

## Chapter 20

# LOREL (DAUS) KAY

Mendocino, California

June 19th, 1996

**VM = Vivian Moses; LK = Lorel Kay; EK = Eric Kay; SM = Sheila Moses**

**VM:** This is a conversation with Lorel Kay on June 19th, 1996 in Mendocino.

Can you tell us how your scientific life started and how you ever came to be in Calvin's lab.?

**LK:** I was always interested in science from the time I was in high school and, as far as getting into Calvin's lab. goes, I was doing graduate work at the University of Michigan and one of the professors, Professor Westerham (*spelling?*), had asked Melvin Calvin to come and give us a seminar on photosynthesis, path of carbon type of thing. I listened and I was absolutely fascinated so that I got over my natural reluctance to talk to people and I went down to the rostrum and I said: "Professor Calvin, is there any possibility I can work in your lab.?" When I came out (*to California*) job interviewing, I went up to the Radiation Lab. and I got the job.

**VM:** What did he say to you when you asked him that?

**LK:** He said, "Get in touch with Mr. So and So (whose name I have forgotten) who handles personnel" and so that's what I did.

**VM:** You were a chemist then, were you?

**LK:** I was strictly an organic chemist. I did my graduate research pouring two stinky chemicals together and getting a lachrymator. Pretty much organic chemistry.

**VM:** When you heard Calvin give this talk, was he doing his chromatograms and photosynthesis stuff?

**LK:** Yes and that was what was so fascinating. The type of thing that I had been doing was organic synthesis and just the idea of doing these separations on something as simple as paper chromatography was just mind-blowing.

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**VM:** You hadn't any special thing you wanted to do in his lab. when you went there?

**LK:** No.

**VM:** Were you a graduate student, did you go to him as a graduate student or did you have a PhD?

**LK:** No. I was looking for a position, a money-producing position. I had finished the graduate work at Michigan.

**VM:** One of those things one has to have to live on.

**LK:** Right.

**VM:** So you came there. What happened when you arrived in Berkeley?

**LK:** I started working in Donner Lab. and at first I worked with somebody who was working actually on nucleic acids. We were hydrolysing them and sending them through columns and we came out with this fascinating little piece of information as, gee, there is the same molar quantity of two of the bases as the other two bases. Isn't that remarkable? I have forgotten the man's name now that I was assisting in that.

**VM:** This was before the great event?

**LK:** Absolutely. Not long before the great event. So in '52, when I was on a visit to Europe, we went to the Cavendish Labs., we saw the labs. that were to produce this. It was really fascinating.

**VM:** I was just trying to remember, you can't remember who it was that you did this DNA work with in Donner?

**LK:** No, I don't.

**VM:** Ed Bennett, I think, had been in Kalckar's lab., hadn't he, in Copenhagen?

**LK:** Yes, but that was later.

**SM:** May I ask what year was it that you started?

**LK:** I started in '49, early in '49 as a matter of fact.

**VM:** When you first actually got to the lab., Day 1, Hour 1: who did you meet and how were you received, what happened, what did it look like when you got there?

**LK:** That was a little bit awkward. It turned out that when I did the physical exam my blood count was a little bit off and I had a spleen that they could feel so they wouldn't let me start to work immediately. We were monitored all the time for radiation and so

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on. I kind of hung around for a while. I can't really remember: we were up in the second floor of Donner (*actually it was the third floor*); of course, Ed Bennett was there, this other man that I worked with. I worked with a young woman called Marilyn Meinke. We did propionic acid metabolism in mouse liver slices. Those data came out...my maiden name was Daus and I called that the Daus Mouse paper when it was published.

**VM:** When you say that you couldn't work at all, did they literally not let you work or you couldn't work with radioactive materials?

**LK:** They literally would not let me start working until my blood count became within the range. They figured it might have been a subclinical case of mononucleosis. But anyway, here I was, with something I had never heard of, "Why can't I work in this lab.?" It resolved itself. I can't really remember the very beginning. I am sure I must have talked to Bert Tolbert at that time because Bert was probably the first person I really would have talked to in the lab. when you ask what happened right at the beginning. I was probably talking to Bert. Somebody called Dorothy Mabee was also working in that particular lab.

