

Chapter 50

LUISE STANGE

Göttingen

May 23rd, 1997

VM = Vivian Moses; LS = Luise Stange; SM = Sheila Moses

VM: This is a conversation with Luise Stange in Göttingen on Friday, the 23rd of May, 1997.

Luise: what was your original science education and what led you to go to Berkeley when you went there?

LS: Well I was a student of biology and chemistry at first in Göttingen and then later in Freiburg-im-Breisgau. I did my PhD in Freiburg with Professor Oehrkers who was a geneticist. I was interested in genetics and also in the physiology of development. For a short time after my promotion I had a job in industrial research with sugar beets but I was not very content. I was offered a place in Cologne at the *Institut der Entwicklungsphysiologie*...

VM: That's Developmental Physiology.

LS: Developmental Physiology...and there I started my *Habitation* work and this was on regeneration in plants, especially the question, the following question interested me: what happens in the soil which changes from nature stage and return to the cell cycle activities? Of course, to have a new cell division, it's very important that DNA can be synthesised, which is not the case in nature cells. Therefore, I tried to get more information about nucleic acid synthesis in these cells. For this I used labelling with P^{32} but I found out immediately that it isn't very specific because so many cell compounds become labelled with P^{32} .

At the time I was offered a grant from the German Science Ministry because they wanted after the war young scientists to be educated in the United States, especially in radioisotope techniques. Therefore, I wrote at first to Andrew Benson because I had read his papers. He answered that the best place would be to go to Dr. Calvin's lab. to Berkeley. I wrote to Dr. Calvin and I was very glad that he accepted me. Actually, he wrote to me that he was interested in my work but that we should do something of mutual interest. He was working with *Chlorella* and when I came to Berkeley we decided that I should try to grow *Chlorella* synchronously which was just developed at the time by a Japanese group and also a German group. My task was at first to cultivate *Chlorella* in synchronous cultures.

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VM: Can I go back to your approach. How did you travel to Berkeley?

LS: At that time I took a plane, still with propeller, from Paris to New York, it was Air France. Then in New York I had to change. There was just a holiday, I think it was Labor Day...can this be in September?...so there was a delay and I had to stay there for one night. It was very interesting to see New York.

VM: This was the first time you had been in America?

LS: This was the first time I came to New York, the first time and I had to phone my landlady I would arrive a day later so I came later. Then I flew to San Francisco and I didn't know how to come. They told me that you have to change immediately and get a helicopter to go to Oakland. In a rush I came to Oakland and took a taxi and arrived during the night at 2511 Hearst Avenue.

VM: You had already arranged this apartment?

LS: Yes.

VM: Had you done it or had they done it from the lab.?

LS: I can't remember this; probably I got a suggestion from the lab. 2511 Hearst was just at the border of the campus.

VM: So, you arrived at your apartment in the middle of the night?

LS: The landlady was still awake or waiting for me and was very nice and helpful.

VM: You had things in the apartment to live? You had furniture and bed linens?

LS: Yes.

VM: So the next morning, what did you do?

LS: I went to the lab. and there I met at first I met, Dr. Bennett, Dr. Ed Bennett and I was astonished seeing him. He was very friendly and said "Hello, Luise, please come in". He immediately put his legs on the table, which is not usual in Germany. I was a little bit astonished but I didn't say anything, of course. Then he wanted to know what I intended to do. At first he hoped, apparently, that I could work together with him on his rats but I said no, I didn't want to work with rats, I wanted to work with *Chlorella*. Pretty soon I came then to the Old Radiation Laboratory and a lab. space was given to me. And I almost think that Dr. Vivian Moses was the next one — no, I think at first it was Ozzie Holm-Hansen; he was at first responsible to help me at the beginning. And then in a very dark lab. upstairs in this Old Radiation Laboratory and very close to my desk was the desk of my later friend, Erminio Lombardi, who was always singing Italian opera because he had done this in Milano before. And I remember downstairs there was Ning Pon and he had wonderful records playing during the night because We three worked very often during the night Ning Pon,

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Erminio Lombardi and me. Americans always finished their work at 4:30, I think, about this, while we intended to get as much work done as we could in a short time.

VM: Your first scientific contact on the algae was really with Ozzie, but had you met Calvin yet at this point?

