TRANSCRIPT OF I. M. KOLTHOFF INTERVIEW -- MARCH 1983

INTERVIEWER, DR. HERBERT LAITINEN

(Requested by American Chemical Society for posterity)

L: Well, Piet, I brought along a book that I'm sure you'll recognize.

K: I do?

L: Yes. This is Ostwald's famous book.

K: Wilhelm Ostwald.

L: Yes. Do you remember the occasion that you got this?

K: Yes, it was many years ago. I was a sophomore in the University of Utrecht (Holland) and went to a sale; I saw twelve books on a chair and one of those books was the book by Wilhelm Ostwald "Scientific Fundamentals of Analytical Chemistry" (translated from German). I bought the 12 books for one Dutch florin, which is still the same as it is now, 40 American cents.

L: (Chuckles)

K: The other books I didn't keep, but this has been a book which I have studied and studied.

L: Well, that's obvious from all the notations. I notice there are some in English and some in German and some in Dutch.

K. Probably some in Dutch. Yes. Because in 1912 I had no idea that I ever would settle in the USA.

L: Yes, depending on when you happened to be reading it.

K: He really opened my eyes that analytical chemistry was simply following exact procedures, how you precipitated, how you washed, how you dried, etc., but not a word was said of solubility product, hydrogen ion concentrations... partly, I guess, the reason was that physical chemistry really was born only in the 1890s. There had been quite a few studies made before. I think that when I got the electrochemistry Palladium award I referred to Clausius who in 1853 (Poggendorf's Annale) wrote an article about electrolytes in which he said, "When the current passes through ..." and he used these words, "cations move to the cathode and the anions go to the anode.." and further "as a matter of fact, these particles ..." and I got quite excited when I read that, .."these particles are also present when no current flows through." So, actually, the Arrhenius theory was given. Bryce Crawford gave a talk on "The time was not ripe"; well, I guess the time was not ripe to say that electrolytes did dissociate into the ions.

L: There is something that has always puzzled me about this and that is that, if I remember well, Kohlrausch's work on conductivity preceded this by a couple of decades, back in 1879 or something like this. And how was it, then, that he did such exact, meticulous work without really understanding apparently the ionization --- or did he, in fact?

K: Yes, he did to some extent.

L: Yes.

K: Because, coming back to Ostwald, Ostwald had helped Arrhenius in getting his degree, because his professors in Stockholm didn't want to accept that theory. It is very strange, as I say, Arrhenius actually mentions the name of Clausius, that is the reason why I looked it up. I was studying all these articles in the Zeitschrift für physikalische Chemie. Ostwald is famous for all the writing he has done, many books on history, etc. and they were interesting. In the first volumes of the Zeitschrift für physikalische Chemie Nernst wrote several classical fundamental papers on electrochemistry and physical chemistry in general.

L: Which, by the way, brings us to **the point that Nernst** is not mentioned in Ostwald's book.

K: Well, the strange thing is that Nernst worked in Ostwald's laboratory and at the end of his article on the Nernst equation, he acknowledges that he -- Nernst acknowledges the information, or help or discussion, I think he gratefully mentions the discussions that he had had with Herrn Geheimrat Ostwald.

L: I think you said once that even in the later editions of the book Ostwald never mentioned ...

K: Never mentioned Nernst. He may have had a jealous feeling about it.

L: Maybe he was just too good or so ---

K: Maybe. Maybe. Nernst might have been a little bit too much convinced of his superiority.

L: Yes, wasn't that true of some of the early physical chemists? I think you mentioned this, their antics at meetings ---

K: Yes, it was in 1924, I went to a meeting of the Deutsche Bunsen Gesellschaft and the first lecture -- Bodenstein was the president, Bodenstein was famous for his kinetics etc., and he asked at eight o'clock a.m. when the meeting started - and it was an overcrowded program, Nernst was the first lecturer - that nobody was allowed to talk longer than 20 minutes. Nernst disagreed with the Debye-Hückel theory and he talked and talked, and Fajans, who was the second lecturer (you never forget these things, they stick to you) Fajans argued with Nernst. Nernst had determined the heat of dilution of lithium chloride, I believe, and it didn't check with the values of what he should have found according to the Debye-Hückel theory. And Fajans had done something similar. Before he started his lecture there was a little arguing and when he started the lecture Bodenstein said,"Please, will you think of the time, you have twenty minutes, we are late already." And Fajans said - I must say it in German - "Ich werde allerdings nicht so lange sprechen wie der Herr Geheimrat."

(I will not speak as long as Herr Geheimrat Nernst has spoken.) So there's where I learned to know Fajans. Later, in the Hitler period Fajans went from Munchen to Michigan.

L: I was wondering if you could go back a little bit to when you first started in the university. I believe the story was that you had to get a degree at the Technical University (in Delft) was that it? Because of your background ...?

K: Well, yes, it is a kind of a relatively uninteresting story, but it explains why I went to the University of Utrecht. When you came from the kind of high school I went to, and I was always very very grateful that I went to that school, the only trouble was that when you wanted to register in a university you had to have had Latin and Greek. And we got modern languages, we got sciences, three years chemistry, three years physics -- three years all sciences in a very thorough way. In the continental European countries you had Technical Universities, you had Veterinary Universities -- one is connected now in Utrecht with the main university -- but they still have an Agricultural University in Wageningen -- you see sometimes papers, especially in colloid chemistry from that university. So I registered in the Technical University in Delft but then I was told that I should take two years mathematics because there was no space in the chemistry lab and that would have been at least two years which I didn't want to do and so I learned about a professor of pharmaceutical chemistry who also was a professor of analytical chemistry, because analytical chemistry was not taught in any university in the School of Chemistry -- that was taught by my ex-boss, Nicholaas Schoorl (S_C_H_O_O_R_L) and, well, he was also professor in toxicology, I think it had a slightly different name, but the name doesn't matter, it really was toxicology, but they had it in a different name (bromatology). Then food analysis, because they got in that period, let's see, I came to the University in 1911 and they started in different cities to have food and drug laboratories to see whether the milk --- if e.g. water had been added or something else, whether the drinking water was good, etc. The lab directors had got their education in that pharmaceutical lab. And it happened that Schoorl had had the same education as I had had, but he had taken the two years of Latin and Greek which were required before you could register in a university. --- Really crazy.