**VM:** In the same room?

**LK:** In the same room — Dorothy Mabee, Marilyn Meinke and I were in that room, and I think we also did this hydrolysis of the DNA stuff.

**VM:** You were presumably aware of the fact of the group being split between the two buildings.

**LK:** Oh sure.

**VM:** Did you get to know people in both places?

**LK:** Absolutely. Remember, other people undoubtedly have told you that we had weekly meetings so you knew what was going on in both (*places*). At that time, Vicki Haas (is that her last name?)...

**VM:** I think so.

**LK:** ...she was working over there, Al Bassham was there, Martha Kirk must have been there at that time. I wasn't working directly with them. Sure; we knew what was going on in ORL.

**VM:** When you arrived they were already in those two buildings, originally they hadn't been, they had all been in Donner, but by the time you were there they were in the two buildings.

**LK:** I think so. The downstairs (*of Donner*) was Gofman's group at that time.

**VM:** How did you progress in your own work in Donner? How long were you there?

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**LK:** I was about two years, I guess, in Donner or two and a half and then some of us decided that — we had all these people visiting, postdoctoral years from Europe and everybody was going to Europe. So there were going to be four of us who went to Europe and actually, it ended up with just two of us going. At that point, that was about two years — Dave Kritchevsky, for example, his name must have come up.

**VM:** Yes; he's in Philadelphia at the Wistar Institute.

**LK:** Right, exactly. (*He had*) a wonderful sense of humour. That was one of the incentives: hey, let's go to Europe. As I say, two out of four of us actually did. When I came back and I wanted to work again, then it meant going over into the photosynthetic group which I really was very excited about.

**VM:** Did you actually have to resign your job in order to go to Europe?

**LK:** We did because we were doing the “Hey, we're going to stay until our money runs out.” Which is basically what we did; we were over there about five months. We had enough money to get back and then we needed a job. The woman that I went with had worked for Gofman.

**VM:** So you came back and presumably went back to the same people.

**LK:** Yes: “Can I have my job back again?” And they said “yes” so I took it.

**VM:** Then what did you do?

**LK:** Once I got over into the ORL?

**VM:** Well, presumably whoever...Did you think you would go back into Donner, doing the same sort of things you had been doing before?

**LK:** I can't really remember that. I don't know. I just know I was very pleased to be in the photosynthetic group. Because, let's face it, I think that had a little bit more status. In Donner there were a number of small but not very coordinated efforts going on. Dot Mabee was working on carcinogens and there was this little DNA project going on and we were doing animal metabolism and that kind of thing. I think (*Eu*)gene Jorgensen, for example, was working in one of the labs., not the lab. that I worked in but on the second floor of Donner.

**VM:** If I can hark back to Donner for just a bit before we get to ORL: did people in Donner in the separate rooms each know what the others were doing?

**LK:** That was partly this community that worked for Calvin. Yeah; we had the meetings. Rapoport, for example, another faculty member, would sit in on these. Somebody would present what they were working on. I think we knew what was going on.

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**VM:** During the day, in the course of your ordinary activities, were you always wandering up and down the corridor in and out of other people's rooms? Was it that sort of thing?

**LK:** No. I think you were doing your work where you worked. You had a lab. bench, you'd go down to the animal room, say, if you had to sacrifice an animal. So you knew what was going on elsewhere and you went elsewhere in the building but it wasn't too much of a going in and talking to somebody and then coming back.

**CM:** Was that different in ORL?

**LK:** ORL, of course, was kind of one great big open barn kind of place, rather than separate rooms.

**VM:** Did that encourage people, in your experience, to interact more directly?

**LK:** Probably, yes. Plus in ORL pretty much we were all working on aspects of the same problem. You had photosynthesis and I was doing the sugar degradations over there as were a couple of other people; we were all using the same chromatography room so you were in and out there. I think there was definitely more interaction in ORL than there had been in Donner.

