

Chapter 5

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Berkeley (California)

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VM = Vivian Moses; MT = Marilyn Taylor; SM = Sheila Moses

VM: This conversation is with Marilyn on May 23rd, 1996 in Berkeley.

Since you probably know more about this whole business than anybody else anywhere ever, let's start with how you got involved in the first place. How did you join the group?

MT: I was working at Cutter Lab., part-time, after I graduated from college and I saw an advertisement in the paper for a secretary at what was called the Radiation Lab. at that time I put my application in and several months later I got a telephone call from the then personnel manager who was named Mr. William Bigelow — I don't know whether you remember him or not?

VM: Vaguely.

MT: Vaguely. They set me up for an interview, I filled out voluminous papers, including clearance papers. In order to work there you had to have a Q-clearance, so all the clearance stuff went through before they even interviewed me. If I hadn't been "suitable" that way, they would never have bothered with me.

VM: Can I interrupt you at this point to tell you — which year was this?

MT: 1948.

VM: OK. Thirteen years later, when we were in LSB (*Life Sciences Building*), I was once visited by the security guys who were running a check on you and they asked me whether I could give them whatever they wanted, I don't remember what it was now. I pointed out that I wasn't a citizen, which I wasn't at that stage, and they said that didn't matter. I was very touched to be asked. You got through, I take it?

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MT: Yes. As an aside, it was only two years ago that I gave up my clearance.

VM: They are still running it, are they?

MT: Yes, they do it every, I think, eight years now and there was just no point in my continuing to have a clearance. So it was a rather long-lived clearance. I got through the clearance procedure and went to the personnel office, and Mr. Bigelow talked to me. He explained there was this job with Professor Calvin and his group, he explained what the group was — it was a small group of chemists located in two different places — in the Donner and in the Old Radiation Lab. and that Professor Calvin had had very excellent secretaries, the only unfortunate thing was they all got pregnant! They were coming and going and he be very excited about them and they were very good and then, bang, they would go.

I went and interviewed the four senior staff people (*of the group*) at that time: there was Bert Tolbert, Peter Yankwich, Jim Reid and Charlie Heidelberger. They all interviewed me and I seemed suitable to them. At that time, of course, Bert asked all these pertinent questions like: when was I going to get married and, if I got married, when was I going to have children?

VM: This was all before that had happened.

MT: Yes. All before this had happened. You couldn't ask those questions now. It would not be proper. After they had decided that I would be "suitable", then Dr. Calvin interviewed me. He bounced into the office. He was much more rotund in those days. We talked a bit and he asked what kind of work I had done, which was technical secretarial. And, you know, he said "OK, you'll do."

VM: Did you know any chemistry?

MT: I had chemistry at college, Chemistry 1A/B and Chemistry 8 and then I had been working in an engineering group for some years. I knew enough chemistry. I overlapped with the girl who was there, Betty — can't remember her last name, her husband was a zoologist (*it was Betty Cohen*). In about a week she clued me into everything and off she went and I came.

VM: How big an organisation was it at that time?

MT: Let's see. There were four senior staff in Donner, Martha Kirk was there, two other women who were technicians — well, they actually had BS degrees and they called them research associates, we had one graduate student. In ORL, there was Andy (*Benson*), who was in charge, Al Bassham, Sam Aronoff, not Bill Stepka, two or three other people — Tom Goodale, Vicki Lynch (*Vicki Haas*, actually), the dishwashers and people like that. That was about the total group. There wasn't any other faculty associated with the group at that time. Calvin was it.

VM: How close were the two groups of people?

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MT: There was very good co-ordination back and forth. In such things as counting (*of radioactivity*), for instance, the Donner had the best counting room so that's why a lot of the counting was brought over and done in Donner. Peter Yankwich was the one who was most involved in developing the counting techniques, both in ORL and in Donner. Bert and his people would synthesise the compounds that were then used with the algae and also in the animal studies, and various other things. The work in ORL would have been very difficult if they hadn't had such close in-house support for making the radioactive compounds which didn't exist then. There were very few sources of that kind of thing.

VM: Although they were separated by a couple of hundred yards, they considered themselves a unitary group?