In 1915 we moved over from an old lab to a new building, and we could go through a corridor to the Van't Hoff lab, that was the chemistry building where I spent quite a bit of my time, especially with Kruyt. Kruyt (K_R_U_Y_T) was the colloid chemist who would put a book in front of his window when he could see me -- funny you don't forget those things -- that meant, "O.K., you can come over, I'm not too busy." And we would talk quite a bit. He gave one year colloid chemistry and one year phase rule. Professor Cohen who was the favorite student of Van't Hoff, and who has written the most fascinating book about Van't Hoff -- it was out of print in my young days already -- I've read -- I think we had a copy of it in Minnesota-- it's a very, very fascinating book. But Cohen, professor in physical chemistry, looked down upon analytical chemists, he looked down, not on my boss, Schoorl, but I had nothing but trouble with him. And, I didn't take it very easy. I recall in 1917 I had written a paper in the Dutch Chemical Weekly on "The Use of Electrical Conductance to Get the Salt Content of Water"- which has mixtures of electrolytes - it was not a highly scientific paper, I should say that; the treatment was mainly from a practical viewpoint, but he told his students, "That article by Kolthoff, you shouldn't read that it is all nonsense what it contains." Well, immediately after his lecture was over all his students would come through the corridor and come to tell me what Prof. Cohen had said. And I went - of course I was pretty much upset - to see him and said, " Well, I want you to explain to me what is really wrong , why you have said this." Well, at the end he had to admit that he shouldn't have said it but he didn't say it in those words . And I said at the end, "Well, the least thing you can do is at the next lecture take back the words you have used and tell them that it was a decent article." Well, I said it probably in a less polite way; at least he went to complain to Schoorl about it, and I said to Schoorl, "Cohen can go to hell as far as I'm concerned .." Schoorl said, "You can't talk about a professor that way ..." You see, he was the professor again, and we usually had disagreements although we got along beautifully; he has missed me when I came to this country.

L: When you started your first experiments, I think you said something to the effect once that you had access to some physical chemistry equipment in a laboratory on Saturdays, was that it?

K: Well, it was --- I think what you were referring to is that I wrote a paper in 1915, and I think it was my first paper in the Dutch Chemisch Weekblad, on the dissociation constants of phosphoric acid. But we had no equipment to do potentiometric work. Schoorl helped me later to get equipment that I could use, e.g. a slidewire etc. When I was busy with that paper on the dissociation constants of phosphoric acid, I went every Saturday afternoon with my bottles, walking twenty minutes to the lab of Ringer who was the professor in physiological chemistry and a very precise experimentalist, precise in pH measurements; he had an excellent outfit for it. But then later I used the slidewire instead of a couple of rheostats and used a Lippman capillary electrometer. You watch through a microscope the surface of the mercury in a capillary and when there is a potential difference,mercury moves up or down. We also didn't have a good galvanometer for the purpose, so at the beginning the work was done with primitive equipment.

L: Well, now, it was about, earlier (1913) wasn't it when this paper of Hildebrand's came out and that inspired you. I think it was -- was it Schoorl? -- who suggested to you to read this?

K: Yes, he apparently found out that I was interested. He was interested himself, but with his busy life, and his favorite thing was what he hoped to do after he retired -- to sit down and do more microchemistry. Why he guessed that I would be interested, I don't know.

The first gravimetric determination I had to make was the sulfur determination as barium sulfate - and I'm not famous for having much patience - so I'm quite sure that I didn't wait every time when I washed the filter that all the water had gone through. I knew what the answer had to be, so I knew myself that I got a high value. I had to record it to be honest, and he took it apparently that I had been honest, and said, "Well, you could have got coprecipitation. You may read two papers on coprecipitation in the Journal of the American Chemical Society in 1912." Let's see, our friend and colleague, E. B. Sandell, in our book in 1936 we still refer to the coprecipitation paper in the JACS in 1912.

After gravimetric analysis we started volumetric analysis, and I didn't understand the color change of the two indicators phenolphthalein and methyl orange and liked to know more about it. Schoorl had written in 1904 a scientific paper on indicators which I didn't understand at all; but then he referred me to a paper which had become quite famous by S.P.L. Sørensen in the Carlsbad Laboratory in Valby (near Copenhagen). He had published it in 1909, I think in French, and he gave a German version in the Biochemische Zeitschrift in 1910. I remember that the German was more familiar to me than the French. Sørensen did all the fundamental work, the indicator error, the salt error, the protein error, and he developed a whole set of buffer solutions which for years have been used before they were replaced by the Mansfield Clark buffers. It was Sørensen who developed the concept of pH which is still used the present day.

L: Did you very meet Sørensen?

K: Yes, he visited in 1917, but Schoorl took most of the time to keep him in his office, but I was greatly honored to have him come and he impressed me by his simplicity about his own work. He was easy to talk to. He was a great person, S.P.L. Sørensen. And, as I say, completely unassuming and complimenting me on my work on acid-base indicators.

L: Well, getting back to this paper of Hildebrand, that was the one on potentiometric titrations of acids and bases. I remember that he had introduced a simple hydrogen electrode and referred to work done in Nernst's laboratory. But Hildebrand, of course, never considered himself an analytical chemist as he was a physical chemist.

K: Yes. But he wrote me a very nice letter in 1949 when I got the Nichols Medal. In my talk I referred to Hildebrand as one of my teachers. And I think this talk I gave was put in print in the C&E News. Hildebrand read it and wrote me a letter -- I never forget things like that -- "You found gold where others only found dust."

K and L: Chuckles.

K: We have remained good friends and I mourned his recent death at the ripe age of 101. But since 1949 we have been corresponding on and on. I went to his 90th birthday symposium and his birthday at the same time -- I didn't go to the 100th one and not to the 101st which he had last November. I also mentioned in the talk in 1949 Leonor Michaelis who had written two books, one called "Hydrogen Ion Concentration", of course in German, and the other, "Oxidation Reduction Indicators", which he abbreviated to Redox Indicators. I think I've told you that story with Mansfield Clark: I used the abbreviation when I was already at Minnesota. The title of the paper was something about Redox Indicators, and Clark as the referee said the paper was O.K. but I had to change the abbreviation "Redox" because it could be misunderstood as meaning "Red Ox".

L: (Chuckles) Red Ox.

K: So that was that.

L: Well, maybe this, uh, it would be a good idea to talk a little about that first visit you made to this country in 1924, was it?

K: I think that Charles Foulk was really the man who started that idea. He had been working in a -- in Ostwald's laboratory and visited me in Utrecht and said, "Why don't you come over and visit us?" I said that my salary was such that I hardly got enough to pay even to get over, and he said you can make some, we can find ways to have you talk to some companies and they can pay fifty bucks or something. Well, anyhow, in 1924 I did go over to a meeting in Toronto. From there I went to Princeton where I stayed not with Furman, apparently his house was at that time not suitable to have guests, as I stayed with Charley Smyth (the physical chemist), a man who did quite a bit of work on dielectric constants and so on. But I got well acquainted with Furman and probably the idea grew at that time that we might together get a book out on potentiometric titrations.

L: That was the first time you ever met Furman then?

K: Yes, that's the first time. Then I visited Ohio State University where Foulk was, and you should know better than anyone else about his "dead stop end point", 1929, because you were the man who really gave a quantitative interpretation in your thesis of the potentials and potential jumps, etc.

