Chapter 1

MELVIN CALVIN

Berkeley (California)

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VM = Vivian Moses; MC = Melvin Calvin; MT = Marilyn Taylor; JO = John Otvos

SM = Sheila Moses

VM: I thought we might start by thinking about how the whole photosynthesis, how the biochemistry of photosynthesis started. Because you (*Calvin*) were really a physical-organic chemist at the beginning and you decided to go into what was essentially biochemistry.

MC: I don't know whether or not I made a conscious decision, but it happened.

VM: Did you see it as going into biochemistry, or for you, was it just more chemistry?

MC: I didn't really think about that at all. I just did what I had to do. I didn't plan to go in this way or that way; I just went. Does that make sense to you?

VM: But you chose to start with looking at the fixation of carbon dioxide. How did you tumble to that? What prompted you to get ...?

MC: I had CO_2 I had radioactive carbon, carbon-14.

VM: And because of your earlier interest in pigments, it fitted together with that.

MC: No**,** I had lots of it so I had to do something with it. That was all there was to it.

VM: But why did you decide to do that instead of doing something else with it?

MC: Because that was an obvious thing to do. That's was the main sink for CO_2 in the world. So, I thought, well we'll do that. There wasn't anything subtle about it.

VM: You then began to develop this group of people.

MC: Well, I didn't develop a group of people. They came.

VM: They must have come to something. They must have known where to come.

MC: I guess so, I didn't really look for them, if that's what you mean. I didn't organise it. They came. You don't understand what I'm saying?

VM: I'm trying to think what it was like on Day 1, you know. You decide that you are going to do this. It was, as you say, the obvious thing to do. You had a lab. You had any experience?

MC: The Old Radiation Laboratory; it was an old wooden building.

VM: Yes, I remember that.

MC: I had the algae cultures in one corner of it, it one corner of the lab. I did that for many years, I don't know, how many years (5-l0 years, maybe more), I lived in that building. It was an old wooden building, until they tore it down.

VM: Do you remember what it was like at the beginning, when that lab didn't have anything in it and you had to decide what was to go into it.

MC: No, I don't remember that. I just put stuff down where I had to put it down, that's all.

VM: You, by yourself. Were there other people?

MC: There were other people--graduate students, post-docs.

VM: Do you remember who they were at that time?

MC: We can look it up.

MT: Not really, not that far back. But Andy was there, Andy Benson.

MC: I guess he was, but he wasn't crucial to the action. He was there, but I don't remember him doing something that made a difference.

VM: So there were original. graduate students and postdocs who were in with you from the very beginning.

MC: I guess so.

VM: We can try and look them up.

MT: Al was one of your graduate students, Al Bassham. When he came back from the war, he came back to get his Ph.D. He had a BS degree at Berkeley and then he went into the Navy. So he came back and somehow he made the connection with you. He was

one of your original graduate students. And you had Sam Aronoff; yes, he was a chemist. You knew him from your war work; he worked with you on the chelates. And then there were some BS chemists like Tom Goodale and people like that...

MC: I don't remember.

MT: ...and Vicki Lynch was there.

MC: Yes, I remember her.

MT: She was a microbiologist; how she got there I don't know.

MC: I've no idea.

VM: I'll get back to some of them. Maybe they can fill it in from the other side of the picture. So, you started with this group of people.

MC: No, I started by myself.

VM: Literally by yourself?

MC: Yes. Then, we added people to the group.

VM: Had you ever handled biological material, living things, at that stage? So, you did it from the ground up.

MC: Yes, I had the algae culture in the corner and that was the heart of it. There were four black-bottom vessels in a shaker in a thermostat. And we kept them going all the time and harvested them when we needed them. That went on for years; I don't know how many years, but a long time.

VM: Presumably you were aware of the work that Sam Ruben and Martin Kamen had done.

MC: Yes, I was well aware of that. But they had moved away, for some reason. I don't know where they went, but they weren't there.

VM: But you'd seen the sort of set-up that they had had presumably.

MC: I didn't copy that. It was all done in the Old Radiation which is this building here, old wooden building, and I don't recall how we got there. I just went there. I think Ernest gave us the building.

VM: Some of the things that struck me...

MC: The cyclotron room was next door; it wasn't there but the empty room was there.

VM: But, you never had an office in that building, did you? Your office was always in chemistry.

MC: Oh, but I had an office there. Why?

VM: Well, I was wondering you see: when you went into that place, in the morning or whenever you went it, and you used to go in there every day as I remember, you didn't go and hide yourself in an office.

MC: No, certainly not, the office was out in the open anyhow. It was all glass lined. There was no closed-in area at all. How shall I tell you that?

VM: Well, I remember it. So, you used to go in there and just talk to people, as I remember.

MC: Yes.

VM: And what did you do; you grabbed the first guy you saw?

MC: I don't remember that. I usually had some particular part of it going in my head and that's what I would pursue. Something like that. There wasn't any profound thought about it.

VM: But you didn't do any lab work yourself...

MC: Oh yes I did.

VM: ...with your own hands there?

MC: Yes, it was possible to do that and I did occasionally something. I don't know that anybody liked it, but I did it.

VM: I remember that you used to spend a lot of time actually sitting with people at their desks, going through all the stuff. When did the big white table start, do you remember, where one laid out all the chromatograms? Who thought of that one?

MC:: That was later. That wasn't at the beginning.

MT: That was when we got chromatography. Remember, at first they didn't have chromatography and when it was necessary to have a place to spread out the chromatograms, that's when Mr. Norman made the table.

VM: Mr. Norman made the table?

MT: Mr. Norman made the table.

VM: And that became the social focus of the whole group, didn't it? That's where the coffee was.

MC: It wasn't done deliberately. It was an accident, I guess.

VM: But that's the way it worked out, and I remember that led, then, to your concept in the Round Building (*Melvin Calvin Laboratory, MCL*) of having a focus in the middle so that people would concentrate around it.

MC: Well, I guess so. I think there was some kind of relation. But I didn't think about it that way.

VM: The other thing that occurred to me is the seminar, the Friday morning seminar programme. Did that start from the very beginning?

MC: Very close. I can't tell you exactly but it was close to the beginning of the work in the ORL?

VM: How did you think of this God-awful time of 8:00 o'clock on Friday morning?

MC: How did I think of it?

VM: Well, you always got up very early, didn't you?

MT: Vivian, he taught a class at eight o'clock three days a week, so what's another day?

VM: I remember that. Some of us weren't used to thinking at all at eight o'clock in the morning.

MC: I was there all the time.

MT: I think that teaching the class at 8:00 o'clock, three days a week, was habit.

MC: It woke me up!

MT: What's one more day a week, on Friday morning!

