

## Chapter 42

### OTTO KANDLER (with Traudl Kandler)

Freising, Bavaria

March 15th, 1997

VM = Vivian Moses; OK = Otto Kandler; TK = Traudl Kandler

*(Editorial note: parts of this recording were unclear or incomprehensible. With the respondent's agreement, the transcript has been edited to alter these sections and so does not correspond exactly with the recording; the edited sections appear in italics.)*

**VM:** This is a conversation with Otto and Traudl Kandler in Freising on the 15th of March, 1997.

I remember that you were in Berkeley in 1956-1957 and you came in the middle of the year.

**OH:** In September, I guess.

**VM:** How did you come there? What brought you to Berkeley?

**OK:** I had an offer from the Rockefeller Foundation to spend a year in the States, I think it was in '53 when they offered it to me. In those days I worked in photosynthesis and I had some doubts about the correctness of Calvin's scheme which was not perfect in those days anyhow. *I did not like the idea that photosynthesis is a reversal of glycolysis because it involves so much ATP and it requires that the distribution of  $C^{14}$  in hexose is symmetrical.* This was not definitely shown in those days because the degradation was done with *Lactobacillus casei* which gives symmetry anyhow. Since I learned my biochemistry from Feodor Lynen I also used his method to determine the phosphorylation rate which was stopping metabolism by cyanide to measure the increase of inorganic phosphate which was supposed to be more or less equal to the *phosphorylation rate* a few seconds before. I transferred this *method* to *Chlorella* because the Chemical Institute was bombed *during the war*. Lynen worked in the Botanical Institute.

**VM:** In Munich?

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**OK:** In Munich.

**VM:** And you worked with him there?

**OK:** I worked not with him but next door.

**TK:** Because he had no institute; his institute was ruined.

**OK:** And Holzer was one of his assistants. I got good friendship with Holzer and I always looked at what he was doing. Then I heard in a lecture by Lynen how he made *phosphorylation kinetics*: every two seconds he took a sample. I was very impressed by this. When I saw his assistant *Holzer doing such experiments, just shaking his Erlenmeyer flask with the yeast suspension, taking out samples every few seconds and putting them in trichloroacetic acid*, I said this is not difficult; I can do the same thing with *Chlorella*.

**VM:** Probably not accurate, either.

**OK:** I did the same with *Chlorella* and this is why I came to this technique. I found out that inorganic phosphate pool goes up and down with light on and switching off the light, in *Chlorella*. I said this was a sign of light phosphorylation. I did this already, when was it? In '47.

**VM:** Long before Massini did it in Calvin's lab.

**OK:** Yes, long before. I published a paper in 1950. *At first* I just measured these changes in the inorganic phosphate level *during the light/dark and the dark/light transients* and then I used the cyanide technique. I was a little disappointed because I thought I should pick up a much stronger increase of inorganic phosphate *in the dark* with cyanide. It actually wasn't more than I got with *Chlorella* after glucose feeding. *If one calculates the phosphorylation rate on the basis of a stoichiometric coupling of photosynthesis with phosphorylation, one would expect a 3-4 fold higher phosphorylation rate during light saturation than in the dark under saturated glucose feeding. So I wondered why I could not pick up the expected much higher phosphorylation rates in the light. On the other, I was convinced that a light-driven phosphorylation exists, as evidenced by enhanced glucose uptake in the light.* It's converted to sucrose and other things. And so I thought, well, this glucose uptake is also a measure of photophosphorylation. The rate I calculated from the glucose uptake and from the change after cyanide stopping was about the same. And so I concluded that light phosphorylation is *a reality* but it does not support the full photosynthesis according to the Calvin cycle. If you use two ATP for each CO<sub>2</sub>, and you need an extra one to convert glucose to starch; *the phosphorylation rate in the light* should be much higher.

**VM:** Than you could generate by light phosphorylation?

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**OK:** Than you could generate by light phosphorylation, yes. Then in '54, I think it was, Martin Gibbs came to Munich and gave a talk in Lynen's seminar and told about his *Leuconostoc* study. I thought this is the method to use, to look if Calvin is right, that the *distribution of label in the freshly-synthesised glucose* is symmetric. I decided to spend half a year with Martin Gibbs to learn this technique and then to go to Calvin and to do experiments with  $P^{32}$  which was difficult to do in our country because it was still — we didn't have a military government any more but it was still complicated to get permission to work with isotopes.