L: Did you then visit Michigan?

K: Yeah, I think I went there via Northwestern University because I could make a few pennies there. A man who manufactured candy had a problem: his candy became opaque. Of course, I said immediately that everything was due to a wrong pH. I forget now how I solved it, but he sent ma a "nice little present" for having done it and the "nice little present" was $25. I don't know how much money he had made by getting good candy. From Evanston I went to Michigan. I stil l recall that I was pretty young looking, just thirty years, but probably looked more like eighteen, that there were four gentlemen waiting because several people apparently were visiting Michigan -- the University of Michigan -- and I went to the four when there was nobody left on the platform and introduced myself. They looked pretty disappointed but we became on very good terms. Willard, I always think of Willard and Fenwick, he had the girl, Florence Fenwick in 1922, and they have written very first class papers. Well, I think since we mentioned the three names, Foulk, Furman, and Willard, there was a kind of friendship between us. It was only a year later -- and there was no thought in my mind that I would ever get an invitation to come to any university in USA -- that Willard and Mrs. Willard visited me in Utrecht. Mrs. Willard and I still exchange Christmas card but I sent the last one to the wrong address, at least I just got it back.

L: You didn't visit Minnesota that trip, is that right?

K: No, I never had been to Minnesota. No, when Furman came over in '27, the idea was we would work together for a year; then I got in '27 an invitation from the University of Minnesota.

L: Well, how did that come about? Because you never visited there, or did you know anyone in particular?

K: No, but I think they got some information from Kruyt. Kruyt, I think, was one of the earlier lecturers at Cornell University -- they have those three-month kind of lectureships and that -- I know because Kruyt told me later that he had been approached by S. C. Lind, who was the head of chemistry, also, in your time I think Lind was still the head of chemistry --- and he wanted to get more research done at the University of Minnesota. Because the graduate schools were of a young age -- and people like Sam Lind, he got his degree in Paris, Hildebrand got it abroad, and G. N. Lewis got it abroad --- I can give you a whole list of names, people who were leading American scientists, who all had gotten a degree in Europe, and Lind felt very strongly that we should give more scientific education, so he was on the lookout to get more good people who would be interested in promoting research.

L: Well, I wondered if Lind had a specific objective of making analytical chemistry more scientific or --- but not this special, just to promote in general research in ...

K: No, I don't think.

L: Just to promote in general ---

K: I had published quite a bit so that would make me a good guy to go on publishing.

L: Sure.

K: -- and no, all I recall is that neither the president, nor Dean Ford of the Graduate School -- he was Dean Ford at the time -- your time too -- wanted to see me. Lind and Dean Leland -- Leland was the Dean of what later was the Institute of Technology, where Lind became first dean.

L: Yes, he was dean when I entered there.

K: Well, the two had sent that cable to me to ask me to come for a year. But at Minnesota they had had bad experiences with a Scandinavian professor who was in the Medical School, who couldn't get used, I suppose, to the fact that the people who did the common work (janitors) would be treated politely, as human beings and not in a haughty way: "you do this..." and if he (or she) didn't do it at the same time they'd get terribly mad and rude. Anyhow, the President didn't want to have another European coming. Until close to Christmas, I had been giving a talk in Wisconsin and was asked to go there by, I forgot the name, it may come later, the head of chemistry at that time -- you might know his name, but I ---

L: Sure. Oh ---

K: It doesn't matter, it probably will come later. Anyhow, I went to Lind at that time and said, "Well, I'm not going to tell them in Holland ---

L: You are speaking of Ferrington Daniels.

K: Yes, well, referring to Daniels -- but he will not -- he had quite a bit to say, he was a famous physical chemist, Ferrington Daniels. The name sounds so familiar that he could have been the head of chemistry. It doesn't matter very much who it really was ---

L: But I recall that Meloche... Well, it wasn't Meloche, was it?

K: No, Meloche wasn't there yet. No, I think they were looking out also for someone who would do some research. My appointment was for one year, with the understanding they could say after one year, you can go home, and I could say, I'm going home. Then, I said to Lind (in December) I'm not going to wait until May or June and then tell Holland I'm not coming back. If you can't make up your mind,or anything like that... and he said, no, I want you to stay, we can make a formal agreement, and to make a long story short, it was on Christmas Eve afternoon that I had a meeting with President Coffman who wanted to know if I'd like to stay, etc. Later we became very friendly, Coffman and I, we played billiards together and it was very pleasant. So this thing was really settled at that time that I would stay. But I still was of the opinion , or rather had in mind, that I expected to stay for a couple of years and then go back to Holland. And in 1931 it was almost settled that I would go to Amsterdam. I never talked about that, but as you described in the I.T. paper on my 85th birthday that "the good professor" would always leave for Holland in the middle of June and come back towards the end of September, not mentioning that I had a special trunk made -- the top of it was full of books and work that I could do. When that vacancy in Amsterdam came to naught I settled for good at Minnesota. The last time that I have seen Schoorl alive, and you know the tremendous respect that I have had for the man and the gratitude that I have had for him -- he was retiring in 1941, and in '39 I left just two days before war was declared, one day before Hitler moved into Poland and I was then in a hurry to leave. Knowing that Schoorl would retire, I told him "When you retire, I would be perfectly happy to accept an invitation to become Professor of Analytical Chemistry, but it should come from the chemistry department, and he said, "leave that to the faculty, I don't think they'll change that. But if we keep things as they are I would prefer to have someone else (E. H. Vogelenzang, a good friend and student of mine ; he died last spring but for all the years that I have been here, Vogelenzang and I have been very good friends. He was more pharmaceutical but actually he got his degree in astronomy, a very strange combination, a very good scientist, but that's the last time I saw Schoorl, who died in 1942, and I never heard about it until after the war because there was no connection anymore --no mail or any other information came through during the war. And one of his ex-students, who had also become a Professor of Pharmaceutical Chemistry -- I didn't care much for him -- but he wrote an excellent article about Schoorl, and I learned that Schoorl had had cancer, it was a terrible death and he suffered terribly. At the time when he retired, and he hoped that he would go sit with the microscope and do all the work that he had planned to do -- but they asked him to write (in Dutch, of course) what the main work was that he had been doing. I have a copy of that article and also a copy of the obituary which I only got a few years ago, someone wrote an obituary, and it was very good. I think it was eight, nine years ago in 1975 that the Analytical Section of the Royal Netherlands Chemical Society celebrated its fiftieth anniversary, and I had been instrumental in my young days to initiate that Section, and they asked me to give a talk and I used this opportunity to talk about, for half an hour, about Schoorl -- he never got the recognition from the chemical side for all the good and fine scientific things he had done.

