

## Chapter 17

### VIVIAN MOSES

Berkeley, California

June 16th and 29th, 1996

VM = Vivian Moses; SM = Sheila Moses

**SM:** It is Sunday, June 16th, 1996 in Berkeley, California. I am Sheila, Vivian Moses' wife, interviewing him about his part in the Calvin group during the 1950s and early 1960's. Having been married for eighteen months we came to Berkeley together in 1956.

How did you first hear about Melvin Calvin?

**VM:** There are, I suppose, three strands in my wanting to come to Berkeley. I had been working with a graduate student, whose name was Glenn Bartlett, at University College in London on the metabolism of a fungus with which I was particularly associated, using radioactive techniques and also using paper chromatography and radioautography which we tried to work up following the example in a paper, I think by a man called Vickery but I'm not sure. It wasn't too difficult to get the chromatograms and to get the radioautographs but what was extremely difficult, and I subsequently found out why this had been the case, was to identify what the spots were. It had taken Calvin and his group many years to do this and I and my graduate student were simply not up to it. That was one strand. The second strand was that it was very much in the mood in England in the fifties for postdocs. to go to America for a period in order to pursue their scientific careers. Naturally, I wanted to do that too and I was alert to the possibilities of where to go. The third point was that someone whom I knew, someone in the family had suggested to me that if I was going to go to America it would be very nice to go to Berkeley because Berkeley was a good place, good academically and also very pleasant. Furthermore, if I went to Berkeley inevitably one would then see the east coast whereas if you just went to the east coast you might not see the west coast.

So, I was very receptive when Calvin's lecture which he gave, I suppose, towards the end of 1955 at the Institution of Electrical Engineers, as I remember, in London on his photosynthesis work was announced and I went. I think I must have known about him at that time otherwise, I don't see why I would have gone. I was terribly

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impressed with what he was doing and what he was saying, and this was clearly going to be the answer to all my technical-scientific problems.

**SM:** Were you able to meet him on that occasion?

**VM:** No. I have to confess that it didn't occur to me in the (*lecture*) hall itself to try and meet him. I knew already that it was very difficult if you weren't known to anybody on an occasion like that actually to attract his attention. So I didn't. It took me a little while, I suppose a day or two, to wind myself up to the idea of just what to write to him. By the time I decided to do that and decided what to say and so forth, and found out where he was, he had gone. So I missed him on that occasion and then had to write to him in Berkeley. So, that was how I came to know about Calvin and came to want to go to him.

**SM:** I now understand what decided you that you would like to do that. How did you go about it?

**VM:** I needed to get money. I wrote to Calvin and expressed an interest — I can't remember the exact terms — and he wrote back what I subsequently found was pretty much his standard letter that he wrote to people whom he didn't know. That was: "Get yourself money on a competitive basis and come, and if you can't get quite enough money (I'm not sure whether he said this explicitly to me but it was certainly what he said to many people), if you can't get quite enough money then we'll supplement it". The point he made was an entirely valid one, I think. For people he didn't know and who were not specifically recommended by colleagues and close acquaintances of his, he needed some filter to ensure that they were at least of reasonable quality. The filter he used was their ability to get money.

So I started to look for money. As I did not have a permanent faculty position at University College at that time (I was the equivalent of an assistant lecturer), as far as I know I was not eligible for a Rockefeller award. Looking around for possible sources of money — and I cannot now remember how many possibilities I found at the time — the one I eventually applied for and got was a University of London Postgraduate Travelling Studentship, at which I was successful. This paid the princely sum of £1,000 which, at that time, was \$2,800, to cover all my expenses for a year — travel as well as living expenses.

**SM:** For me to ask you how you reckoned that this would be enough is rather silly because obviously I know. Tell me how you managed to work this out. After all, you had a wife, you needed both to travel and to live in Berkeley for that period.

**VM:** At that time it was very easy for British-born citizens to obtain American immigration visas. As you were willing and able to work, and you were a British-born citizen, you applied and were granted an immigration visa. And the intention was that you would work when you got there and indeed, that's exactly what happened. For the first year, at least, you certainly earned more than I did with my grant. Between us, of course, we managed. We were taking a gamble but, as far as I remember, the advice we got

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from other people it wasn't that difficult for people with your professional experience to get jobs and that's what we counted on.

**SM:** And what made you decide to stay...no, perhaps that's a little premature. Of course, I know that you arrived in Berkeley in the autumn of 1956. How were you greeted and what did you think of the place and the people you met on that first occasion?

**VM:** Perhaps I should say just a little bit about how we actually made the journey. We travelled by sea on a Dutch ship called the *Ryndam* which took eight days to cross the Atlantic. It was quite exciting, even the boarding of the ship. We boarded at Southampton but the ship wasn't in the dock — it was out in the roadstead and we were taken out on a lighter — not just us, but other people as well. And at night time, as I remember, but I'm not quite sure about that. Eight days of wallowing on the Atlantic were not entirely to my taste, and we landed in Hoboken, New Jersey, that's where the Holland-America line had its base, then at any rate. We were met by members of our family, as it happened, and we stayed for a few days in New York and made arrangements to ship direct to Berkeley, or direct to Oakland, a trunk — or maybe more than one trunk full of our possessions because we were advised to take things like bed linens, blankets, etc. to the United States since we were likely to get an unfurnished apartment. We did all that, stayed for a few days in New York and went across country by Greyhound bus. Again, there were two reasons for that. Firstly, it was the cheapest way of travelling and secondly we thought we would get some idea of the country. And, indeed, we did. We had never been anywhere before where we had travelled over such vast distances by land and although there was only modest comfort at times, it was a fascinating journey.

So we stopped for a few hours in Chicago to change buses and we also met some friends who lived there. We travelled across country, stayed in Reno overnight so as not to arrive in Berkeley at a ridiculous hour. Finally, we came one day to Oakland bus station at about midday and were met by Paul Hayes who was then the, I suppose you would call him the laboratory manager. He picked us up, took us to the lab. I can't remember now whether I actually met Calvin on coming into the door. I met him pretty soon because we stayed that night in his house. I must say the organisation was incredibly efficient. There was a secretary in the Old Radiation Lab. called Dee Lea Harrison who immediately took charge of us and set about finding a flat. I think, but I'm not quite sure, even on that first afternoon she had already started to drive us around. But if it wasn't on the first afternoon, it was certainly the next morning. We found a flat very quickly on Bonita Avenue at \$75 a month, which was unfurnished, or at least partly unfurnished. I remember that the lab. in general always seemed to have a stock of odds and ends of furniture and household goods, and so forth to lend to postdocs. who came on a temporary periods and all sorts of people brought stuff. I have a distinct memory of Calvin himself coming upstairs with a couch, a cane couch, on his head — or something like that. He certainly was a contributor and he certainly helped. He used to do things like that; he would join in and carry stuff around.

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**SM:** Did it strike you then that this manner of greeting and informality was in enormous contrast with your experience of British university departments and the way that people behaved towards each other?

**VM:** Well, a total contrast, of course. Perhaps I should say at this point in order to make my own position clear. By the time I came here I had actually six years of research experience (by the time I came to Berkeley), three years as a research student at University College in the Botany Department (what was then the Botany Department), and then followed by three years as an assistant lecturer in the same department, but with very light teaching duties, so it was essentially still doing research. My formal supervisor as a research student was Professor W. H. Pearsall who was an elderly gentleman, rather deaf, he had an old fashioned microphone and amplifier which, as far as I remember, he wore on his chest. The story around the department was that if he didn't want to hear anything, he would simply turn it off and sit there, not hearing and looking benign. He was really not very experienced in the sorts of things I was interested in and actually didn't play much of a role in supervising me. But there were three younger people in the department with whom in fact I worked and from whom I learned what I learned.

And so, among that small group of people there was friendship and they were all much of a muchness in age, each within a relatively few years: although I was the youngest, the others couldn't have been more than three or four years older than I was. But there was certainly not the enthusiasm in the place that I found when I first came to Berkeley. It was really a revelation to see what went on. Interestingly enough, ten years later another person, Ian Morris, came to Calvin's lab. exactly from the same place that I had come from, from the Botany Department at University College, and I was very amused and entertained to see that his reactions to Calvin's lab. really paralleled my own very closely. I was, in a sense, reliving my first arrival in the place when Ian came.

**SM:** So after this first impression of the contrast which you have just described between the manners, if you like, between people in university department in England and in how it struck you in California, there was, of course, the Calvins' personal hospitality and helpfulness and informality and warmth. How did California strike you after post-war austerity England?

**VM:** There's one thing, of course, I must correct, that is I was not aware of all the British university scene, only the bit that I saw myself and so that was only some of it. Maybe other places were indeed different.

**SM:** Of course, but this was your experience.

**VM:** Correspondingly, in Berkeley, although in time I got to know other places, for the first couple of years I really had very little contact with groups on the campus other than Calvin's group. So the comparison I can make is only between University College before I came and Calvin's group when I was here.

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California, in general, was a very exciting place. There were all sorts of new things to see that we hadn't experienced before, many of them trivial now — supermarkets and parking meters and lots of other things like that. On the other hand, because we were native English speakers there were no difficulties of communication at all. I say, at all — very occasionally there would be a lack of understanding of a word in either direction. On our way across country, you may remember, we stopped in Laramie, Wyoming, and you wanted a coca-cola and I had great difficulty in making myself understood in a bar. Fortunately there was a Canadian there who felt, no doubt, that he was midway between the two cultures and he was able to translate my version of coca-cola to the local version and get the drink. That happened very rarely. There were obviously differences in language, differences in usage, differences in pronunciation, but they hardly interfered. We rapidly found the local way of saying things and people seemed to have no difficulty in understanding us.

The style of life was not unfamiliar. After all, we had been to the cinema enough and we knew roughly what to expect. There were mistakes that we made simply because we made the wrong associations. We would occasionally go into a post office to look for a public telephone because in Britain, at that time, the telephones were owned by the post office. We found here that they weren't and you had to look elsewhere. Aside from minor things like that, there was really very little difficulty. Before very long in our flat we were able to buy a car. That was a very exciting thing, this was the first car we owned; I think we must have answered an ad in the newspaper. One day a red 1950 Plymouth convertible drew up outside our door, this was the car that was being offered, and we were, of course, terribly excited and we bought it for \$250. It had 97,000 miles on the clock at the time and, by and large, it didn't do that badly. One or two things went wrong, but on the whole it served all the time we were in Berkeley and eventually took us back across the country on a rather zigzag route. We finally left it with my cousin in New York to sell on our behalf. Eventually he told us he got \$20 for it. We owed him a few dollars for something he had sent us in the meantime. Anyway, that's what happened with us. Freeways were something else, of course, that we hadn't seen at that time. We quickly got used to it.

**SM:** Clearly language within the working group was something else because there were other postdocs. who came from other European countries and elsewhere. So, tell me something about who the people in the group were and the people with whom you worked mostly, and then, perhaps, something about the social activity both inside and outside the lab.

**VM:** When I first got here (*to Berkeley*), whenever it was that I saw Calvin for the first time to talk science and what I might do, he suggested that I should work with Ozzie Holm-Hansen on the behaviour of deuterated *Chlorella*. To relate it to what other people have said, Ann Hughes at that time had already been working on deuteration in mice, deutering their drinking water, and because of a lot of experience that was already in hand with photosynthesis, as far as I could tell, for no specifically good reason but simply because it might be interesting, Calvin (I presume it was him) had thought it might be a good idea to look at *Chlorella* in a deuterated form. Because I didn't know the techniques and the ways of the lab. and so forth, obviously he thought it a good idea that I should work with somebody who did, and that someone

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was Ozzie Holm-Hansen who was a plant physiologist/plant biochemist and clearly we could easily talk to one another. He knew how to do everything and, therefore, I would learn from him. So, that's the first thing I started to do.

**SM:** Can you tell me for my own information, what is deuteration?

**VM:** Yes. Hydrogen atoms exist in three forms which are distinguished primarily by their weights. The common form of hydrogen has a unit weight of 1; there is a heavier form which has a unit weight of 2 and yet a heavier form, which is also radioactive, with a unit weight of 3. Because the hydrogen atoms differ in their weights, there will also be differences in the way in which they perform chemically because, if you like, the sluggishness with which the heavier isotopes will move. So, there are energetic problems associated with that. Hydrogen is a very important element from all sorts of points of view. It's heavily involved in determining the three-dimensional structures and stabilising those structures of nucleic acids and proteins. It is to be expected, and indeed it was known at the time, that the heavier isotopes of hydrogen would have effects on the stability and structures of these large molecules. So that replacing the ordinary hydrogen in an organism by one of the heavier varieties would be expected to have consequences not necessarily easy to predict. Of the two heavier isotopes, deuterium is stable and not radioactive and, therefore, not in the least dangerous to use. The other one, tritium, is radioactive and there are many more limitations, therefore, on the way in which you can handle it.

What we tried to do was to replace all the hydrogen in *Chlorella* with this heavy version called deuterium, simply because it has two units of weight. We were not successful at that time; I think later people did do that. We could not get the *Chlorella* to grow if the deuterium concentration in their water was more than 60% but with such bugs we conducted photosynthesis experiments in what, by then, had become the traditional Calvin mode of lollipops and radioactive carbon dioxide and so on. It provided for me a vehicle of learning the ropes and, in the course of a few weeks, I suppose, I became as skilled as the rest of them in doing all the things that had to be done.

**SM:** Describe a "lollipop" and its function.

**VM:** A "lollipop" was simply a glass vessel about 4 or 5 inches in diameter, circular in view, flattened so that the space between the two sides of the lollipop was relatively narrow — I would say something like 5 mm — with an opening at the top for pouring liquid in and a large stopcock at the bottom. The idea was that you put the algal culture of *Chlorella* which looked like a green liquid, but it was actually microscopic plants in the aqueous medium, in the lollipop, shone lights from both sides so the algae were very highly illuminated, squirted in whatever radioactive material you wished to study the algal conversion of and, when you were ready to take a sample you opened the stopcock, which was a large stop-cock, and the liquid suddenly fell out straight into boiling alcohol and killed the plants very quickly, and, as it were, "froze" everything for later investigation. It was called a lollipop simply because it looked like the top end of a lollipop on a stick.

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**SM:** Obviously, it must have done from the way in which you describe it. You are working with Ozzie (*Holm-Hansen*) on this problem which you say was later worked on and solved by others, with whom else did you work and what else did you do?

**VM:** The only person that I knew in the lab. whom I had known before we got to Berkeley was someone we had only met a few weeks before; that was Bob Rabin who at that time was in the biochemistry department at University College. A mutual friend of ours told me that we were both going to go to Berkeley so we made a point of meeting one another and so when I got to Berkeley, I can't remember which of us came first, but anyhow he was the only other person I knew. At that time, there was a very strong overseas contingent, although the staff members of the lab. — Calvin himself and the people who were in the senior and continuing positions, people like Bert Tolbert (who may have left by the time I got there; I really can't remember), Dick Lemmon, Ed Bennett, Al Bassham, Ozzie, I thought (although subsequently it turned out that Ozzie was not as permanent as I had imagined at the time), people like that and of course the secretaries and the technicians and so on — were essentially all Americans. The transitory population of graduate students and postdocs. and academic visitors had a high overseas component. Most of them at that time, my guess is, from England and the countries of Western Europe. Germany and Switzerland tended to be well represented but people also came from Scandinavia and from the Low Countries and from France. There were one or two Japanese, not many, one or two and, as far as I remember, nobody at that time from China or from Hong Kong or Southeast Asia or from India, that I can remember.