VM: So those started from the very beginning and was this a planned organisation or did you just sit around and chat when you first started?

MC: I'm sorry; I don't understand what you mean.

VM: Well, did you decide who was going to speak a long time ahead? How did you decide who was going to give the seminar?

MC: Just that day.

VM: Oh, really?

MC: Yes: I looked around and asked somebody to talk

VM: Just at the drop of a hat, like that?

MC: Yes.

VM: By the time I knew it, they were told the day before.

MC: Oh, really.

VM: Yes, and then, these kids used to stay up all night getting their stuff ready.

MC: Well, I don't remember that but I would look round and ask somebody to talk. That was simple. There was nothing planned about it. I just wanted to get people whom I didn't hear about, whom I didn't see every day or whom I didn't talk to, to get up and talk.

VM: And you always sat at the same position, didn't you, at the right-hand side of the table, and this long table became part of the scene.

MT: Originally, when I first came, they had the seminars in the Donner Library, you remember that?

MC: No.

MT: Yes, that was on the second floor. There was nowhere in ORL basically for a place like this. But there were chairs and they came into the Donner Library but you still sat in the same position and they had chairs sort of like the seminar room in Gilman Hall.

MC: I don't remember that, but I'm sure that's true. I can't remember the details of that. I don't visualise it at all, but I dare say you are right.

VM: After that, we used to go into the Faculty Club before the Round House was built. There was a Faculty Club room that we used to squeeze into: it was a bit small.

MT: It was the Lewis-Latimer Room.

VM: I don't remember the name of the room but there was a room somewhere that we used to use.

MC: I don't remember that. You are probably right. I don't know how you remember all these things.

VM: I was there!

MC: Well, so was I!! But, that doesn't mean much — to me anyhow.

VM: When you started — I read the thing (*oral history*) you did with Norberg and there's a lot of information there about how you came to have an establishment in Donner.

MC: That was John Lawrence's idea, and I had the top floor, at least part of the top floor of Donner, for some years (I don't how many years, but for quite a while). I don't remember why that was so. I think John wanted it, and I wanted it. I didn't have any other place. So that's how it happened.

VM: The people in Donner were largely concerned with the synthetic work, weren't they?

MC: Almost all of them.

VM: Did you ever see a distinct separation between that group and the photosynthesis group?

MC: I don't know what you mean by that.

VM: Well, did you regard the whole thing, the Donner and the ORL people, as one, as part of one thing.

MC: I think so.

VM: Was there a lot of interaction between them?

MC: I can't answer that. I think so.

MT: Sometimes, Vivian, the people in Donner would make the (*synthetic radioactive*) compounds that the people in ORL would use. So, without Bert and the people who were doing the synthetic work, when you needed something over here (*in ORL*) radioactive, they made it there and then you could use it in your photosynthesis studies. So it was really an integrated operation.

MC: I guess so. I can't remember how it worked, but that sounds reasonable to me.

VM: So people moved freely back and forth between the two groups.

MC: More or less but there really weren't very many movements because they were different kinds of people. Some of them were analytical biochemists and the others were synthetic organic chemists and there wasn't much moving around.

VM: They were all one group when it came to seminars and activities like that.

MC: Yes, but they were different talents.

VM: Everybody knew everybody else, that sort of thing. When did you begin to attract this enormous range of people coming from other places, the foreigners, the others from inside the US?

MC: I don't know; they just came. I didn't make any effort about that.

VM: At the beginning, as I understand it, there really wasn't anybody else working on photosynthesis, or on this aspect of photosynthesis, at all when you started.

MC: In the world, you mean?

VM: In the world.

MC: I think you are probably right, but I can't be sure of this.

VM: How did you begin to publicise what you were doing?

MC: I published papers.

VM: Did you go to conferences?

MC: Yes, but the papers that I published were the important part. The conferences were important, but they weren't the heart of it. The heart of it were the papers.

VM: So people would then approach you and want to come?

MC: Yes.

VM: And how did you begin to respond? What did you say to these people when they said "I want to come" and they probably said "find me money", didn't they? They usually said that.

MC: Yes, of course.

VM: So how were you able to respond to that at the beginning?

MC: Well, I had the money. I don't know where I got it; Ernest, I guess. Lawrence. So, I just asked him and he gave it to me.

VM: That's a very favourable situation (*laughter*) — it doesn't happen any more like that, I'm afraid.

MC: Well, I don't remember really.

VM: So, you were able to accommodate these people from the very early stages?

MT: You probably remember, Vivian, from your own experience that Dr. Calvin would tell you to try and get a fellowship of your own; particularly for foreign people the Atomic Energy Commission money wasn't so easy to get for foreign people. So, you (*Calvin*) would say to foreigners: "get your own money" and then if you need a supplement, we can help you. Or, if you come for a year on your own we can take care of you for the second year. A lot of the people from Britain and France would come on their own money for a year and then we could take care of them after that.

VM: Were the original people mostly Americans and did the foreigners begin to come later?

MC: You are asking a question I just can't answer. I just don't know. It seems to me they were there all the time.

VM: They were certainly there for a long time.

MT: When I came in 1948, I remember that there were two foreigners then, an English person (*in Donner*) and I think there was one in ORL but I don't remember who they were. And the others were American.

VM: There was a guy called Ted Abraham.

MT: He's the first one I remember. He's a "Sir" now.

VM: He's in Oxford and he had to do with cephalosporins later on; that's how he made his name later on. He must be retired now but I think he's still in Oxford. I must go and talk to him as well about the early days.

MT: He was in Donner, he was not in ORL, and he was there about six months.

MC: Who are you talking about?

MT: Ted Abraham.

MC: I don't remember him very well; the name is familiar.

MT: You (*Calvin*) visited him when you were Eastman Professor in Oxford in 1967-68 you had social and chemical contact with him again.

VM: When you first started publishing in this field, in the path of carbon field, did you get responses from the botanists. Here were you a chemist, it seemed invading their field, did they respond? Did you get reactions from them?

MC: I don't remember, but I think there was some kind of reaction. I just don't remember.

VM: At the time that you started, really nothing at all was known, was it. Did you have any even vague ideas yourself at that time about what might be the mechanism?

MC: No, not the slightest.

VM: You weren't guessing?

MC: No. Very early on I found PGA (phosphoglyceric acid) and I had to figure out where it came from, and that's how it got started.

VM: I remember you told that story about sitting in the car outside the freezer store (at Grove and Cedar Sts. in Berkeley) and it came to you.

MC: Yes, I remember that.

VM: It's a true story, is it?

MC: Yes. My wife was in there, doing something, I don't know what.

VM: And you were on a red zone apparently, and a bit nervous I guess.

MC: How did you know that?