MT: Oh yes. Of course, we always had parties together. You remember this in ORL, we had all the parties there and the people from Donner would come over. The Donner people were not as intimately associated with the photosynthesis work, or even as intimately associated with Calvin, but they were still a very important part of what was going on. When Professor (*Ernest*) Lawrence asked him (*Calvin*) to develop the group, he (*Lawrence*) was interested in animal studies as well as the plants because of Dr. John Lawrence (*Ernest Lawrence's brother, who headed a medical research group in the Donner Laboratory*). That worked into the Donner end of it and we were much more closely associated with Donner at the beginning, when I first went to work there, than later on.

VM: Calvin was very concerned, wasn't he, with the early isotope work and was an of the authors of the book (*"Isotopic Carbon"*)?

MT: That book was one of my first projects. That had just been written and the final typing had been done when I showed up.

VM: It wasn't a case that Calvin was much more oriented on the photosynthesis, he was spread across both of the things?

MT: He was but his real interest...At that time, actually, paper chromatography hadn't come into the work and the efforts which were going on in ORL were very laborious and things weren't happening near as fast. So, therefore, sometimes things were happening more quickly in Donner with the animal studies that Martha (*Kirk and Charles Heidelberger*) was involved in than the work in ORL with all the chemistry that had to go on. When (*paper*) chromatography came in it opened up a whole new world for them.

VM: Calvin always liked to be at the forefront of new developments.

MT: Oh yes, I think that he was. The whole chromatographic technique really got them going.

VM: So, you started off in Donner?

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MT: The word just spread. We would have many, many more applicants, even applicants with money, than we could fit in, particularly before the building was created.

VM: I remember that by the time that I came, he (*Calvin*) was already adopting the view that it wasn't enough just to have money, you had to get the money competitively because he didn't know who people were and he had no way of judging. Did he start that very early?

MT: Yes. Very early on. The actual AEC (*Atomic Energy Commission*) budget wasn't all that great and we didn't have, at that time, very many US postdocs. You had the graduate students — there was always a group of those, and you had the senior staff and then you had people like Martha (*Kirk*) and Ann Hughes but you didn't have an awful lot of US postdocs. They started coming later.

VM: They didn't exist?

MT: They just didn't exist; I don't think anywhere they existed. The foreign people came in, and then the word spread, and then the US people came. He was always very careful to have competitive fellowship types, with the possibility of support (*from here*) for a second year or a third year or whatever.

VM: As far as the budget to run the lab. as a whole is concerned, that was all arranged by him?

MT: Yes: he would talk to Professor Lawrence, or Ed(*win*) McMillan (*Lawrence's successor as director of LBL*) or whatever and the (*budget request*) went through The Hill. At that time, things were great. You always problems if you wanted some huge piece of equipment, you had to justify it. Basically, our budget was never a real problem.

VM: So there was not the competitive atmosphere in budgeting that came in later.

MT: No. you didn't spend your whole time writing grant proposals. In fact, one of Bert's jobs, and that was also true for Dick Lemmon when he took over, was as budget officer. Later Paul Hayes took on some of that (*work*) when we had the building. They always knew how much money they needed for people, pretty much, but it was the equipment and so forth (*that was negotiable*). It was their (*Bert's and Dick's*) jobs to write the reports, get it to The Hill (*and have it sent on to Washington for final approval*). If there was any arguing or discussion, that was their function.

VM: As I recall, it was a fairly simple format that you had to say how you'd fulfilled last year's expectations...

MT: List of publications.

VM: ...what you were doing this year and what your aspirations next year were. You had this routine of shifting stuff up, year by year (*between the budgetary categories*).

VM: Was there a feeling, both in the group, and perhaps among others, that it was a well-funded, unusually well-funded, or was that not really the case? Were others like that, too?

VM: Did it give rise to trouble? Did people get envious?

VM: In the early days, particularly before chromatography took hold, did they ever think that this problem was going to be too difficult and they wouldn't be able to do it?

VM: Did you get the feeling?

VM: When you first started, ORL was fully occupied, was it, by his group?

VM: You mean not just for the Calvin group?

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MT: No. The machine shop was actually a Radiation Lab. facility — people in Crocker used it, people in Donner used it, I think even some people from Chemistry used it. That was what I call a lab.-wide facility. It was a great thing to have there.

VM: Eventually, the Calvin group occupied all the building except maybe for some of those machine shops.