L: Well, I think this might be a good time to get into this aspect of introducing really a scientific spirit into analytical chemistry, because I think if your students were to be asked what is your real contribution to science, it would be that. That you are foremost in promoting the scientific approach to analytical chemistry rather than the simply empirical one. But, well, for example, one way I would -- you could -- introduce this is the kind of work that you were doing back in Holland and continued at Minnesota on precipitates -- the thing that characterized that was that it was not just devoted to a single determination, one at a time, but rather to the principles governing a whole class of precipitates, for example. And the fact that, really, that work was not considered acceptable in the traditional analytical journal and you had to publish in other ---

K: We published quite a bit in Z. Anorg. Anal. Chemie for organic and inorganic chemistry

L: Sure, and in J. Phys. Chem.

K: We had a few in the JACS, on electrometric titrations ---

L: Oh yes. Of course, in those days, they did publish some analytical papers, but I was thinking even of the Analytical Edition, when it was first set up in 1929.

K: It was pretty old-fashioned, the early edition, I resented the title, "Industrial and Engineering Chemistry, Analytical Edition".

L: It was, indeed.

K: Or the fact that each paper had to have a procedure, or an analysis, that was part of the directions to the authors. And -- your papers didn't have that.

Well, but what I will say tomorrow very soon after starting is that, if I put the clock back fifty years, and if Proctor and Gamble had given -- had made the award possible fifty years ago , then there would have been slightly less than 25% chance for me to be the first recipient because Furman and Willard, and I think, well, Ernie Swift certainly should be mentioned, he wrote books, outstanding scientifically speaking, and then, of course, the man at Ohio State University, who was Charles Foulk. So there were several of us all working to get analytical chemistry recognized as a scientific discipline, as a science you might say. I recall, for example, at an ACS meeting I was told -- I was giving the first talk in the afternoon at 1:30 -- I was told just, oh,five minutes before, that Furman had been the first one to get the Fisher Award. That was the first one. I was quite happy to announce it. I don't think there had been ever any jealousy - I used to spend every year, when I made the trip to Holland, I always stayed a day and a half overnight in Princeton, because I reacall that Furman took great pride in it -- he had a student -- I never have mentioned that to anyone -- not to you -- they adopted the names and they put that on their door, the students in the place where the university --if you have a moment, it's necessary to find the right word for it -- where they all lived together like in a dormitory -- yes, that's the word I wanted to get -- and when one had adopted the name Kolthoff, Furman was very happy with that. There was no -- I don't think there has been anything like jealousy between the two of us. I should include Willard also and Foulk.

L: Well, of course, in later years I got to know these two very well, too, and I consider them very good friends. Willard for many years went to meetings, even after his retirement and enjoyed just being around the analytical scene.

K: Well, I thought that it might be -- there may be, in all probability there should be, some descendants of Furman and some descendants of Willard in the audience, and I think they will enjoy hearing that there was that understanding in that we all worked for the same purpose to get recognition of analytical chemistry as a scientific discipline. So, I don't think it will be out of place -- that it's a proper thing to say something like it.

L: No, certainly ---
Well, looking back now at your 60 years or more, you have of course had several different specialties within analytical chemistry. I mean, your early work on -- you mentioned pH and buffers and potentiometric, and the precipitates and so on. I think you have an especially good story to tell about the time that you really entered electrochemistry and polarography. That was the famous trip of Heyrovsky, yes?

K: That was quite a bit later.

L: That's right.

K: What is his name, you mentioned his name ... I'll fill it in ... I've written it down because I have trouble with names, --- Vic Meloche -- he had a polarograph, before Jim Lingane had finished his work using a polarimeter. We didn't have a polarograph, because it was only in 1940, I believe, we got one. It was strange enough, because in 1924 I had a Czech , Oldrich Tomicek, who became later a professor in analytical chemistry at Charles University in Prague, but he worked with me for a year on potentiometric titrations in Utrecht and since that time I had been going to Prague every two years, so I became acquainted with Heyrovsky, but at the time I never realized he would probably show me something about the analytical applications of polarography.

L: You are speaking of Heyrovsky ---

K: About Heyrovsky --- I really think that I suddently became very convinced that he had a big thing in 1933 in the famous week that Otto Hahn and Heyrovsky visited together for a week in Minnesota and I said to Jim Lingane that I really thought we should study the fundamentals and do something about it. I don't know what he is doing but--- (referring to the technician on the side of the set)

L: Well, I think maybe he is suggesting that this would be a good time to take a break.

K: Good. We have been talking for quite a while.

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L: We were talking about Heyrovsky's visit, and you mentioned that Otto Hahn visited that same week. Well, I'm thinking that -- this brought to my mind -- was the fact that you had used tracers, radioactive tracers, in several of your precipitate studies. And was -- to what extent was Hahn an inspiration to you in starting that? Or did you start it long before that time?

K: No, Hahn was interested in adsorption on precipitates and used the radioactive method also for that purpose. I think with Charles Rosenbloom we had shown --- well, a rather interesting effect that if you precipitate at room temperatures, say, a 0.1 molar sulfate with 0.1 molar lead and added thorium B very soon after the precipitation, say, after one minute or so, and with shaking for a short time the thorium B distributed itself as if it was a liquid, forming a mixed crystal, a homogeneous mixed crystal. So, it meant that after formation of a precipitate, that very <u>rapid</u> formation of a precipitate, that you will get quite a few recrystallizations before you get normal crystals. Hahn was very much interested in that particular effect, and I still recall that he also came from Cornell University; the few people I refer to apparently all came from Cornell University after the three-month lectureship that they have.

L: The Baker Lectures ...

K: Yes, the Baker Lectures. Yes. And I recall that I picked Hahn up at the depot, the Great Northern Depot, in my car, and that they had written to me from Cornell, "Don't touch any politics with him because he'll blow up like anything." So, I thought, well, if he feels that way about the Hitler situation, I'll simply avoid it. But, I hadn't driven one block, I believe, when he showed me a cable that he'd gotten from Max Planck, which read, "Please don't go to California" -- that was his plan to go from Minnesota to California -- "but come back as soon as possible and save whatever can be saved." I used to get from Hahn, I think every year, around New Year or Christmas, a card, and he wrote in German then, "How often (wie oft) denke ich an die schönen freien Tage ..." (How often do I think of the pleasant free days of that week in Minnesota). But he had to dismiss the woman to whom he owed quite a bit, Lise Meitner; she was his right hand -- there's still a question as to how much she is responsible for his atom splitting. He had to dismiss her and that did quite a bit of harm; not quite a bit of harm, a tremendous harm that Hitler has done to Germany. I recall Hahn mentioned two names of people I knew --- one I knew quite well later -- but that is Haber and Freundlich who shared-- Haber was the head, the director of what was at that time the Kaiser Wilhelm Institute in Berlin-Dahlem, close to Berlin, and Freundlich worked in that Institute, that both left Germany because they had to dismiss all their Jewish workers and they didn't want to do it, Haber went to Switzerland, but apparently he never recovered. I often thought of it and Freundlich -- here I must come back to my annual trips again to Holland. Whenever I came through London, often got off at Southampton then to London, I could take not the ferry but the boat to Rotterdam. But, I don't know why, F. G. Donnan was always so kind to me -- he insisted -- he said, "Whenever you come, I'd like you to come in." And I recall that at that time Freundlich was --- Freundlich had gone to the University of London where Donnan was, but Donnan said, "We've got so many refugees that we can't offer him a permanent job or anything." I said, "Well, I think they would be very happy in Minnesota to get Freundlich, but I am still a Dutch subject and even though --- the Rockefeller Foundation had told me in the early Hitler days, "If you know of any people who would be an asset to our country, then please get them." I had something to do with Fajans coming to Michigan. But I had to be kind of careful, I said to Donnan, I'm sure that we'd like to have him and I'll get Ross Gortner, who was a member of the Academy (NAS) -- the biochemistry building is called the Gortner Building -- so he was quite a man, a prestigious man, in Minnesota. I'd rather have him do the work to get Freundlich invited. You know Heller? He was --- isn't that the name, Wilfried Heller? He was in the Chicago group.