**VM:** At that time, what was your position here in Germany?

**OK:** I was an assistant at the university. Each professor in our country in the older days had a couple of assistants.

**VM:** Who was your professor?

**OK:** My professor was in those days (you don't know him) was Leo Brauner...

**TK:** A botanist?

**OK:** Yes, a botanist. In the early days I didn't have *a full professor of botany* when I was a student. But then it was Leo Brauner and then it was Otto Renner.

**VM:** What was your own background? What were your academic studies before you did research?

**TK:** I'm sorry. It was Renner first and then Brauner.

**OK:** It was Renner first and then Brauner, yes. I took botany and chemistry for examination for the doctor thesis. I took physics because Gerlach, a physicist, gave an excellent lecture and so I went to him. My main training was talking with Holzer, the assistant of Feodor Lynen, and watching him.

**TK:** You didn't have a teacher, actually, because the Institute because after the war there were no professors.

**OK:** After the war there was no professor *left* because they were all by the Nazi party.

**TK:** And were all removed. We had not...

**OK:** The Nazis were actually Green/Red; it was a combination of Greens and Socialists. They believed in anthroposophy and so on, and *many were* vegetarian. I only realised this nowadays, how this really worked. Biology was a big thing during the Third Reich.

**VM:** So it was easy for you to study biology? That was a...

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**OK:** But we had nobody. Yes, there were three *young ladies who taught us elementary botany. We also* invited professors *such as Bünning and Egle* from *other universities*. I don't know whether you know Egle; he worked in photosynthesis after the war. But Purson, you may...

**VM:** Purson? Where? Is he here? Is he German?

**OK:** He is German, yes.

**VM:** No, I think this is before my time.

**OK:** Yes, this is before your time,

**VM:** I was never a botanist, I was a microbiologist, biochemist. It's interesting because many of the people who worked at the beginning in photosynthesis were all chemists, particularly the Americans. All the Berkeley photosynthesis people were really chemists. Only Van Niel — he was probably a microbiologist.

**OK:** A microbiologist; yes, he was a very famous microbiologist.

**VM:** So you planned to spend half a year in each of the two places?

**OK:** Yes, because I wanted to learn at first the breakdown of glucose, carbon by carbon, just to be sure that it really was symmetric; or if it's not symmetric. It was almost symmetric but not quite. It's complicated, you know the story, probably. Then I went to Calvin to do the  $P^{32}$  experiment. *I expected a very strong uptake and fast labelling of phosphorylated sugars but it was not as strong as I expected* from my cyanide work. When I arrived, I realised that Bradley had already finished his thesis but I didn't know that he worked on *phosphorylation*. Calvin didn't tell it to me. I talked with Calvin in Brussels at the Biochemical Congress in '54.

**VM:** Was that when you first met Calvin?

**OK:** Yes, I met him first in Brussels, I think it was in '54 or '55.

**VM:** He was in Europe in that summer of 1955. You arranged with him then to come to Berkeley.

**TK:** The International Biochemical Congress.

**OK:** Yes, the International Biochemical Congress in Brussels; I was there, too.

**VM:** And you arranged with him that that you would...

**OK:** I arranged with him then.

**VM:** Did he know about your own work?

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**OK:** I think he didn't recognise it. I told him about it but I think even later on he didn't recognise it, really, because he never cited it.

**VM:** Did he encourage you to come when you said you wanted to come?

**OK:** Yes. He was always happy to have someone come if it didn't cost any money for him. He was very friendly of course. I talked in Brussels about this phosphorylation business but I think he didn't get it to his mind what it means.

**VM:** I think there were times when he did not allow things to get to his mind that he didn't want to hear.

**OK:** He was not very happy when I talked in Berkeley about it. Only then when I arrived in Berkeley I realised that Bradley had done exactly the experiments I had wanted to try. I was very satisfied reading his thesis and looking at (*his results*). It showed there is not very strong, not very fast phosphorylation in the light — the *labelling of ATP* was very sluggish,. Of course, Calvin in the discussion always said, "Well, you know, there is a compartmentation in the cell and it just doesn't get into the chloroplast. This is probably the explanation why the exchange, the labelling, is so sluggish".