**VM:** When you got into the photosynthesis group, at what stage were they and what did you start to do?

**LK:** I was pretty much on the sugar end of it; I was the sedoheptulose person. At one time I probably knew more about the middle carbon of sedoheptulose from very quick — you know from the first half second of photosynthesis — anybody else in the world: expert (*in*) a very small area.

**VM:** Rather important.

**LK:** I started on sugars and, of course, we separated the phosphates on the chromatography, the technique was already, you know it was just a technique that we used but almost each of the carbons in the 7-carbon chain had to be snipped apart by a separate method. Sometimes you'd get two of them together and then you'd get one of another (*sugar*) and your data wasn't as neat and clean as perhaps you might have wished it to be. But you were working with very low amounts of radioactivity that got in in this very short-term sugar phosphate.

**VM:** Did you develop the methodologies yourself?

**LK:** No.

**VM:** Where did they come from?

**LK:** Out of the literature.

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**VM:** I see. There were established methods for doing this.

**LK:** Yeah, they were pretty much. For example, I don't remember exactly, but I think I had to make some aldolase from rabbit tissue at one time as the method of snipping off one of the carbons. I hadn't done it before; you know, it was right out of the literature. I would say that probably on things like that it was probably either Andy or Al that may have suggested where to look for these things. I have forgotten whether the people, wasn't it Ann that was working on the ribulose?

**VM:** Ann? Which Ann was that?

**LK:** What's her last name?

**VM:** Zweiffler?

**LK:** Yeah.

**VM:** The one who married Bert Tolbert?

**LK:** Yeah.

**VM:** I don't know.

**LK:** I'm a little dubious on these names. Whether she was using exactly the same methods that we used on the sedoheptulose, I'm not sure.

**VM:** Can you remember at that time, when you were doing those degradations of sedoheptulose and other people were doing degradations of other sugars, and, presumably, day by day and week by week you were accumulating results and no doubt discussing them, was this the time when people were drawing schemes and trying to relate the information you had?

**LK:** Absolutely. I remember that in some of the early days the data that I was getting didn't fit with the two CO<sub>2</sub>s coming onto the recipient at the same time. So, there was almost, you know, a little pressure: "hey, are you sure that this data is perfectly good?". I would say "to the limits of how good I know it is, it says this and not that". Yeah, schemes were always coming up and being abandoned.

**VM:** I'm trying to get a sense of the frequency with which you did this. Were you talking, as it were, every day, did this topic come up again: what's the latest today and what do we make of it, or was it less frequent than that?

**LK:** Certainly with Calvin it was much less frequent. With Andy, who was in the lab. all the time, it was more if I was having problems getting something. And each one of these degradations, you had to do the experiment in the lollipop and basically the things that I was working on were mainly the soybean things rather than the algae. So, you would do your experiment and then you had to do the extractions and then you would have to run your chromatograms and then you would have to do the X-ray

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things from them. Then you would have to cut them (*the radioactive spots*) out, elute them — elute.

**VM:** Elute.

**LK:** OK. See even with this time some of the technical language has certainly escaped me!

**VM:** But much of it is obviously still there.

**LK:** So it would take quite a while. Then you would have to have the correct — well, for example, when something had happened, was when for one of the degradations I had to have carrier sedoheptulose. So, we went out on a sedum collecting expedition and got a whole basketfull of sedum that is supposed, the particular genus of sedum, that was supposed to have sedoheptulose.

**VM:** Where did you find it?

**LK:** It was up near where Calvin's house was. I don't think it was on his lot but it was on somebody else's lot. We came back with the basket. That was kind of a fun process, to do the mashing, like making wine perhaps? Mash it down, boil it down, eventually crystallise it and then I had my carrier to put in with the sedoheptulose that had the radioactivity in it from the short-term photosynthesis.