VM: You told it, I think, when you got your Nobel Prize. You did a press conference and I remember you telling that story. These are the sort of stories that will make good reading.

MC: I remember sitting there, I don't remember doing anything, just sitting there, while she was inside doing whatever had to be done. I didn't go in, but that's all I can remember about it.

VM: When you started you had a completely open mind as to what might be happening in photosynthesis.

MC: I didn't know what was going on. That was the point. There was no preconceived notion about what might be there. There wasn't any evidence, there wasn't anything.

VM: The only piece of work that I know which was relevant to what you did and even preceded your own work was a paper which I think might have been a theoretical paper in about 1943 by a guy called Zilversmit who described the kinetic sequence that he would expect from a line of metabolites, one following the other (the word escapes me for the moment) — a line of intermediates — and he had a theoretical paper in which he said that the initial radioactivity should be 100% in the first one and then it should decline. Just the way you, in fact, did the PGA identification. Did you know about that?

MC: Not that I can recall. But I may have known it. I don't recall it.

VM: It's exactly what you actually did.

MC: Yes, but I don't remember it. I don't remember it as a fact. I believe you!

VM: It was only two or three years before you did it so you might very well have been aware at the time.

MC: Very likely, but I can't tell you more about it than that.

VM: In terms of actually running the lab., clearly you were absolutely immersed in the science of it; did you also have a big burden running it administratively? Did it take a lot of your time?

MC: I don't think so. I just didn't do it.

VM: It had to be done.

MC: Well, not much.

MT: It was done by others: Bert (*Tolbert*) did it. You set it up so that the Donner office was the administrative office and took care of the budget. The person over there was Bert and then later Dick Lemmon. They took care of the budget, they took care of the personnel, they took care of all that kind of stuff so you did not have to do it.

MC: I wouldn't do it!

MT: You got somebody to do it for you, and it was done. So you could spend your whole time on science.

MC: I did! I didn't like that kind of work!

MT: But, unfortunately it has to be done whether you like it or not. A group can't go without somebody doing that kind of work.

VM: Was all your research essentially at that time in these two groups? Did you have additional activity in the Chemistry Department as well?

MT: I had to teach.

VM: But not research in the Chemistry building at that point.

MC: I don't think so.

MT: You actually had Janet Splitter: remember Janet Splitter, who worked all those years on the stilbene problem down in basement of Old Chemistry. And you had one of your graduate students, Gilbert Seely, he wasn't in the photosynthesis or anything, he was a (*indecipherable*) Chemistry grad. student working on chemical problems down in the basement of Old Chemistry. You always had somebody down there.

MC: I hear you, and you are undoubtedly right, but I don't remember it.

MT: You had (*Gustav*) Utzinger, a Swiss man, who came on the Rockefeller money, and he was down there working on what I called really "chemical" problems as opposed to anything in ORL or the Donner. So there were always one or two that you had.

MC: I guess you're right. I don't really remember that, Marilyn, but I guess you're right.

VM: And these people really played very little part then in the ORL activities?

MT: That's correct, but they were still part of the group from the point of seminars, and this kind of thing. They were working on what I called strictly organic chemical problems.

VM: Then there were the other people who were not part of your own group who also occupied space in ORL. Rapoport's group, for example; Rapoport had one or more people...

MC: Not much.

VM: ...but he was there. He had some representation in the group.

MC: Not that I can remember. It made no impression on me.

VM: I'm trying to think of the name of the guy. There was a guy called Mel Look. Wasn't he one of Rapoport's people?

MT: Yes, he was one of Rap's. people — and Clark Lagarias: you remember him? A grad. student. doing organic chemistry related somewhat to photosynthesis.

VM: Not really. There were one or two people there.

MT: Yes, mostly graduate students who were doing organic chemistry related somewhat to photosynthesis.

VM: There weren't any others at that time. I think the others all came rather later after the initial photosynthesis people, after the initial photosynthesis period. People like Tinoco and Rod Park...

MC: They were much later.

VM: Much later, weren't they? I have to limit his effort to something otherwise I won't live long enough, so I am going to limit it to the path of carbon period, from the beginning to about '56, '57, just at the time I came and wrote the last two of the papers.

While you were teaching in Chemistry, did you have a lot of teaching to do?

MC: No, very little.

VM: You used to teach, I remember, was it a freshman chemistry, or

MC: No, a sophomore, course, a sophomore organic course first year organic.

VM: And you did that for years and years?

MC: Yes.

VM: Did you enjoy doing that?

MC: Not particularly. I did it, though.

VM: You had to, I guess.

MC: I earned my living!!

VM: You didn't have to run lab. classes or anything like that, did you?

MC: No.

VM: Or grade papers?

MC: No, I had graduate students and teaching assistants to do that.

VM: So you were able to spend all your time essentially thinking about research, reading and actually being in the building

MC: I had to prepare the lectures, which wasn't very hard to do.

VM: Was it the sort of course that didn't change very much from year to year, was it an introductory course?

MC: Yes, an introductory course. It was a sophomore course in organic chemistry and I did that for many years, but that's all I can tell you.

VM: Can we come on, as the photosynthesis began to develop and you began to get information, the first big thing, apart from PGA, that seems to me happened was your shift away from ion exchange chromatography to paper chromatography.

MC: I never used ion exchange very much.

VM: But you had to use it at the beginning?

MC: At the beginning, yes.

VM: How did it happen, how did you tumble to the paper chromatography possibility?

MC: You get a picture of the whole thing all at once.

VM: I know. But how did you realise this was the thing to do?

MC: I can't answer that. I don't really know. It seems to me that was obvious, so obvious that I didn't think about it even.

VM: Somebody has to take the first step and actually do something about it, produce a tank and paper and solvents and all the rest of the stuff. Somebody has to decide that today were are going to this; we haven't done it before. You don't remember how it started? What about the radioautography? I think that was rather an original thing at the time.

MC: **Yes.** All the work was done with radiocarbon. So, I could always get a picture of what I had even though I didn't know what they were, what the spots were. I got paper after paper with spots on them, and the problem was, what are the spots?

VM: I first met you in 1955 when you came to London to give a talk; I came (*to Berkeley*) the following year, and I remember you showing this slide, you were talking to the Chemical Society or perhaps the Chemical Engineers it was, and you had this radiochromatogram on the screen and I remember you saying to get the spots is easy, it's putting the names on them that takes the ten years! You and your colleagues spent a lot of time doing that.

MC: That's all we did. That was the $64 question.

VM: Then, of course, to find out what was happening carbon by carbon inside these compounds.

MC: That was another matter. First you had to find out what they (*the compounds*) were before you could do anything else.

VM: How did you do this? Inspired guesses? Where do you start, with a mess of spots on a piece of film?

MC: I'm trying to remember, and I'm not sure I can.