We, of course, became variously friendly with all sorts of people. There was a lot of community among the postdocs. because they were temporary, they were living in apartments which were not their permanent homes, they had no families for the most part. They might have been married but they didn't have any children and they were not established in the place. Therefore, all the domestic responsibilities which befell the senior members, or at least the permanent members of lab., didn't apply to the postdocs. So they had much more socially in common and ended to mix primarily, I suppose, with one another. Although we knew the permanent staff (Calvin and the others), we knew them well and we saw them socially to a reasonable degree, we were closest in fact to people more of our own age and more of our own, as it were, standing in the lab.

**SM:** As I remember, the permanent members of staff were hospitable, would invite us and other transient visitors to their homes, and generally the atmosphere was a very friendly one socially. There were also, as I remember, weekend trips and picnics and things of that sort among the group.

**VM:** There were all those sorts of things, I think. There were occasional invitations to people's homes. I think they were not that frequent. but they certainly occurred, and there were gatherings, parties, whatever you like to call them, of greater or lesser magnitude. And, indeed, we must remember when we first arrived we stayed with the Calvins for maybe it was only a day or two because we got a flat very quickly. But later, when we came back in 1960 with our own kid, then we stayed with them for something like 10 days until we found ourselves a suitable apartment. So the Calvins

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had a basement in their house which I think they used for that often and they were very important vehicles for helping people to get settled because they were able to offer them at least temporary accommodation until they got fixed up. So many people certainly stayed at Calvin's house for a shorter or longer period while they got settled in.

**SM:** I think that much of their hospitality and their warmth was engendered by Genevieve, Melvin Calvin's wife, in which, of course, he joined enthusiastically. But she was enormously hospitable and kind and helpful. So apart from the social aspect of things outside the lab., tell me about how the group worked together, the physical aspects of the lab. and what difference this may or may not have made to the working of the group.

**VM:** Before I do so, let me say that it's true: Genevieve, of course, was very responsible for and was very conscious of the people in the lab. But, so was Melvin, in a rather different way. I think everybody felt that he always did his best for people in making sure they got jobs when they left him. Many people had come as postdocs. or as students without necessarily having positions to return to when they left his group. I think the general feeling was that he was very assiduous in helping people to get settled when they left him. One of the points I remember coming up for discussion at some time, but I can't place it and I can't really remember who said what in this connection, was the reluctance of people to accept as postdocs. those whom they felt they might have great difficulty in placing afterwards. There was certainly in later years, I think, the opportunity to have postdocs. who were much older than the norm, people who might have been in their forties, and there was reluctance, not necessarily because the people were not good or not suitable, but because of the feeling that it would be very difficult to help them to get a position later because of their age and difficulties of older postdocs. and so forth.

I think Calvin was always very responsible and to the best of my knowledge those of his senior staff who sought ultimately to have faculty positions he helped as much as he could. Now, I think it has been generally recognised that he had great difficulty — and many people have said this — great difficulty in securing faculty positions in Berkeley for his own staff. Indeed, only two of them — Rod Park and Ken Sauer, as far as I remember — ever succeeded in doing that. Other people, for one reason or another who, in the end, wanted to have faculty positions which they did not have as Radiation Lab. employees in Calvin's lab., felt they had to leave; some of them, of course, did that.

**SM:** Since you are talking about personalities and those who were part of your life in the lab. and became friends, I think that this might be a point at which to mention one particular group member and this was Ning Pon. Perhaps you would like to say something about him.

**VM:** Ning was certainly one of the "characters" in the lab. I have to say that in that group of people, and I can't really remember how many there were at the time when I first joined in '56, some tens of people at any rate, there were clearly some people more colourful than others. Ning Pon was, I think, one of the colourful ones. He was a



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graduate student at this time. He had been there for some years, two or three years at least, and he had some time to go. He was a very friendly person and he worked closely with our friend Bob Rabin and so naturally, both inside the lab. and socially, so we saw a lot of Ning. We saw a lot, actually, of lots of other people as well.

It soon became clear to me in the context of the lab. that collaborations between people were highly encouraged. This was not something that I felt had happened in London before I ever got here but was very much the name of the game in Berkeley. The pattern was that people would talk to one another — I was going to say continuously; certainly continually — they were talking all the time. The place in which they worked, the Old Radiation Lab., at least for the photosynthesis group where I was, was centred around this big white table that everybody talks about which was the social and academic centre of the building. It was the place where people gathered to drink coffee, it was the place where the discussions took place because it offered the opportunity for laying out the large chromatograms and other bits of paper, so you could put a lot of stuff on one flat surface and get a number of people around to look at it and talk about it. It was very much the centre of things and by the very nature of the building, which was rather an open building inside with a...well, there *were* walls and doors but not as many as...people did not work in cubby holes... there was a lot of gathering around this table. On such occasions, it was clear that people would take the attitude in discussing problems, they would say “why don’t we do so and so?” Faced with something that needed to be resolved, someone would say “why don’t we do this, or why don’t we do it that way?” and somebody else would join in as say “we could modify and so forth...” And before very long you would find a new collaboration had been started. In addition to whatever it might have been that those people had been doing before, they added a new thing. This was continuously going on — people were constantly forming and reforming collaborative associations. And, of course, it happened to me just as it happened to everybody else.

So I started working with Ozzie on the deuterium problem, and I can’t remember the sequence with which I worked with other people, but in the first year I must have worked with three or four other people as well — Chris Van Sumere is an obvious one I can remember doing a paper with. I can’t really remember exactly what the sequence was: Luise Stange, but she might have been in a later year. I don’t remember exactly what it was; I would have to look up my papers for that. In the course of the two years that I was there, I think I got something like 15 publications (two years as a postdoc., this is), 15 publications with various people in these collaborative groups, most of them with Ozzie because he was the person that I was closest to but there were others as well with whom I collaborated here and there.

**SM:** You mention the two years that you were there and, of course, I know that you were there for two years. This was rather unusual for a postdoc. How did it come about that you stayed for a second year?

**VM:** As it got towards the end of the first year, or at least when the end of the first year was in sight, I felt I had so many things going which would not be able to be completed were I to leave literally at the end of the first year, and, incidentally, there

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was nothing special for me to leave for because I had no job waiting back in England, that I said to Calvin that I would very much like to stay on. I suppose I said to him, could he help? What he said was that he appreciated the situation, he recognised how much was going on, he was clearly favourable to the idea of me staying on and so he offered me a year's salary. That year's salary was what I needed in order to be able to stay on for the second year. Of course, you had a job which could simply continue and so that part of it was assured and, therefore, we could continue on our second year on a somewhat more favourable financial basis since the salary that Calvin was offering me was a bit more than the university had. We were actually all set up and, of course, had everything running by then. I remember thinking at some time during that second year, or perhaps by the end of it, that had I had to leave after one year, I would have had no more than about three papers but I think by the end of two years there was something like 15.

**SM:** You had found this a good situation in which to work and, obviously, you had found Dr. Calvin responsive to what you were doing.

**VM:** Sure. I was well aware, it wasn't that I worked that much harder in the second year it was simply that the things that I had got going in the first year came to fruition in the second year, and there were new things as well. Staying for two years simply was more than twice as good as one year would have been, simply for this reason. So that was all very satisfactory. Within the lab. itself there was this continuous discussion going on. It was the year that Al Bassham was away in Oxford...

**SM:** This was '55/'56?

**VM:** '56/'57. Al Bassham was away — can't remember the exact sequence — but this was towards the end of the time when the carbon cycle was being finally wrapped up. There was a lot of debate both inside the lab. and between the lab. and other workers in other places, about just what was right and what was wrong. There was not just a lot of debate, there was heated argument, very heated argument in some cases. During that year one of the things that I think all of us will remember was the arrival of Otto Kandler. I should say that there were two German postdocs. in the lab that we were already friendly with, as far as I remember: one of them was Helmut and Hildegard Simon; Helmut is an organic chemist, really, from Munich (at least, he's in Munich now — I can't remember where he came from at the time), and the other was Helmut Metzner and his wife Barbara and he was a plant physiologist/plant biochemist and he came from Tübingen. They were postdocs. as we were; Metzner was a bit older but Simon was about our age; Metzner must have been about five years older, something like that. They were part of the gang, if you like, part of the gang of these postdocs. Another one that I remember was Duncan Shaw and his wife Elizabeth. Duncan Shaw was English and he had been an RAF reserve fighter pilot and I remember that he had crashed at least one fighter, maybe even more than one, and we used to rib him and ask him whether he had to pay for it out of his wages of sixpence a week or something of that sort! There were those two and I mentioned Luise Stange and Ozzie was really a postdoc., as he saw it anyway, he stayed in the lab. for only three years, and he and his wife Harriet also lived roughly in the same part of

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Berkeley as we did. So there was a gang of all these people. Oh, I'm sorry: Chris Van Sumere who came without his wife because she, I think, had just had a baby.

**SM:** He was, of course from Belgium and Luise from Germany.

**VM:** He was from Gent. Luise at that time was from — Göttingen? Can't remember; she was German, anyway. There was also a Japanese called Kojiro Nishida and his wife, whose name I don't remember, who was from Kanazawa in Japan; he was someone with whom Ozzie and I collaborated at one stage. So there were all these people around and we were stable. We'd been there for six months roughly when Otto Kandler arrived.

Now, as I understand it, the reason Otto Kandler arrived was that he had been on a Rockefeller fellowship with Martin Gibbs on the East Coast somewhere. Martin Gibbs had many points of contention with Calvin and with Calvin's group about details of the results and, in turn, their interpretation. And, as I remember but really it may need correction, the Rockefeller people had suggested to Kandler that he should split his fellowship half time between Martin Gibbs on the East Coast and Calvin on the West Coast. The story was that Kandler really didn't want to do this but they insisted anyway. He arrived and there ensued, as far as I could tell, six months of heated argument between him and Calvin. One could see them in this glassed-in office (*in ORL*), which was Al's (*Bassham*) in his absence, waving their arms and scribbling furiously on blackboards. You couldn't hear what they were saying. I think, as far as I can remember, you could hear that voices were raised but I think you couldn't hear the details. Anyway, they went hammer and tongs at it for many times, for many, many hours. Part of the upshot was, there was, as a result of this, one final overall discussion where all the outstanding issues were hammered out in, as it were, plenary session, among all the interested parties in the lab. and we all gathered, as far as I remembered, we all gathered in the seminar room that we used in the Faculty Club. By that time the group was holding its Friday morning seminars in the Faculty Club (*in the Lewis-Latimer Room*), and I think we used that room: there was certainly nothing in ORL which was big enough for us. We all gathered around the table and went at it for...again, I remember it as being days. Maybe I'm wrong but it seemed a long time. We finally arrived at an agreed position. I can't really remember whether Kandler also subscribed to that agreed position at the time but anyhow that's what happened.

In the meantime, of course, there were lots of incidents around the lab. One of the ones I remember — and it must have taken place at that time — took place on a Friday afternoon. We had the habit of going to Laval's, on the north side of campus just at the beginning of Euclid (*Avenue*), which had a beer garden and people would go there on Friday afternoon, fourish or so, and have beer before dispersing for the weekend, although I shall come back to the question of what this dispersing meant. Calvin would sometimes come but not always. One Friday afternoon, Duncan Shaw and I (Ozzie Holm-Hansen said he was there as well but I don't remember that he was) — but anyhow, it doesn't matter. Duncan Shaw and I were in the big lab in ORL, as far as I remember standing in front of the blackboard which was above a drying oven or something like that, or maybe it was just a cabinet, and we were

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doodling fantasies on the blackboard. We came up with a new carbon cycle for photosynthesis which started off with a polymerisation of carbon dioxide to make what we called “polycarbon dioxide”, this was the first enzyme “carbon dioxide polymerase”, then we doodled on from there; I can’t remember the details. We headed this thing the *Dephlogisticated Soot Cycle*. (You remember? You don’t. Phlogiston was an old alchemical theory about combustion and I’m not sure I’ve got it right but anyhow, it doesn’t matter.) *Dephlogisticated Soot Cycle* — and we were doodling away like this, putting the finishing touches, when Melvin came through the door and said something like “Hey, what are you doing?” We were embarrassed at what we were doing and so we took an cloth, ready to rub the whole thing out. He said “hold it, hold it. There might be something in it!” We had then to persuade him that there really wasn’t anything in it, and he would be better off drinking beer.

**SM:** Was this typical of his approach and his reaction to whatever anybody happened to be doing anywhere?

**VM:** I think, you see, he is a man, was a man at that time, full of ideas, not all of them stood up to much scrutiny, but he was very prolific with his ideas and they were often provocative, that’s to be sure but actually they didn’t all stand up to scrutiny. And he was very receptive to other people doing the same thing and he would be equally critical. You could propose anything to him, providing you could support it because he would start by looking at the holes in whatever you said. He was entirely happy to have these things presented to him. I expect there were lots of occasions like this. It was his practice to come into the lab. often, maybe every day, I just can’t remember how often. He would come in and he would actually sit down at people’s desks. The pattern in the lab. was that everybody had a lab. bench, a flat working surface with shelving in front of them, and across the shelves would be the other half of the bench with somebody else working. Behind every person there would be a chemical rack upon which you could hang equipment and that was shared, in turn, by the person behind you and at the end of your bench, nearest the wall, there would be a writing desk and that’s where you would sit; that would be your base.

It was commonly the case that Calvin would come into the lab. and, I don’t know how often he knew who he was looking for or he just picked on people by chance, then he would start talking unless, I suppose, someone was obviously desperately engaged in something where they couldn’t be disturbed. He would start talking to individual people at their desks, he would want to see the latest piece of paper with the result, whatever form it might be — chromatograms, graphs, read-outs from this or that machine. He would literally sit and pore (*over the material*) with the experimentalists, demand explanations, want to know why this had happened, why that had happened, offer explanations. There would be argument and discussions in an attempt for him to understand and to resolve what was going on. Sometimes, of course, he did this at the big white table and I suppose the first thing he would do if he saw a discussion going there, he would tend to join it. But there wasn’t always something going on and then he would go to the other people. This was very commonplace. He was actually very acquainted at that time with what people were doing individually. He was familiar with the technology and the techniques and he often had things to say about it. I can’t remember whether they were useful or not but

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he certainly was familiar. He would discuss the nature of the chemicals you were using or, in chromatography, he would have suggestions about what this material might be or how one might change this or that or the other. He was very much a part of the technology although he wasn't an experimentalist himself any more.

Later on, of course, it changed. Later on, when we got into the round building and the whole organisation became bigger he became less familiar with that part of things and he spent much less time in the lab. actually talking to people. He adopted the tendency of bringing people into his office to talk about (*their work*). Things eventually changed as the lab. became bigger and as the senior staff became older and, in fact, took responsibility themselves more for the graduate students who were all usually still in Calvin's name. Whereas in the early days, he took personal responsibility for the graduate students, later on, in fact, they were supervised by the senior staff and only notionally by him.

