigrate. Hitler also caused Debye and quite a few other people to leave.

Every year toward Christmas or New Year's, I used to get a card from Otto Hahn. He would write it in German. This story is funny. (You might as well include a few funny things about famous people like Otto Hahn.) Otto Hahn wrote to me about "how

often do I think of 'schönen und freien Tage' (beautiful and free days) in Minneapolis." What's funny you ask? Oh, I often ask people from abroad, "Did you have any difficulties when you started teaching in English?" I asked Hahn this question. He said, "No, I have my students correct me." So, in his first lecture, he talked about "a-nions" and "cay-tions". The students told him that that was "an-ions" and "cat-ions". So, that evening while in the faculty club he ordered his dinner with "on-ions". They didn't understand that he wanted onions. I mean that is completely logical. That was the same week that Heyrovsky was here. That was when Jim Lingane decided he would work on polarography for his doctor's thesis.

TSELOS: That was the next question that I was going to ask you. How did you begin to study this topic and why did you find this technique to be so fascinating?

KOLTHOFF: Well, as I told you, I got acquainted with Heyrovsky in 1924. I had this Czech friend, Oldrich Tomicek, who became a professor of analytical chemistry in Prague. Well, may I interrupt this story to say something personal, and that I didn't use to defend myself against accusations of being a Communist. After the war I went to Czechoslovakia a couple of times. I was there when their president was buried. What was his name? Masaryk?

TSELOS: This was in 1948, at the time of the coup?

KOLTHOFF: It was after they had become Communist, I guess, after the Communists had moved in. Well, it really doesn't make much difference.

TSELOS: We were talking about...

KOLTHOFF: Being in Prague at the time that the president was being buried. At that time before I started a technical talk, I said that Jim Lingane and I had written a book on polarography in 1939 that was dedicated to Heyrovsky and contained a footnote with the motto of my university in Holland at Utrecht (16). It read: "May the sun of justice shine on us." I then continued, "I hope that the sun of justice will shine on your people again." You could absolutely hear a pin drop. Only about twenty or twenty-five people left. I was told that they would have gone to the police to report my remarks. I was very happy about that; yet, I never have been complimented by all the people who considered me to be a Communist, for having said that.

TSELOS: During the 1930s you took an active role in assisting the relocation of a number of European scientists to this country. How did you get involved in that?

KOLTHOFF: No, I couldn't tell you; I really don't know. I would think that it might have been through, you had mentioned him, the biochemist [Ross] Gortner. I think that it was through Gortner. What I do recall very well is that the Rockefeller Foundation had said that these European scientists will be an asset to this

country and if so, go ahead and relocate them in this country. They said that we will pay the universities to take them. We will pay the first year's salary and half of the salary of the second year, and if they are actually very good, the university should keep and finance them.

TSELOS: But how did you contact the Rockefeller Foundation?

KOLTHOFF: I think that Gortner must have done something.

TSELOS: Oh, I see.

KOLTHOFF: That was a touchy subject and I had to be careful when doing these things. It was to our advantage to get some of the people like [Herbert] Freundlich.

F.G. Donnan, a famous chemist in London, took a liking to me. I was greatly honored and since I went to Holland every summer, I usually took a ferry across the North Sea and got off at Southhampton. Donnan said, "I wish you people could do something about Freundlich." He said, "We have so many refugees that there is no money available to take care of so many people." So, you understand, I got Gortner involved again because I did not want to do anything directly with bringing the people over. Once I got into a terrible quarrel with Gene Ormandy; never talked to him since.

TSELOS: The conductor?

KOLTHOFF: Yes. I lived in our old Campus Club and that was right next to, what is the name of the concert hall?

MEEHAN: Northrop Auditorium. The building is still there.

KOLTHOFF: The Campus Club was there and while coming out of it I saw Ormandy coming from rehearsal. I said, "Gene, I want to talk to you." He said, "I am busy, very busy." I said, "I am busy, too." My famous last words were, "You can go straight to hell!" He would not listen. I said, "Do you know any outstanding musicians or people who are composers?" All that I got out of him at that time was "I'm busy, I'm very busy."

TSELOS: When you were at Minnesota, in the 1930s, who were your outstanding students at that time?