MT: Yes, they occupied the downstairs. Probably within two years after I got there (1950) they had the whole building except for the shops.

VM: It was, of course, this open building, the doors were never shut, what doors there were, were never shut. Donner was very different, was it?

MT: Donner was like Latimer, like a hotel, with the doors opening (*into the corridors*), so therefore, you couldn't personally have the same kind of interaction that you did in ORL. The Latimer is that way; most of the campus buildings are that way. It's very hard, you have to make a real effort, to get together and talk about stuff. Whereas in ORL, you were all there together and that was also true when the Calvin Lab. was built.

VM: Do you think that the people in Donner failed to mix as well as the ORLers because of this?

MT: Yes, I think so. We had a very tiny little room for coffee. (*When I first started working in the Calvin group*) we used to have it in the hyperbaric chamber out behind the building that was connected with a corridor which was where experiments on the Air Force were done during the war. There was this big chamber and we would huddle around (*it with our coffee cups*), there were mice (*in their cages!*) on all the counter tops. We all made an effort to go down and do that (*i.e. have coffee*) but it wasn't the same. You had to walk a long way (*down three flights of stairs*), and it wasn't a very pleasant area (*for socialising*).

VM: I haven't heard of this hyperbaric chamber.

MT: Maybe I have the wrong word You should talk to Bert about that; he's the one who would really know the history. It was an enclosed chamber, inside this big room, called the Donner Annex, and there some experiments done there during the war for oxygen deprivation in aviators, or something like that. There was this great round thing (*chamber*) in the middle of this room, and these counters around the edges. We crowded in there. That was knocked down — I can't remember — maybe at the end of the fifties, maybe earlier, when they put the Donner expansion on.

VM: You said there were mice in there at the time you were coffee?

MT: Yes, sure. Safety has changed!

VM: Oh yes. You can't drink coffee in mice rooms.

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MT: If we hadn't had that — then we moved up to some other little room (*on the third floor of Donner*), there were chemicals all over the place. None of us seem to be any the worse for wear for this.

VM: The group occupied only a fairly small proportion of the Donner building?

MT: Yes, the third floor, one side. And then the big lab. at the end, across the corridor (*from the other space*). It was all synthetic (*organic chemistry*) work. Martha's (*Kirk*) animal work and Ann Hughes' animal work was done in the same area, in other words, the animals were right there.

VM: But, of course, the Donner people all went to the Friday seminars.

MT: The seminars originally were in Donner, in the Library. The ORL people would come over for that. Everyone would all be there at eight o'clock in the morning

VM: What shifted it out of the Donner Library?

MT: I think when they (*the people from ORL*) moved to LSB (*the Life Sciences Building*).

VM: At one time they were held in the Faculty Club.

MT: Yes, that was when they went to LSB: in the Lewis-Latimer Room. That's the room they can put a partition down the middle and they moved from the Donner to the Lewis-Latimer Room.

VM: The Donner people, of course, never moved out of Donner until they went to the Round House; they occupied that space all the time.

MT: Yes, they did. The Donner people just went from Donner to the new building.

VM: Coming back, if I may, to the time around when you came. The way you described it there must have been something like a dozen people or so in each of the two sites.

MT: Maybe 30 or 35 total.

VM: It was as big as that even then.

MT: Well, if you took in the graduate students and stuff like that (*office people, dishwashers, etc.*), the BS people level people who came and went, some stayed a while, some didn't. A sort of a floating population there...

VM: You saw it, as you did see it, from the centre as Calvin's secretary. Presumably there were many more inquiries from people wanting to come than actually finally turned up.

MT: I never it but I have a big book in the office of all the statistics through 1980 of how many people were there, what countries they came from and all this kind of thing. I

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used to have great big folders full (*of correspondence*) from people who had asked to come and never made it. In general, I think, the inquiries were from very excellent people. All the people were eager to do something and some were fortunate enough to get money.

VM: One of the things that has been very striking among the people we have so far talked to (not many yet) is how relaxed the whole thing was and, in particular, how unstuffy Calvin was.

MT: He was not stuffy.

VM: Not in the least.

MT: No.

VM: Particularly people who had come from foreign countries were not used to seeing (*someone of his stature who was so relaxed*). Was Calvin unusual in that, or was that the style of everybody around here at the time?