**SM:** Can we take things chronologically a bit later on about the later period. Tell me some more about the nature of the building. It was called ORL because it was the Old Radiation Lab., I understand, and it was the shape that it was because I think the 37-inch cyclotron had been situated in it. Am I correct or would you like to...?

**VM:** The 37-inch cyclotron had been located in it. It was certainly gone by the time that I got there. I don't think the shape of the building had anything to do with it. The building was much older than that, as I understand it. It was a first world war temporary building, I think we'd been told, and the cyclotron came 20 years later.

**SM:** But the shape of the lab. might have reflected that.

**VM:** How the building had been built originally I have no idea. By the time we got there, the ground floor was largely open labs. in the part that we worked in. I think there was another part of the building which housed the machine shop to which we had access; they were not part of the Calvin group. The structure of the building, as far as we were concerned, was to have at least one big lab. on the main floor with several people working in it (I can't remember how many) and there were other labs. also with multiple occupancy as well as one or two offices. There was this glassed-in office of Al's, I remember in particular. Upstairs there were more labs. And, indeed, my office was upstairs and the first year I certainly shared with Ozzie and with Kojiro Nishida. I must say that that office of ours, of which I have a picture somewhere, looked as if an earthquake had hit it. It was always strewn with papers — all three of us were totally untidy when it came to our papers and all three desks were covered with stuff. Our lab. bench was also upstairs, I think in another biggish lab. but not as big or not as densely populated as the lab. downstairs. Then there were chromatographic rooms upstairs and I really can't remember what else. In the basement there had been built a concrete-lined room for doing the radioactivity counting of the chromatograms.

Perhaps I should explain that the technique generally was to use two-dimensional paper chromatography for the analyses we did of radioactive carbon. There were a number of points to be noted about these. The first thing was that the radioactivity

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actually had to be deposited on the corner of the paper and dried down there. The papers were then dangled in a tank with some very noxious solvents associated with it.

*(Tape turned over)*

So the person doing the work put the papers in the tank, I was always smelly-ish of course, and when you put the solvent in it got smellier and then you closed the lid and left the papers to...let the solvent travel across the paper for however many hours it took.

**SM:** Am I correct in remembering the smell which you would then pick up was phenol and you used to come home smelling like that?

**VM:** Yes indeed. Phenol was one of the solvents and it was also a corrosive solvent and you had to be careful not to get it on your fingers. As far as I remember, but I'm not sure about this, we didn't use rubber gloves. But I really can't remember.

Then you had to take the papers out of the tanks in order to dry them and the way in which this was done was to use some stainless steel paper clips to make sure the papers remained attached to a sleeve over which they were draped: anybody interested in the details had better look up the relevant paper. And you then lifted up this sodden wet piece of paper, held in place by two or three paper clips, and delicately took it to a drying rack in some sort of oven, hood really, fume cupboard. During this process, you got the full force of the vapours in your face unless you wore the "space helmet", which someone had thoughtfully provided, which was literally a dome-shaped thing which fitted over your head and had an air supply in it from an external source which would keep the vapours out of your face. But, in practice, I don't remember that people used it. I think I tried it once and it was so unpleasant that I held my breath or something like that while I carried the paper across.

Every now and again, you would jerk the papers a bit too hard and they would tear; of course, they were wet paper and wet paper has no tensile strength. And if you weren't careful, these papers would occasionally simply tear and rip off and fall on the floor and that would be the end of that one. But most of them survived and you then dangled them in this hood arrangement and left them, I suppose overnight, and eventually they would be dry. They never stopped smelling, incidentally; they were always stinking of the solvent but, of course, they were not so strongly smelling as when they were wet.

You took the dry pieces of paper and then, in a dark room, and I can't remember now where this dark room was, but in a dark room you folded them round some X-ray film and put the folded paper round the film in a special envelope and let it sit for a protracted period for the radioactivity to cause a latent image on the film, and eventually you'd develop the film, all in a dark room.

**SM:** Was this not the dark room in the basement?

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**VM:** No. I don't remember. There might have been a dark room in the basement; I just can't remember where the darkroom was but somewhere in the building there was a darkroom. Of course, you didn't spend that much time in there. It had a dim light, enough to see and it wasn't a very skilled operation; you quickly got used to what you were doing and you'd spend a minimum time in this dark room putting the papers next to the film and putting them in their envelopes and so forth, and later doing the actual development.

Having done all that and developed the films, the next thing to do was to mark up on the sheets of paper...let me back-track: align each sheet of paper with its relevant film; they were numbered and somebody had also produced a little rubber stamp which you could wet with radioactive ink, stamp it on your chromatogram and that would produce an image on the film. So you could actually like the film up very accurately with the paper afterwards and, using a box with a light inside in order to be able to see through properly, then you would mark out, trace out with a pencil, the positions of the various black areas (which represented interesting compounds) from the film onto the paper.

You then had to "count" the radioactivity, that is to say, measure how much radioactivity there was in each of the spots and you did this with an end-window Geiger counter. They were quite large, by the time I was there, they must have been about 2 or 2-1/2 inches in diameter and they had replaceable Mylar windows on them because the windows used to tear after not too long and so every now and again you could replace the window easily enough. Later on I think we used gold-sputtered Mylar windows; they were rather better for some purpose or other. Anyhow, the upshot was that in order to measure each particular spot, you had to make sure that the adjacent spots were not interfering in any way with the counting measurement being made at the time. Because carbon-14 is a low-energy isotope, the  $\beta$ -particles it emits are actually stopped effectively by sheets of paper, or certainly thin sheets of card like index card. And so the technique was to shield all the other material, apart from the bit you wanted to count, with bits of index card and then gently place the Geiger counter down exactly on the bit you wanted to measure.

It was a most painful activity. There were many such spots on each chromatogram and it was very easy to run large numbers of chromatograms, so one was faced often with dozens if not hundreds of these operations that one was actually trying to measure. It was one of the most boring things I have ever done in my life; it's true enough that if there was anybody else in the room you could sit and talk to them but since you were measuring each spot usually for a minute or two, you were constantly having to change things and carefully arrange the cards round some other pattern on the paper and do it again. It was awful.

**SM:** You mentioned the toxicity of the solvents. You are also talking now about carbon-14 and radioactive materials. How much care was there taken as far as health and safety were concerned?

**VM:** As I remember, it was minimal. People were aware of the fact that they were using radioactivity and that in principle it was dangerous; they didn't slop it around —

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occasionally, I suppose, it got spilled — and occasionally, I think, people were actually called in to clean up spills but I don't remember anything very dramatic. Minor spills you would clean up yourself, minor spills on the bench you would clean up yourself. You would be reasonably careful but not fantastically careful. I think the practices we used then would probably not be permitted nowadays according to current regulations.

Anyhow, we used to spend lots of time there for counting these spots on chromatograms and it was on that, on the data from those measurements, that everything depended. And so it was very important to get it done. Ideally, the spots should have been counted on both sides of the paper because the paper itself absorbed part of the carbon radiation and it was by no means clear — in fact later on it became clear that it wasn't the case — it was by no means clear, even in the early days, that the two sides of the paper would give equal levels of counting but I think the problems of manually doing anything about this were just too awful to contemplate and people didn't do anything about it. Later on, I became interested in the problem and recognised the need to count both sides of the paper, and at that point developed alternative methods where you didn't have to do it by hand. Anyhow, that's another story which came much later.

So a lot of time was spent in this underground counting room, underground — it was a semi-basement; you entered it from outside the building down a few steps and I think somebody said that it had been built specially and had been concrete-lined in order to shield out the radiation from the Crocker cyclotron which was in an adjacent building and, without shielding, caused impossible background levels before the shielding had been put in.

So that's what we spent our time doing. We did the experiments on the lab. bench; I don't remember that there were any special places where we used our radioactivity. As far as I remember they were on the lab. bench. We prepared the chromatograms more-or-less on the lab. bench, they were run in the special chromatographic rooms. Of course, these were smelly and people were aware of the toxic effects of the solvents and so they were careful not to run them in the open lab., and then they were dried in the drying places wherever they were and then counted downstairs in the semi-basement. So that was the practice for much of the time that I was there, and other people: that's what we spent our time doing — and talking. Of course, we would bring our material up onto the big white table and discuss what it means and do the calculations and write the papers; things like that.

**SM:** As I remember, as it came to the end of the two-year period, we had undertaken according to the terms of your visa particularly, to return to England. At that time Dr. Calvin was anxious that you should stay on. Would you like to say something about that and why you decided not to and what happened subsequently.

**VM:** What happened was that, as I mentioned before, I did not have a job waiting for me in England and Calvin knew this and Calvin had already been employing me for a year; as it had turned out, I think he employed me for about a year and a quarter. He suggested, therefore, that perhaps I shouldn't go back and I should stay there and join



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his staff permanently. That was the implication; whether he put it in those words or not, I don't remember. And I said I had a visa which required that I return to Britain for two years and he said it would be possible, or he might be able (I can't remember what he said), to get a waiver of visa requirements through a private Act of Congress; it's apparently much less grand than it sounds to do this, and enable me to stay. At that stage, however, I was really undecided. There were rumours that the situation in Britain was changing out of the post-war gloom. I wasn't sure that I wanted to stay in Berkeley on a permanent basis and so what he said was "Well, go back, if you want to, and take six months or so to think about how you want to respond to the offer and take it from there".

So that's exactly what we did. We went back to England in the autumn of 1958 and there wasn't much difficulty getting a fellowship; I got one at King's College Hospital Medical School but it was before the period when the new universities were being founded in England, which wasn't until the early sixties; there were no more than rumours, as I remember, at that time and in the two years that I actually spent in England, I think there was only one job that came up which I might have wanted. The job prospects were really not very good. And so after I'd been there for six months we decided that I would take up this offer of Calvin's and we told him that that's what we would like to do but that I had already started doing all sorts of things and that I would like to sit out the two years that the original visa requirement had stipulated and that would enable me to complete things in England.

Of course, I'd already had the experience that two years in Berkeley were worth much more than one year and I felt the same about the time I spent at King's College Hospital as well, and that it was very worth while staying on for two years. So that's how it came that we came back eventually in October 1960 to a permanent, that's to say open-ended, staff appointment. I don't remember now what was written on paper. There was a letter of appointment of some sort; don't remember quite what it said. It must have stipulated a salary and I think the lab. was rather generous in paying removal costs of our stuff across the Atlantic.

**SM:** I'm sure that it must have been signed "Greetings and very truly yours". (*Calvin's standard salutation at the end of his letters*).

**VM:** It probably was but it might not have come from Calvin. There was a formal letter, I'm sure, that came from the Personnel Office on The Hill and incidentally one of the points was...At that point I had to get an immigration visa which still was easy for native-born British citizens and, of course, I was employed for five years in the lab. without being an American citizen and therefore didn't have to sign any of the loyalty oaths or anything of that sort until eventually I did become a citizen, when the five years was up, and then I was employed on the same basis as anybody else.

However, to come back, if I may, to some of the social activities in the lab. that I can recall. The people worked very long hours; that is to say, the lab. was always populated. I suppose there might have been times when there was nobody in it but they were rather few. I think people were working there — somebody or other seemed to be working there day or night. It was often the practice, since we lived

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fairly close, for us to go in in the evening, at least for a little while in order to do something: to start something, to stop something, to put a chromatogram on or to take a chromatogram off, or something like that. So it was notionally a matter of five or ten minutes; it probably lasted into half an hour or an hour. It happened, I think, most evenings and there was always somebody there. I think the lights were always on, there was always a car around and the place was always populated. I never stayed there all night and I don't know literally whether other people did; it was the impression one had.

**SM:** As I remember it, we never went out on a social occasion in the evening without returning via the lab.

**VM:** That's right. So it was the sort of place where people were always busy and always active. Our association with the postdocs., as I remember, was very close. That's to say, we spent lots of our evening with friends, partly your friends, of course, that you had made in your own work, and partly friends that we had from Calvin's group and one or two other places on campus. But as I remember, there weren't many friends we had from other departments on the campus; they were all from the Calvin group as far as I remember, or essentially all.

So as postdocs. without domestic obligation, we were able to spend our evenings together and often our weekends together. There were many visits to the countryside, various places up and down the state, to the mountains, to the deserts and so forth. Camping was the common style of doing so because people had neither the money nor the willingness for the most part to stay in hotels although I subsequently found I much preferred hotels to camping! So we'd go for the weekend to Yosemite or to one of the other National Parks, or to some of the desert areas or whatever, and that was a common activity for people to do. Groups would go up. They would go up in the winter to go skiing and we tried that not very successfully on one or two occasions. And of course they went in the summer time and in the other parts of the year.

**SM:** I remember, actually, backpacking into Lake of the Woods — with help!

**VM:** That's right; we did. We were — I think neither of us was very good at mountaineering and the carrying that was involved. And then there were one or two nasty things that happened. I remember once when we had arrived in Yosemite quite late at night and pitched our tent, and I was still asleep in the morning and reluctant to get up, Ozzie Holm-Hansen brought the tent down on my head in order to try and get me moving. And I was very incensed at that and insisted that he put it up again before I would consider moving. However, by and large it wasn't bad and one got a bit scruffy after a weekend out in the wilds and you came back and cleaned up. We would eat food which we'd barbecued or did various things to; all not terribly much to my taste but I suppose I got used to it at the time. Inside the lab...There were also beach parties: I remember one party we had on the beach at Stinson Beach or somewhere like that and I think there may have been more than one of those.

**SM:** You've forgotten Death Valley.

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**VM:** We went to Death Valley, I think, fairly soon after we arrived, and I remember that you cooked curry for us in pitch darkness or by the light of a torch or something like that, and you inadvertently put rather a lot of curry powder in and it was the hottest curry we've ever had. There were reminiscences which were nice.

There was one very good trip we took together with Luise Stange and Erminio Lombardi which was to the Southwest, to Grand Canyon and Bryce and Zion and places — Monument Valley. That was all very nice. We had one story there where we were on a side road in Monument Valley and the car slipped off the dirt road and got stuck on its axle and we simply couldn't move it. There was nobody around; I think perhaps one motorist passed by but there was nobody around to give us a ride. So we were about four miles from Goulding's Trading Post, across the main road through Monument Valley, and I remember — I suppose Erminio and Luise stayed with the car — while we set off trying to get help. We got down to the main road and waited for a truck to come and we flagged one down and it stopped. It was driven by an Indian, an American Indian. We explained at considerable length what our problem was and what we needed and he looked at us totally without blinking, and without saying a word, put the lorry into gear and just drove off! So we realised there was nothing for it but to walk the other couple of miles to Goulding's and eventually they sent something out and towed us free. There were always these stories associated with going on trips.

On that same trip, Luise Stange, who was much more athletic than Erminio or we were, hiked down to the bottom (*of Grand Canyon*) and back again in one day, 5,000 feet down and 5,000 feet back up. I think we sunned ourselves on the top (*at South Rim*) and waited for her to get back.