LS: Yes, I had met him, probably at the second day or so, and he said “hello, Luise” (this was unusual for me, too, the first name, which is not used in Germany). He was very friendly but we didn’t have a very long speech at that time. We didn’t have a very long discussion but also when I had set up the tube with the algae — he was interested to see the method for the synchronous culture.

VM: So you started to talk science with Ozzie Holm-Hansen.

LS: Yes. And pretty soon Dr. Vivian Moses joined us. I think he had the same room like Ozzie Holm-Hansen./

VM: Yes, Ozzie and I shared an office.

LS: I was very thankful because I had not education for a biochemist or biologist and so learned many small things from Dr. Moses, Vivian. Remember, for instance, shaking the beaker. I had to put it on the lab. desk so that it was shaken horizontally and not in the hand, not hand-shaken. I remember this very well.

VM: I think I have forgotten that!

LS: Later in my own lab. I always remember that if I had a beaker in my hand what Dr. Vivian Moses had told me at that time.

VM: What was it that you agreed with Ozzie, and perhaps with me, to work on at the time?

LS: I was interested in nucleic acids and so, of course, my idea was to follow nucleic acid synthesis through the cell cycle. But to get these nucleic acids labelled is not difficult because the methods were available in the laboratory. But to isolate these nucleic acids from other material, it was difficult. There I had close cooperation with Vivian Moses and we together worked out a method to fractionate this material. At first we could some of extract the soluble material, in which we were not very much interested, only the radioactivity of the whole fraction, and the insoluble material...this had to be fractionated further further. We decided to do this by enzyme digestion. Actually, the results of this work has been published in *Biochimica et Biophysica Acta* together with Dr. Moses and Dr. Calvin.

VM: What you were interested in was to look at nucleic acid synthesis in synchronised *Chlorella*.

LS: Right.

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VM: Were you interested in that? I think the lab. had no synchronised *Chlorella* at that time.

LS: I had to synchronise these *Chlorella* cells and there I could rely on the papers of the Japanese fellows, Tamiya and co-workers, and then, in Göttingen and Pliser (*spelling?*) and co-workers. It is done by special light-dark changes and nutritional changes but, of course, we had to check if the culture was really synchronous. We did this by microscopic analysis of the cell size. Of course, at the beginning of the cell cycle, the cells have to be very small and then, at the end of the cell cycle before they divide, they had to be very big. So they had to be uniform. This was hard work: I had to do every day to check the cell situation in my cultures. It worked out and it was published also in another paper, not the fractionation paper (this is only the method), but another paper “Short-time $C^{14}O_2$ incorporation, with synchronously-growing *Chlorella*”.

VM: How did you set up the synchronous cultures? What did you use? What did you use to grow the cells?

LS: The nutrient medium?

VM: Well, did you use the continuous culture tubes in which the other algae were growing?

LS: Yes.

VM: You used one of those tubes?

LS: Yes.

VM: And you had a light-dark regime and a nutritional regime?

LS: Yes, yes.

VM: And the cells you measured through the microscope, visually, counting cells, estimating cell sizes, every day. How often did you have to make the measurements?

LS: I checked it every 12 hours because the cell cycle was about 36 hours so I had 4 measurements during the cell cycle. This was enough to judge about the synchrony of the culture.

VM: What do you remember about the way you worked in the lab.? Did you tend to work by yourself? Did you work with other people? Did you collaborate in experiments? How did it go?

LS: Actually, with my project I was rather alone because until then nobody was interested in labelling nucleic acids by $C^{14}O_2$. But with the technical things I got help from every side. Also I remember Pat Smith, she was in charge of the algae culture, and she helped me quite a little bit with the preparation of the nutrient solutions. I learned

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many practical things. Of course, in the analysis of the material I learned from Vivian Moses how to make hydrolysis and sealed tubes — I had never done this before — and many other things, many small things which are very important in lab. work. I got support from everybody. When I asked somebody, I got help from all sides. But of course, the people were more interested in photosynthesis and not in cell cycle analysis, although Dr. Calvin was very interested in the project and I had to give a seminar and prepared myself very carefully in the library about the cell cycle. This theoretical work has helped me later very much in my work at home because, until the end of my career, I was interested in the cell cycle and its regulation because it is a very complex process and it's very closely related to the process of differentiation. So it was a very fruitful time from the theoretical point of view for me.