KOLTHOFF: Well, there were quite a few. Jim Lingane, who got a job at Harvard. He's retired now. Herb Laitinen went to Illinois.

MEEHAN: Don't forget Sandell.

KOLTHOFF: Right. Sandell was one of my first students. Who else was there? A fellow who went to Ohio State University. It must have been quite a number.

MEEHAN: There's Henry Yutzy.

KOLTHOFF: Yes. He became a vice president of Eastman Kodak. Pete Carr, I think, has figured out that more than nine hundred teachers in analytical chemistry trace their training back to me, so to speak. There's six or seven generations of them. I think that I had quite a good group, like the man who came from Harvard. He always remained supersensitive, but he was a very good worker.

TSELOS: How did you get involved, in the beginning, in the rubber project? Could you tell me something about the nature of the work? More particularly, can you tell me whether the two of you were assigned specific problems? Or were you assigned a general topic and then expected to choose the areas that you wanted to pursue?

KOLTHOFF: Our whole problem was that we simply did not know what the problems were. No, I am not joking. In the very beginning, I said to Maurice Visscher, "I'm so damned mad. I'd like to publish that the big companies, Goodyear, Goodrich, Firestone, all disliked the idea that university people would come in and stick their noses in their business." We didn't know what the problems were. You asked what problem? We didn't have a problem. We didn't know the problem. Surely, we could analyze those things and so I recall that Ed Meehan and I went together to Goodyear and Goodrich and were told, "We want to know the purity of the standard substances which are being used to make rubber, but there is a committee working on them already so we don't need you for that."

Without admitting so, they needed us pretty badly because there was one substance that they needed desperately to know about. One of our men had worked on that topic particularly. The substance was used in a very small amount, yet it determined whether the rubber would be soft or hard. If that substance was not used, the rubber would get ebonized. The Japs had taken New Guinea and most of it was made from...

MEEHAN: Coconut oil.

KOLTHOFF: And then it was put in a kind of a shaker so that we could do the same thing that the plants do when making the rubber. It was not a good rubber, but at least we could see how the speed of shaking the mixture affected it. It took quite a while before that was found out. First, we had to get a method to determine how much was present and how that changed during the polymerization. That became a very major issue. We would then write to the big plants because we couldn't say whether it was a better rubber or not. They had to test it in a pilot plant. Finally, it appeared that the shaking was being done much too intensely and had to be done with considerably less speed.

TSELOS: How long did it take for you to get effective cooperation with the companies, that is, to find out what they were doing and how what you were working on related to their production problems? Or did they ever really communicate?

KOLTHOFF: Well, every month we had meetings and called together the people who worked in universities and the chemists from the companies.

MEEHAN: It was a pretty free exchange.

KOLTHOFF: I also think that we knew by that time the problems encountered in order to get a better quality rubber. We knew certain technical things and started working on them. Many of the problems were being discussed at those meetings.

TSELOS: So, once you got in direct communication with industrial chemists, there was better communication than there was in the beginning when you were just dealing with the executives?

KOLTHOFF: Yes. We became good friends. We had major difficulties to work out in the beginning.

TSELOS: Did you travel extensively from plant sites in addition to doing your laboratory work?

KOLTHOFF: Ed and I went together at the beginning because...

MEEHAN: Yes, I remember visiting several plants.

KOLTHOFF: But, they wouldn't tell us the problems at that time and we didn't know them. I think that the substance that we spoke about is mercaptan, which was the first really major thing that we did work on. Well, they also wanted to determine the composition of styrene and polystyrene because these two substances were also used to form rubber. They wanted to know how much of one to use and how much of the other. Also how they were affected by temperature, and how they were affecting other things. Later, I think, they started to appreciate the cooperation of the academic chemists. You know, that was all secret work. Don't you remember that Du Pont, I think, used ferricyanide instead of the other oxidizing agent. I think it was Du Pont. Gordon Guss left some secret work on the train.

MEEHAN: That's kind of comical, in that respect. There was a document marked secret and confidential and he left it in his berth.

KOLTHOFF: That was Guss, wasn't it Meehan? He died at a young age. He got his Ph.D. here, too. He went to South Dakota.