**SM:** She was a keen botanist apart from anything else.

**VM:** She was and much keener on all natural things than any of the rest of us were.

Then, inside the lab., there were, apart from the daily social gatherings at coffee times — we had lunch together: I seem to remember that we went to a cafeteria which I don't think exists any more, somewhere on the south side of campus. It was some sort of university cafeteria, roughly between ORL and Sather Gate, but where it was and what it was I really don't remember except that we went there and I have a distinct memory of Ozzie and I and Utz Blass standing in line one day, arguing about how you pronounced the name of the animal that looks like a horse and has black and white stripes. I insisted it was "zebra" and Ozzie insisted it was "zeebra" and we consulted Utz. Utz said "Of course, you're both wrong; it's actually 'tsaybra'." (*in the German style*). We must have gone there; I think we went there every day; I can't remember very clearly.

In the lab. itself, or as lab. functions, there was an annual picnic which I think was held usually in Tilden Park on one of the campsites there; everybody turned out and there was the usual park-type melee of picnic with barbecued this and soft drinks and people wandering around...

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**SM:** And hot dogs and marshmallows.

**VM:** Yes, and playing silly games, children around, of course. The people with children would always bring their children to things like that. And we also, of course, had lab. Xmas parties which were held in the big room in ORL and people from both parts of the Calvin group, the Donner people as well, came in and I think we gave each other gifts and I think there was a rule that we...not sure what the arrangement was. You either.. either the gifts were made anonymously or they weren't prescribed. I have an idea that you...everybody had a recipient nominated but the recipient didn't know where the gift came from. And you were limited to 25¢. Obviously, the idea was to be as funny as possible within the confines of this limitation. So we did that and that was one of those social occasions; they were always very free and easy and, of course, the Calvins came, Gen Calvin came, probably their kids came — I really don't remember.

**SM:** Was the punch alcoholic? Because in those days you weren't permitted to have alcohol on campus; can you remember?

**VM:** Just don't remember. I really can't. Was it punch that we had?

**SM:** There must have been something.

**VM:** There must have been something; I don't remember. There were also farewell parties, I remember. I can't really remember when they were.

**SM:** There was a particular gift that was given each time.

**VM:** That's right. Everyone who left the lab. received a little cylinder, a little plastic cylinder which had been — if only I could remember exactly what it had been. It had been irradiated in the cyclotron in some way and then tapped with a nail at one point and this produced a feathery structure inside the cylinder which was, as it were, the "trademark" of the lab. Somewhere I've a piece of paper which describes actually what it is but I can't remember for sure. So everybody got this as a leaving symbol of their time in the lab. and I think, since I left twice, I actually had two of them.

**SM:** Before we go on to the differences during your later period with Dr Calvin's group, you mentioned in passing the Friday morning seminars. Would you like to say something about their character?

**VM:** Well, they were very characteristic. The first thing that everybody commented on was their time at eight o'clock on a Friday morning. Calvin was a very early riser and was very active the mornings. He tended to go to bed early and progressively wound down as the evening wore on. So he was a morning man and, since classes started in Berkeley at eight o'clock in the morning and so on, he felt it entirely reasonable to have a seminar at that time. I must say I never encountered any such thing; all the ones I'd been to before than had been around tea-time. Anyway, there was this thing at eight o'clock in the morning that the British contingent always objected to it, but it didn't do us any good. And the format was entirely ritualistic, almost. I understand

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that, before I ever knew it, it had been a gathering where the whole group came together on Friday mornings and Calvin would look round the table and choose at random .

**SM:** Excuse me, the whole group constitutes both the ORL group and the Donner group?

**VM:** That's right. And Calvin would choose, not at random perhaps, but anyhow would choose someone to talk. By the time I got there, the decision was made at the Thursday lunch. The senior staff of the lab., a group that I subsequently joined when I came back, used to meet for lunch on Thursdays at the Faculty Club to decide lab. business and, at that point, the Friday morning seminarist was chosen and was informed after lunch that he was going to talk. I think people had some idea that it had been sometime since they'd talked, or they knew they'd come across something good and that Calvin knew about it and would like it to be talked about, so people were not totally surprised but you never knew for sure until one or other of the lunchers on Thursday came back and told the seminarist that he was on. And people then began to prepare frenziedly for Friday morning and some of them were up all night getting their stuff ready, getting all their illustrations. Some of the people who were less confident at speaking, I suppose, were making preparations.

This went on for years — must have been ten years or more because it was not until sometime in the mid-sixties that we began to get fed up with this. The people were really not producing very good seminars and were getting so disruptive that we persuaded Calvin that we ought to have a programme of seminars where people knew ahead of time that they were going to have to talk at some time and could get ready. And, furthermore, you could then space people out and it would have the dual advantage of giving people time but also making sure that people knew that they would, sooner or later, have to talk and make sure that they had something to say at that time. But that came later; that came sometime in the mid-to-late sixties, by which time all sorts of things were very different.

So the seminars themselves had a characteristic form. There was a long table at the time when I first knew them in a room in the Faculty Club and the format was later preserved in the round building. A long table and Calvin sat , if you had your back to the blackboard, Calvin sat at the front of the room at the left-hand side of the table. As far as the speaker was concerned, at the left-hand corner of the table. He always sat there. The senior staff, for the most part, tended to sit round the table and the more junior people tended to sit on other chairs distributed around the room, although there were some of the juniors at the table as well because there were enough chairs.

And so, come eight o'clock or a few minutes after, the seminarist would speak. I don't know if this is literally true, but the impression I have is that it was rare that the speaker got past the first sentence without Calvin interrupting them with a question. Some people, of course, felt that this was very off-putting, the ones who were readily intimidated or didn't speak English very well or whatever, went into some sort of "tizz" when this happened. Other people took it in their stride. Some people even told him to wait and contain his patience until they'd presented their stuff. Calvin was always the first one but other people did it too and, at various times during the

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presentation, there would be interruptions from the floor and this was to be expected. And, in a way, it took the place of an open discussion at the end because the discussion had actually taken place during the course of somebody's talk so that, by the time we'd got to nine o'clock or soon after, the subject had usually been talked out and that was the end of it and people then packed it in and people went home.

**SM:** They didn't go home, surely they ... ?

**VM:** Well no, they went on to work. I'm sorry. We also had to write quarterly reports. At that time, in the fifties and for much of the sixties, all the support for the lab. — or essentially all the support for it — came via a block grant to Calvin from the AEC and our obligation was to write an annual report, I think, in which we had to say what we had done in this year, what we were planning — how was it — what we had achieved in the last year, what we were doing at the present time and what we were planning for next year; and this was an ongoing thing and, as it were, one moved the story down a notch each time you used it, so that next year's proposals in the following year became this year's actuality and the year after that became history. So we had to do that and the system was such that that was sufficient for Calvin to make a case to raise the money he needed.

Internally, we would write the quarterly reports for a publication which was actually called 'The Quarterly Reports', which was properly typed up and presented in some simple bound copy in the lab. and that was, in a way, the first form of record beyond people's original lab. notebooks and often formed the basis for subsequent publications.

And so there were lots and lots of these quarterly reports and what followed then was the UCRL reports, the University of California Radiation Lab. reports, which were rather like full papers but in typescript and not formally published by a journal. So people did that and it was expected that people would write a quarterly report every quarter or at least most quarters. It was thought that everybody must do enough work in a quarter to enable them to write a few pages of quarterly report, so that was very common. As far as I remember, the technique for writing was that people did not have typewriters then. In the beginning, in the fifties, people wrote longhand, gave it to the typists, of whom there were a number in the group, either in Donner or in ORL or in both, and they typed it up and made all the corrections so that typists had to retype things several times often in order to get rid of all the mistakes. This was before the days of memory typewriters or word processors.

**SM:** You mentioned in passing typists as support staff. I understand that there were other support staff. You also just mentioned the quarterly reports in order that Calvin could justify his funding. From that I understand that, beyond that, no-one in the lab had any worries about funding.

**VM:** That's right. I think Calvin made the case for funding on the basis of the formal application we submitted which was this three-year view of the lab's. work — next year, this year and last year.

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**SM:** What was the source of the funding?

**VM:** The source of the funding was the Atomic Energy Commission via the Radiation Lab. That I think paid for essentially all the funding except Calvin's academic salary would have been different, but the people who worked in the lab. were paid out of that fund and the funds of the lab. itself.

**SM:** And was there any problem about the equipment that you felt to be necessary, and so on?

**VM:** No. At that time the supply of resources was very lavish. The impression one had, and I think this was subsequently borne out when I joined the senior staff and attended the Thursday lunch, that pieces of equipment with a reasonable case were usually funded pretty easily — unless they were vastly expensive. If you wanted to buy enormous things then, clearly, you had to look more carefully but run-of-the-mill things which, as I remember, were pretty lavish by other lab. standards, really were discussed but there was not much doubt about them. Chemicals and biochemicals and radioactive materials and so forth, I don't really think we discussed them at all. We just bought them. There was a mechanism for buying them. You told Paul Hayes, or the storekeeper or whoever it was, that you wanted these things and they came.

**SM:** And what other support staff were there in addition to secretaries?

**VM:** Well, one other thing, may I say. The problem with postdocs. was rather more difficult. There was enough money in the budget, certainly in the later years when I knew more about it, to support a number of postdocs. each year. Now, some of the postdocs. came with their own money, as I mentioned earlier, and they didn't need support — or not much support . Some of them stayed on for a second year, as I had, and were funded by the lab., and so that used up postdoc. salary money, and then there were some people who were specifically recommended by people that Calvin knew and trusted and was prepared to offer them a salary on the basis of their recommendation. We, the rest of the senior staff, would also make the case that there were individual people who might have written to us and wanted to come who were worthy for support. So there was some debate as to how we were going to distribute this postdoc. money. I think, in the end, it was Calvin's decision how to do it but he certainly heard the views and received the advice of other people on the senior staff as to what to do.

There was some unrest late in the day, in the sixties, when — after very careful juggling that everybody was gradually reaching a consensus — we found out that over the telephone to one of his friends Calvin had promised another postdoctoral salary to someone and threw out our calculations and so on, but I don't know anything about this in the fifties because I wasn't party to it. So I got the impression that most of the postdocs. certainly came with their own money. That is it say, they had secured it competitively from some source or other and received no more than a supplement from the lab.

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**SM:** You mentioned this earlier. Is there more about funding you need to say, or can we talk about the support staff?

**VM:** I don't think I know any more about the funding. The place was very lavishly supported in general terms. There were secretaries, I seem to remember two in Donner. Marilyn Taylor and Norma Werdelin, were — I think — the two in Donner and there was at least one who lived in ORL who was Dea Lee Harrison. There may have been others whom I can't remember. We had access to a carpenter and to a machine shop, I think, maybe one of the main Radiation Lab. machine shops was present in ORL and we, as a Radiation Lab. unit, had access to it so all of that sort of stuff was very well done. There were safety people around when you needed them. There were, inside the lab., ladies who washed the dishes, washed the glassware, who ordered the chemicals, who ordered the supplies. I don't remember that there was any shortage of support staff at all, certainly by any other standards that I'd been used to at the time, so it was very well set up.

**SM:** I think I remember your mentioning that there was a glassblower?

**VM:** There was a glassblower. Yes, I'm sorry, there was. There must also have been someone responsible for electrical work, but I can't remember who it was. So things really worked very well. I think there were also ways in which you could get to the Chemistry Department under some circumstances and have work done if you needed to, but I don't remember that any of this was an issue at all. It all went very well.

**SM:** You've talked at fair length about how the group worked and how it interacted socially. This was during the first period when you were there as a postdoc. and, then again, during the later period when you came back as a permanent member of the staff. Were there notable changes during that time, or would you like to go on, if there weren't, to plans for the building that would be housing the group in the future and how this came about?

**VM:** The big change that had occurred between my leaving in '58 and coming back in 1960 was the fact that ORL had gone and the group who had been there had had to leave because the building was demolished to make way for Latimer Hall, the new Chemistry building. The ORL people had been relocated to the basement of Life Sciences Building and this had many detrimental effects. For one thing, they were much further both from Donner and from Calvin's office in Chemistry. Remember, all this time Calvin had been a Professor of Chemistry and had activities and obligations in the Chemistry Department, so his office was always in Chemistry and the old Chemistry building where he was literally next door to ORL so it was a few steps for him to go from one to another and not much further to go to Donner. And then, with the move to LSB, it was several minutes walk away down the hill and much more awkward. So that was one factor.

The second factor was, in LSB, the Calvin group was distributed along a corridor in a number of rooms, bigger and smaller. There was at least one big lab., maybe two, but the atmosphere was never the same as it was in ORL. That was quite sure, and I personally occupied an office and it had bench facilities on it and it was much more



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isolated than it ever had been before. So that was one thing. The second thing was that, when I came back I was a senior staffer on an indefinite employment contract and so I could begin to look at things differently because I was not there on the short term that I knew I was going to be the first time. And so I began to become involved in scientific work which was likely to take much longer to come to fruition. I began to work with people on that basis. For example, the work that I did with Karl Lonberg at that period and then extended through to other people like David McBrien later on was of that sort. I also had my own technician, Julie Chung, I think was the first one. Chang?, Chung? — can't remember now. And she and I worked together in my room but it was a less satisfactory arrangement than it had been.

**SM:** When you came back in 1960 had the work been completed which led to Dr Calvin's award of the Nobel Prize?

**VM:** Yes, I think it had. The lab was proportionately much less involved with photosynthesis than it had been and the nature of involvement with photosynthesis had changed. Al Bassham was always, throughout his career, primarily involved with carbon pathways in photosynthesis and their ongoing consequences, and he was the one that carried the flag for that right the way through. But, by 1960 when I came back, much of the emphasis had shifted towards what remained still unknown in photosynthesis and that was the nature of the light reaction and so the whole emphasis had moved much more towards the physical end of chemistry or biophysics, if you like, as distinct from biochemistry. I am not a biophysicist and so my involvement with photosynthesis, as far as I remember, did not carry on — was not renewed after 1960. In fact, I was involved only between '56 and '58 during that first period. When I came back I was involved always with other things.

**SM:** You mentioned that you are not a biophysicist. I don't think you have mentioned, in fact, what your background has been?

**VM:** Well, my first degree was in biochemistry at Cambridge. It was primarily in microbiological biochemistry and then I took a PhD in microbiology at University College in London. So essentially I started out by being a microbial biochemist, but certainly not a physicist. So, the way the lab. had shifted its emphasis in the photosynthesis area was nothing that I became involved with later on at all.

**SM:** So the group, in fact, consisted of several types of scientific approach?

**VM:** It had always done that ever since I knew it. You have to remember, Calvin himself is a physical organic chemist. That was his training and that's his basic understanding. He later involved himself in all sorts of other things but that's where he starts from. Some of the people, in fact, I would say many of the people that originally associated with him were chemists. The biological involvement tended to come rather later and I think that much of the early work, and he said this and it is really fairly clear, much of the early work in photosynthesis was very much a chemist's view of how to approach the problems. It had of course been influenced — it must have been influenced — by the work of Ruben and Kamen before the war in similar sorts of things. But even there, Ruben and Kamen were chemists and, although they worked with Hassid, who

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was a plant biochemist, the emphasis was chemistry. So that I think from the very beginnings the Calvin group were very chemical, always very chemical, and there was a strong emphasis all the way through. There were always people there who were themselves chemists, had been trained as chemists, and were looking at whatever it was they were doing very much from a chemical point of view.