**TK:** It was never published, didn't you say?

**OK:** It was not really very well published (the Bradley paper). It was in the local...

**VM:** UCRL (*University of California Radiation Laboratory*) report.

**VM:** It wasn't published as paper? I just don't remember.

**OK:** I don't think so. At least I didn't see it. I read it in the report and in the original dissertation.

**VM:** I don't have a list of Bradley's papers but I have a list of Calvin's papers. Probably, if it had been published, he would have published it with Calvin. I will look it up when I get home to see whether it's there.

**OK:** There is one Calvin and Bradley (*paper*), but it's not very detailed on this subject. It's clear because it was difficult for him to explain it. He had to argue. If you like to push something, you don't like to argue too much! It looks as if you didn't know it really.

**VM:** So you arrived in Berkeley in September 1956, intending to stay for about six months? That was your plan?

**OK:** I think I never wrote him. I just talked with him in Brussels and all the other things were handled by (*Gerard*) Pomerat; this was the Fellowship Adviser (*of The Rockefeller Foundation*). I don't remember that I wrote a letter to him.

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**VM:** The only thing I wonder is that in those days everybody had to have clearance to work in that lab. Foreign citizens were more of a problem. I remember I had to get clearance. I presume you got clearance somehow.

**OK:** I think Pomerat took care of this.

**VM:** You arrived one day and Calvin was there when you arrived?

**OK:** Yes. They had arranged a house for us; we lived on McKee Road...

**TK:** McKee Avenue.

**OK:** McKee Avenue.

**VM:** McKee something, anyway. Americans don't worry too much the "avenue" or the "road". You bought a car, did you, when you got there?

**OK:** We came by car. We bought a car in Brookhaven. Because after three days we found out that Brookhaven is... even buy a can of milk or something you had to have a car! We bought a car for \$80.

**VM:** And you drove it all the way?

**OK:** Yes and I sold it for \$100 when we left.

**VM:** Congratulations! Very good! OK, so there you are in Berkeley on the first day or the second day and you have a house on McKee and a car.

**OK:** We drove up to Calvin the next day and we got stuck because our car was too weak to make it up the steep hill.

**TK:** I think we had to walk the last part!

**VM:** I remember that you moved into the Old Radiation Lab. with the rest of us.

**OK:** Yes, that's right.

**VM:** I remember you spent a lot of time with Calvin in that office which had glass walls, but I don't remember where you were working, which lab bench you had.

**OK:** Together with (*Helmut*) Simon, I shared a bench with Simon. They slept, they spent the night...

**TK:** They spent the night...(*Do you know*) the story of how we met? We didn't know each other.

**VM:** No. Come on; tell me. They told me they came late and they had to a night in a hotel in San Francisco.

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**TK:** We had been in Berkeley for two or three days and the Calvins invited us for dinner. We came there late in the afternoon, five or six, and Hildegard and Helmut Simon were already there because they had spent the night there. Somehow, they didn't dare to leave the room. They noticed that the Calvins had some visitors; they didn't know who it was. After a while they thought "We must go out, it's getting late" and this was us. They saw us for the first time.

**VM:** Did you know they were going to be there?

**TK:** We have been very good friends ever since that evening.

**OK:** In those days, they were in Berlin with Weygand, they were not in Munich. Only afterwards, Professor Weygand moved to Munich and they moved with him to Munich.

**VM:** Yes; Helmut told this morning his story. So anyway, I'm placing you in the lab. and so you're starting to work.

**OK:** And then, of course, I did CO<sub>2</sub> fixation because I wanted to see what cyanide does with photosynthesis. This was the main purpose. I also knew that Meyerhof showed that triose is an intermediate by stopping with cyanide and with *hydroxylamine*. I thought I could do the same thing with photosynthesis because *dihydroxyacetone phosphate* was tricky in those days. You only had a very faint spot of triose phosphate so the point was: is it really there? I thought it should accumulate if you put cyanide in. I was surprised when it didn't accumulate but the hamamelonic acid accumulated. So you know that story about the hamamelonic acid with...

**VM:** I remember there was a story. That was an addition product, presumably; a cyanide addition product.

**OK:** Yes, it was of ribulose diphosphate. When I came back to Munich, Helmut prepared some labelled cyanide and we did the same experiment as Rabin did in Calvin's lab. I came to the same conclusion. There was some tricky thing. We *degraded the isolated hamamelonic acid which should have been labelled only in the carboxyl group* but we didn't find it. It was dispersed. That may not have been very clear by the degradation. But I *accepted* that it was a cyanide artefact.