MT: I think it was pretty relaxed. I don't know about too many of the other Chemistry professors. But certainly people like Luis Alvarez had that same attitude, and Segre was pretty relaxed. So, I think these people did have an attitude of friendship toward their group. Because they were in an organisation like the Radiation. Lab. that protected them from so much of the outside difficulties of the world, they could be that way. These groups were always very close-knit. The Alvarez group was very close-knit, the Segre group was close-knit. It was hard for me to judge the Chemistry groups. There weren't really any big groups at that time (like the Rapoport group which evolved later on). There were almost like individual professors with two or three grad. students and a couple of postdocs. Bigger groups in chemistry came along later. Calvin was always busy, he was never what you would call a relaxed person from the point of being laid back, he was always pushing forward. But it was very informal.

VM: From the time when you knew him, did he have this business of getting up very early in the morning and going to bed very early at night?

MT: Always. He used to teach eight o'clock classes, they used to be on Saturdays even. He has always been an early morning person. He'd would pop into ORL at ten minutes of eight before his class...

VM: Just to make sure.

MT: Yes.

VM: And he was fat in the early days?

MT: I have some interesting pictures of him.

MT: Oh yes. In the Old Chemistry Building, not his second office, but the first one which was on the court yard there with the atrium, there were wooden floors and he used to put his cigarette down and grind it out.

MT: Very heavily. And he was very rotund.

MT: Yes.

MT: In 1949, when he was 38 years old.

MT: I don't remember now. I think it was at home; I think I would have remembered had it happened at the office. It was quite severe, very severe. He off for several months.

MT: Pretty much. Very severe heart attack. But I think if he hadn't had the heart attack then, and if he had had his first heart attack when he was 45, that would probably have been it. If he hadn't had the heart attack at age 38 he would have kept on with the same bad habits that he had with the smoking and eating.

MT: He was in Kaiser.

MT: Not in hospital. When he went home, he was told not to come back to the office for several months. He had people come up to the house and I would go up every day with the mail and he would answer the mail and I would bring up whoever he wanted to see that day. I would make coffee and they would talk, and we would all go back down again. He was working but he was at home and he was only doing it so many hours a day and then Gen would say “go now; time is up”.

MT: Yes, and he kept it off. He went on a very strict diet from the point of view of his cholesterol and even now, when he could afford it, he says “I can’t have any ice cream”. He is thin as rail; it would do him good to have a bowl of ice cream, maybe.

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VM: There was a recurrence, wasn't there, much less severe — in the sixties?

MT: He has had several in the sixties. There was one in the late sixties or early seventies...

VM: When he was off for three months or so?

MT: Yes...and then he has had several bouts of congestive heart failure and various other things. I think he really did pay attention to his diet and various other health matters after the (*initial*) heart attack.

VM: He has always maintained a great deal of liveliness in spite of whatever physical problems he may have had.

MT: It's only in the last two or three years that he has sort of diminished his psychological energy.

VM: To get back to the ORL group in the late forties, early fifties. Calvin at that time presumably was the way he was later. He would take an intense interest day by day in what was going on in the lab., looking at data, etc.

MT: Every day.

VM: But he never did any experiments himself, did he?

MT: From what I remember in writing that book (*"Following the Trail of Light"*) he was doing experiments early on, probably in '47 — maybe in '48, I don't know — but that sort of ended. Other people were coming in, things were going too fast, he had his teaching, as the group got bigger he had to pay some attention administratively to the group. I would say — and you could check this with Al (*Bassham*) or Andy (*Benson*) — probably not much after '48 or '49 at the outside, maybe even earlier, he just didn't do experiments himself.

VM: You say "his administrative responsibilities": he never was actually much of administrator in the lab. himself.

MT: No, but he was appointed to various university committees which required his attention. One was the Educational Policy Committee. In addition to his Rad. Lab. stuff, he did have university responsibilities. If you are put on these things you have to do some work and that took a little time. Plus, he started travelling, giving talks and that took him away from the lab.

VM: When did that occur?

MT: He had been going to things like ACS (*American Chemical Society*) meetings throughout his career since he was at Berkeley. But I think one of his first international trips was to England in '49. From then on, it just burgeoned. He was always going. He took a sabbatical in '50 and was in Europe for five months.