When I got there in 1956 there were already physicists. There was Power Sogo in particular, who was developing NMR and ESR and things that I actually knew little about. There were, in Donner, people like Dick Lemmon who is a chemist and, I think many of the people in Donner — not all — many of the people in Donner were also chemists and there was a large chemical activity there producing isotopes and looking at the chemistry of isotopic carbon which was a Donner-based activity. Some of the postdocs. in ORL were chemists. Helmut Simon was a chemist, Duncan Shaw — as far as I remember — was a chemist. I can't remember what all of them were but many of them tended towards the chemical. And I was, if anything, rather towards the biological end or the biochemical end rather than the chemical end.

**SM:** Who else was there who were biologists?

**VM:** Well, Ozzie, he was a biologist. Chris van Sumere was a biologist. Bob Rabin was very much a biochemist. Ning, I'm not sure what Ning was originally. He might have been a chemist. There were not many straight biologists, if any.

**SM:** And clearly there must have been an advantage in the interdisciplinary character of this group in that people learned a great deal from each other and were able to help each other with lots of different problems.

**VM:** Yes. It's difficult to know quite how this worked. It was certainly true, and I suspect that the presence of Calvin as a cement between these various activities was maybe the most important factor because Calvin seemed to be able to encompass all of these things rather well, that is to say he could interact with the physicists, it seemed to me, pretty much on a physics level even though he wasn't himself a physicist. Better, I think, than some of the other people could do because remember that his was always an overview of the situation. He was never, in my experience, involved in lab. work; he never did it himself — sometime in his life, no doubt, but not when I knew him. Because of his understanding and the breadth of his experience and all the time that he spent, I think he had by far the best unifying view of anybody in the group and I think he knew enough of all these areas to be creative and constructive in all of them.

Now, for the rest of us who were involved day to day in doing our own experiments, I think that was probably less true, that is to say, we listened to what other people in other disciplines were saying but I think we found it much more difficult to be constructive and creative in them.

**SM:** Is it usual for groups to be interdisciplinary in that manner?

**VM:** Not in my experience but my experience, of course, is limited. Calvin's group, I suspect, was very unusual for its time and may even be very unusual now but, of

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course, groups now, forty, fifty years later, are not the same, not constructed the same as they were then. But for a group to have been chemically based with extensions into physics on the one side and biology on the other, led by a man with that sort of experience and view, and having furthermore that sort of funding stability which might have been a very important factor since people were not having to worry about where next year's funding was coming from. I don't even think Calvin had to worry about it. I didn't get the impression that he worried; I think he was pretty confident that he knew where it was going to come from. So I don't think that funding was a worry in that sense; it was well-funded, lavishly you might say, and people were pretty confident of its stability.

Another important factor was the hierarchy, or rather the lack of hierarchy inside the group. Calvin was the leader, Calvin was the only academic; there was no...what shall I say?...there was no conceivable competition on the part of anybody else to lead the group. Calvin was the acknowledged and only leader of the group and beyond that there wasn't really a hierarchy. There were permanent employees — people like Al Bassham and Dick Lemmon and do forth, *de facto* permanent employees, and there were the transients: the postdocs. and the graduate students. I don't think there was any conflict between these various groups of people; they each recognised where they were and I think each recognised that they were not one of the others. So no-one was going to be Calvin; the postdocs. for the most part didn't necessarily want to stay there and therefore weren't looking for permanent positions, and so it went. This produced, I think, a very relaxed set of relationships between people because they weren't competing for anything among themselves. All they were concerned with, naturally, they wanted to get their names on the papers that they'd contributed to but there seemed, as far as I can remember, no conflict of any sort with that sort of thing.

**SM:** And, of course, collaboration.

**VM:** And collaboration. I think it was...everybody was almost euphorically collaborating, if you like; in a way which I have personally not experienced elsewhere, people were working together for something which was coincidentally for the common good of the group and their own individual benefit and interests. They were absolutely coincidental at that time as far as I can tell.

**SM:** It sounds very exciting as an atmosphere in which to do science, as it were.

**VM:** I think it was and, as I keep saying, in my experience I haven't encountered another environment which paralleled that one. But, of course, there may have been others which I don't know about and there may have been...

**SM:** OK. Talking of environments, you had mentioned briefly or begun to talk about the necessary move to LSB which you returned to in 1960 and therefore the physical fragmentation to a greater extent of the group as a result of which...

(New tape)

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**SM:** Today is Saturday, June 29th, 1996 and this conversation concludes my interview with Vivian Moses started on June 16th.

When we were last talking, our discussion ended on our return to Berkeley in 1960. However, you have pointed out to me that there are several things which should have been mentioned during the earlier period. Perhaps you would like to talk about some of those now.

**VM:** The first thing to talk about is the actual scientific work that I did when I was in ORL in that period from 1956-58. I think I mentioned that Melvin suggested when I first arrived that I should work with Ozzie Holm-Hansen on photosynthesis in deuterated *Chlorella*. as a way partly of studying that problem and also as a way of getting into the technology and learning the tricks of the trade. And so I did start to do that in the lab. on the second floor American, first floor European — the upper floor anyway. There was a lab. there with several benches in it, at least more than one. I think Ozzie and I both worked up there as well as one or two other people; it certainly wasn't as big as the lab. downstairs. We also had an office there (I may have mentioned this already; forgive me if I am repeating myself); it was one which I shared with Ozzie and I think with Kojiro Nishida. It was a fair old mess; none of us was tidy. Before very long I rapidly found out the style of collaboration in the lab. and the fact that everybody was interested in what everybody else was doing, very much the feeling that we were all working in the same area and what each of us did was of interest to the others. There was continual chatting, continuous chatting one might almost say, about the science and everything else under the sun. We all got to know one another and what we were doing very quickly. There was an endless set of comments like "why don't we...?" whenever ideas came up. Groups would form and reform in order to pursue particular ideas that came up in conversation — many of the conversations round that big white table that people keep talking about, at coffee time but also at lunch and the other times we met.

I can't remember the order in which these things happened. Ozzie was certainly my main collaborator, I think, throughout the period I was there. Not every paper that I did was with him but many were. Then I did some work with Ozzie and Chris Van Sumere and I think that was on the discriminatory effects of photosynthesis against carbon-14. I also did something with Nishida and Ozzie, I can't remember now what that was without referring to the papers. I did some work with Luise Stange and Ed Bennett in which we looked at the further incorporation of radioactivity beyond the first intermediates into the macromolecules of the cell and we tried to take the cell to pieces in a gross chemical way to find out where things were. As far as I remember: this is all without looking up the papers.

**SM:** And we're talking now of the period between 1956 and 1958?

**VM:** Yes. It was all done in that period and all done in ORL.

During that time also I worked with Ning Pon and Al Bassham (I think maybe almost the only paper I did with Al Bassham) in which we looked at two things. I can't remember again which authors were which. It was my first interest at that time in this

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whole question of compartmentation which later occupied me for quite a lot and subcellular architecture. We did two papers. One of them was to try to take spinach leaves to pieces and find out what each of the bits did and see whether they would together to account for the activity of the whole cell, all with regard to photosynthesis. And the other was an investigation, as far as I remember, in *Chlorella* of the interaction of the respiratory and photosynthetic mechanisms which, of course, biochemically overlap to a great degree although the respiration and photosynthesis are moving in opposite directions. As we knew that both of these things could happen in *Chlorella* we were interested in their interrelations. That was a paper I think that I did with Al and Ozzie and — can't remember who the other one was. One of the things about that paper which was a little bit disappointing: it was the first paper, as it happens, ever submitted to the *Journal of Molecular Biology*; I had hoped it would be volume 1, page 1, but some fellow named Paul Doty, although he submitted his paper later us, actually got the first slot so we were the second paper of that one.

**SM:** You means this was the first paper, or one of the first papers in the existence of that journal?

**VM:** That's right and that journal became subsequently a very prestigious journal and it carried the new name of molecular biology which was not common at that time. I think the journal actually came out in 1959; we probably submitted the manuscript right at the time that I was leaving or maybe it was even after I left that the paper was actually submitted. Anyway, the journal came out I think in 1959 and we had the second paper in the journal.

There were lots of papers that I published in that period. As far as I remember there was something like 17 papers, although some of them may have been conference reports, at the Biochemical Society after I went back to England. Even if there were, there couldn't have been more than two or three of those so there were something like a dozen or 15 actual papers and one or two reviews. I felt that was very promising. That's why I was so impressed with the whole organisation.

**SM:** Were there not a couple that were significant in the winding up of the Calvin cycle?

**VM:** Yes. Together with Melvin, I think just with Melvin, I published the last two papers in the *Path of Carbon* series, numbers XXII and XXIII. The first one tentatively identifying the  $\beta$ -carboxylic acid which had been postulated as the addition compound between carbon dioxide and ribulose diphosphate. The second one was the final identification of erythrose-4-phosphate, not a particularly remarkable thing, but I did it. It was a chromatographic exercise simply to find the thing. That wound up everything.

There was, I found out only last year, Andy Benson had published a paper XXIV in the series, but that was not until 1970. It was quite different and really not related to the first twenty-three. You could say that I published the last two papers in the series.

Something else that I did which is of some interest: at the time that I was there the Path of Carbon, or at least the initial stages of the Path of Carbon, the so-called

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Calvin or Calvin-Benson cycle, was pretty much complete. I may or may not have mentioned earlier on about the big discussion that we had after the arrival of Otto Kandler when there was a great arguing of all the loose ends with the final agreement that that's the way the cycle stood.

**SM:** You did mention this controversy quite graphically actually.

**VM:** Right. Because of the success of the path of carbon there was naturally interest in looking at the other elements involved. There are three other elements and there are stories associated with all of them, two of them with me and one of them not. The one not with me was oxygen and that was done by a Swedish lady called Ingrid Fogelström-Fineman and she and others, I guess, tried to use oxygen-18 as a marker and then to irradiate it into fluorine-18 which was radioactive and could be measured. Let me say to complete the story at this stage what they tried to do. Oxygen-18 can be bombarded, I think with neutrons (*actually with protons*) in a cyclotron, and it absorbs a neutron (*proton*) and emits a proton (*neutron*) and is converted to fluorine-18. So that oxygen-18, a stable isotope, becomes fluorine-18, radioactive with a half-life of a couple of hours. The idea then was to feed oxygen-18 as a tracer to photosynthesising systems, to run conventional chromatograms of the products, but only in one dimension, and then to elute the chromatograms sideways onto a strip of tantalum, as I remember, in which some of the resolution of the chromatogram would have been destroyed because the elution would have been done through a series of serrated points cut in the paper to produce a lot of little drops, some of which would have contained oxygen-18 material and some not. This tantalum strip was then bent into a circle with the dried spots on the outside, fixed to a wheel and it was that wheel that was rotated in front of the cyclotron beam. Then, when they'd irradiated it enough they put it against photographic film and got an image in the usual way. That was an attempt, I remember not very successful, to trace the path of oxygen in photosynthesis.

What I subsequently did was to use tritiated water to trace the path of hydrogen. Now, with carbon dioxide and using radioactive carbon one can, of course, control the amount of CO<sub>2</sub> one admits to the system and so the carbon dioxide itself, or the bicarbonate in the form in which we actually used it, was intensely radioactive. There wasn't much radioactivity but there wasn't much carbonate either. What there was very radioactive. You can't get to that state with water. Inevitably there is a lot of water around all of these systems and in order to make water sufficiently radioactive to have any hope of using it as a tracer, you have to use enormous quantities of radioactivity. That's what I planned to do.

Whereas in the course of the normal carbon experiments we would use a few microcuries of radioactive carbonate per experiment, in the one I did with tritium I used 5 curies which is getting on for a million times as much radioactivity simply in order to overcome the diluting effect of the ordinary water which had to be present. That was potentially a dangerous thing to do. It certainly couldn't be done in the open lab. the way the other experiments were done at that time, or with minimal precautions. The whole thing had to be done in a sealed glove box which had to be worked by extending one's hands into gloves into the inside of the box so that one

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was actually isolated from the material itself. That meant that all the equipment that we normally used had to be reconstructed in a miniature form in order to go inside this glove box which I would guess was something 4 or 5 feet long plus perhaps 2 feet deep; it couldn't have been much more than that or one wouldn't have been able to reach the back from the outside.

**SM:** Was it necessary, if these things were miniaturised, to use some sort of magnification in order to see what you were doing?

**VM:** No. They weren't miniaturised in that sense. They weren't tiny but they were compact so that the incubation vessel, I remember, was a little shaker bath which was about 6 inches by about 4 inches and it was constructed in small scale, not miniaturised in a minute sense. Also, it could be pushed inside this glove box.

**SM:** This way you were hopefully completely insulated from radioactivity.

**VM:** Insulated and isolated. Throughout the time that I was working in the box, literally all the hours I spent in the box, there was a Radiation Lab. monitor (that's a person, not a machine) with a machine (*i.e. a radioactivity counter*) next to me, monitoring all the time what was coming out of the box, if anything, to make sure that there were no escapes and that I wasn't receiving any radiation. There was no danger to me so long as the box remained sealed because the tritium radiation is very weak and would not have penetrated through the gloves I was using or the glass screen of the box. So long as none escaped, then I was quite safe. It's now been 40 years and there's no obvious sign except I don't have as much hair as I used to!

**SM:** Were there not audible Geiger counter noises?

**VM:** No, I can't remember how it was done. There was something either visible or audible. I think there was a chart recorder which traced something that one could see immediately if anything went wrong.

One of the problems was that the amount of tritium which actually got incorporated into the materials in which we were interested was, of course, a very tiny proportion of the stuff that we introduced. Part of the technique, an essential part of the technique that we used was to evaporate all the water away when we finally spotted the material onto the chromatogram. In the course of that evaporation all this radioactivity would be evaporated, too and we had to get rid of essentially all of the 5 curies. This could not be released in any normal way. I have to say that the carbonate that we used in our  $C^{14}$  experiments I think just went into drums or large bottles for disposal, which were sitting around the lab. I can't really remember what we did with them. I don't think we threw it down the sink; that wasn't permitted. We had collection bottles. This tritium was much more dangerous. It was in water which, of course, evaporates and so had we just put it in bottles of that sort it would have evaporated into the lab. and that would have been very dangerous. So all the material that came out, all the tritiated water that had been removed from the samples, was distilled out in an evaporating set-up and collected in a cold finger, cooled by dry ice...

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**SM:** A cold finger?