VM: I guess you guys stood around and argued about the toss what this or what that might be.

MC: I guess that's what must have happened, although there wasn't much of an argument. I said "that was it".

VM: Yes, but you could have been wrong.

MC: I was.

VM: Some of the time.

MC: I don't remember how it worked, exactly in detail, except that I did a lot of identification, mostly from the position of the spots (*on the film*). A few of them are known and that was enough to be benchmarks for this position. Then, I could guess at the others and eventually sort them out. That's how it happened as far as I can tell.

JO: Finally what you had to do though, wasn't it, to synthesise these compounds containing carbon-14, pure compounds, and see how they migrated?

MC: That was the very last thing.

JO: That was the confirmation.

MC: I didn't do very much of that.

JO: But that was the final confirmation.

MC: I didn't do much of that.

VM: You then embarked on this big program of taking the compounds to pieces and analysing them.

MC: That was a big job.

VM: And that's really an organic chemistry job to start with, isn't it? Do you remember who were the people who developed the methods for that?

MC: I did!!

VM: Who worked with you to do it?

MC: You can look in the history and see. I can't remember. I can remember PGA and I can remember the chemistry I did there to hydrolyse the phosphate off and then chop it up. The first thing you get is the CO_2 off the end, and then you get the other two. I remember doing that myself, personally. But that's all I can remember.

VM: Presumably the other guys developed methods for all the other sugars and then you put it together.

MC: Yes.

VM: At some stage, you, someone else, must have begun to realise that the thing was cyclic in its nature.

MC: I hear you, and I'm trying to remember how that happened. Well, it's just reasonable. You had to have something regenerated in order to keep the thing (*the cycle*) going. That was the fact, that's where the idea came from. It didn't start from CO_2, it started from something else: it starts from PGA — well, it starts from ribulose diphosphate which then picked up the CO_2 to make two PGAs. The recognition of that was a major, major step. To realise that was the first reaction from the CO_2.

VM: I suppose that became obvious once you'd identified PGA as the initial compound. It had to be something...

MC: Yes, but you had to find the ribulose because where did the PGA come from?

VM: You may not remember this, but I'll tell you something that I remember. You remember we used to go out to beer on Friday afternoons?

MC: No, but go ahead.

VM: We did. We used to finish early and we used to go to Laval's, up on Euclid. You didn't always come. We tried to drag you, but you weren't too keen. But sometimes we got you there. We used to go up and drink beer, the whole gang. One Friday afternoon, when it was getting close to beer time, you walked into ORL, into one of the big labs. there, and Duncan Shaw — do you remember Duncan Shaw? He was an ex-fighter pilot from Britain — he and I were playing on the blackboard with schemes. We produced the "dephlogisticated soot cycle" (*laughter*), and it started out with carbon dioxide polymerase to make polycarbon dioxide. You walked in through the door and, in your usual style, you said "What's that?" We took an eraser and started take it off, and you said "Hold it! Hold it! There may be something in it!". (*laughter*). But then we then got you out for beer pretty quickly after that because there wasn't anything in it. Those were the days when there was still argument about just how the cycle went. I remember that there were a lot of arguments about how all the wiggles went.

MC: It's surprising that you remember all that.

VM: Well, It was an important part of my life.

MC: But I don't remember it.

VM: You'd done so much of it that you don't remember the bits. I remember there was a time that there was a lot of argument going on between you and, what's the guy's name? Martin Gibbs was it? I don't remember what the details of this argument were.

MC: It was pretty violent.

VM: What was the nature of the argument?

MC: I don't remember but it was not very pleasant; that's all I can remember.

VM: Presumably they were contesting data or the significance of data. It lasted some months.

MC: More than that.

VM: It gradually died down and I think there was plain sailing after that.

MC: How do you remember all that?

VM: I don't know how I remember it. It just is there, you know.

MC: I hear you, I hear you. But the only reason it's coming to me is because you are telling me about it. That brings it up, awakens it in my head.

VM: I obviously don't remember everything either and in the course of this conversation you say things, and Marilyn says things, which remind me of stuff that I have forgotten. You don't remember everything. But, I have been doing a fair amount of thinking about this (*oral history*) and I have also been reading some of the papers, and it brings it back. You know, we have, like everyone else who was here, we have photographs all over the place, mementoes, those little plastic things, so it is all part of our lives. That's why I think this project is really so interesting and why I'm looking forward to meeting the other guys and seeing whether they all remember the same thing, for example. I don't know how much of what you say is going to be remembered by the other guys. We'll find out; I'll come back and tell you.

MC: I'd like to hear it.

VM: I have just started. It was a nice idea to start talking to you because, after all, you were the originator of the whole thing and we happened to be in California. We are on our way back from Australia and New Zealand, visiting our family in Los Angeles, and Marilyn thought it would be a nice idea if we came up now and talked to you. So, we did that. We hope to come back next spring and get down and do the hard work on this.

MC: By that time you will know more about it.

VM: I will have refreshed my memory more and there are a few people in England that I can get to easily in the meantime; and then we can come back and do as many as we can while we are here, including, I hope, I'd like to get to people like Sam Aronoff.

MC: I don't know where he is.

MT: He's in Canada but I don't know... He was at Simon Fraser University, Dean of Science there. So now probably the best thing that might work is to write them.

VM: Well, we can try. And then there's Alice Holtham, isn't there?

MT: I have already talked to her about this. I will see her in Seattle in March and I gave you her name and address.

VM: And is she willing to divulge?

MT: I'm sure she would be happy to participate.

MC: I don't remember her.

MT: She was the secretary in ORL, she used to do the drawing and type the papers over there. She's the one who put the fisherman in *Path XXI*.

VM: You remember the fish and the fisherman? Well, I have to remind you. This was Alex Wilson, wasn't it?

MT: Yes.

VM: Alex Wilson's big paper. The way I heard it from somebody or other recently, there was a drawing of this whole business (*apparatus*) with a tank in it. Apparently the JACS (*Journal of the American Chemical Society*) didn't like the size of the figure and they said it was too big and would occupy too much space and they would have to compress it. In order to get their own back on the publishers, they drew a little picture of a fisherman sitting on the edge of the tank (*in the apparatus*)

MC: I remember that.

VM: And when it was reproduced in the *Journal* it was so small, it's there, but it's so small that you need a microscope to see it (*laughter*). I gather that Alice Holtham was one of the people responsible...

MT: She was one of the protagonists on that.

VM: So, you know, stuff like that makes life entertaining. But you didn't know about that?

MC: No.

VM: In your discussion with Norberg (*oral history, University of California at Berkeley, 1976-80*) you commented there that you thought that interesting though the Massini experiment was with the light-dark, you thought that the critical thing was the Wilson experiment where he turned off the carbon dioxide. You remember there were these two experiments to prove the cycle. Massini did an experiment in which he started with the algae and the light and then he turned the light off and he got an accumulation of... What did he get an accumulation of?