**VM:** I guess those were the days before you could buy it commercially; you had to make it yourself.

**LK:** We certainly didn't buy it commercially. We made it ourselves.

**VM:** Did you know what sedum looked like yourself?

**LK:** That particular sedum?

**VM:** At the time when you needed to collect it?

**LK:** I know what sedums, the general kind of sedums. I don't think I knew what that particular sedum looked like. Remember, we had plant physiologists in the group, we had lots of people — that was the good thing about it. You had people who knew physics, people who knew chemistry, people who knew plant physiology and everybody brought these things together.

**VM:** Can I hark back just for one minute while I think about it? When you first started and you first had this talk with Calvin in Michigan, and you were a chemist and this was not the sort of chemistry you had heard about before, presumably — was it not a bit, what shall I say, ambitious, did it not seem a big jump to go from where you then stood into this new area?

**LK:** Sure it did. On the other hand, I think it was probably the kind of thing that I really had been hungering for even when I was in straight organic chemistry. In other

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words, getting closer to the natural world was definitely something that I think was more normal for me than making lachrymators.

**VM:** Had you done biochemistry before?

**LK:** I never had any biochemistry.

**VM:** Or biology courses of any sort?

**LK:** Only in high school. I had the interest in plants, was very strong, the interest in the out-of-doors was very strong. So the idea of doing something that was more related to that than straight chemicals. Perhaps if I had come a little different way, I probably might like to have gone into biochemistry. But, you know, at the time, when you make these decisions, chemistry seemed a very good place to go.

**VM:** OK; back to the sedoheptulose and the degradations at the time then. I recall that the earlier degradations had been of the hexoses and this had enabled people to get an idea of how the hexoses were formed. Then you were in the next stage, trying to work out this complex of what came next.

**LK:** That's right. That was both the sedoheptulose and the ribulose diphosphate which eventually turned out to be *the* acceptor.

**VM:** Who were the other people around you at the time who were doing degradations? Can you recall them?

**LK:** Anne, and I, of course Andy was involved...

**VM:** Al (*Bassham*) would have been an interested party, I expect.

**LK:** Absolutely. Certainly the method of doing the short-term photosynthesis with the radioactive CO<sub>2</sub>, that was Al's set-up, and we were certainly working together on all of those experiments. I am not sure I know who else had been on that sugar degradation. The names are on the sugar degradation paper and you can look it up.

**VM:** That's the big paper, Path XXI, isn't it? I can't remember.

**LK:** There were the two papers, you know, that they cited in the Nobel, the one was the reservoir and that was the one that Alex Wilson was involved in and I'm not sure again who else was working on that, and then the sugar degradations. I didn't do anything with the ribulose. I was strictly the 7-carbon kid!

**VM:** These kinetic experiments were being done, as you say, by Alex. Were you also there when Peter Massini did his (*experiments*)?

**LK:** Oh yes, absolutely, sure. That's a name I had forgotten.

**VM:** I have found him as well. He is retired and lives in Switzerland.



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**LK:** Oh. Where does he live?

**VM:** Switzerland. Those people presumably were talking to you as avidly as you were talking to them about what was going on.

**LK:** I can't really remember that much about who you talked with about what.

**VM:** In the photosynthesis, in ORL, can you remember which room you were in, who your neighbours were?

**LK:** Sure. I remember the room, I'm not sure about the neighbours. I can see the room. I was in the room, the office, if you came in from the east side, the offices were to the right and the cold room was to the left. And then as you went through that kind of corridor you came into a big room and that's the room that I was in. There were a couple of aisles down there. I was on the left side of that aisle, for most things. You know, there was a rack...

**VM:** That was your home bench?

**LK:** That was my home bench, where I boiled down the sedum and stuff like that. But there were the racks where we did the original experiments; those were both in that room, there was a hood in the next room over. I couldn't place other people at the benches, though, who was actually where.

**VM:** You presumably spent a fair amount of your time running chromatograms which, I think, were upstairs.

**LK:** They were upstairs, yes.

**VM:** I can't remember where the dark room now was, where you put the things against film — must have been around somewhere.