**VM:** Yes, a glass vessel, sitting in dry ice which was very cold and, of course, froze out all the water that was distilled off. We collected it in this dry ice trap and then I remember we had designed this cold finger in such a way that it could be disconnected from the distillation line, sealed inside the box, and then we put it...The finger also contained vermiculite which is some mineral material which is good at absorbing water. So we finished up with this cold finger, sealed, containing the tritiated water in vermiculite, and that was then placed in a steel cylinder with a screw cap. It was only then that I was allowed to pass it out of the box with great checks on the monitor as we actually opened the lid or opened the vent and took this stuff out, this sealed vessel out. The vessel, I think, was encased in concrete together with other so-called low-energy waste and dumped at sea. Once I had got this pipe out of the box I was finished with it.

**SM:** From the way you describe the security...

**VM:** Safety.

**SM:** ...the safety measures that were taken with regard to this kind of experiment, it seems that they were far more serious than your earlier rather casual remarks about possibly throwing things down the sink. So I am sure that nothing was thrown down the sink.

**VM:** No, I don't think things were thrown down the sink, but I think they were kept, as I said, in storage bottles of some sort for disposal. But people were not terribly worried about the ordinary amounts of radioactivity we used around the lab. People were careful but not manic about it. But when it came to these quantities of tritium, then there was a totally different response and it was quite clear that I could only do it if I were properly supervised and proper safety measures were taken which, of course, was very sensible.

So we got a paper out of that which was somewhat interesting, it wasn't that fascinating, but it showed something. I remember that on our way back across America, when we got to the East Coast we went to visit Racker, Efraim Racker in his lab. in the New York Public Health Service at the foot of East 14th Street (everybody remembered this address: the foot of East 14th Street!) and he was interested in these sorts of things which is why I went to see him. I think we were bantering one another over lunch as to how much radioactivity we had used in our experiments and he told me how much phosphorus he had used in some of his and I told him how much tritium I had used in some of mine. I think I won that particular contest.

**SM:** How is he?

**VM:** How is he? I'm not sure I have ever seen him since then. I'm not sure that he is still alive but I don't think he died young.



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The last thing that I want to say in this context was nothing that actually came off but it was a good story.

**SM:** You mean succeeded?

**VM:** It was never put into practice for reasons which I will describe. That was the question of nitrogen. Although nitrogen is not directly involved in photosynthesis it is, of course, an essential element in all subsequent metabolism involving amino acids and all the other things. Nitrogen, of course, has no convenient radioactive isotope to use in the same way that one can use carbon. The radioactive isotopes of nitrogen all have very short (*half*-)lives and there are only some heavy isotopes, one in particular ( $N^{15}$ ), which people use extensively. However, the use of heavy isotopes as tracers is much, much more cumbersome than the use of radioactive ones because it is not easy to measure their presence or their quantity; you have to perform much more elaborate analyses. You certainly had to in those days.

So we conceived the idea of possibly going around the back door and the tip for this actually came from something that I remember Andy Benson had done which I heard about (maybe he had published it at that time and I had read about it). He had been looking for phosphorus compounds by irradiating normal phosphate compounds in a reactor, or near a reactor, and getting, I think, an n-gamma reaction which converted  $P^{31}$ , the ordinary form of phosphorus, into  $P^{32}$ , the radioactive form without altering the compound in which the phosphorus was located. It was, as it were: he tickled the compound and the phosphorus in it became radioactive and therefore he could see which compounds contained phosphorus and do on.

Working from that idea, and I think this now must have been rather later — this was probably in the early sixties, I can't quite remember when it was — we came up, I and somebody else — and I can not for the life of me remember who it was but the sort of person it might well have been was Karl Lonberg because he and I would think up schemes of this sort — we were tipped off by the embarrassment the Atomic Energy Commission was having at that time in its bomb testing programme. They were very anxious to show that their activities were also directed to peaceful uses of atomic energy which was, indeed, part of their remit. I think they had solicited experiments from people which could be used in association with the bomb tests they were doing. That is to say, they were going to blow off bombs, wherever they did this, to suit their own purposes and they invited other scientists to contribute experiments which could be placed in suitable locations with respect to this blowing off bombs.

**SM:** You mean experiments which were physically located near the explosion of bombs in order to see a reaction as a result of the blowing up of the bombs?

**VM:** Exactly, to use the radiation which came off the bomb, or the compression wave, or one of those factors for something other than just the bomb tests, for some scientific purpose. We conceived an idea; you may not know this, but I'll tell you. Carbon-14 in the first instance is made by neutron bombardment of nitrogen-14 and the neutron is absorbed and a proton emitted, and nitrogen-14 gets converted to carbon-14.

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Atomic bombs emit a large number of neutrons. So we had this idea that if we grew cells totally on  $N^{15}$ , the heavy isotope, which would not be toxic to them and then used  $N^{14}$  as a tracer, if we could so arrange things that the results of the analyses were radiated by neutrons from the atomic bomb, all the  $N^{14}$  would be converted to  $C^{14}$  which would be radioactive and we would then be able to find it on the chromatograms. We were very skilled at finding things on chromatograms and identifying them when we'd done so. That was the idea, to use the (*atomic*) bomb for this purpose.

We knew from Andy Benson's work that the paper that we used in chromatography was not very tolerant to high levels of radiation by neutrons and would crumble. We realised we couldn't use paper for this but would have to use something else. The other material which came to mind was to use silica gel spread onto glass. Of course, the glass would be OK and the silica gel, as an inorganic material, would also survive. The idea, then, was to run chromatograms on these silica gel plates, big plates if necessary, and then put them in a suitable position to be irradiated by the bomb blasts.

So the next thing to find out was what was the flux — let me put the thing in the right order, how close could we get a sample to the bomb? was one question. With that answer, what was the flux of neutrons going to be like at that point? and, therefore, could we expect to get sufficient conversion? The third answer was what would the shock wave be like at that point on our sample? There was great reluctance to give us this information. I suppose it came under some element of secrecy. Eventually, they told us that we could actually get to within 700 yards of the bomb, that was the closest, and they told us what the flux would be, and that it was OK, it was enough. It would have given us a reasonable result. When they told us what the effect of the shock wave would be like, it would be like hitting this glass plate with a sledge hammer. So, we realised we couldn't do that.

Then we fell back onto another idea. At the Atomic Energy Establishment at Hanford River, I think in Washington, there was a reactor which had — I don't know exactly what this was like — but it had somehow ports into which you could put samples which could be lowered into the depths of the reactor. There you could leave them for a protracted period and they would get irradiated. Apparently, the cumulative flux there would have been sufficient over the course of days or weeks or whatever to give us the effect we wanted. You realise the flux with the bomb is an instant flux — it happens and then it is finished. In the reactor, it just keeps going on at some rate, obviously less than the bomb, but you leave it (*the sample*) for a long time. So we inquired about the possibility of doing this at Hanford River. These ports were, I think, 6 inches in diameter so that we would have had to have miniaturised things compared with the way we normally did them because we ran our chromatograms on sheets of paper which were about 18 inches by 24. That didn't matter; we could envisage doing this thing on small sheets of glass. There would be no shock wave, no physical hazard to the material. The only problem was to gain access to the reactor. It didn't sound quite as attractive as the bomb one. We were looking forward to writing a paper in one of the scientific journals in which the method section would say "take one atomic bomb...". That sort of thing had not appeared in biochemical journals

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before. Anyhow, we were prepared to do this with the Hanford reactor but we couldn't get anywhere near it. There was a guy called Admiral Rickover (*Hyman G. Rickover*) who apparently totally monopolised these ports for use on experiments intended for nuclear submarines. We just never got anywhere near it. It remained a story of the sorts of ways in which we would think and it was one of the many things that never saw the light of day.

One of the other things, I think, that I was involved in that first period...

**SM:** We're back in the first time?

**VM:** I'm sorry; the bomb thing might have been later, as I said. In the '56-'58 period, another thing I was involved in, together with almost everybody else in the lab. as far as I remember, was preparing a display for the Brussels World Fair of 1958. The bit that I was involved in, in particular, was the photosynthesis display. What that consisted of was a series of painted, back-illuminated panels showing in progressive enlargement the photosynthetic apparatus. That is to say, it would start with a leaf, go to a cell, from a cell to a chloroplast, from a chloroplast to the components within the chloroplast. There was a series of painted panels which for a very long time actually lived in the hallways of the round building after it was built and apparently were taken away only in the 1970s when the storeroom was turned into a lab.; the storeroom in that building was turned into a lab. so the whole corridor became littered with store cupboards and they had to take away these wall-mounted panels which were obstructing...

**SM:** Are you about to talk about the Brussels exhibition and what was sent in?

**VM:** I am, this is all the Brussels exhibition, yes. So there were these panels and the second item was a very large working model of the photosynthesis cycle. I can't quite remember how big it was but at a guess it was 10 or 12 feet square. It was a large panel, translucent plastic panel, which had the chemistry of the photosynthetic reduction cycle marked on it and behind it was a series of lights, worked by microswitches, which represented the introduction of radioactive carbon dioxide and the lights showed how each atom within the various compounds of the cycle progressively became more and more radioactive. When this thing had gone around the appropriate number of times, the idea was that a sugar cube would be delivered to the audience from one corner of the thing. On this sugar cube would say: "This sugar cube represents the amount of light which falls on so many square meters of land over such and such a period". A lot of time was spent putting this thing together and I can't be sure that I ever actually saw the thing working. It was sent to Brussels and was on display, and I think it was working. Paul Hayes was the one who was the lab. representative in Brussels. He spent a lot of time getting the stuff there and he was in attendance in Brussels throughout the exhibition. I never saw it in Brussels. It came back after the exhibition was over and, again, was mounted — I don't know where it was mounted to start with — I think in LSB; but later it was against the wall by the back door of the round building, for many years until finally it went away.

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It never worked properly when it came back. The system was based on a series of microswitches; it was before the days of electronic switching. Those microswitches never worked properly and I think the sugar cube was never actually delivered at any time because something broke down in the negotiations with the sugar company (C&H I think it was, but I'm not quite sure).

One of the things I had to do was to help write the brochure, our bit of the brochure, that went together with the exhibits, and this brochure was written in English, I remember.

**SM:** What was the Brussels fair?

**VM:** It was a world fair; it was one of those fairs that are put on from time to time which show all the world's industry and technology and culture, like the 1851 or the New York fair of 1919 or the San Francisco fair of 1939. They have these things every now and again.

Our bit of it was, of course, written in English. It was then apparently translated into Flemish and edited in Belgium in Flemish and then retranslated into English for our comments and at that point I think I went to town on it a second time. I'd originally written at least much of it and the editing in Flemish had really knocked hell out of it. I never went there, as I said, didn't see the exhibition. At some time later, with great delight, I remember receiving a certificate from the US State Department — remember I was not an American citizen at that time — and the certificate, which I still have somewhere, says something, thanks me for my help for the Brussels World's Fair and it says that citizens like me that make our country great; words to that effect. I was very pleased to get that.

**SM:** Right. As a matter of interest and curiosity you talked about a number of papers which were produced with your name on them during the '56-'58 period. Do you remember, by any chance, roughly how many there were?

**VM:** Well yes, I said, I think there were 17, but three of those, I think, might have been conference reports, that is to say, preliminary presentation of data which then appeared as a paper so they would be duplicates.

**SM:** The sheer quantity would seem to me to reflect the amount of activity that went on in the group and the amount of collaboration that went on because clearly these were working in different directions with different people over the period.

**VM:** Yes, you are right. There were, of course, common themes, but you are right. That does reflect, and also reflects how much more one can do in the second year if one has two years. I remember thinking at the end of one year that I would have had only about three papers and so much was started which could not have been finished in one year but could be finished in two.

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**SM:** Going back to the earlier period, in order that we can conclude that and go further, you mention something about relationships between individuals and the informality of the group.

**VM:** Everybody was on first name terms within the group except Calvin. Calvin was always called “Dr. Calvin” or “Professor Calvin” by absolutely everybody except his wife who always referred to him as “Melvin”. When she talked about him she always referred to him in that way.

**SM:** Hopefully when addressing him she didn’t call him “Dr. Calvin” either!

**VM:** I don’t remember what she called him. “Honey”, I think she called him. She called him “honey” and he called her “Babe”...

**SM:** Right.

**VM:** ...as I remember. But nobody ever called Calvin by his first name. Within the lab., as I said, with one minor exception, and that was, of course, among the Germans, there were in our period in 1956 three or four sets of Germans (husbands and wives in some cases) in the lab. and there was a hierarchy between them which was not reflected anywhere else. We were particularly friendly with one of these German postdocs. who was not the most senior among them and we were very impressed that while everybody called everybody else by their first names, he referred to his colleague as “Sie”..

**SM:** ...which is the formal mode of address in German.

**VM:** Right. They themselves were very friendly, they spent all of their time together, because they came from the same place. I asked our German friend why did he not call his colleague “Du”. He said in the nature of things his colleague was the senior and until he was invited to do so by the colleague he couldn’t do so. Apart from that, everybody else was on a first name terms.

**SM:** Is this in any way in contrast to experience in British labs.?

**VM:** In British labs. fortunately...When I did my PhD, actually we did call everybody by our first names except the professor. Everybody called him “professor” or “prof”. When I went back to England in 1971, I was — I don’t know quite what the word is — I was embarrassed and I found it hilarious that all the secretaries would insist on calling me “prof”. It was only after much trouble, much persuasion that I got any of them to call me “Vivian” which eventually they did. One of them, whom I have known now since 1975 (21 years) since she was about in her early twenties, still calls me “prof”; she can’t be persuaded to change.

Anyway, this all went on in Calvin’s lab. without change until I would say, late in the sixties. By that time Mel Klein and I (Mel Klein had joined the group in 1963, I think), Mel Klein and I decided that we would put an end to this and we would call him Melvin; we had known him long enough. We wondered whether there would be

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any reaction. There was none at all. I don't know whether he noticed the fact that we called him Melvin but we suddenly one day decided from now on we would call him Melvin and we just did; and there was no response.

**SM:** As I remember, Dr. Calvin himself was completely informal in his conversation with everybody.

**VM:** I think he might very well have been oblivious of this or maybe he felt that that's what they wanted to do, let them get on with it. He simply didn't react when we changed this. Now, another 20-30 years on, I think everybody now calls him Melvin. All the people he has known over those years call him Melvin. But it took them a very long time to do so.

**SM:** Is there anything else, during the earlier period, that you cant to cover, because to have some form of chronology seems to make some sort of sense....

**VM:** There are one or two things. There is some continuity; they run on a bit into the next thing...One of the things I would like to comment on was Calvin's lecturing style which I first met when I heard him talk in London in the autumn of 1955, as I think I mentioned earlier on. He was a very enthusiastic ebullient lecturer, very informal. He had a lot of good stuff to present. The audience wanted to hear what he had to say and, I guess, he knew that and he acted in accordance with that. Instead of standing behind the rostrum or behind the lab. bench, or whatever it may have been, he would tend to come round to the front of it, as I remember, and sit on the front of the bench and talk off the cuff — he never had any notes and he would use the slides as prompts, as most scientists do anyway. He would sit there and he would be showing the slides and waving his arms around and explaining in his enthusiastic way how it had happened, what it all meant, and so on. The only trouble with that was that he lost all track of time and he would just go on and on and on, and suddenly he would look at his watch or the clock and realise how late in his allotted time he was. Then he would ruin, or partly ruin, everything by gabbling through what he felt had to be said of important results which he hadn't yet got too. People recognised that the quality of the lecture would change about ten minutes before closing time as Melvin got to that point.