L: That's right.

K: He died in a car accident.

L: Yes, this last summer.

K: I didn't know that, but he asked me to write for the Colloid Section of the ACS, which had a hundredth birthday celebration, about Freundlich at Minnesota --- but --

L: Yes, Heller wrote a biography of Freundlich that was published.

K: Yes, it was a kind of a booklet, -- little booklet.

L: I remember ----- quite well in his last days -- several years -- in --

K: Yes, that was at the time.

L: In fact, my oral exam was the very first one that he had attended, I remember ---

K: Really?

L: Yes, he was a very -- he lived up to his name, a very freundlich----

K: Yes, a very fine -- a real gentleman.

L: But did you have anything to do with Debye coming over?

K: No. The only thing that I recall, well, I saw quite a bit of Debye and at the time (1940) he wrote to me "What should I do with my citizenship?" And I answered him in Dutch, of course, he wrote me in Dutch, "Take your first papers out because you have to wait five years before you can get your second papers and then two years you have to wait, but you don't bind yourself to anything by taking the first papers out -- do it right away." Which he did, he was kind of grateful for the cheap advice which I gave him (and he became) I think that was the main reason why Debye left Germany (he was still a Dutch subject). He was a professor in Utrecht when I was a freshman. He was a very young professor at that time, I think he was 28 or 29, one of the youngest professors they ever had. The other professor in physics who was more experimental and less theoretical, made life pretty unpleasant for Debye and after a year he went to Germany, but he didn't want to give up his Dutch citizenship and particularly not in the Hitler days and then things became tight and they wanted to force him to take German citizenship and that's the reason that he left but he immediately realized that he wouldn't go back anymore to Europe and became an American citizen.

L: Well, this bring us, I think, rather logically to the rubber program days of Debye, uh, would you like to recount how it was that you became involved in the synthetic rubber program?

K: Well, I couldn't say exactly how I became involved in it. I know you, Herb, helped me considerably when I was after my operation for three months in the hospital in pretty bad pain and you came over once and told me about the determination of peroxide in soap that gave me the idea to get to induced reactions. I think one of my students, Avrom Medalia, got his Ph.D. on the hydrogen peroxide-ferrous ion reaction.

L: Well, I remember -- Of course, from our viewpoint, Speed Marvel was the one that moved us into the polymer program, but it never was quite clear to me how you and he at the same time --- did he involve you or the other way around? Or both independently?

K: Well, I never found out. But, of course, there was the point, we didn't know (Ed Meehan and I supervised together) and we didn't know what the problems were. We went to Goodrich and to Goodyear and we went to U.S. Rubber, you did the same, and they probably told you the same. They didn't want the university people to stick their nose in their business. I really planned to write some thing about it after the war; they didn't behave like good citizens. They only would tell us that "We don't need you because we have a committee on the purity of the butadene, we have a committee on the purity of styrene.." So, there were no other problems. The problem really came with the mercaptan, you know.

L: Yes.

K: Walter Harris, a candidate for the Ph.D. developed the voltammetric titration of mercaptan in latex.

L: Yes, well, it was a beautiful example of the cooperation between universities and industry in the end. I mean, it was a little slow in getting ...

K: And, yes, it became much better the longer it lasted.

L:Because I remember so well the arguments we'd get in --you would get in with people like Kharash -- Oh, I remember the argument about the role of oxygen and whether this was a retarder or an inhibitor. Remember that? I still remember that you ---

K: That was the organic chemist from Chicago, not Harkins, but Kharash.

L: From Chicago, no ----

K: Heller went to wiith with Harkins ...

L: That's right. His name slips me just now, I'm sorry.

K: A well known organic chemistry, Kharash, he objected always terribly about the kinetics, not realizing how in very dilute solutions the kinetics would change if he would mix larger quantities together and would get a faster reaction than when you mixed much more dilute solutions.

L: I think, yes, there was a sampling problem here, too, that they didn't get really valid analytical results.

K: Valid, yes. The complaint was that the mice nibbled on the rubber which contained antioxidant. Bill Baker of the Bell Labs said - and this was meant as a joke, that the mice and rats liked the synthetic rubber so much .... now, how do you call those animals again, mice and rats in general ...?

L; Oh, rodents.

K: Yes, rodents. Bill Baker said, "Next meeting Piet will have developed a rodentometric titration."

K and L: Laughter.

K: Funny, those things you don't forget. Bill and I became really good friends.

L: That was a period of strong personalities and some jealousies, I guess, but still there was tremendous cooperation.

K: Yes, Harkins was rather mean.

L: Yes, he could be.

K: Very mean. He had attacked one of our theoretical interpretations in a mean way. I recall that I said to Ed Meehan, "Ed, I have too much respect for what he has done in his life but I wouldn't take it lying down and I would probably regret later to say pretty mean things-- why don't you give a talk. As a matter of fact, people thought so much of Ed Meehan, of the way he would present the things, that they usually made him the first morning speaker. And Ed always did the perfect job in talking about it.

L: Well, I remember, McBain for example, and Harkins, getting into wild disagreements about ---

K: Yes, I think McBain is the man who introduced micelles, and then he sent a wire to Harkins. Harkins was a kind of intolerant man in recognizing McBain's role of micelles in the production of rubber.

L: That's a pity because Harkins himself had done a great deal, but he -- he resented bitterly the fact that, for example, Langmuir had gotten the Nobel Prize for his surface work and that he had not been recognized. And well, he made no bones about it. He complained right and left. Ed Meehan and I once stopped there to visit Heller, and in fact we saw Harkins and Harkins ushered us into his office and he had pictures on the wall showing various discoveries that he had not received credit for. It's a really remarkable thing, because we were just young assistant professors then. We were nothing in terms of influence. But he made his position known.