**VM:** Was Calvin very interested in what you were doing? Did he talk to you a lot at the beginning?

**OK:** Not too much. He became very interested when I had this unknown spot; I think that was the first time that he put the keto acid into the scheme was a few months later. Now he saw that it could be trapped. But he didn't talk too much with me.

I then came back to Munich to work on the hamamelose. *hamamelose is found in almost all plants, especially in Primulaceae; they have lots of hamamelose — it's the main sugar. I still wonder where it comes from. Probably it is made from fructose*

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diphosphate because Bick showed in my lab. that aldolase can manage to make hamamelose from it.

**VM:** There was a period in Berkeley, I remember, when you used to argue a lot with Calvin and you were in that office with the glass wall and we could see you on the blackboard, the two of you, arguing. What were you arguing about?

**OK:** How he explains this very sluggish phosphorylation. If it's really necessary to have so much phosphate. I always thought it *would be* stupid to *build* something, drag it down half way and then spend again so much energy to bring it up. Even today nobody has shown, to my knowledge, that the turnover of phosphate really has this high rate. You can show it very easily in isolated chloroplasts, making phosphorylation with chloroplasts. You get high rates, you get 300-400 micromoles, but not in whole cells. But as Calvin argued, the *sluggish* exchange *between* chloroplasts *and* cytoplasm *may* prevent *its* detection.

**VM:** You weren't satisfied with that explanation?

**OK:** I was not satisfied with that explanation and I spent a lot of my time, I wasted some of my time, to find the trick to show the other way. But it didn't work. I also did much unpublished work about the exchange. If you feed labelled glucose, position-labelled glucose, and look how fast it equilibrates, all these things indicate that the exchange between chloroplast and cytoplasm is very good. This is why it took me a long time to drop this issue.

**VM:** When you were there, I'm sure when you were there, the argument grew very intense, I remember, and Calvin was clearly...

**TK:** I didn't know that.

**VM:** You didn't know that?

**TK:** He always told me about his ideas and about the argument but I didn't know that there was such a discussion behind glass doors.

**VM:** There was a room there which had been Al Bassham's office, I think. Al Bassham was away and I don't know whether you occupied the office but when Calvin came by...

**OK:** No, I didn't occupy the office.

**VM:** ...they used to go in this office and shut the door and you could see, and on the blackboard, but we were on the outside and we could see but we couldn't hear or we couldn't hear clearly. Obviously, we understood what the nature of the discussion was. I remember that we then had a meeting of everybody in the building really to discuss this issue. Do you remember that?

**OK:** Yes.



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**VM:** As far as I remember, we resolved it. What did you feel, in the end, about the whole business? Do you think that there really was a discrepancy between your results and his results? Were you happy in the end with the cycle? What was your feeling?

**OK:** I still have some doubts. You know the paper of (*Elias*) Greenbaum, probably, from Oak Ridge?

**VM:** No. I left this field, I have to tell you, in 1958. I have not been involved with these things since then.

**OK:** The Greenbaum paper is exactly what I feel. He has a *Chlamydomonas* dominance mutant (*Editor: correct?*) which has no photosynthesis system I. Of course, this was after the Calvin time already. I always thought that there was some special system for the phosphorylation and I associated always the Photosystem I with phosphorylation and Photosystem II with real CO<sub>2</sub> reduction. He has a *Chlamydomonas* dominance which has no Photosystem I and still it makes *oxigenic* photosynthesis and the quantum yield is very good. He says that Arnon was right, there is no Z scheme, no obligate. You can *do* it with Photosystem II alone.

**VM:** What does this mean for the path of carbon?

**OK:** He (*Greenbaum*) still accepts the Calvin cycle. It wouldn't necessarily change the path of carbon. I am not quite *sure what he means*. He does need less *quanta*; he gets a better quantum yield. But if you split the two PGA and have to reduce both, you spend a lot of energy so the quantum yield shouldn't get much lower. I don't actually see what he feels about the Calvin cycle. In his paper there is no evidence that it is Calvin cycle.

**VM:** So you still have some reservations about some parts of the Calvin cycle?

**OK:** Yes. There may be still some way that you don't need so much phosphate.

**VM:** Does anybody still work on the path of carbon in photosynthesis? I would have thought it's long dead, isn't it?

**OK:** I don't think so. The only point where one could start is this mutant now of Greenbaum. He actually should do the old Calvin-type experiment.