TSELOS: What kind of supervision did the government officials exercise over the project? Was it a matter of simply writing a check and letting you do your work or did they try to exercise close supervision? I'm wondering about the relationship between your lab work and government people.

KOLTHOFF: No. There was a special office in Washington that... What was it called?

TSELOS: Rubber Research or something like that.

KOLTHOFF: I suppose that as long as they got some value for the money, they were satisfied.

MEEHAN: They required a report every month.

TSELOS: The people that you were dealing with, that is on the government side of it, were they essentially non-scientific administrators?

MEEHAN: Williams was the first one and he was a chemist.

KOLTHOFF: Yes. And Bill, Bill...

MEEHAN: Baker.

KOLTHOFF: Well, Bill Baker knows quite a bit about our things in that period. We became close friends.

TSELOS: Did you feel that the government's pouring these funds into synthetic rubber research and a number of other areas caused a major and favorable postwar change in attitude towards government funding of research? Did you notice that?

KOLTHOFF: Definitely, because the three branches of the armed forces, Army and Navy and Air Force, immediately started making funds available. And they did not have to be for things that they were directly interested in. I'm quite sure about that because I worked for a year or two with the Air Force. It had been recognized that the scientists played a very important role in the war effort.

TSELOS: How much of your research after World War II was devoted to things related to synthetic rubber? Did any of that continue?

KOLTHOFF: Practically none. We did develop quite a number of interesting problems from the rubber program that had nothing to do anymore with rubber; induced reactions, for example. Several functions were, at least to me, and I think to you also, kind of new. Things like micelles and chain transfer agents--those things we could apply in our academic work. It is interesting that technical scientists would be used to great advantage at the time when the country would really need to have them go into practical applications. They never would have started anything in this field or any other field for which there was a shortage. There must have been other fields as well. Research that Meehan and I had been conducting shows typically how a purely academic thing can be turned to practical advantage. And not only me, but Speed Marvel. No, I think that the war has been--the rubber crisis has been--a good example of how scientists would be used by the government to great advantage.

TSELOS: There is a question that comes up here about the relationship between these political charges against you in the early 1950s and your government work? Did these charges by people like McCarthy and some of the other people ever affect your security clearance?

KOLTHOFF: Well, I don't know. I often got visited by some government person asking about trips to Russia, Yugoslavia, Czechoslovakia. I often got visited by someone, but it never dawned on me then, as it could now, that they suspected me.

TSELOS: Of course I suspect many of the visits were simply to get information from you about your trip, rather than....

KOLTHOFF: That's the way I always took it.

TSELOS: I was going to ask you a rather general question. Why have almost no analytical chemists ever been awarded the Nobel Prize in chemistry? That's one thing that is rather noticeable as you look over the fields of people who received the Nobel Prize. I think that you've really gone a long way towards answering that question in terms of your comments about the long-standing prejudice against the field.

KOLTHOFF: Well it is not only that, you see. I really think that you must make a big discovery or develop a new technique that will find a very general application. It has to be something which...now, like you have in the medical field, it is easier there to answer it than in a science field, but it still must open a new avenue to follow. I don't think that there will be...well, there is an analytical chemist who is a Britisher and got the Nobel Prize.

MEEHAN: Martin, I think.

KOLTHOFF: I think he's the only one, although I don't know if he calls himself an analytical chemist. I should have asked him when I met him once. Another analytical chemist may win a Nobel Prize if what he develops has broad, general, and new applications.

MEEHAN: Well, the style is changing now.

TSELOS: You've received quite a number of awards, Nichols, Fisher, Gibbs, and so on. Would you single out any one of these as having been particularly gratifying to you for certain reasons?

KOLTHOFF: They are all gratifying. It is difficult to answer your question because I would think that the amount of satisfaction one feels is determined more by the quality of the group of people who have gotten it over the years. This determines whether it is an honor to be a member of one group or not. I would think that people who have received the Gibbs Medal will have gotten a few of the other medals as well. I think that the

committees go over the names and reputations of the candidates very carefully. They have plenty of difficulty in deciding who to honor.

TSELOS: Thank you very much for your insights and comments about your long and productive career as an analytical chemist.

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