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VM: Where did he go?

MT: It was Norway, mostly: he spoke in England, he spoke in Germany (*he also went to Italy*).

VM: He was getting better known, I guess, by then.

MT: The first path of carbon paper came out in '48. In the meantime, he had all these other interests, the chelate work (which just came out all of a sudden as a result of declassification at the end of the war). And all the synthetic (*organic chemistry and reaction mechanism*) papers were coming out from the Donner. His name was becoming very well known in many fields.

VM: Was he talking about all those things or was he talking mostly about photosynthesis?

MT: If you look at the list of his speeches, he was talking about (*everything*). When he got into the thioctic acid later on, he even talked about the stilbene work he did with Janet Splitter — he was talking about everything. Of course, his favourite thing was the photosynthesis. He also enjoyed the deuterium work that you guys did, he thought that was fun. He had things that he liked the best to talk about.

VM: When people came into the lab., when this turnover of postdocs and graduate students came in the lab., did he actually think ahead of time about what they were going to do? Or did they shop around?

MT: Most people, as you know, in their (*initial*) letters (*of inquiry*) said we've done so and so and we'd like to use this knowledge in the field of so and so. Most of them did not come in and say "I want to work only on this problem". So they would come in, and they would have a talk, and Andy and Al and everybody would be in there, and there would be some interesting problem discussed, and he (*Calvin*) would say "why don't you try that?" And off they'd go!

VM: He wasn't dogmatic about what they should do?

MT: No. He didn't say "you have to work on this". There were some things that were so exciting that he was always happy to have more than one pair of hands working on it. But the people that came with preconceived ideas about what they were going to do — they wanted only "this" — didn't have as good a time. There were some who did that: (*Gustav*) Utzinger was one. He only wanted to work on whatever he was working on down in Old Chemistry, the organic chemistry problems. I don't mean that he didn't get something out of it (*his visit*), and because he came on his own private money, there was no reason he couldn't do that within the scope of the whole group. In the ORL group, the photosynthesis thing was going so fast that there was always something new and exciting, particularly when you go down a little further toward the late fifties/early sixties and you get into ESR, NMR — all that kind of thing broadened the whole program.

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VM: There were plenty of rumours, about which I am very vague, about differences of opinion between Calvin's group and other groups, the Arnon group was one.

MT: That's why Nate Tolbert wanted you to talk to Bob Buchanan.

VM: How did you see it from your point of view? What do you remember of it?

MT: I certainly remember hearing about it but I couldn't scientifically judge anything, because I'm not a scientist. I think that Dr. Calvin sometimes rushed into print a little sooner than he might have if he hadn't had someone breathing on him in a way. I think Arnon was as unique a character in his way as Calvin in his, and there was also Martin Gibbs. So, they wouldn't all love each other. There was definitely conflict with Arnon.

VM: These conflicts were not just technical ones — were they personally antagonistic?

MT: I don't know. I can't answer that question.

VM: You never saw them together?

MT: I never saw them together. Al (*Bassham*) can tell you more about that.

VM: I remember when Otto Kandler came, there was intense argument, wasn't there, for many months?

MT: Yes. Again, I wasn't into it from the scientific point of view. But you could see it personally in the lab. Just walking into the lab. and they would be going at it, verbalising.

VM: Did you get the impression that for all their arguments they disliked one another?

MT: I don't think so. I don't think Kandler disliked him. I don't know what Calvin felt for Otto Kandler. They didn't have that much contact after Kandler left. Calvin kept up close contact with some of the people (*who had been here*), but I don't think Otto was one of them.

VM: Wasn't there a time when Trudi Kandler and Gen (*Calvin*) decided to try and make peace between them?

MT: I don't know.

VM: I'll have to talk to some of the others and see whether they remember that. One of the things that was very striking when we (*first*) came (*in 1956*) was the social atmosphere in the lab.

MT: It was wonderful.

VM: And it was like that from the beginning?

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MT: Yes, much more so in ORL than in Donner, again because of the physical closeness of the people. There was always a lot of social contact, lot of parties, weekend skiing, the camping, the beach parties, party, party, party. Very, very good. That even maintains itself today, surprisingly enough, in the round building, with the various groups in there now — they are always doing things together.