VM: When you worked in the lab. there, you participated, presumably, in the discussions which were going on all the time in the lab. Did other people look at your chromatograms and your results, and discuss with you what was going on?

LS: Yes. At first, Dr. Bennett was also interested. One of the papers on short-time incubation. This was not so close to my original interest, because we didn't analyse the nucleic acid synthesis in this paper, and he was interested because I think he was advised to help me — he was the first person I met there — and I learned from him quite a little bit also. But most people there were interested in photosynthesis, of course.

VM: So you did feel a bit to one side of those people?

LS: Yes. Although not without interest of other side. I have to mention still that I brought with me my experimental plant which is a liverwort *Viyella* (*spelling?*); it is a single cell layer so you can look into the cells directly in this plant. Dr. Calvin allowed me to do incorporation experiments with this plant too. I think because of some experience with German workers he didn't want me to do only my own project. He wanted to integrate me into the group. I understood this quite well.

VM: Which, of course, he did.

LS: He did.

VM: Was this type of laboratory very different from your experience in Germany before you came to America? You mentioned Bennett putting his feet on the table and people using first names.

LS: Yes. Of course, you know this old building, this was quite different from our institutes.

VM: How was it different from your institutes?

LS: In Cologne we had a completely new institute, with modern laboratories, and this was like a barrack or temporary building. It *was* because the second time I came to

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Berkeley they were in the Life Science Building because a new building was erected at the time.

VM: But did you find it an unpleasant building to work in?

LS: Not at all because the people were all very engaged in their work, very busy and helpful, and the atmosphere was very exciting. It was even more active because it was, of course, a research laboratory and in my institute there was the teaching of students. This concentration to the research was a wonderful time for me.

VM: Apart from the building, did you find the American style of working, as you saw it at that time, different from the German style that you had come from?

LS: We had very good facilities and I had the impression that they had lots of money and especially with this radioactive labelling. They had lots of experience. I think that in their methods they were ahead of us. This was the reason why they asked me from the (*German*) government to learn over there.

LS: When you had been in Germany before, and perhaps also when you came back, what was the relationship between the professors and the other people? Was it more formal than in the American situation?

LS: Yes it was, more formal. Not today any more, but at that time it was more formal. The students used to say “Herr Professor”, “Frau Professor”. Today they don’t do it any more. When they go out of my room they say “Tschüss”. I don’t like this; I think Tschüss, this is so...

VM: Do they now call you by your first name?

LS: No, they don’t do that.

VM: They still give you a title?

LS: Usually not a title: “Frau Stange”.

VM: Oh, I see: it’s that sort of title. But not “Luise”.

LS: No, they don’t do, no. You see, in Germany we have this difference between “Du” and “Sie” and you don’t have this in English language, so it already makes a difference.

VM: The German students use “Sie” and are more formal.

LS: There are now younger professors who ask that they shall say “Du”, but I don’t like this specially because I think there is a difference between a teacher and a student. It’s not a value, but a difference in the function.

VM: So, anyhow: America in 1957 — by the way you came in ’57 didn’t you?

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LS: In '57.

VM: For a year, you said...

LS: For a year.

VM: ...the first time.

VM: At that period it was less formal than Germany at the same time, in your experience: in '57.

LS: Yes, it was.

VM: How about the social life in the lab? You say you worked day and night, but I am sure you relaxed as well with the other people. What did you do?

LS: It was a wonderful time, you know, this country, with the mountains, high mountains...in winter we went skiing, and I enjoyed this very much. We even had a trip to Sequoia National Park and we slept there on the ice. It was wonderful; we crossed a creek and I lost a shoe in the creek and Ozzie Holm-Hansen gave me one of his spare shoes. And there is a picture even of different shoes on my feet. It was a wonderful trip. Then, of course, in summer we also had wonderful trips. Of course, I remember the trip together with Vivian and Sheila Moses and Erminio Lombardi to all the national parks in the Southwest. It was a wonderful time.

VM: It was a wonderful time for all of us. You acted very freely socially with people in the lab.?

LS: Yes. It was a first Christmas away from home and I was invited at the Holy Evening to Karl Lombardi's house. I remember this quite well...they had a goose...

VM: to Karl Lombardi?