**VM:** Nobody has the equipment any more...I will tell you later about the equipment.

**TK:** It's not very difficult.

**OK:** It's not too difficult. You can set up a chromatogram very quickly. I could even do it as an emeritus now. (*Laughter*) It would be difficult today because of the new laws to work with isotopes.

**VM:** Is it difficult?

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**OK:** Yes, it's difficult. It's even difficult to work with chemicals.

**VM:** Anyhow — to take you back 40 years because modern history, I'm not doing: I'm doing old history! You worked on the hamamelonic acid afterwards and that's what you did in Berkeley?

**OK:** In Berkeley I made a lot of fixations and chromatograms. Essentially I tried to catch the keto acid and used hydroxylamine and other possible things under a variety of extraction conditions. I used the cyanide and the *various kinds* of application of cyanide.

**VM:** Did you find it? Do you think you found it?

**OK:** I trapped successfully the keto acid, of course, in the form of hamamelonic acid. But Helmut (*Simon*) told me right away, that it could have been a cyanohydrin synthesis. I am not a well-trained chemist but fortunately Helmut who had the experience and was a very good chemist and helped me in this respect.

**VM:** Did you publish that with Calvin?

**OK:** No. I published a paper on the finding of this unknown. He helped me to write it but not with his name.

**VM:** Looking back on that lab as you try and remember it forty years ago: was it a very different type of environment from the one you had previously known in Germany?

**OK:** It was different, yes, of course. I had two labs. there and I was very fortunate that Lynen was in the Botanical Institute so it was not "pure" botanical institute...

**TK:** Traditional.

**OK:** Holzer's work was not so much different than the work of the people in the Calvin lab.

**VM:** You had already experienced this very free atmosphere of talking between people in the lab. already before you went there?

**OK:** It was not unfree *in* Germany.

**TK:** After the war.

**OK:** After the war I think the people in the lab. had no restrictions.

**VM:** No, but there was an atmosphere in Berkeley, at least many of us who came from Europe experienced this, the easy relationship the Americans have with one another.

**TK:** We had experienced this already in Brookhaven.

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**OK:** This is not specific for the scientific world. It was a general way. For example, the rich man talks with the person in the filling station. This was different in Germany than in America. Also the connection between the students and professors was, of course, quite different.

**TK:** Much more casual (*in America*): coffee breaks were very important and very new to us when we arrived in Brookhaven National Laboratory. We had had this experience already there. Coffee breaks were where people got together and discussed their daily work. So this was very new.

**OK:** In our institutes, in those days, there weren't as many people. Put it this way: my connections, for instance, with Holzer (*in Germany*) was not much different than the connection to...

**TK:** It was a very small group.

**OK:** Well, of course, he was the only assistant of Lynen; there was only one. It was much simpler.

**TK:** The standard of living was very different, of course.

**VM:** That was for all of us from Europe in various ways.

**TK:** When did you arrive?

**VM:** When you did. I think two weeks later or something.

**OK:** The exchange in the laboratory was not so much different but it was a very stimulating atmosphere, of course.

**VM:** The way you describe some of your interactions with Calvin suggests that Calvin was, of course, very concerned that his ideas should be accepted by the scientific world.

**OK:** Of course; that's natural.

**VM:** Did you find him an original thinker?

**OK:** Of course, yes. He was certainly an original thinker.

**VM:** I recently wrote an obituary for him in which I stressed that so many of us thought he was a great scientist. Do you think he was a great scientist?

**OK:** I think so. I don't know the other things he did. Later on his ideas about the petroleum plants, I think this was a little trivial, he had not so much physiological experience with plants.

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**TK:** As scientists grow older they try to do something which has more practical relevance and which is more understood by the public. Remember your own activity with “Waldsterben” (*death of forests*). We have this problem in Germany where some pseudoscientists claim that we have a general dying of our forests and Otto thought this was wrong and he also tried to change the discussion. This was also trivial; it was not solid science, actually.

**VM:** When Calvin was involved with the petroleum plants, he was already in his seventies, he was no longer director of the lab., he didn’t have the responsibility and, of course, the atmosphere, after the photosynthesis, was never the same. During the early days there was a strong focus, you know, and everybody in the building worked toward the same objective. Then it was finished; people did other things.

What did you think of the building? People have talked with happy memories of that wooden building. Did you have happy memories?