**LK:** I'm not sure I can remember either.

**VM:** It might even have been downstairs in the basement somewhere with the counting room.

**LK:** I certainly remember the chromatogram room but I don't remember the dark room.

**VM:** Did you write some of the papers?

**LK:** No. I usually gave the data to other people who wrote the papers.

**VM:** We talked earlier on, before we started recording, about all the social activities that were going on in the lab. and you clearly had pleasant and strong memories of those sorts of things.

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**LK:** Oh yes.

**VM:** What struck you about it as a group? You were one of the young people in a group of lots of young people.

**LK:** They were young and enthusiastic people and we spent time not only at work but much of our social life outside of work was with people from the lab. and it was usually outdoor activities — hiking in the he summer and skiing in the winter. We would take off often on, late on Friday night and get back late on Sunday and be back in the lab. It was a really wonderful sense of camaraderie that permeated both working together and playing together.

**VM:** Did almost everybody go?

**LK:** That's hard to say. No, not always. There were a couple of things that I remember once and I don't remember whether that was still — I think I was still at Donner, when we went as an entire group, including Dr. and Mrs. Calvin (Gen and Melvin came too) and we all went camping at Twin Lakes near Sonora Pass. That was really a fun trip and I think some of the people went mountain climbing, some went swimming, and so on. That was one instance I know when pretty much the entire group went. It was over some three-day weekend or something like that. Often it was smaller groups but sometimes a pretty large group. For example, there was kind of a standard climb of Mt. Lassen on skis on Memorial Day. One time from there we went on, some of us, up to Mt. Shasta and went up to (*that?*) Shasta. Rosemarie and Hans Ostwald, for example, went on a number of the ski trips. So there were different groups that went. Then there were some people that, I guess, weren't interested in that.

**VM:** When you went out on these activities, did you continue to talk science, as you remember, as you climbed the mountains, or...

**LK:** I can't remember that!

**VM:** I bet some of the time but not all of the time. Did people work all hours of the day and night in the lab.?

**LK:** I'm not sure that I really remember that. I don't think we kept 8-5 hours and if you were doing an experiment that needed to keep going, you would go. And there were probably some people that did more all-hours than I did. I was a 23-27 year old "kid" and I was very interested in the life that wasn't in school. I was really enjoying that part of working there. It was a wonderful group to work with, the science was exciting and after you had been in school for how many years it's been really great to do these outdoor things, that we hadn't been able to do before.

**VM:** Did you have lots of friends in Berkeley outside the group or were your friends mostly inside?

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**LK:** I lived with a number of other young women and some of them were certainly on these trips, also interested in that.

**VM:** So when the group went out, the Calvin group went out, there tended to be other people who friends coming along...

**LK:** Sometimes, sure, both roommates and I had a friend who was up at Davis at the time and she would come sometimes. I would say at the time it was the Calvin group that was kind of the main group.

**VM:** Looking back on it now, and we'll talk a little bit about the contact you have kept with them, looking back to how it was when you knew it, did you find it a remarkable phenomenon? Had you experienced other things like that? What do you think made it work the way it did?

**LK:** I don't know. When I was a sophomore at UCLA I had worked for Dr. Dunn who actually was in biochemistry down there, doing...eluting out amino acids and checking their polarity and things like that. That certainly was not this kind of a group. I think I knew at the time that it was a very special group.

**VM:** What do you think made it special?

**LK:** The excitement of the project, the fact that everybody was young and interested in it, the fact that there were so many people coming from different, from Europe because of the excitement of the project, the enthusiasm of Calvin himself which, of course, was what had gotten me there. I think part of the unity of the group came from Gen who really provided warmth, a real warmth. For example, when our child was born, Gen was giving me motherly advice, how to take care of this baby and she gave me this lovely little figure, bronze figure of a little boy — here it is — and I thought that was the most charming gift to give to somebody who has just had a baby. Not something for the baby to wear or something; this is one of my prize possessions and Eric and I really enjoy it. That was the kind of warmth, I think, that Gen provided to people in the group.

**VM:** Did you see her often? Did you see Gen often?

**LK:** She would come to the lab. and there would be affairs where she was there. They didn't usually go on these "outings" kind of things except for the one time that I told you about. But there were certainly get-togethers. We knew her, yes.