MC: PGA.

VM: He got an accumulation of PGA, that's right and the ribulose went down. He turned the light on and it went back again. And Wilson did the experiment of dropping the carbon dioxide concentration and seeing these kinetic waves around the cycle. You don't remember?

MC: It doesn't matter; you remember.

VM: Well, I remember that but what I was going to ask you is that you commented to Norberg that you thought the Wilson experiment was the more significant of the two and I wondered why you felt that.

MC: I have no idea; I don't even remember what it was.

VM: I didn't see this, but apparently Wilson had two large vessels of carbon dioxide and he switched from one to the other, so he could drop the concentration from 1% or something to essentially zero just by turning a tap; it was a flush-through system of some sort. He followed the radioactivity in the intermediates and he saw these interactive waves of concentration as the shock went around the cycle one way and went backwards around the cycle the other way. You don't remember that? You should read that paper; it's a good paper!

I think perhaps we might have a little break after my next question. Do you remember what it was like when the whole group finally realised that you had proven the cycle? You had got the complete system. How did you celebrate? Did you have a party?

MC: No.

VM: No party?

MC: There were parties all the time.

VM: So there was no special reason for a party.

MC: Not that I can recall, but that doesn't mean much.

VM: I remember in about 1958 there was the Brussels World Fair. Do you remember we built that enormous display of the photosynthesis cycle with the coloured lights? Well, it stood in the back end of the Round House, near the back door.

MT: Yes, it was finally taken away. We had the panels inside the Calvin Lab. for years.

VM: There were these panels with the cycle on it, with all the labelled carbon atoms, and as it originally worked there was a series of coloured lights which went around showing the progressive labelling of the cycle components. And I remember the way it was designed, it was going to drop a sugar cube out of it (*laughter*). But for some reason either they wouldn't produce the sugar or the thing didn't work at the end, so it didn't actually produce much sugar. You don't remember this thing? There must be pictures.

MT: Oh yes, I think there are some...

MC: If I see the pictures, I guess I'll remember.

VM: That was really the final seal of approval that this cycle actually existed**.**

MT: Paul Hayes was the one engineered the panels and got them built on the Hill and got them taken to Brussels, got them installed over there for the World Fair and everything. He baby-sat them for a while over too.

VM: So can we take a break?

(*After a break)*

MT: Professor Calvin asked me: do I remember when the definite cycle occurred? The best way would be to go back into the literature; just check it, you know, with the literature. Or go back and look in some of the other biographical information that I sent you.

MC: Well, there's a biography...

MT: Vivian has all that.

MC: The autobiography that I wrote.

MT: He has all that.

VM: I have that and I don't have all the papers but I have references to all the papers and over the course of the next few months I'll actually go through all the stuff and remind myself of all of these things. I think the idea of how it became a cycle is a very interesting one; I realise it's difficult to cast your mind back over that period and work out how it happened but I...

MC: I have no idea; I can reason it but I can't visualise it. I don't remember.

VM: Do you have any sense, during the whole business, of whether there were many false starts, when you had to backtrack?

MC: Yes, always, always: lot's of them. I can't remember what they were, but there were lots of mistakes which had to be corrected — or didn't have to be corrected but had to be ignored.

VM: So you went down blind alleys and then realised after some period that this wasn't going to the right place and so...

MC: I guess that's the way it worked. You're asking me now something which is not there, not in my head.

VM: Well, I remember one or two blind alleys, rather later than that. You remember Helmut Metzner and methyl phosphate? Do you remember that one?

MC: I remember Metzner, faintly.

VM: You don't remember methyl phosphate?

MC: What about it?

VM: Well, he came up with this new spot. You must remember how exciting new spots always were! Our lives were governed by spots. And this was going to be the answer

to something or other, I don't remember now. It wasn't my project and I don't remember myself exactly what it is.

It turned out that the methyl phosphate (I think it was phosphate-labelled) came from the methanol that he had been using to kill the algae with but until that was realised, of course, it was very exciting. There was virtually a whole new theory developing on that!

MC: I can believe it.

VM: Well, I think that's what happened: that the excitement at that time was such that people were actually dreaming up things day by day. There were these ideas just floating around all over the place and people were talking to one another.

MC: Where did you work, in which building?

VM: I started working in ORL.

MC: You did.

VM: When I first came you suggested I work on deuterated algae and I worked with Ozzie Holm-Hansen to start with.

MC: Yes, I remember.

VM: You said that would be a good way for me to learn the ropes and it certainly was: a few weeks of that stuff with Ozzie and I knew how to do it. And then I worked on a whole series of other things including I did the last two papers in *The Path of Carbon*. You remember we found traces of the carboxylic acid. We did some experiments: we'd improved the chromatography and there were some new spots which looked as if it might be the dicarboxylic acid we were looking for but, of course, it fell to pieces very easily and we did some tests on it. I remember there was an organic chemist who came through the lab. at that time and I think his name was Angel. Does it ring any bells? From Australia, I think he was, quite an old man, elderly at any rate. Older, I think, than either of us was at the time. And I remember you talking to him and asking him what he thought of the breakdown patterns we were seeing and whether this was consistent with the dicarboxylic acid.

And then I also found the erythrose, erythrose phosphate, and I think that was the last paper until Andy did one much later, number 24 I think it was, which was ten years or twelve years after that. There was the cleaning up at the end but there was all the excitement of finding things — and the cycle was wrapped up but there were always bits round the edge which were not entirely clear and we spent a lot of time working on those. So I did that and then I did a number of other things. We began to develop the idea of how the whole thing fitted together in the cell, what the organisation was inside the cell and we began to take cells to pieces and see what the bits did.

MC: I don't know how you remember all that.

VM: I don't know *how* I remember it; it's just part of it.

MT: Well, part of it, Vivian, was that it was such an exciting period. In other words, when you're excited about something, personally or technically or whatever, you're going to remember it. And it was exciting, even for a non-chemist like myself, you could walk through there and feel how exciting it was.

VM: One of the most exciting episodes that I remember was a guy called Ian Morris, whom you may remember — do you?

MC: The name is familiar but I don't remember him.

VM: He came here in about 1966...

MC: Australian, wasn't he?

VM: No, English. He came in '66 as I remember, about ten years after I'd come for the first time. He came from the same department in London as I came and underwent exactly the same shock of seeing a place like this that I had. I had come from this stuffy English department into the excitement and ferment of the lab. here and Ian Morris did exactly the same. I just watched him: I watched him react the way I'd reacted ten years earlier.

MT: And he's now living in the States.