LS: No, no, no: Karl Lonberg...and he had a goose, a fried goose...and then we sat down and listened to Handel's Messiah. It was wonderful because I like this music very much. The next morning, the first Christmas day, I was invited to Martha Kirk's house out there in Walnut Creek; I think it was Walnut creek, some place there. Her parents were still alive and I got some presents under the Christmas tree and everybody was so nice. A wonderful dinner; I think it was a dinner. They were all so...*was heisst "Gastfreundlich"*? Hospitable, very hospitable. They always took me with their car, I didn't have a car then. Even I did only my driver's license over there at the end of the year, so they had to take me in their car.

VM: So you never had a car at any time in California?

LS: No, in California not.

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VM: How well did you speak English when you first arrived?

LS: Oh, you see, I don't think it was too good but it was good that I was alone so I was forced to speak always English. There had been German couples and they always spoke German when they were in their house, in their living apartment. I was forced to speak only English and I think I improved very much at the time. I remember when I came home to Cologne I even used English constructions because in English it is much easier than in German because of the use of participles which shortens the whole sentence. German is much more complicated in the construction.

VM: So you learned to do this in the year in California?

LS: Yes, yes, yes, I did.

VM: Was it tiring to have to speak English all the time?

LS: Not at all.

VM: So you must have spoken quite well when you came in order to be able to build and feel fairly comfortable.

LS: Of course, I learned English in school but I had never been in an English-speaking country before. This was my first experience in an English-speaking country.

VM: When you went back to Germany at the end of...when was it? In the summer of '58?

LS: In the fall.

VM: Were there parties when you left?

LS: Yes, there was a goodbye party.

VM: Where was your goodbye party, do you remember?

LS: Somewhere outside and we had to go there. There were some eucalyptus trees and they made a barbecue; I have even pictures of this.

VM: A lab. party, people from the lab. came?

LS: Yes, from the lab. Dr. Calvin was also there. He joined the goodbye party. They gave a card to me with all the names on it; it was like a gun with a big *Kugel*: *was ist das?*

VM: Bullet.

LS: Bullet to shoot me back because I stayed there 'til the last minute. I think I still keep this card somewhere! Could be: all the names were on it — very nice, yes.

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VM: What did you think of Calvin as the leader of the group? Did you find him an interesting person, exciting person? How did you deal with him?

LS: It was always interesting. I met him mostly in the seminar. This was very stimulating because he had always new ideas and important questions. Also when I gave the seminar it was very good for me that he had the questions which partly I couldn't answer. I had to look afterward for the answer. Then when we had to go thorough the manuscripts, this was important for me too because at that time I learned that in every publication you have pictures or graphs and it is necessary to have under the graph a complete description of what is shown. Dr. Calvin said to me "Luise, you have to write a paper. Nobody has time; you have a paper like — you have only look at the pictures that you know what has been done." This I followed in all my further publications. It was very important counsel.

VM: Had you published any papers at all before you went to Berkeley?

LS: Oh yes, I did.

VM: before you went to Berkeley?

LS: Yes, but only in German.

VM: When you wrote papers from that lab. there, did you write them yourself? You could write English well enough to make a first draft?

LS: I think I got quite a bit of help from Dr. Bennett and Dr. Moses in writing these papers; yes. Especially the fractionation paper, the fractionation of the insoluble material; I remember that Dr. Moses was sitting in front of his machine and he started with the introduction and the introduction always is to cite other papers.

VM: But I noticed that, at least on many of these papers, perhaps all of them, you are the first author.

LS: Yes, because I had done most of the work which is included in these papers.

VM: If you were the first author, then probably you did most of the work and you wrote the first draft? That was usually the style.

LS: But you helped me, especially with the fractionation paper and Dr. Bennett. Maybe the last one I have written mostly myself but Dr. Calvin went over it and asked something to improve it.

VM: You left Berkeley in the fall of '58 and came back to Germany. What did you do when you came back?

LS: I went back to Cologne...

VM: ...to the same lab. that you had been before?

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LS: Yes. Because before I went to Berkeley in February '57, did my *Habilitation* (what is usual at German universities) so I was then a *Dozent* in that laboratory. I stayed there until '63 or '64, I don't know; don't remember exactly.

VM: But you went back to Berkeley for a period?

LS: Yes, in '60.. Actually, I wanted to go back to Berkeley earlier but they were moving out of the Old Radiation Laboratory into the Life Sciences Building. My father was very old at that time and actually he died in December '59. That was the reason I postponed my second visit to Berkeley. Finally in '60 I could come a second time.