**VM:** You said that she gave you that figure when you first child was born. Were you married while you were still working in the lab.?

**LK:** Yes. Eric and I met each other after I got back from the trip to Europe when I first really started working in ORL. He actually was a friend of the young woman that I had gone to Europe with. We met and very shortly decided that we would get married. We met, I guess, in August and were married the next February. Before I left the lab. I had my first child. I kept on working after that.

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**VM:** Apart from the formality of getting married, was it duly celebrated in the lab. as well?

**LK:** I'm sure. Althea Van made chicken salad for our wedding and she was one of the dishwashers. On the other hand, the wedding was a very small wedding and there really weren't many people from the lab. who came to the wedding.

**VM:** I was wondering whether in the lab., apart from the formal arrangements you had made for the wedding itself, whether informally people were celebrating your wedding, your marriage. I seem to remember it as a place where anything which could be celebrated was celebrated.

**LK:** I don't remember that particularly. I remember more getting ready to get married; I was more focused at the time on Eric than I was on things in the lab. I can't really remember that.

**VM:** Can you remember any other people getting married while you were in the lab.? Nobody has actually mentioned this in all of the people we have talked to. Maybe you were the only one.

**LK:** I don't know. I really don't remember. Alice (*Lauber, née Holtham*) was married after and Marilyn, I think, was already married.

**VM:** I don't know; I must ask Marilyn.

**LK:** I really don't know. Dick, I know, was married sometime during that time but he may have been married even at the time I was in Europe. I don't really remember. I was focused at the time on my life and the changes in my life.

**VM:** What about the other sorts of parties they had, Christmas parties? Which I seem to remember as a time when celebrations took place.

**LK:** I am sure they did but I can't give you particulars. I remember: there was certainly some kind of exchange of presents because I remember when Kazuo and (*indecipherable*) (*Shibata*)... he was the person that gave me something at one of these parties and it was a lovely hand-written little poem about eight by two (*inches?*) in Japanese on a gold kind of a rice paper of some sort. That was in ORL. In Donner I don't remember any parties especially. Maybe we didn't, I don't know. I remember that one party at least where we even exchanged gifts.

**VM:** Then at some stage you presumably decided to leave.

**LK:** Eric was going back to school.

**VM:** I see: in Berkeley, presumably?

**LK:** No, this was University of Washington. So that was the reason we left.

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**VM:** What sort of contact have you kept with people since then?

**LK:** A number of them I keep in sort of...well, Alice, more than yearly, Dick Lemmon more than yearly, Ed Bennett Christmas card, Ed Melly (*spelling?*) Christmas card, and when I go to the lab. I see people...

**VM:** Did you get down there reasonably often?

**LK:** No.

**VM:** Is it special occasions when you happen to be in Berkeley, you drop in?

**LK :** It's more if I happen to be in Berkeley, sometimes I will drop in. But there are very few people there now that I know.

**VM:** During the years there have been people, over the years since the time you left until the present time...

**LK:** We didn't live near there. When I left, we were 3-1/2 years in Seattle and then we moved to San Jose and I had three kids and I was working and I didn't get up to Berkeley very often.

**VM:** I want to come back to the Berkeley scene but, now that you have mentioned it: briefly what did you do with your life after you left Berkeley? You said kids and you were working and moving...

**LK:** When we went to Seattle, I worked for Frank Huennekens in the Biochemistry Department. It was, I think, a 3/4 time thing (*i.e. job*) because I already one child and later on I had another. (*Note: Frank Huennekens was a former associate of Melvin Calvin in the Chemistry Department.*) So that was good; I enjoyed doing that. We were working on folinic acid, one-carbon metabolism still, but folinic acid and a number of other biochemical approaches to things. Then, when Eric finished with his doctorate, we went to San Jose and then we had our third child and I stayed as just a mother, I was very busy for about five years. As soon as the youngest one started to school, I in the meantime had gotten a teaching credential so I started teaching because that fitted very well with having three school-age kids. Then, we went to Berkeley again; Eric was on a sabbatical in Berkeley; while we were there I did some volunteer work at the Lawrence Hall of Science but I didn't work while we were up there (*in Berkeley*). I also worked in a special project of teaching math to, I guess you used to be able to say, economically disadvantaged kids in the San Jose area. It was a really fun project. As far as the teaching that I've done, I think that was the most rewarding kind of thing that I've done.