VM: Well, he unfortunately isn't with us any more.

MT: I did not know that.

VM: He became the director, I think it's called the Bigelow lab. in Maine, the Bigelow Marine lab. or some such name as that, and eventually became very fat, I think, and eventually he had a heart attack and it killed him.

MT: I remember about you first coming; I was expecting a woman! (*Laughter*)

VM: So many people were, I'm afraid.

MT: It was a shock when Vivian walked in! (*More laughter*) Looking back on it, and becoming more familiar with British names as I've gotten older, I'm not surprised but at the time... And there was no way from the piece of paper you can tell.

VM: It was a name that was fairly common just around the time when I was born and I have come across a number of people called that but it has really died out certainly as a boys' name since then but there are a few of us carrying the flag. I'm stuck with it — there's not much I can do any more.

SM: I don't think that you can possibly have any idea of how exciting it was for newcomers to your group. It was so dynamic. This is an experience that must have befallen many people who have worked for you over the years.

MC: I have no recollection.

SM: This comes from your personality and your way of doing things.

MC: I suppose; I don't think in those terms at all so when you say it I understand it, but that's all.

SM: But it's a great thing to have been able to do, to inspire people in this way.

MT: We're probably down to the third generation now, when you think about it. There are people who have come through, graduate students who have established there own little biodynamic groups or whatever they have wanted to call them. It's way of life that scientifically gets transferred around. Murray Goodman will tell you that. If you talk to Murray, he's very articulate on that subject. It's true. You learn how to do these things and as a scientific parent you transfer that to the next generation.

SM: I think it may have, literally as a scientific parent, gone as far as Kevin (*our son*) who is terribly excited about his work. He is a molecular geneticist and he is excited about his work in the way that Vivian has been and the way that you (*Calvin*) have been. I think it all rubs off from you, going down as it were. So this must be true for many other people.

VM: When you started the group, you really had little idea of what it would turn into, did you?

MC: None at all. As a matter of fact, there wasn't any group, it was me. Then we added people here and there and pretty soon we had a group. (*To Marilyn*:) Were you in ORL?

MT: No, I was always in Donner where the administrative offices were.

MC: I remember that, but all the work was in ORL.

MT: The synthetic work and the animal work, and things like Charlie Heidelberger, was done in Donner. Your heart was in ORL but you actually had another group of people doing the radioactive work...

MC: There was a lot of synthesis.

MT: ...synthesis and animal work. People would synthesise the compounds and people like Charlie Heidelberger would do animal studies with them, and Ed Bennett did animal studies and Gerard (*Milhaud*) did animal studies. They were over in the Donner; that's where the animals were. So, ORL was the heart, but you had to have a little bit

of assistance from Donner or you couldn't have gotten some of the things done that you got done in ORL like having the radioactive compounds

MC: I think you're right. I don't remember but you're right.

MT: I know because I was there.

VM: How did you come to get ORL?

MC: Ernest gave it to me.

VM: He was just able to do that?

MC: Oh yes!

VM: He was "king", was he?

MC: Oh yes!

VM: Did you go in before the (*37"*) cyclotron was taken out?

MC: There was a cyclotron next door (*the 60" cyclotron in the Crocker Laboratory*).

MT: The 37" cyclotron had gone, though.

MC: It had gone by that time?

MT: Yes.

MC: But the room was empty.

MT: That's right.

VM: So you moved into an empty building, essentially, and brought all your stuff or whatever in there.

MC: Well, gradually built it up.

VM: By the time I got there (*1956*) you were occupying the whole building essentially.

MC: I didn't ever more into the (*special room*) where the cyclotron was; there was a room there and I never really worked there. I could go in there, but I never really worked there. Most of the work was in the next door lab.

MT: You had a machine shop in there, too, you remember, which was very handy: a machine shop and a glass shop, which stayed in ORL until it was destroyed. We had the rest of the building, except for the shops, which was very fortunate situation.

VM: Were you aware during the early heyday of the group just how remarkable it was?

MC: No, I hadn't the slightest idea.

VM: Really. It didn't occur to you that not everybody operated in this manner? Let me tell you what was so exciting about the thing. First of all, there was a large group of people, all working in the same direction. That was unusual in biology at that time. I know it must have happened on the Manhattan Project and things like that but in biological research you didn't find that in the late forties and early fifties. Secondly, you were obviously the major contributor. There was a guy there who was the guiding stimulus. I hadn't come across this before. No doubt there had been other people in that position, but I hadn't seen it. The third thing was that there was a lot of money, not just money dropping around but what money buys: you wanted something, you got it. There wasn't any question: you cannot do this experiment because we cannot afford to buy equipment/supplies.

MC: Ernest gave me the money.

VM: I know but it was a very fortunate situation that it worked like that. Had you been stuck for money it would be have been much more difficult.

MC: I suppose.

VM: Then there was the idea, which was new for me at the time, that people were not reserved unto themselves. Everybody was interested in what everybody else was doing. They used to join in. You had collaborations, breaking and reforming.

MC: I had a seminar with everybody there; Friday mornings, eight o'clock.

MT: They all remember that!

VM: They were very scared of you.

MC: Really? Why?

VM: I'll tell you why. Because you would interrupt them...

MC: Oh yes, but that was my job.

MT: You patterned that after Gilbert Lewis — you had a good teacher.

VM: But some of the people were not used to it they would start, and you would get them in the first sentence...

MT: That was as far as they got.

VM: ...and some of the younger graduate students who hadn't seen much of this before were not ready for it. Some of them responded well enough but there were occasions when they were intimidated by this. It was a very vigorous discussion that went on.

MC: I can believe that because I was interested in what was going on and that was the only way to find out.

MT: Later on, in the Round House, when people got bigger and you had the formal senior staff and everything, the speaker would get like a week's notice. It first was a day, first it was 24 hours, then schedules were made up and you would get a week's notice and there would be a printed form used to tell exactly what speaker and what day. That was a late period in the evolution (*of the seminar format*).

VM: I don't know whether you were aware of them even at the time: there were some interesting hierarchical things that began to develop as time went on. In the beginning the only person who had a place at the seminar table was you. You always sat at that same right-hand corner. Everybody else, just sat around, whoever came in next. In the course of time it got formalised. The senior staff sat at the table and the junior people sat in the chairs around.

MT: The grad. students were in the back of the room!

VM: There was a lectern where you stood with your stuff: it all became high tech. with buttons to press for the slides.

MT: That was in the Round Building; that was once we got into the Calvin lab.

VM: In the beginning it wasn't like that. In the beginning, I don't even know whether there were slides; there were just blackboards and waving arms. And you brought the stuff and laid it out on the table for everyone to see.

MC: The chromatograms.