K: I recall that I visited Chicago, or the University of Chicago, in 1924 when I was on a lecture tour.

L: Yes?

K: And Harkins went out of his way to be very nice. He really went out of his way, and I was very much impressed, of course, I knew his name by his work, and I felt extremely flattered until I got this mean attack on our work in the rubber program.

L: Well, G. L. Clark had worked with Harkins ..

K: Oh, no?

L: Yes, Oh yes, he did his -- that's where he learned his X-ray work. And, of course, had a very long career then.

K: W. Mansfield Clark?

L: No, G. L. Clark, my former boss at Illinois. Well, OK ---

K: Was he the head of chemistry?

L: No, No, he was --- he was the head of the analytical chemistry division, until I succeeded him in 1953.

K: I don't know that name ...

L: Yes, Oh yes, he did -- Smith and Clark, you ---

K: Oh --

L: Well, it was a curious thing because Clark, being an X-ray specialist, was not really recognized as an analytical chemist. So I've said in more recent years, I think, that he was a generation ahead of his time because he turned out a good many Ph.D.'s who didn't then work -- they weren't called analytical chemists, they were called physicists. But in fact, they were doing analysis of a very important kind and looking at X-ray structure and various things.

K: Well, I think you were to ask me about the recognition of anlytical chemistry, although you got the degree yourself, not in analytical chemistry as a major but in physical chemistry --- that analytical chemistry was really considered ---- a good analytical chemist was supposed to be a good analyst and do the analyst work, and was not consulted for any scientific solution and there would have been plenty of problems in the various industries -- Vernon Stenger will talk about that -- what kind of type of work they asked him to do -- that would be work by analysts. I think it explained the fact that I asked McDougal if he would accept majors in physical chemistry and minors in analytical if they would satisfy whatever the requirements were in physical and minor in analytical chemistry, and allow me to be major advisor. I think this was the situation until the end of the war, and, maybe, I think that Stan Bruckenstein is still an exception. I think, maybe, he got a major in physical, I don't know.

L: But things certainly did change with respect to analytical becoming a science in its own right, special due to you.

K: Well, I think that McDougal has been a great help to all of us and that you and I would go down and he would help ~~us~~ with the thermodynamics and that was so with several candidates that they satisfied the requirements for the physical and were interested in the analytical things, but not to become the "determinator" -- this word originates with Lundell.

L: Yes, I think he did distinguish between the analysts and determinators. That analysts deal -- dealt with real samples and ----

K: Well, yes, someone can make --- can make a real analysis.

L: Yes. Well, I was thinking now that looking back from your perspective of these years and so on, what is your feeling about the future of analytical chemistry?

K; Well, I was afraid that I would get a question like that. I haven't given it much thought, except, if I may remain personal for a moment, my interest always has been in the solution chemistry, which was of importance for analytical chemistry. And I don't think that we should say that the chemistry is getting out of analytical chemistry, that I think the solution chemistry is no longer playing the big role which it has had for so many years, but that chemometrics becomes more and more important and more applied. Now I believe, in quite a number of problems - also in the Philips Labs, not the Phillips, but the Philips in Holland in Eindhoven -- a big company -- how they appreciate their analytical chemists because they had to consult them over and over and over again. And that was mainly for things which were more or less chemometric but also real industrial problems. So, I would think that several of us -- you have been interested in the solution chemistry -- and on that basis I knew the physical chemistry and applied the physical chemistry to the problems in analytical chemistry -- that there's a gradual replacement in their use to analytical chemistry and it may change, the overall, well; the science will become more and more the science of measuring things. But, it still corresponds to the definition of Ostwald -- that an analytical chemist makes use of properties -- properties, that is emphasized by Ostwald over and over and, of course, now properties, how a surface can be analyzed, and what a composition of the surface is and all these things which are probably very interesting; they are outside my own field, I can't grasp all those things anymore and stick mainly to the solution chemistry. But I think that the science as such can be maintained, if the understanding of what is being measured is realized. What is your own opinion about it?

L: Well, this is about you, not about me ....(chuckle)

K: Well ... (chuckle)

L: Well, I did want to cover another aspect of your work, and this I think is rather remarkable that it wasn't that long ago, really, in terms of your total career, that you became interested in these crown ethers and so on. And I wondered if you would care to tell us how you got interested in this field and --- so on, now we haven't talked about non aqueous solvents, it's a major field in itself. But I think that sort of overlapped, didn't it?

K: Partly, because from the little I had read about crown ethers it impressed me that the alkali metal ions and the alkaline earth metal ions are so easily complexed with some crown ethers. I think that was the thing that particularly impressed me. As a matter of between you and me and the doorpost, I thought that tomorrow morning I would start saying a few words about crown ethers and a new analytical application of them. A referee of one of our papers thought it was a good paper, but he wondered what analytical chemistry had to do with it ---- I didn't mention that probably, but I may start now for a few minutes to say -- well, about our latest work is --- and I think it is quite worthwhile to mention that, because it is an important analytical application of the physico-chemical properties of crown ethers.

L: Yes, but for the record though. I mean ---

K: Oh, well, what impressed me really only recently, I started to realize what Pedersen, the man who really just before he retired from Du Pont - but for a different purpose - he made the crown ethers and he made, I think, a hundred or something of them. And all the crown ethers have a cavity, they are all macrocyclic molecules, and they have a cavity and in the cavity are ether oxygens, and we call them crown ethers because every oxygen is connected in the simplest crown ether with two CH_2 groups. If it has six ether oxygens in it and then it has 12 CH_2 groups, that is six times three atoms is 18, we call that 18-crown-six. It happens that 18-crown-6 just fits a potassium ion. Pedersen made quite a few applications. For example, salts--- I think he used as an example potassium permanganate -- if he imprisoned the potassium into 18-crown-6 it would become soluble in benzene or other organic apolar solvents. I don't know, I was thinking about it, and it really means that the crown ether is still hydrophilic in the sense that these oxygens, when they are not being bound by ion dipoles interaction with the ions which they complex that it is hydrophilic, but it becomes hydrophobic once you put that ion into it. If that is true, then it should be possible to do the opposite thing -- instead of making the complexed salt more water soluble it should become less soluble in water. When the salt contains a lipophilic anion and the cation is complexed in a crown ether, it can be extracted in an immiscible organic solvent. Ion-selective electrodes have been manufactured based on the above property.