**OK:** Yes, of course.

**VM:** Do you think it was a good building for doing science?

**OK:** It was a good building because especially they didn’t care very much about the regulations. This is always nice to work with.

**VM:** Nobody died, as far as I know.

**OK:** And we still have our hair.

**VM:** Some of us!

**OK:** I didn’t lose any!

**VM:** Did you ever go back to Berkeley to see the new building?

**OK:** Yes. I wouldn’t like to work there.

**VM:** Why not?

**OK :** It looked so...

**TK:** Sterile?

**OK:** It looked so sterile, this large room; maybe it’s different if you really worked there.

**TK:** Maybe it’s just nostalgia.

**OK:** If you had not worked in the old building, it had a look...if today you would come back without the experience, you would say it would be impossible to work there!

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**VM:** In the old building?

**OK:** Yes. I wouldn't be happy in this circus...

**VM:** ...the Calvin Circus.

**OK:** The Calvin Circus, yes. I visited once Bassham there.

**TK:** And Arnon.

**OK:** But Arnon was not there.

**VM:** Arnon was not in that building.

**OK:** This was a sacrilege.

**VM:** Do you know why Calvin was so antipathetic to Arnon?

**OK:** Of course; both struggled for a Nobel Prize.

**VM:** Was Arnon against Calvin like Calvin was against Arnon?

**OK:** I don't think so, that he was so strong against Calvin personally. Well, it was another personality there; you couldn't judge it so easily what he really feels.

**TK:** I never heard any negative remark by Arnon about Calvin.

**OK:** In this respect Arnon was (you understand German) more *vornehm*.

**TK:** Very reserved.

**VM:** You think even then, in the mid-fifties, Calvin and Arnon were both thinking strongly of Nobel Prizes?

**OK:** I think so. Arnon thought his photophosphorylation was important. Arnon visited me in '54 (something like that) and he gave a very detailed description of my work in '56 but later on he doesn't mention it any more.

**TK:** Why didn't you go to Arnon?                      Why didn't you want to go to Arnon instead of Calvin?

**OK:** I was interested in the path of carbon; photophosphorylation was an old story for me. I was convinced it works and you can use it for sugar assimilation and all those things, so this was not a problem. It only was a problem together with the Calvin cycle if it really is strong enough. That it was there, that's no question, but the stoichiometry was the important thing and it was not easily shown. Of course, the excuse Calvin uses is legal and is feasible to me, too. But it was just wishful thinking

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on my side that this exchange is not the right explanation and in some way one should be able to show it. This paper now of Greenbaum is really surprising.

**VM:** Where is it (*published*)?

**OK:** I don't have a copy with me. It's in the Proceedings of the National Academy of Science.

**VM:** Roughly when?

**OK:** That last one is one year ago (*from March 1997*).

**VM:** I'll find it. Can we finish by my asking you about your career when you came back to Germany after you had been in Calvin's lab?

**OK:** I had my assistantship (*in the Botanical Institute*) and I hoped, of course, to get a call for a professorship. I had a call before for microbiology. This didn't work any more because they switched their mind in Köln and they changed — this was what we call an *Extraordinariat*. It was a half-professorship in those days...

**VM:** In Cologne?

**OK:** In Cologne, yes, and they decided to make genetics and microbiology and this was not my job. This is why it didn't work. There were some people in Germany who were annoyed with me because I had had an argument with Calvin.

**VM:** Oh, really? They were annoyed with you?

**OK:** Yes, yes; very much.

**VM:** They thought that a young man shouldn't argue with Calvin? Why were they annoyed?

**OK:** I think Calvin made nasty remarks. I have not heard the remarks but from the echo I got, I had the impression that he...and some of our...you know there is always a certain establishment around the scientists, so this is not the right way...

**TK:** You didn't behave well!

**OK:** ...and so I didn't get right away a call for a professorship.

**VM:** Do you think that had an effect?

**OK:** Of course it had, a strong effect. In those days the head of the Dairy Science Research Station in Weihenstephan became free and the man who had the directorship came from our Institute 20 years ago, or 25. And so they asked again in the Botanical Institute if somebody was interested. I was interested: in those days botany was a very poor science and I was Dozent in those days, no help at all (a technician), but

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there were three assistantships in this (*Dairy Science*) Institute, and I knew the predecessor and I was anyhow interested in microbiology. So, I said “Why don’t I go there?” So I took the Dairy Science and the botanists, of course, were very surprised that I went into Dairy Science.