VM: Why did you want to go back? I know that it's a nice place and so on; but specifically?

LS: No: Dr. Calvin actually wrote a letter of recommendation so I could get money again. And he wrote that I couldn't reap the fruit of the trees which I had planted. Actually, from my first visit there had been the two papers, fractionation and short-time incorporation. And then incorporation in the insoluble material: this is from the second stay, from the second visit. So I had prepared the method, I had worked out the synchronous culture but I had not yet labelled the nucleic acids during the synchronous growth to the cell cycle.

VM: When you went back a second time, the lab....the people were then in Life Sciences Building?

LS: Yes, They were in Life Science Building and I had to use the steady-state machine of Dr. Bassham. I got quite a little bit of help from Martha Kirk at that time.

VM: How long did you stay that second visit?

LS: About four months at that time.

VM: You didn't got back to your old apartment, did you, on Hearst Avenue?

LS: Yes, I did.

VM: To the same building?

LS: To the same building, not the same apartment, in the same building, to Mrs. Lee.

VM: How did you notice the change in the group which had moved from ORL to the Life Sciences Building? Was the atmosphere different at that time?

LS: I don't think — there had been some new people...one from Britain, I don't remember his name exactly now...a couple of Britons, some new people have been there, some foreigners, guests in the laboratory, but the American staff was the same.

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VM: Were you able in that second period to complete the work that you had wanted to do? Was there enough time?

LS: There was enough time although I took with me some chromatograms to measure them in Cologne to finish it, but it was enough so that we could publish this last paper from this period about the incorporation of $C^{14}O_2$ in nucleic acids during the cell cycle.

VM: That was the last time you worked in Berkeley?

LS: This was the last time you worked in Berkeley.

VM: But you went back from another visit later. When was that?

LS: I think it was in '78.

VM: When the new building, the round building.

LS: It was in the new building. I joined a seminar and Dr. Calvin was very nice. He told the people, most of them I didn't know, that I had been working there before twice and it was very nice to be in this group again which was guided by him.

VM: You had worked in ORL and then you had worked in Life Sciences Building and then in '78 you saw the round building.

LS: Yes.

VM: What did you think of it?

LS: I think it's a marvellous building, probably...you see, I didn't work there...

VM: I understand.

LS: I had the impression that they had very good working conditions in this new building now.

VM: You know that that building was an attempt to recreate the atmosphere of ORL in a modern building. Do you think it was successful?

LS: No, it was completely different, I think. As I said before, this was a special atmosphere in this Old Radiation Laboratory. We had lots of fun. It was very nice. But it was a special atmosphere because of this old building but this active group, this combination; it something special.

VM: It was not possible to reproduce that at a later stage?

LS: No, I don't think so, no, no.

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VM: OK. Let's come back to 1960, November 1960 — October/November — when you went back to Germany again. And again, back to Cologne?

LS: Again, back to Cologne, yes.

VM: You stayed, I think you said, until '63 or '64.

LS: '63/'64. And then I was offered a professorship, I was already a professor in Cologne but it was called *Ausserplanprofessor*; it's only the title. Actually, you have to publish some work. Of course, I published something else beside these papers. Then, two professors have to be asked if a person who has his *Habilitation*, taught for some time, is able to have a chair. If there is no chair, they get the title *Ausserplanmäßiger-professor*. One of the professors who was asked to recommend it was Dr. Calvin and he wrote, apparently — I have never read it — but my professor said it was a very good recommendation and the one was Folke Skoog in Madison, Wisconsin. Apparently that was also...He was interested always in my work about regeneration.

VM: Did you know him personally?

LS: Yes, very well. He visited us several times in Cologne and Hannover and we had many interests in common.

VM: You know, of course, that Ozzie was a student of his?

LS: Yes, I know, I know.

VM: When you left Cologne, where did you go?

LS: To Hannover. At that time it was called the Technical University; it's now University. I worked there in the Botanical Institute but I had the Division of Plant Physiology.

VM: That was your title, Professor of Plant Physiology?

LS: Yes. At the same time, I was teaching at the medical...it's called medical high school (*Hochschule*; it is different from the English high school — a medical university, you might say). I enjoyed this work very much with the medical students. It was general biology and we had them also in our practical courses for medical students.