The new analytical application which we made is that an acid with a lipophilic anion - like dichloropicric acid - in aqueous solution precipitates when H^+ is complexed with 18-crown-6. Doyle Britton is making an X-ray of the crystals of $L(HPiCl_2)_2$ (L is the crown-ether). Bartsch has published a paper in the JACS only a few months ago in which he made from nitric acid $L(HNO_3)_2$ and from various other acids he made an LHA complex -- by precipitating it in organic solvents with diethylether. You can precipitate $L(HPiCl_2)_2$ in water. I said, well,let us take the potassium salt, which is already relatively slightly soluble in water -- potassium dichloropicrate -- it is 0.015 molar soluble, and add 18-crown-6 ether to it and got a beautiful crystalline precipitate; not specific because sodium fits less, but still does it. Lithium does not do it but rubidium and cesium and alkaline earth ions -- they all can be precipitated as $LM^{n+}(PiCl_2)_n$. I want to try a few more hydrophobic and lipophilic anions (dipicrylaminate is one) and probably a few more, but I'd like to write a Communication to the Editor about this precipitation because it is a very logical thing this should happen, but I don't know, it had taken me a couple of years before I really started to apply the above principle.

L: Well, it is good that you can still get an excitement out of chemistry ---

K: Yes, I still have that --- can get a great deal of satisfaction.

L; There is another thing I would still like to explore with you, Piet. I know that over the years you have taken more than the average amount of interest in,well, social and political events as well as scientific ones. And, I wonder if you'd just care to comment on what you think of the responsibility of the scientist in this respect. I mean, does he have a social responsibility for his discoveries? And if so, how should he exercise it?

K: Well, this is quite a question you know, Herb. I would think that if you work to get weapons of destruction that you have to consult with your own conscience as to whether you should do it or not do it. I don't think it is possible to give a definite answer because, why did I accept the invitation to work on the rubber situation and let all other work go; because we had to get rid of Hitler. And if we and our allies would not have succeeded, the world would be a completely different kindof world, whether we would have a communistic world or have a world under types like Hitler, then it might take a few hundred years before democracy would restore itself. So, I can fully understand that scientists who have worked together to get the atom bomb to work when they saw this first explosion in New Mexico they had the feeling "Oh, God, what have we done!" I mean it was a thing of terror what had been created and , in a way, without talking politics but the talk is now about getting more nuclear weapons, that we should get, and I think that any scientist would feel that it is absolutely essential to get an understanding with Russia, not only about the nuclear weapons, because it will involve also an understanding that war should become an impossible thing. Because if it is a last desperate effort to save your country, you are going to use anything possible and use --- nuclear weapons. I definitely feel that a scientist must consult with his own conscience, that he will accept an invitation from the government to do things, well, there are so many things that a chemist can do -- the grass no longer will grow in the Far East, you know what I'm talking about -- herbicides have been used to make it impossible. I don't know, many of the things that the scientists are doing can be used for the benefit of mankind and can be used for the destruction of mankind. I don't think a simple answer is possible that ---- conscience ----

L: No, of course not. But we certainly could say that the scientist has a responsibility, I think, to --- to educate the general public, wouldn't you say so?

K: Yes.

L: As to, well, for example, the significance of nuclear weapons compared to conventional ones. But on the other hand, the question of environmental pollution by chemicals, that this can't be just generalized. Chemicals are not simple things, they are many things. And most people haven't the foggiest meaning of quantitative aspects of this. In other words, you can have a scare if you have anything approaching even background levels -- which is one extreme. And the other extreme would be ignoring the horrors of , say, nuclear war, at its extreme. So wouldn't you say that the scientist then has a certain responsibility to -- to educate the general popolation?

K: Yes, but I think there are quite different things that the scientist can do which can be used for the benfit of mankind and for the destruction of mankind. I mean, all this cloning work that is going on now, I think is a typical example how it can be misused for the destruction of mankind. And an outstanding man, without ever realizing it,can make it possible to destroy mankind by applying what he had done and apply it for the destruction of mankind. So, I think in a general way the scientist should take an interest in government ....

L: Well how should he influence the democratic process? Should he run for office? Or should he influence people that do?

K: Well, I think there's no objection if he would give up his work as a scientist -- who is the man, the chemist...

L: Charlie Price, perhaps you're thinking of?

K: Yes, Charlie Price.

L: He didn't get elected, though.

K: Hasn't he been once....

L: I don't think he has...there are others I believe who have...

L: Well, the question I ask is this: Which is more valuable--to have one, or a very small number, of well educated scientists and extremely good scientists, in a political body like the House--is that more important? Or is it more important that the whole House have at least a minimal appreciation for the methods of science?

K: Yes, I think it would be asking too much to tell a man who can do things which can lead to the destruction of mankind, that he has to give all his work up and go in the government, he should make it clear that his contributions could be used for the destruction of mankind. But why he personally would have to do it, after all, they can call him to a committee in the House or Senate, to explain what he is doing, what harm can it do, etc. and he can give them a clear picture of what it is. Don't you agree that that would be...?

L: Well, I've always felt that perhaps a more important thing than to have a few scientists in the legislature would be to have more training of lawyers who are the ones that make the laws, after all, and become our politicians; have them have deeper understanding of the methods of science and aims, and--and the accomplishments. Because a legislator, I feel is in a terribly difficult position in making decisions about things he knows nothing about. And--and this is especially true in the modern world of scientific matters. So somehow I think we should take steps...

K: Yes, well, if you see the behavior of the general public in Europe and more and more in this country about the nuclear weapons, that we have to get to an understanding and we are running fast to destruction, they are all big words and pessimistic, and I feel that it is almost unavoidable, unless it is really possible to get to an understanding. And now, of course, the questions comes up that we claim we, I mean, if the country claims, that we will not be the first ones to use nuclear weapons, and the Russians say it -- the Russians say they have many more weapons to destroy Europe -- than we to defend them -- so there has to be an understanding between the two superpowers that would make war an impossibility -- the thing should be settled someway, like, well, there are these organizations of which I am a member, the World Federalists, for example, have been, I think a member for as many years as it exists, and there are more similar organizations. The scientists can exert a direct influence. And the bigger organizations, like the World Federalists, should be able to exert some pressure on the government.

L: Well, what do you think of the role of scientific international organizations, such as IUPAC, do you think that's appreciable or negligible.

K: Well, I think at the moment, they don't have much of it. It would be, I think, very desirable to have an organization like ICSU, really taking a direct interest in the nuclear situation. Whether it would do any good, it's pretty hard to say, it will all become finally a government matter. Except that the public hears enough about it, you have seen on the T.V. probably, all these objections in Europe, Western Germany and Holland, processions of thousands and thousands of people who don't want to have a nuclear war -- well that certainly will have an effect on their government.

K: I think you would deserve the Nobel Prize if you could find a way to really find a way to make it impossible to have it happen.

L: Unfortunately, it's -- some of the popular demonstrations are not necessarily based on very solid principles...

K: No.

L: at times.