VM: And it was from the beginning like that?

MT: It was always that way from the beginning. I've always felt that was one of the wonderful things about that group was the social interaction among everybody and on an equal plane.

SM: Were Calvins part of these group social activities?

MT: On some occasions. I think they went on one ski trip that I know of. The local things like the beach parties at Stinson, they would go, the camping I don't think they ever went. But the were always asked. Partly after Calvin had his heart attack, he may not have wanted to do that kind of thing so much.

VM: When you joined, the use of ORL and Donner had already been in existence for two or three years, I guess. Did they think it was going on forever or did they see the possibility that the building was a temporary structure which wouldn't last?

MT: You mean ORL?

VM: In particular, ORL.

MT: I think they thought it was their home but that's just my own opinion. The problem with the building was the fact that it was so old that the facility was not adequate for the kind of science that was going on in that building. About 1958 when they were starting out Latimer Hall they had to knock it (*ORL*) down because they had to make room for the big new Chemistry complex. I think until maybe the late fifties it just looked like it was our home, for the photosynthesis people not the Donner people. I think it really came as a personal psychological blow to Calvin when they had to knock it down and the group had to move down to the Life Science Building even though they thought they would have adequate space in Latimer Hall. That turned out not to be true, and that's when Calvin went for his own building. There would not have been room in what Calvin was assigned in Latimer Hall for the ORL people and the Donner people and the whole thing; it would just not have been enough room.

VM: Let me try and get the sequence clear. At some stage, it became clear that the Chemistry Department was going to build Latimer Hall. Can you remember when that was?

MT: They knocked ORL down in 1958 so it must have been in the mid-fifties.

MT: First of all, he discussed it with the people on The Hill and the AEC. I think they would have funded it but he would have had to go up to The Hill, and he absolutely didn't want to do that because, by that time, we had people from psychology, biophysics, biochemistry, molecular biology in his group. When it became clear that he did not want to move the group up The Hill, I think he discussed it with (*Glenn*) Seaborg, who was then Chancellor (*of the Berkeley Campus*) and they suggested there that were certain avenues he could apply to and the State (*of California*) would

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provide a certain percentage (*of the construction costs*), the National Institutes of Health and the National Science Foundation. So those three people were...we didn't have too much to do with the State but these other two were (*indecipherable*). He also wrote at that time to all sorts of other foundations — I can't remember, there were so many things going on. This was before computers, before Xeroxes; it was laboriously typed out over and over again. It came down that the NIH would give a certain amount of money, NSF would give a certain amount of money, and the State; and then there was this \$300,000 that the Kettering (*Foundation*) people (*provided*). The AEC equipped the building. So, in a sense, it was joint thing — there was AEC money in the building in the form of equipment.

VM: The design of the building, the, must have begun reasonably early because you have to go out for money on the basis of something; you can't just wave your arms around.

MT: I don't remember the date of the first architects meeting but the senior staff got together and the campus people hired Michael Goodman to be the executive architect.

VM: That was their choice?

MT: That was their choice, that was the campus choice. They had certain people they worked with and he was also on the faculty of the School of Architecture.

So Michael Goodman was the architect and then they had Florence (*Porter*), his assistant, was the liaison between Michael and the people (*i.e., university, senior staff, contractors, and the AEC*). The senior staff got together (*to decide*) what kind of building would they like. What do we want in this building? The whole thing was the kind of space that they'd had in ORL. Even people who had worked in Donner saw how good that was; it wasn't that they wanted to work in a building that was just like a hotel which was just like the facilities they had. With that kind of philosophy, (*the evolution of the design*) was interesting. First of all that was half a circle...

VM: I remember that that was Al Bassham's original design.

MT: ...and then that didn't look too good and it seemed to be a full circle, with the various kinds of room for chromatography, and stuff in the cores. It turned out to be a very practical plan, actually.

VM: As it worked out, the building had very few faults.

MT: It turned out remarkably well and even when they (*later*) had to put the temporary trailers (*for cancer research*) up on the roof it didn't affect it (*the building*) that much. I remember there were these planning meetings with the architect, planning meetings with the campus people, and all the senior staff participated including Paul Hayes. It's too bad Paul didn't keep a record (*of the history of the building design*). He did, actually, have a lot but I think it's been destroyed now, or sent away to the archives, of how the building was created because there was a lot of discussion back and forth.