Then I taught some more at high school and then we had...Oh, in the middle there I also worked for three years for the US Geological Survey as a geophysicist...

**VM:** Was that in Menlo Park

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**LK:** ...in Menlo Park. I was in two groups there: one was studying Alaska earthquakes and the other was analysing data in the attempt to get earthquake prediction. At the time they had what they thought was a pretty good predictor, it turned out to be a false goal. They certainly don't do that anymore. That work was fascinating. Then I guess I got down into teaching again and then Proposition 13 wiped out all the teaching jobs, so I got a job at Lockheed by selling myself as a programmer. At that time I had sent out three...I have done a lot of job searches, so I had three resumes: One as a teacher, one as a chemist and one, even though all I had to put on it was that I had worked at the Survey on some programmes, one as a programmer. I got a job as a programmer at Lockheed and I moved then from Lockheed to IBM as a programmer where I stayed for eleven years.

**VM:** That's a pretty a varied career...

**LK:** Very varied!

**VM:** ...one way and another.

**LK:** Jack of all Trades and Master of None.

**VM:** You said it, not me.

**LK:** Yes, I did!

**VM:** One of the points about which people have commented is the influence of ORL, the building itself, the way that place was set up, its openness, and so on. Did it strike you at the time and what do you think of it now?

**LK:** It was a thunky old building, I can say that.

**VM:** Was it nice to work in? Was it good for the work you were doing?

**LK:** The place itself had sort of a fallen-apart feel to it but the people were fantastic. The openness, I think, was very good. Yes; I really liked the openness much better than the newer lab. in Donner where, you know, you were in your own lab. and you went down a long hall to talk to anybody else. That part was good. It did have kind of a feel of a building that was falling apart.

**VM:** In the end, of course, it was encouraged to fall down and they built a new one, they built a round building trying to embody the philosophy of that.

**LK:** I know that building.

**VM:** How well do you think they did? You've been in there: how did it strike you?

**LK:** Just by the fact that it is so much bigger, I don't think you can get interaction with that many people. The idea behind it, I think, was great. Now it was quite a small group when I was in ORL because that was pretty much in the early days. I think you

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really knew everybody in the group. The times when I have gone to the Bio-Organic Building, it seems like kind of any other building. There are just lots of people scattered around and I don't see how anybody can really know everybody that's working in here. I don't know whether that's true or not. I have never worked there.

**VM:** Well, of course, if you do work in a place and see people every day then you do get to know them better than if you just look in.

**LK:** Oh sure; I don't think you can evaluate that.

**VM:** What's the fisherman story?

**LK:** There certainly was the fisherman story, and you are going to hear about everybody.

**VM:** But tell us.

**LK:** It was a lot of fun because we had a lot of fun with it. Alex Wilson was writing up the work for the paper which was going to be both his thesis work and the paper in the JACS. There was a diagram in it which involved the steady-state bubbling of the gases through the reservoir. The reservoir looked like a beaker, as it was originally drawn, a square beaker, with bubbles in it. Alice Holtham (at that time; [now] Alice Lauber) was doing the drawing and, of course, it hadn't been reduced yet. Just for fun, I don't remember who got the idea of putting the fisherman there but once the idea was there some of us really said "go for it". So the fisherman was placed sitting on the rim of this reservoir and one of the bubbles was converted into a fish and the fishing line, he was catching that fish. We all thought that was very funny. Then, of course, there was the pressure: put it in the paper, put it in the paper! And, again, I'm, not sure who-all knew that this was actually going into that paper but it got past the two referees and it's in the JACS. It's probably the only humorous thing that was ever published in JACS. I was glad to have been there when it happened.