**VM:** This was a professorship in the Dairy Science?

**OK:** Not really; this was a directorship. So I remained a Dozent at the university in Munich. I had my students; once a week I gave a course in plant physiology because I wanted to have students from natural science and not from agriculture. I should have given talks on agricultural microbiology but this was not obligatory, this was a facultative (*i.e. voluntary*) lecture. My predecessor had only four or five students so I said it doesn’t pay to give my lecture for five agricultural students and so I rather preferred to stay at the university and give this course in plant physiology. I had complete freedom in Weihenstephan; I could take natural scientists — I was not forced to have agricultural people in the Institute —and much more money than I would have had in any botanical institute.

**VM:** Eventually you moved into the university itself?

**OK:** One year later already — of course, I did dairy microbiology; I got acquainted with lactobacilli and streptococci and in those days I already had started work on cell walls of bacteria, cell wall chemistry and this I could do very nicely. The chemistry of peptidoglycan is very much modified in the various strains so this became a taxonomic characteristic. It was very fortunate that *Leuconostoc* is used to break down the glucose; it also makes the aroma in butter and cheese. So I had no difficulty in making degradation studies with *Leuconostoc* even in the Dairy Institute.

Very soon I got a professorship at the Technical University in Munich. The Botanical Institute at the Technical University was very small and relatively poor so I kept the Directorship in the Dairy Science which had much more money than the other one. After eight years I got the Chair in Botany at the University of Munich.

**VM:** Which year was that that you moved to the University of Munich?

**OK:** In ’68. In 1959 I moved into the dairy business (in ’58 already) and in ’60 I moved to the Technical University and in ’68 I moved to the University.

**VM:** And you stayed there for the rest of your career?

**OK:** Yes.

**VM:** Are you now formally retired?

**OK:** Yes. I retired at the age of 65 in ’85.

**TK:** In order to start work.

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**VM:** Well, that's what happens.

**OK:** During the time at the Technical University, and also at the University (*of Munich*) I worked mainly on the biosynthesis of branched chain sugars, hamamelose and apiose, and also on other carbohydrates. This was my main interest there. That was one side; on the other side was the variability of oxypeptidoglycan in bacteria and...

**TK:** Excuse me: and the chemotaxonomy. You worked with oligosaccharides, too?

**OK:** This was the neighbourhood of the branched chain sugars I worked with these oligosaccharides, mainly raffinose biosynthesis. From then I got more and more interests in phylogeny and evolution, from the cell walls to the archæbacteria. *This led to* my connection with Carl Woese and this is my main interest right now.

**VM:** Are you still active in the lab yourself?

**OK:** No, I dropped that. For some years I continued to do a little work on cell walls by myself to screen new bacteria but a couple of my former students have chairs now, they are active and are my assistants now! (*Laughter*) I still have good contacts and talk with them.

**VM:** It's nice to see you after so long and to see you in such a healthy and active state.

**OK:** And actually, I'm very happy, after all this CO<sub>2</sub> fixation business. In a few weeks in *Science* there will be a paper on CO<sub>2</sub> fixation under primæval conditions from CO and iron and nickel sulphate.

**VM:** Are you the author of the paper?

**OK:** No, just a friend of the author. The author is Gunther Wechteshäuser.

**VM:** Helmut (*Simon*) was telling me about it.

**OK:** Yes, he took him in his Institute. I think it's really great. So far the only product is activated acetic acid.

**VM:** I think we should stop soon because our host (*Editor: this was recorded in the Simons' house while dinner was being prepared*).

I want to thank you very much for spending the time.

**OK:** It was a great time for me in Berkeley, of course, because to be really in the centre, this was the important thing.

**VM:** You felt that that was the centre at the time?



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**OK:** For photosynthesis it was the centre. There was Calvin and there was Arnon and I was the only one in the lab. of Calvin who could switch between both! I was the only one who visited every fortnight or so and go down without having an official...

**TK:** Perhaps that's the reason why Calvin was not so pleased with you. He knew that you had contact with Arnon.

**OK:** I don't know whether that was so.

**VM:** I still have to talk with Bob Whatley. Do you know him?

**OK:** Yes.

**VM:** Maybe he will be able to help me. No one has really given me a good explanation of why there was so much tension between them. You may be right that it was the Nobel Prize competition.

**OK:** I think it was just competition.

**VM:** Anyway; perhaps we should stop and thank you again.