**SM:** Would it be out of the way to jump ahead a little bit, just on this particular topic, to his preparation of his Nobel Prize address.

**VM:** Indeed; indeed it would. When he was awarded the Nobel Prize in 1961 he was told, I understand, that he had to give a lecture in 40 minutes and that it was as very bad form to exceed that time. This was something which he had never done before. What he did was to work out a lecture that he was going to give and then Paul Hayes and I edited it for him to make sure that we cut out redundancies in order to keep him down to 40 minutes. We timed him bit by bit and he arranged with Gen, his wife, that she would be sitting in the projection booth with a script so that he didn't even have to ask for next slide but she would know when it was due and she would show it, which would save him a few seconds here and there. So, he gave this lecture in Stockholm from his script, as far as I know, and got it through in 40 minutes. I'm afraid in some

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ways it ruined his subsequent lecturing (*style*) because then he became addicted — not addicted, he from time to time at any rate, maybe often, used a script. There was all that spontaneity and ebullience that he had in his earlier years had really largely gone after that. The Nobel Prize exacts a high price in some regard.

**SM:** We've jumped ahead on that, because you were talking about the manner of his lecturing. Were you also going to talk about the way in which he approached the writing of papers?

**VM:** Not just the way he approached the writing of papers. It's the style of writing papers in the lab. By the time I got there, I suppose, this was well established. Many of these papers came out with many names on them because of the number of people participating in the projects. The agreement was that the person who wrote the paper had their name on first. And usually, not always, usually Calvin's name was last and the rest of the names were on alphabetically. I suppose there might have been variants in that but that was the general style of things. It certainly happened that when I wrote papers myself, when I was the first author, that is to say when I wrote the bulk of the paper, which meant that I had done the bulk of the work — I think the person who did the work wrote the paper. That's the way it went. Sometimes all the names after the first a author were alphabetical. As I remember there was never any argument about this. It was an accepted way; everybody thought it was reasonable. Nobody seemed to object to having their names included with lots of others. Everybody wanted their papers published with Calvin. These were all youngish people who were in fairly early stages of their career — I suppose Andy Benson would have been older but he had gone by the time I got there — and so they were pleased to have Calvin's name associated with theirs and saw it was a plus for them. Not every one of my papers was published with Calvin in those early days but most of them were. There were one or two in which he really played no part or in which he had no particular interest — for example, the fungus that I had done my PhD on in London was something I brought with me and I did some work on that as well and that was not something that Calvin was concerned with. So that was published just by me as far as I remember.

In the course of writing these papers, I should say that Calvin was not a passive partner. He would argue over every comma. He was very argumentative over some bits of the paper which he saw as being most important or not to his taste. He was not a sleeping partner, he was a contributor, had often contributed, I would say, usually contributed in the formative ideas, had certainly contributed in the detailed discussion of the results as they came through and also in the discussion of the paper, of the written draft as that was being produced. His name was properly on the papers and the fact that he may have produced in his life 600 papers or so represents contributions that he actually made. He was not the sort of person who would put his name on simply because he was the head of the lab. That wasn't his style.

**SM:** Again, from your first impressions in the earlier period, I can remember your reaction to the eight o'clock Friday morning seminars. Can you tell us something about that?

**VM:** The first reaction was that it was so early in the morning! I had come from a culture where, as far as I can remember, one got into work in time for coffee which was

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something between 10:00 and 10:30. True enough, it was in a city where there were commuting problems, but nevertheless, the idea of eight o'clock in the morning was foreign to me and also to all the other English in particular, of whom there were a fair number in the lab. at that time. Nevertheless, Calvin insisted on it. It was actually ten past eight in the morning that we started. I don't think I need to go into the style of the seminars. Many of the people that have contributed to this oral history have mentioned them. Calvin always sat in the same place, at the end of the table; he always interrupted early in the seminar — some other people also interrupted but not the way he did.

**SM:** Did you find yourself intimidated in the way that some people seem to have?

**VM:** I don't remember being intimidated, but then, he was not...I was about to say he was not aggressive. Perhaps that's not the word to use. He was not attempting to put you down, he was asking questions which he genuinely felt ought to be answered or at least he wanted to know the answers. If you said "I picked up the test tube" and he said "what sort of test tube?" it was because he wanted to know what sort of test tube you were talking about. He was not suggesting that you should have been picking up something else. It was constructive criticism. But I suppose people felt it intimidating because he did it so much. His style, I suppose, was a bit aggressive in the way he asked. And the fact that he started so quickly: as soon as somebody opened their mouth, usually within the first sentence or two he started.

One of the problems with the seminars was that they often went on much too long. They were supposed to be from eight to nine but sometimes he got very wound up and really wouldn't let them go.

**SM:** Who took part in these seminars?

**VM:** Everybody.

**SM:** By everybody, you mean from the Donner group in addition to the ORL group at that time.

**VM:** That's right, and Calvin and all his senior staff and all the postdocs. and all the students and the technicians. Everybody except secretaries and support staff took part in the seminars — were expected to be there and people came; I don't think people failed to come to the seminars. These things would often drag on, well after nine o'clock, and the thing would go on and on and on, and people became more and more restless with Calvin's nagging sometimes at some of the abstruse points. But it was very difficult to get out. The people who could get out were those students who had classes at nine o'clock. You remember, many of the graduate students in American universities take courses and some of them would have course lectures at nine o'clock; they could legitimately leave. Every now and again more of the more senior people would slip out in the general melee in order to get out of this if they could see it happening. It was a problem and it did go on often and people got fed up with it.



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**SM:** Am I correct in remembering that the way that Dr. Calvin felt about this situation as in every other situation was “we are all here to learn”, can there ever be enough time spent learning?

**VM:** Yes, that’s true. What he wasn’t sensitive to was other people’s needs to learn. If he needed to learn something, he presumed everybody else was interested in it. He never, that I remember, adopted the approach, “I’d like to pursue this with you when it’s finished”. He probably did but I don’t remember it. He would pursue it there and then, in the seminar, and keep everybody else around while he did it. So, he was not sensitive to the fact that some people might want to get out.

**SM:** Instant mental gratification.

**VM:** Yes; I think that’s a fair way of putting it.

**SM:** Have you now exhausted this earlier period so that we can get on to where we had chronologically left the interview last time, which was your return in 1960. Of course, the most exciting thing about that period early on was Dr. Calvin’s having been awarded the Nobel Prize for Chemistry.

**VM:** No that’s not the earlier period, that’s the later period; that’s 1961.

**SM:** Yes: I said “on your return”. So you came back and, I think we had spoken about this earlier, to find that the group had been physically fragmented because it had been necessary to leave ORL because of building work on campus or what have you and so the inmates of ORL had gone to the Life Sciences Building.

*(Tape turned over)*

**SM:** Right: so the group had been physically fragmented — well not any more than before — but into different places.

**VM:** It was not more fragmented than it had been. It was simply that the ORL contingent was moved bodily to Life Sciences when I was not there. That must have been some time in 1959 when I was not in Berkeley. So when I came back, that’s where they were, physically much further away and under much less pleasant circumstances along a corridor — I think I mentioned this — along a corridor in separate rooms to a considerable extent although there were one or two big labs. The atmosphere was totally different.

**SM:** And plans for a new building has already begun, had they not?

**VM:** I think they had already begun by then and I remember being party to the discussion. The plans had not been laid by the time I came back in 1960 so that the form of the new building was still very much up for discussion. I suppose that the first things that had to happen, and I was not aware of this, was the raising of the money and other people have talked about where this came from and how they did it. So I think until the money came, there was no point in serious architectural designing of the building.

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By some time in late 1960 or early 1961, I suppose, and perhaps even before then — I can't really remember when it started; it will be in the records, of course — there was the planning for the new building. Everybody had been very impressed with ORL, not literally for its properties, it was a crumbly old building, but the fact that it had been so open and the fact that people had been placed together so effectively.

**SM:** And encouraged to collaborate visually and actually.

**VM:** People felt that the building had played some significant role in that. It wasn't just the people but it was the way the people were physically dispersed with respect to one another which encouraged that sort of activity. People in the lab. felt that was a significant fact also in the scientific success of the group. They didn't want to lose it. They saw dramatically what a difference it made when they moved to LSB and they had earlier seen, of course, what a difference there had been between ORL and Donner. Donner was a building based on corridors. It was quite clear that the atmosphere in the two places was different.

Various people have said that in Donner they worked in rooms and stayed in rooms. In ORL there wasn't that; you moved freely around the building, you weren't conscious of being placed in rooms. So when it came to the opportunity of having a new building, naturally the thoughts turned to how do you reconcile the design for a new building with what had been perceived as being so successful in ORL. From that grew the idea that we ought to have large labs. The main working areas ought to be large and ought to be open, allowing maximum communication. At the same time, of course, it was recognised that there were other activities like smelly things, or noisy things or whatever which really had to be kept in separate rooms. One needed some combination of large open spaces plus small protected areas for chromatography and centrifuges and other sorts of things.

I don't know at which stage the idea of a semi-circular working lab. arose. I don't know whose idea it was. I certainly don't remember being present when it first came up. The idea did arise that the labs. would be of that form, semi-circular, with the lab. benches arranged like the spokes of a wheel around the periphery, around the wall. Everybody would have, as they had had in ORL, a lab. bench with a chemical rack behind them and a writing desk at the far end with a maximum degree of privacy; the far end against the wall. Within that...

**SM:** Far end against the window, I think.

**VM:** ...against the window wall, against the external wall; there would be some windows and some not. There was some discussion about how big a window you could have to do with the heating, lighting, air conditioning, etc. At the inner end of the benches there would be a space for a walkway — no wall, but a walkway — and then there would be equipment which would be common equipment used by the people who were working at the various benches — things like spectrophotometers, instruments, balances, all sorts of instruments — would be placed in this inner area. In the very middle of the room there would be the big white table, at least on one floor there would be the big white table; that would be the focus. As there were to be two floors

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like that, the lower floor had a circular coffee table. In fact, the second floor coffee table became the social centre of the building. The big white table became the scientific centre for the third floor only at that point.

**SM:** Once you were in the round building, of course, both groups were together. The Donner group was there as well as the group which had been in ORL and subsequently in LSB. Did they work discretely? Was there one (*group*) on one floor and one on the other? How did it work?

**VM:** To a degree there was. What happened was that the staff members...I should say that at each end of the arc containing the lab. benches around the semi-circular labs, were two glassed-in offices. Those were to be the homes of the staff people, people like me, Dick Lemmon, Al Bassham, Ed Bennett and all the others; we each had one of these glassed-in offices. There were to be eight of those, four on each of the upper two floors and, naturally, each of us had our own group of activities near our offices on the same floor as our offices. The original idea was that people would be all jumbled up, at least within a floor and maybe within the two floors; can't remember that.

**SM:** What do you mean by people being "jumbled up"?

**VM:** That's to say, people in my group would be scattered anywhere on the semi-circle, not necessarily next to other people in my group.

**SM:** Do you mean the animal people might be next to the photosynthesis people, too?

**VM:** Yes, certainly my people and Al Bassham's people, and I can't remember who else were on that floor, would all be mixed up, in order deliberately not to separate them. In practice, that tended to die away over the course of the years and we tended to occupy blocks. But, they were open blocks, that is to say there were no physical barriers and there was talking back and forth and movement back and forth across the room. So it wasn't much of a barrier.

**SM:** You mention the semi-circular lab, but you didn't actually mention that this was part of a circular building which might not have been the case.

**VM:** Just a minute; that's right. The original design was for the semi-circular labs., and where the other "buildings" (*should be "facilities"*) were to be placed was not so decided. The original design, which I remember Al Bassham came up with (I think he came up with), was to have a semi-circle on a block and the block would be conventional rooms for the service facilities. The architects felt that would be too fussy for a building of the size this one was to be and it was they, I think, Michael Goodman the architect, who suggested it should be completely round and that would also then solve the problem of not having to worry about which way it was sited.

**SM:** Do you know, or have you ever known, what the diameter of the building is?

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**VM:** I have known and I don't know. I think it is a 24,000 square feet. I think it is 30,000 square feet gross and 24,000 square feet net on three floors which would enable one to work it out but I'm not going to do it right now (*i.e. about 110 feet diameter*).

This last point, that the architects felt that a circular building could be sited — you didn't have to consider which way you were going to twist it — and therefore the access road and all the rest of it could be built for maximal convenience for other considerations. It didn't matter where you put the front door and the back door whereas on a block you would have more restrictions. So that's how it was done. All the other rooms around the edges were service rooms.

The ground floor was never like that. The ground floor had a central storeroom, which was very large, and all the labs. were in separate rooms around the periphery. The ground floor was offices and physical equipment for physics and for some reason, which I don't know at this stage, the physicists did not want big labs.; they wanted smaller rooms.

**SM:** I think the reason for physics being on the ground floor is that often the machinery is very heavy.

**VM:** That might have been a contributory factor but I think that the...I don't really remember what the argument was for what you put on each floor.

So, the building was constructed on that basis and, I think, it actually worked very well. It was clearly impossible to rebuild ORL in its original guise somewhere else — no one was going to build an old wooden building at that stage — the building had to be a modern building and I think that the compromise between modern design and modern construction and the openness of ORL was achieved very successfully. But there were limitations. I should say first of all that in the construction of the building itself, I think, I have only ever spotted two faults in that building from the point of view of the original users. One of them was that the lift (*elevator*) that was installed was a hydraulic lift which had been terribly slow; a hydraulic lift operating by a ram from below rather than from cables from above; it always crawled up and down. The other thing: we forgot to put floor drains in the cold rooms, as I remember, but apart from that it worked very well.

However, seeing what has happened to that building since the Calvin group stopped being the Calvin group, has shown that it's actually not as flexible as one would like. In its present form, where it is occupied by a number of pretty discrete groups, there has been a tendency to erect barriers between the groups on the various floors, barriers made by pieces of equipment and cupboards all over the places and big refrigerators. The place now looks a mess and it certainly doesn't fulfil the function that it was originally designed for.

**SM:** The ethos of the group is not as it was and it's not a group. You have just said it. It's several groups and they have sought to separate themselves.

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OK. Let's get back to where we were. Talking about the new building; the new building is opened, it's working, and you feel that it worked well. On your return, you were a staff member rather than being a postdoc. for a limited period. Did this make a great deal of difference to the manner of your working or your responsibilities or how you felt about things?

**VM:** It made one big difference which I was very conscious of from the very beginning and that was that I had a job which was permanent, or at least open-ended and, therefore, I wasn't so conscious of having to start lots of things in order to get lots and lots of papers in a short period of time which I was certainly alert to in the first period. I could then devote myself to a much longer-term development of ideas, such ideas as I might have had. That's indeed what happened. I never again worked on photosynthesis after 1958. I was aware of what was going on because Al Bassham was in the same room. Even in LSB I knew what he was doing and then in the round building he was on the same floor, and so I was always aware more or less of what was going on but never did it myself. I was embarked on a number of other things, which are not really terribly relevant to this discussion, but which then I was able to pursue over the long term with a group of my own, more or less, in the way I have described — postdocs. and students who chose to work with me over the eleven years that I spent there.