The enthusiasm was there, the *(first)* day I walked in and it certainly lasted through the time Calvin was there in the round building. People were very enthusiastic in that group, even the people in Donner who sometimes felt a little isolated from what was going on, particularly when the other group went to LSB. We *(the Donner people)* didn't feel we were as much a part of it *(the Calvin group as a whole)* but that was just because of the physical distance. But everybody *(in the Round House)* was enthusiastic about what was going on. With the things happening in photosynthesis, it was very exciting but there were also things happening in animal biochemistry and in Dick Lemmon's radiation chemistry and the chemical evolution experiments. There was just exciting science going on *(in the Round House)*, in all different directions.

VM: The photosynthesis was, of course, the anchor by which, at least, the ORL contingent functioned, but lots of stuff spun out from that. There were people who were very much a central part of that group who were not actually working on photosynthesis, or not directly. Anything that they did reflected back because there was so much back and forth communication which went on all the time.

MT: It was amazing that that happened. It even happened, actually, when we got into the new building. The third floor was supposed to be mostly photosynthesis — it turned out to be a lot of other things in addition — that reflected back not only on photosynthesis but, if you want to call it, structural biology now — the whole idea, the different things they were doing.

VM: Nevertheless, there was a difference in atmosphere, wasn't there. I wasn't there at the time when the move actually took place out of ORL: I left with it functioning and came back to find everybody buried in the basement of LSB. Obviously, things were different. Good though the Round House has been, it never quite recaptured that atmosphere (*of ORL*).

MT: No, it didn't.

VM: Maybe that's simply because it was just later. People were older and science had moved on and they were thinking different things, as well as the structure of the building.

MT: Calvin made a comment one time, about it used to be he would go into ORL, day or night, the graduate students would be there, they would be working, and the postdocs; everybody would be working. “Now”, he said, “it’s like a business — it’s a 9:00 to 5:00 or 8:00 to 5:00 or something.” That was ten years or more ago that he said that.

SM: Some of the other people we interviewed, they made the point that during that later period he was less around because he had to be in Washington or he had to be in other places. It was his presence at the time in ORL that made the real difference.

MT: I think that made a tremendous difference. I think that as he progressed, or whatever you want to call it, and got involved in things in the National Academy of Sciences, the President's Science Advisory Committee, all sorts of other things that he was

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involved in, he was gone at least half of the time for some years. Therefore, those people who came through during that period of time didn't get the nurturing from Calvin personally that they had gotten earlier on (*i.e., that earlier people had received*). That was just one of the reflections of fame, if you want to call it that. People tend to get involved: they are asked to do something, they feel they need to serve and so one service leads to another service and pretty soon it just snowballs.

VM: I think there were a number of factors which were important, as you say. One of them was that when the group started, Calvin was very much the leader and the senior man. But as the years went on, everybody got older and they (*the senior staff*) were all becoming pretty well known, the leaders of the various groups, and themselves being invited to international conferences. So although Calvin was still the leader he couldn't quite be the leader in the same way as he had been a lot earlier. The fact that he was being involved in things more and more remote from the daily activities of the lab., just changed the character. So perhaps that was one of the contributing factors for the Round House simply being different from the earlier days of ORL. It was a younger period altogether for everybody.

MT: I think that is true of organisations. In other words, as the organisation gets bigger and more important then the leaders are asked to do this and that and they just don't have the physical time to spend. There was nothing wrong with the building itself. Everything was very conducive, I am talking about the Round House now, to interaction. The building itself contributed to interaction. But he was gone, a lot. There's no question about it.

VM: It's surprising, thinking back, how well, even by modern standards, ORL functioned as a building. Because it was actually a very crummy building as a building, very old fashioned in the way it was put together. And yet I don't recall at the time that there was any great difficulty in working in it. I think that everything seemed to work well enough, even in this funny old building.

MT: You had good equipment. In other words, even though the building was old, you weren't suffering with really old equipment. When something was needed, you were able to buy it, you were able to set things up in funny spaces, but, nevertheless, the equipment you had, there was never a problem. You could always get stuff, or have it made. Even though you may not have physical glamour, you had everything you needed to do your work, plus all the people.