VM: That's right. It became much more formalised as time went on, inevitably, I think.

MT: (*Indecipherable*)

MC: You are bringing up things that have been buried for years.

VM: That's one of the things I wanted to do. I realised that you would not have thought about many of these things and I wanted to try and bring them up.

MC: Well, you're doing it.

MT: One of the things that is interesting is from Professor Calvin's 69th birthday. We had a big party over in the Round Building, with cake and everything, and Al and I had sent letters to people saying would you write a letter to Professor Calvin and tell him

what your experience was and so forth. Those letters were fantastic. They would be a source of information for you, Vivian.

MC: Where are they?

MT: Right here. If you read those letters, you will get the enthusiasm and how it changed people's scientific lives and what they remembered. Your going over to Laval's was in your letter, Vivian.

VM: Well: you can see how important that was!

MT: You should go back to Laval's; the beer is still good. That was an interesting thing because people recollected what affected them. It showed more of what the lab was like, not so much what the science was like, but what living in that lab was like.

VM: That's really what I want to do. The science has all been written; I don't want to write that all over again, that's old hat. But the lab., apart from these sorts of records, doesn't exist in a form which is accessible to anybody outside the circle.

MC: That's true.

VM: I think that's a pity, because it was a rather unusual place and it would be nice to...

MC: I suppose; if you can do it.

VM: If I can do it. Well all I can do to start with is to try to collect some information. How exactly to handle it I don't know. Sheila and I started talking about it and the first thing was that clearly we had different ideas of what might be done. I'm inclined to write it in an almost a story form.

MC: What's the matter with that?

VM: Well, nothing, if I can do it: I've never done anything like that before.

MC: Well, that all right.

VM: Yes, there's always a first time. I'll try; I think I'' try. But, there might be other things to write about as well. There might be actual lessons to be learned as well as a story. I don't know — this is the first morning and I have got a lot of tapes to fill in before I've got the whole story. And now there's all this collection of letters; we'll have to have a look at those.

MT: Well, you can borrow them. They are all Xerox copies of the letters. Professor Calvin has the originals of these letters in his house. But I Xeroxed them all and I have them in the office here.

VM: Perhaps we can have a look at them during this trip and see how they are. Are there hundreds and hundreds?

MT: Oh yes. We got a fantastic response.

MC: To what?

MT: Well, Al and I decided that since you were having a big party in the Round House to celebrate your 69th birthday. There was a huge cake over there in the Round House and Ed McMillan came and Don McLaughlin, people who were involved in creating the building, Bob Connick and everything. And we asked people to write letters to say what had being in the Calvin Lab meant to them, what was their experience. Some of them were funny; everybody had their own ideas. John (*Otvos*) wrote one. These all came in and before you (*Professor Calvin*) took them home (and I don't know where they are up there), I Xeroxed them all...

MC: Oh that's good.

MT: ... and I had them bound up. They're probably lost if they're up at the house; I don't know where they are.

MC: I have no idea.

MT: These are Xeroxes. Some of the letters came in a little late, so they're stuck in out of order, etc. It was a very interesting response, a very good response. People loved what they did here. They were excited about it. They were excited about being in Berkeley. Where else could you go to breathe tear gas when you walked to the bank among other things.

VM: That was post-photosynthesis.

MT: Yes, but it wasn't post-lab. There were a lot of people — this is not just about photosynthesis I'm talking about. There was a lot of excitement in Berkeley then — there still is!!

JO: I have a short addendum about your discussions about the seminars and the questions. Somebody told me this when I first arrived, just what you said about how people used to sit around the table, and occasionally when Professor Calvin was late, he would come dashing in and he would glance at the screen and before he hit the chair he was asking questions. (*Laughter*)

VM: You were a great question asker. Something else occurred to me just in the last few minutes. When did you know that ORL was going to be demolished?

MC: I don't really know.

VM: Presumably you had fair warning that something was going to happen.

MC: I think so.

MT: Dick Lemmon probably would be your best source for that. The ORL was destroyed in 1958 and they had to find space for us in the Life Sciences Building. It was probably at least one year. You see, the planning for Latimer had been done and we knew it was going to come here, so it was probably at least a year before because I do remember that Dr. Calvin and Ed McMillan stood out there and watched it go and I did, too, watch it come down. We watched the big ball hit it, demolish it.

VM: You can't remember, can you, what your thinking was at the time the building was going? What it would do to the operation?

MC: No.

MT: It spread it out even further.

MC: It stopped it.

VM: That must have been a matter of concern to you at the time.

MC: I suppose it was.

VM: The thing that occurs to me, just at this moment, is that the whole of that operation in ORL was actually very tied up with the nature of the building. The open plan of the building was something that was an essential aspect of the way everybody interacted. And you commented in your Norberg responses, somewhere, to this open plan aspect. Did you not learn something of this from your time in Manchester with Polanyi? Did he have an open plan lab. there?

MC: I don't recall.

SM: You have talked about the fact that the lab. in Polanyi's group, there were doors between the labs. and also doors opened onto the corridors.

MC: Yes.

SM: This meant that people could move in many directions and encounter each other.

MC: I can remember that.

VM: ORL had very few doors. It didn't have none: it had one on the outside, which is around somewhere?

MT: It's in the Smithsonian. (*Laughter*)

VM: Inside it had few doors and I remember Al had a glassed-in office but apart from that the place was pretty open. I think that encouraged people just to move around. There was very little sense of "my space" and "your space".

MC: Your desk was yours.

VM: Your desk, but not much more than that.

MT: I think the counting room was the only other room which was separate because you had to keep the counting room...

VM: That was underneath

MT: It was downstairs and you had to keep that separate.

VM: When the threat came to demolish that building there must have been some concern about what it's going to be like without it. I remember the basement down in LSB was a very different operation.

MC: We weren't there very long.

VM: Four years, five years.

MT: Five years between the time you moved down there and the time we walked into that round building.

MC: I hadn't realised that.

VM: Do you remember anything about the planning for the round building, the concept? Did you think it up, did someone else think it up?

MC: No, I did.

MT: And the senior staff, too. You had meetings with... and the campus architects...

MC: What was the name of that architect?

MT: Michael Goodman. First it was going to be like a half-circle and that didn't work out and then we had a full circle. It's all written out in some of the things I have given to Vivian already. It was Dr. Calvin's idea and the senior staff supplemented it. Paul Hayes was the one who bird-dogged it.

VM: You wanted to retain some of the aspects of ORL, some of the interactive aspects, and was it your concept, as far as you remember, to have this round building with people facing the middle?

MC: Yes.

VM: Do you think it worked?

MC: Yes it worked — it's still there.

VM: The building is there but did it work the way you hoped it would? You felt satisfied with the building the way it was put together?