**VM:** And your name is one of those on the paper, isn't it?

**LK:** No, not on that paper.

**VM:** Oh, not on that paper. That's right: that's Alex.

**LK:** There was the other. You know, the two things that came together in the Calvin.

**SM:** You mention Alex, this was Alex Wilson, I take it.

**LK:** This was Alex Wilson. He had been a graduate student and this was the work for his doctorate. The paper, I don't know whether it went into his thesis, but it certainly went into JACS. I have the reprint somewhere to prove it.

**VM:** Yes, we've seen it. It is there, as you say, it's very small, once it was reduced.

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**LK:** Once it was reduced it was very small and I can see how the referees didn't notice it, but it's still kind of funny.

**VM:** The fisherman story apparently had repercussions, as your husband Eric will tell us.

**EK:** Recently at Stanford one of the graduate students in the group that I am associated with found out that I had been to Berkeley earlier. It turned out that the way we identified both his family's affiliation and my affiliation with Berkeley was through his father. The way that came up is that he essentially said "If you knew my father, then you surely know about the fisherman story". Of course, I immediately said "indeed I know about the fisherman story". That clearly identified Bruce as Alex Wilson's son, the very same Alex Wilson that worked with Lorel at an earlier time.

*(Tape turned over)*

**VM:** In this lab., clearly many people had an influence on ideas and so forth and Calvin, I guess...do you feel was the main source of originality?

**LK:** No, I certainly don't. I think Andy had a great deal on some of it on the smaller issues. Somebody like Al. I never worked really closely with Dick Lemmon but I would say there was a great deal of originality there. But on the big issues, I think that where I heard the big theories come from was from Calvin. I think I mentioned earlier than when I would take my data over, sometimes I had to argue that the data was right even though it didn't fit with, say, this dicarboxylation theory that I think was published actually before I went to the lab. I was bringing in data that didn't agree with that and he tried to talk me out of it and I said "no".

I made use of this later on when I was teaching high school because to me it seems that it's really a measure of greatness to have good ideas, to test it, and if it doesn't fit, you abandon it. This is very hard to get over to kids who want something to be right or wrong and not to stick their necks out. I would tell this to kids who were in my classes that I once worked for a guy that eventually got the Nobel Prize and he published something that was absolutely wrong. Then, what did we do? We got more data and then he published something that is now actually in your biology book, because it was in the book I was teaching from.

**VM:** Presumably there were lots of false leads at various times in the lab.?

**LK:** Certainly there were, at the lower level where we were doing things in the lab.; everything that you do didn't always work. Obviously, I mean that's part of science. Then you would do it again in another way and eventually you get data that you feel is accurate enough to use, to draw more conclusions from.

**VM:** When you observed Calvin in discussion about ideas, some of which may have come from him, although, no doubt, he defended his ideas, was he willing to listen to good arguments against them?



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**LK:** Obviously, he did; sure, obviously. We did have these weekly meetings, you know, and everybody would be there and some people would be more vocal than others. I guess I mentioned that Rapoport would be there and sometimes they would even argue about what was going on. Definitely; obviously he was open to new ideas but he was willing to jump ahead. There for a while he was very interested in the origin of life kind of things and some people were doing some experiments to see what kind of things were there. Obviously that's still an open question. So everybody still has to have an open mind as to how it may really have happened.

The neat thing about Calvin is that he was willing to think about these very universal questions. He was able to do the physical chemistry and the physics. Some of the people, and I didn't interact very much with them, were really much more interested in the quantum part of the photosynthetic use...the ATP and the energy end of it, let's say that, and I didn't follow that that much. He was certainly able to have ideas in that area as well as in the path of carbon biochemical end of it.

**VM:** He might have been quite unusual in not using his authority to push through ideas which were untenable and would be more influenced by argument, I think, than perhaps some people of similar calibre might have been.

**LK:** Could be.

**VM:** That's the sort of story one might...

**LK:** That's what we sort of feel that some of the discussion...

**VM:** Well, thank you very much for telling us all this and for inviting us to stay among the redwoods of Mendocino. It has been pleasant.

**LK:** Well, you've been very welcome.