**SM:** You have sought in the whole of this oral history project to cover the period from the beginning of the work of the photosynthesis group to the operation of the round building as a new lab. Obviously no small part of this during the very end period was the award of Dr. Calvin's Nobel Prize. Would you like to say something about the events and the feeling around that time.

**VM:** As far as I remember, there was talk every year in the late fifties and early sixties that he might get it. I wasn't very conscious of Nobel Prizery at that time but apparently there were lots of rumours always flying around as to who was going to get it. It was said that Calvin was a distinct possibility and it was also said that every year that it was announced he didn't get it he showed signs of disappointment. In the end, he did get it and obviously we were all very pleased and he was very pleased. I think everybody agreed that he regarded it as commendation for the group as a whole and not just for himself. He was the representative of lots of other people. As you can imagine, there was lots of partying that went on, apart from the preparation for the speech, which I have already talked about. There was a big party at his house, I remember, and I think it was on that occasion that a cake in the shape of the new building first made its appearance. I think there are napkins still around, one or two preserved, which have the design of the new building, the shape of the new building, drawn in the corner.

**SM:** As I remember myself, this sort of occasion was very much participated in, initiated by Genevieve as well as Melvin Calvin, so I have myself very warm memories of these particular happenings. Not only was everybody made to feel a part of it, so were there families. On the Calvins' return from Stockholm, as I remember, a gift was brought to each lab. member...

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**VM:** Well, at least to us; I presume to others as well. The party I remember was at his house and it was a large party with lots of people. I think also that it was the only occasion on which we ever met his mother who came up from Los Angeles for the occasion. A sprightly lady at the time.

**SM:** We had always, from the very first time we were in Berkeley, known Genevieve's mother who lived together with them...

**VM:** ...in a cottage on the same property. When Calvin got his Nobel Prize, the University arranged a press conference for him in one of the University buildings to which I went. It was a bit formal and it was literally for the press, the local newspapers, I suppose, and the nationals and television people were there. That's the first time I heard him tell the story about how he thought of PGA. It's his story and he has reported it elsewhere. It goes that he was with his wife in the car and she had gone into what was then a frozen food supply store on the corner of Cedar and Grove (now called Martin Luther King Jr. Blvd.) and the car was parked in the red zone. While he was sitting in the car waiting for her to come out, he realised the significance of what he'd got and that PGA was the answer to it. That's the only story I know in relation to how PGA was dreamed up. He has told it often enough so he believes it.

**SM:** We've talked about the group during the first period that you were here and also after your return in the new building. You have talked about how the building worked as compared with the atmosphere in ORL. Is there anything you would like to talk about before we come to your ultimate decision to leave Berkeley in '71.

**VM:** Only a couple of things. One was that this group was always supported by the Atomic Energy Commission, paid for by them, and was always subject to some of their security considerations. I think that people from outside the group did not have free access to it. I'm not sure about this, but I think it was not easy for people simply to walk into the place. I think there was always some implied limitation on who could gain access to it. I know that people who worked in it had to have some sort of clearance. It couldn't have been very elaborate because there were lots of foreigners there who were not subject to American clearance procedures of the more stringent variety. But I think many of the Americans had what was called "Q" clearance which was some level of clearance.

**SM:** A minimal level.

**VM:** I don't know how minimal it was but it was a factor in the place. The foreigners were in some way vetted. I vaguely remember that there was always difficulty getting people in from Communist countries. There were one or two people who were to have come or, in fact, did come and I think it was much more complicated to get agreement for them to come. I remember Zofia Kasprzyk from Poland, who was there I think in the period I was fifty... around that period. And then there was Nekrassov...

**SM:** Fifty what?

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**VM:** Fifty eight, I think; I can't remember exactly when she was there. (*Lev*) Nekrassov who was there in the early sixties I think also, was, of course, from Russia. It was more elaborate to get agreement for those people to come in.

My contact with security apart from that and getting clearance was very minimal. There were two incidents I remember. One of them was during the period we spent in Life Sciences Building: I remember one afternoon one of the security people arrived in our part of the building and Lynn DuBois, who was then the secretary there, called me as being the senior member of staff who happened to be around at the moment; I guess Al Bassham was out. What this man had come to see was whether I could give him any background information on Marilyn Taylor whose security was coming up for review. I pointed out to him that first of all I had nothing whatsoever to say against Marilyn's presumed loyalty to the United States. I did point out, however, that I was not an American citizen but he seemed to think that made no difference: as long as I answered the question properly that was the only thing he was concerned about.

**SM:** There was one thing which I don't know whether you recall and this was when you were about to go to Russia.

**VM:** Yes, I was about to say that. When I was about to go to Russia to the cancer conference in 1962, which was being paid for, incidentally, by AEC funding, I was solemnly warned by the security people about all the temptations which could come my way when I went to Russia and how I must be careful not to fall for any of them.

**SM:** Not to be lured by the young men, women, or anything else.

**VM:** They didn't mention men, they mentioned only women. This was before men were thought of in that context in this part of the world. They warned me about being lured by young women. But I had to say that there wasn't a young woman in sight that was lure-worthy while I was in Russia, as I remember.

**SM:** Or even seeking to lure.

**VM:** When I came back from Russia on that occasion, I think they had asked me to write a report about my visit; they'd asked me that in advance or they told me they would be asking me. They never did; I don't think I ever wrote a report about the visit. They never showed the slightest bit of interest; they never came back to me and asked me anything.

**SM:** Weren't you also warned against possible approaches to be asked to spy for the other side, as it were?

**VM:** I can't remember now.

**SM:** This was the Cold War period and I think I can remember that element.

We were about to discuss your decision to return to England which you did in 1971. How did this come about?

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**VM:** From a personal point of view, I think there were obviously a number of factors. One of them, family-wise, was that our oldest son — son, oldest kid — was going to go to secondary school in 1971 or thereabouts and that we felt it would be unfair to move him from whichever secondary school he went to once he started. We began to feel that we would prefer him to have the sort of secondary education that we ourselves had had in England. For that, of course, we had to go back. The second factor was, I think, that I had become, for want of a better word, homesick and much as I enjoyed living in Berkeley in the end I decided really I'd rather live there for all sorts of cultural and identification reasons. Then, there was another factor. That was I was then in my early forties and still working as a formally untenured member of Calvin's group, as a senior staffer in the Radiation Lab.

**SM:** Although you were a research scientist, and this is what your particular appointment, was you had been doing some lecturing.

**VM:** I wanted the variety. I spent half my career, twenty years or more, essentially doing research all the time, except for a little bit of guest lecturing. I did an evening course in biochemistry; I had done that for several years in Berkeley, but it wasn't very much lecturing. I felt really that I needed a change; I needed a change in order not to do the same thing for the second half of my career as I had done in the first. I thought at that point I would welcome the variety of an academic career and be able to do teaching and participate in the administrative decision-making and running of an institution; all the rest of those sorts of things that one wanted to do. I thought I would like to do that and therefore wanted a faculty job.

It was very difficult to get a faculty job in Berkeley. There were only two people from Calvin's group who ever did that: one of them was Ken Sauer and the other was Rod Park. Nobody else ever succeeded in getting a faculty job in Berkeley. Al Bassham was an adjunct professor in biochemistry but not a full professor. And so, I realised that if I wanted to get a faculty job I would have to leave Berkeley. Looking out from Berkeley, I didn't want to live in the middle of the United States; looked towards the east coast and having looked that far somehow just over the spires of Boston appeared the spires of London and I was really tempted to go back home and in the end that's, of course, what I did. Simply to say that for the following 22 years, from 1971-1993, I was professor of microbiology at Queen Mary College in London and retired at 65, according to my contract, and in the last several years (about six years) I was also a co-founder and the science director of a small biotech. company called Archæus which was working in enhanced oil recovery and microbial methods for that. I became very involved in biotechnology towards the end.

**SM:** But you have been doing things subsequently. Your activity certainly hasn't ended with your nominal retirement.

**VM:** No. Subsequent to formal retirement from Queen Mary College I have been involved with various publishing, editing and book writing activities — a project on motivations for university biotechnologists to become involved with industry, involved in a human genetics groups and, of course, with this oral history. The oral



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history originally, you may remember, was proposed in 1970-1971 before we went back to England. But then, of course, we did go back to England and the idea arose again last year (that is to say, early in 1995) in a discussion we were having with a man in Bristol. As I was coming here (*to Berkeley*) only a month or so later I used that opportunity to talk about it to Calvin, Al Bassham, Marilyn Taylor and a number of other people — Andy Benson. They all agreed that they would participate. So, we spent several months getting the idea worked out, making contacts, securing our funding and are now doing it (*the interviewing*) in 1996.

**SM:** As I remember the original thought was that there should be an oral history of Dr. Calvin which, in fact, subsequently there was. The idea that there should be one of the whole group is something more recent.

**VM:** Last year when we talked about doing this present project, I first thought about it as a photosynthesis oral history. But it subsequently became clear that we really couldn't isolate the photosynthesis part easily and, instead, it has become an oral history of the Calvin group, the Donner as well as the ORL parts of it, in that early period. We recognised the 1945 and onwards period as being exciting, the beginning of this group. It had to stop somewhere in our story and we decided as a convenient place to end would be the opening of the new building, the move into the new building in November 1963. It's essentially (*the history of*) that 18 year stretch.

**SM:** You have also included a few people who were not part of the group but were part of the background to the work that was being done by the group.

**VM:** That's right. The last thing I might say is thinking of Calvin as a scientist. I have to say that he is the most remarkable scientist I have ever met, or at least met and known well; maybe some of the other people that I met and didn't know well were more remarkable but I didn't know about that. He was, as everybody said, quite outstanding in the fertility of his imagination, in the enthusiasm of his responses and in his ability to co-ordinate and stimulate and activate people. When we first knew him in 1956 and for many years afterwards, this was an overwhelming characteristic of him. It was also very clear in those early days that, apart from his science, he really had little understanding and little interest in other things and indeed appeared quite inexperienced in the affairs of man outside of science. That changed to a considerable degree after his Nobel Prize and, I think, perhaps one of the main factors was his appointment to Kennedy's President's Science Advisory Committee. He began to know the Washington scene. He also travelled a lot. In the early days, I think, he travelled only for conventional scientific purposes, to visit labs. and appear at conferences. I think later on he probably travelled in other respects as well, on a wider basis, and I think this must have broadened his horizons. He certainly became more interested in politics and other matters in the sixties and afterwards than he had done originally.

I remember indeed taking him home, I think, giving him a ride home, one day after he had just come back from a meeting in Washington and he was beginning to wonder whether perhaps he shouldn't give up the directorship of the lab. and devote himself wholly to that sort of activity. I remember discussing with him at the time that if he

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wanted to be a scientist, he simply had to have a scientific base and in my view he should not give up the lab. I never heard any more from him that he was even thinking of it. So I think he's; he was quite extraordinary. Unfortunately, he is now no longer able to do that. In his heyday, he was a most remarkable person and I think that part of the success of the group, a major part of the success of the group, undoubtedly has to accrue to him. He was, of course, also lucky and in the right place at the right time but another person would not have been able to rise to that occasion the way he did.

**SM:** In the sort of response he elicited from those who worked with him, apart from all the other things you have mentioned, what do you feel has been the overall effect of what might be called your own personal Calvin experience on your own professional and personal life?

**VM:** I would say it's major but it's really impossible to say what would have happened if I hadn't come (*to Berkeley*) because one can never say...

**SM:** But you did come.

**VM:** I did and it was a mind-blowing experience. It introduced me to America and to Calvin at the same time. It's a bit difficult to sort out altogether which was which. They were both very stimulating experiences and the fact of the matter is that as a result of them we subsequently immigrated into this country and at that stage intended to stay and indeed did stay for eleven years.

**SM:** In your professional life since leaving Berkeley, when you have been running your own group...

**VM:** I've certainly been influenced by Calvin in all sorts of ways, some of which I recognise and some of which I probably don't. I interrupt people at seminars the way he used to — I probably got it from him. I'm not conscious of which properties I acquired from him but I am sure they are there. I could perhaps try and work them out but I have never done so.

**SM:** So that one might almost say that what might, as in a seminar situation, even be seen as abrasive qualities still come across as admirable in a sense.

**VM:** I think so. I think that the whole point about science is that you must not be afraid to ask questions. Clearly, there are social ways in which you ask questions to elicit responses from people — don't put their backs up. Calvin could put people's backs up and I think in some cases he probably did. But for the members of his group who were used to the way he operated, I think it was not offensive. But for strangers, it might well have been. I have to agree.

**SM:** One now has some sort of feeling of the effect that he has had on you personally and on your way of doing things. Just, finally, back to this oral history project of the group. We have talked about the sort of people who have been asked to participate. How did you put this project together? In what manner was it facilitated?

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**VM:** I'm not quite sure...facilitated?

**SM:** You made approaches...

**VM:** OK; there were two sorts of things. First of all, I had to find the people to talk to. Marilyn Taylor kept very extensive records of people who had been in the lab. Many of the names were familiar to me: either I knew them personally or I knew of them, people who had been there before I was, knew of them from their papers. Many of the people were not difficult to find because Marilyn had the addresses. For some of the others, it was easy to chase them down through the scientific literature, and a couple of letters or e-mails would make contact. I must say that of all the people we have so far approached, by now it must be around 40, not a single one has declined to participate in this programme. They all have very warm feelings of the period and are all happy to say what they can.

In terms of support, I think we have been very fortunate in getting support for our expenses from the Chemistry Department in Berkeley; from the Chemical Heritage Foundation in Philadelphia; from The Royal Society in London; and from Gresham College in London. And, of course, we have also had the hospitality of the Office of the History of Science and Technology (*at the University of California*) in Berkeley and of the Bancroft Library who are going to assist with the archiving of this material. Everybody has been extremely helpful, co-operative and welcoming. It's going very well.

**SM:** Geographically, where will you be talking to people and where have you been talking to people?

**VM:** The biggest concentration of people has still been in Berkeley. We have been in La Jolla to talk to some and in Tucson, Arizona; to Mendocino (*California*). We are going up to Seattle to talk to a couple of people. There are visits to be made in Boulder (*Colorado*) and East Lansing (*Michigan*) on the way back across country and then in the Boston, Washington, and Richmond, Virginia areas. Subsequent to that, we have already seen some people in England, in various parts of England, and there are others to see and then within the next few months we need to go to France, Belgium, Holland, Germany and Switzerland, I think, in order to see maybe another dozen people. That, of course, still won't get everybody. The chances of being able to get to Warsaw to see Zofia (*Kasprzyk*) are not very great but maybe she comes to Western Europe sometimes and we can see her.

**SM:** Fiftyish participants is not a bad representation.

**VM:** Fiftyish should about do it. There were, of course, hundreds of people through that lab. but it's just impossible to see so many of them. And many of them were later than the period in which we are working.

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**SM:** The science, of course, was written at the time the work was done. As I understand it, the purpose of this oral history is to put on record the stories of the people who made the science possible.

**VM:** Yes; that's the way I see it, too.