K: Well, also, people for their own purposes, to belong to a group, let us say, the Communists in Europe, it is quite possible that there has been a majority of a group who elects in favor of democracy to object strenuously to things which they feel can harm their things they believe in, but ----

L: Well, I think, just to close this, the message is that scientists don't have to be sequestered, cloistered in their own subjects, they have a right, a certain responsibility to be social animals, to take part in society.

L: Well, are there any sort of closing thoughts you would like to express?

K: Well, I find it kind of difficult to say something which is of any direct consequence. We have been jumping over quite a few things. I think we both agree that scientist should behave like a responsible citizen and act accordingly. That seems to be the only possible brief concluding remark I could make, and you may run in trouble, like I have run in trouble by thinking, in McCarthy days, express myself that such people like McCarthy can do a tremendous harm to the whole country by not only antagonizing the scientists, but trying to penalize the scientists who do things which ....

L: Well how about advice to young people? How about that--the coming generation--would you think that uh, you would encourage them to enter careers in science? And under what circumstances...

K: Well, this really brings up another question: How should science be taught? I mean in high schools, which is a problem that every country I think is struggling with; we are struggling with it and so they do in Europe.

L: Has it seemed true ---

K: I think that it would be a good subject for ICSU, for example, how should the sciences be taught?

L: Well, Tom Isenhour, today, in his Fisher Award address, referring to the problems of decreasing emphasis on science in schools in this country, and what could scientists themselves do about this. And I was just asking whether that same sort of trend is evident in other countries? Or is it peculiarly American?

K: Well, when I read the Dutch Chemical Magazine - it is published in Dutch - I can see that it is quite a problem in a country like Holland, and that means that Europeans are getting, I mean, the non-communistic democratic countries, are in the process of making the United States of Europe, and it is a more complicated affair than making the United States of America, but the languages will remain in the different countries and you still --- but you can see a goal, you got yourself in Finland, I believe for the Euro-Analytical Group, there's more and more European ---- and I believe, I don't know but I'm pretty sure that the Communists are also a part of the Euro-Analytical ----

L: Yes, Eastern Europeans are. The Russians were represented -- in fact, they have alternate meetings beween the East and the West of Europe, that is the general idea.

K: Oh.

L: Yes.

K: Well, the University of Finland, that is the choice for the fourth Euro-Analytical ....

L: Yes, that's right, that's the fourth one, there was Heidelberg, and then Budapest and -----

K: Well, that partly answers to your question also what scientists can do because there certainly will be talk about those things when they meet.

L: Well, do you think it is realistic to expect that some day there will be a united Europe, a United States of Europe?

K: Yes.

L: It has a long future ---

K: I think, I think it is a necessity almost because there is danger to have two superpowers ---- I am getting tired ----

L: Yes.

K: Two superpowers, yes. Well, there should be a superpower in Europe as well.

L: What about other places like Africa that has so many little tiny countries you lose track of them, isn't that contrary to long hope the trend for some day perhaps should be unification, but I mean on a democratic basis, rather than a colonial one. Do you think that there is any hope for that?

K: I would think so, because it is almost a necessity but there are so many problems involved in this thing, I mean, I'm thinking of the population growth, that can not go on multiplying as has been done in the past -- in countries like the United States and several European countries it becomes a habit that a family doesn't have more than two children for example...

L: I think that there certainly has been progress in a great many parts of the world now...

K: Well...

L: ... in this respect, so that it isn't hopeless thing to contemplate. Do you think terms of--say hundreds of years from now.. (BAD MICROPHONE NOISE) -- it will be easier ( ) I'm just curious to know?

K: Well, personally, I just keep my fingers crossed, but I don't have to worry at my age that there will be an explosion between the two super powers.... (Microphone Noise...) that... if it is true, I suppose it is true, but the

Communist consider it more or less a kind of religion that they have to spread their kind of Communism over the world ---it gave me the feeling that another Hitler is talking,that there has to come a kind of a human understanding that they can determine the way that they want to govern their country, but they can stay out of walking into Czechoslovakia, walking into Hungary --- the things that they have been doing --- and it is a very difficult question, I mean, here the talk we have, what should be done about Cuba---now El Savador, those things can all lead to a war. And there should be some way or other a world organization that should have the power to prevent this ----

L: Now that doesn't seem very realistic, now does it to have at the same time we managed to walk the tight rope now for some decades and if you want to be a little more optimistic you could say maybe that maybe that will deter us from the final, uh, explosion so-to-speak...

K: Yuh, I think both sides...

L: ...and gradually lead to a more unified democracy... Well, I like to feel optimistic and I also like to think in terms of the future for humanity for thousands of years not just decades, because when you think back, after all, we are only two thousand years now...

K: Yuh.

L: ... into the Christian era so it shouldn't boggle our imaginations to think there should be another two thousand...

K: Well, on the other hand, they have to realize that there is only place for so many people on this earth and that that is quite an important question if ---

L: I feel though that this can be solved, I think that we are relatively close to population control, that is, because I see no natural law that says that we must go to such an extent that we can't support ourselves -- I mean, sure, we might double but we can support that -- and I can easily see controlling population growth, but should we---?

K: Yes, but in the poorer countries, you are talking about Africa, there you would have a great difficulty in convincing the people ---

L: Yes.

K: --- in that particular fun that they have in life than they can't have as many children as they would like to have --- it ---

L: Yes, but these are subject to change within a generation after all, I mean within a life time --- in other words, if you make certain societal changes then this doesn't become impossible. Now China is a good example, they are exerting a tremendous influence now to keep the population from going beyond a certain ---

K: Yes, there is a penalty, I believe --- that -- it is a good example, because it has to be done, I mean, the earth cannot provide indefinitely, the food ---

L: Well, I guess we getting near the end of our session; I don't know...

K: Yuh, I think...

L: ... if there is anything...

K: Yuh, I think we...

L: ... more to be said...

K: ...deviated quite a bit from chemistry...and analytical chemistry...

L: Well, I wanted to try to bring out the fact that your interests are rather far reaching...

K: This, I think, we all give quite a bit of thought when you read about the cloning and the possibilities, what can be done in a bad way, how can it be prevented; they have to leave it to the government of the country, and the government I mean not only the president and the cabinet ministers, but the House and the Senate , that they should be sure that they want to do the right thing in important decisions which go beyond the country but our relations with all the other countries. And probably, it would not be a bad thing to educate the school teachers to bring up problems of that nature in the school years that the children would realize that those problems exist and can become very dangerous for their whole future life... Wouldn't you agree? Have things like this discussed in schools, that that would be very important as they become free people to talk about it -- that they have the background....

L: Well, I think that we are in danger of becoming completely illiterate scientifically unless we do things like that, we cannot afford that in modern age. Well, Piet,I think that should button it up, and I enjoyed doing this with you....

(END OF TAPE TWO -- RUNNING TIME 55:22)