VM: And then you stayed there how long?

LS: Nine years...

VM: Until early '70s.

LS: ...until '73. In '72 I was offered a chair in Kassel at the new university and it took a whole year to...because I had this good position in Hannover at the Medical School which also paid my teaching so I took some time until I did get good conditions in

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Kassel. And then I went on there and I had my own group there, in a new institute. It was only a rough building and I could put in everything I needed. It was very nice and I had my own new group there.

VM: That, of course, is just what Calvin did with the Old Radiation Lab. He always had a building in which he could put his own stuff and build it the way he wanted. Were you influenced in what you did in Kassel by what you had seen in Berkeley?

LS: Yes, a little bit. Because then I got offered a share: I said I need to have a radioisotope laboratory. I made the plans for it; it's now too small and just now they build a new one. I have been there for 20 years in Kassel. For this time it was our...for the whole faculty...also for the chemists it was our radiation (?) laboratory, radioisotope laboratory.

VM: How about discussions and seminars when you had your own group? Did you run a seminar the way Calvin used to run a seminar?

LS: Yes I did, with my own group, yes, I did.

VM: You would ask questions all the time?

LS: Yes, I did too.

VM: So it seems that you have been quite influenced later on by the time you spent in...

LS: I must say that also in Cologne, before I went to Berkeley, our institute had a seminar when I was still an assistant at that time.

VM: Looking back on your time in Berkeley, what do you think it did for your career?

LS: Two things, I would say. First of all I learned many new methods in laboratory work, small things which are very important and apparatus; this is more methodological. The other was even theoretically, as I said, because at that time I was only interested in the activation of the cell cycle, but now also in the normal cell cycle where cells divide all the time, I now see the differentiation very close to the cell cycle. Even in my later paper I have some hypothesis that differentiation comes about by differences in cell cycle, at special stages of the cell cycle, which are, of course, metabolically characterised, have influence on neighbour cells and can induce directed transport. Because in differentiation directed transport is a very important phenomenon. So now the cell cycle is really in the centre of my work although I'm interested in cell differentiation. I think it was very important for my theoretical position in research too, my science too.

VM: That was the first thing. You said there were two things that you learned.

LS: No, the methods was first, and this is a theoretical point of view.

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VM: What sort of contact have you kept with people from there? I know that we see you from time to time — not very often, but every few years. What about contacts with people generally from your period in Berkeley?

LS: I had very, very close contact to Martha Kirk. She has been my very, very good friend. She worked for some time in Germany with Ulrich Heber and then with Karl Erismann in Bern. From there she came very often to see us at the weekends in Göttingen. She has been here very, very often. I had close contact with her. Then, of course, to Ed Bennett. He visited me several times, too. We had wonderful trips in Berkeley when I was there in '78 we had wonderful trips again and I am still in contact with him by letters. Otherwise, I have no contact any more. Of course, I had also besides the laboratory I have met other Americans and to them I have very close contact too, completely out of science — a whole story by itself; very interesting people.

VM: It seems that you had a good time and that you retained good memories of the periods in Berkeley and it has been helpful in your career.

LS: It certainly was very helpful. I have the best memories and I am glad that you are here so I can make them up again!

VM: We are very grateful that you have contributed to our collection of interviews and it was not as terrible, I'm sure, as you thought it might be. It was no inquisition!

LS: You see, I tried yesterday evening, but it was too late to read these papers so I could get, give you...but you could read them yourself.

VM: I can indeed.

SM: Did you get to know the Calvin family at all?

LS: Yes I did. I was invited several times to his house and enjoyed this very much. Especially his wife was very, very friendly. She was a very wonderful woman, I think. I have even seen his daughters and the little Noel. I was very much impressed one evening. He was watching television and Dr. Calvin said "now you have to go to bed" and he didn't want and he brought him back but pretty soon he was back again to television! I told people in Germany that children even govern sometimes their very important parents. Mrs. Calvin gave me as a goodbye present an etching perhaps of the tower, of the campanile in Berkeley, and I have it hanging in my working room in Kassel. It was very nice. They are good memories.

VM: I think it remains only to say thank you for contributing and this tape will now be preserved for posterity!

LS: Thank you very much for coming and to honour me with this small work I have done in Berkeley. Thanks you so much.