MC: I didn't think about it. I think so but I didn't pay that much attention to it.

VM: Those of us in the building felt that the building worked well...

MC: It did.

VM: ...but it was different from ORL. It was a modern building with good facilities. ORL didn't have good facilities. It had its own charm, but good facilities wasn't one of them. It wasn't a well designed building.

MC: It wasn't designed.

VM: Somebody must have put it together.

MT; It was built in 1885 or something and it just grew.

VM: Was it as old as that?

MT: Yes. It was a "temporary" building.

VM: Are those temporary buildings still down (*in the centre of campus*)?

MT: If you walk down through the grove they've all been knocked down. It's beautiful grass and flowers and everything; it's just lovely. In fact, the class that I graduated with at Berkeley is giving a memorial glade for people who died in the war down through that area. It is really, really nice. It's worth a walk.

JO: There's one more thing about the building (*the Round House*) being designed and thought up by Dr. Calvin. The evidence for that is that very shortly after our group (*i.e. Calvin's Latimer Hall group*) moved out of the Round House, in 1980 I guess it was, all sorts of conversation barriers were put up on the second floor and now you can't see from one end of the floor to the other end any more. So that area of congeniality disappeared very soon after he left the building.

VM: Got an explanation?

JO: The person in charge of the building (*Professor George Pimentel*) didn't have these same ideas and it went on according to somebody else's concepts.

MT: But the big white table came over to the Chemistry Department for the graduate student lounge on the fourth floor.

VM: I must go an have a look at it.

MT: Is that the round table?

MT: Yes.

VM: The round table or the square table?

MT: The round. The square white table was up on the third floor, the rectangular table actually; it's still up on the third floor in the round building. But if you want to call it the (*round*) coffee club table was on the second floor, the big white Formica top table which, after Professor Calvin moved over to here (*Latimer Hall*) and it didn't seem it would be useful over there, the graduate students over here (*in Chemistry*) it's in their lounge now.

MC: Where's that now?

MT: Fourth floor, 427 or something; it's in Latimer.

VM: What I was less familiar with myself at the time, and so I ask you: What was the atmosphere like in Donner compared with the atmosphere in ORL?

MC: There wasn't any atmosphere there.

VM: Why do you think that was the case?

MC: Well, there wasn't anybody living there.

MT: Don't say that!! I was there for 13 years, and Bert lived there and Dick lived there, and Ed Bennett lived there. It had no atmosphere, I have to agree with that. But part of the thing was that you didn't really care what went on in Donner, so you very seldom came over there. And so, you were always in ORL. If we wanted to see you, people went to ORL. You did not come into Donner.

MC: I didn't come very often.

MT: You rarely came and that was because it (*the Donner Laboratory*) was not your building, like ORL was really your building. John Lawrence controlled the rest of the building and also it had the same kind of architecture as Latimer — you are in little offices with doors opening. It's very difficult to mingle when you have to go from room to room and open the door. There was no real way to have a congenial group.

MC: Yes, but in ORL that wasn't the case.

MT: Donner was a typical lab building, like they are still building all over the campus — corridors, buildings, doors. ORL was a real opportunity which you were able to translate into that other building.

MT: Into the Round House.

MT: Into the Round House. There will never be another building like that.

MC: Why not?

MT: Because first of all it is too small and the university can't afford to put little buildings up any more. Secondly, people don't think that way. Very few people think the way you think. Donner had no atmosphere at all; it was just like Latimer.

VM: Marilyn, of course, is right. I think that the Round House is an expression of the unitary character of the group. It was built for a group, it was custom-built.

MC: Well I designed it.

VM: I know but what Marilyn is saying is that sort of opportunity doesn't exist any more to custom-build a building for a group, or at least it's very rare these days. It was rare then, I suppose.

MC: I expect you're right.

MT: It was pretty rare then.

VM: It was just very fortunate that you were in the right position at the right time to do it.

MC: I don't remember where the money came from. Some of it came from Kettering, didn't it?

MT: Not all of it. The State of California came through, Kettering came through with the last $300,000, the National Institutes of Health and the National Science Foundation built us the building; it was equipped by the AEC. But the actual funds for the building were about four different sources.

MC: I had to go find them

MT: You had to find them. You had to write a lot of proposals — no electric typewriters, no Xerox machines, no computers. I have copies of all those proposals.

VM: All in that office out there?

MT: Yes, it's a gold mine.

VM: Does the Smithsonian know about your office?

MT: No, and the Bancroft (*Library*) doesn't either. One of these days I guess they'll know but I did not send that stuff down there. It's just incredible: from about 1958 to 1960 you were writing proposals for the building, at the time that Glenn Seaborg was chancellor.

MC: For the Round House?

MT: For whatever building you were going to get; it turned out to be round. You had to get the money first. To get an architect you had to have money on hand So, we had to have some money in hand before you could even talk to someone like Michael Goodman to decide what kind of building you wanted. So all these different proposals were sent to different people and the Kettering people supplied the last little bit.

VM: I remember the day that you stuck the spade in and you turned the first sod. It was an wet day, wasn't it, as I remember?

MT: Overcast — it wasn't a very good day.

VM: And the ground was a bit soggy, muddy when you did it, but it was an exciting day, nevertheless, when it happened. Do you remember the day when we marched in...

MT: I remember that.

VM: ...or the day when (*Arne*) Tiselius, was it?, who opened the building formally?

MT: The dedication was very nice — a special seminar and the whole thing. But I was remembering last week, and I think I told John about it, that his (*Calvin's*) first meeting in his office (*in the Round House*) with all the furniture and everything was on November 22nd, 1963. He was talking with Howard Cary, the Cary spectrophotometer man, at the time when (*President*)Kennedy was shot. I went and I told them and I opened the door and said that "President Kennedy has been shot". I think you (*Calvin*) didn't believe me. But I had my radio on, and in an hour or so we all went home. That was your first formal meeting in your office in the Round House.

VM: You hadn't been in the building very long.

MT: No, I think I moved in two days before. Everything was still in packing boxes everywhere.

VM: I wasn't her (*in Berkeley*) at the time. I was in Israel on a month's project with the International Atomic Energy Commission. So, I'd marked all my stuff for shipment up from LSB (*Life Sciences Building*) to where it ought to go in the building and then I took off. I think I wasn't here when you actually moved into the building and started working

MT: It was pretty much about a month that people were moving in..

MC: Which building are you talking about?

MT: The Round House?

MC: It's been so long since I've been there that I have even forgotten it. I wasn't in there very long.

MT: '63-'80 — seventeen years.

MC: Really?

VM: Time flies, eh? I'm wondering whether perhaps we might not close for today and let me and Sheila talk it over and see what we'll think about for tomorrow. We have come through a lot of stuff quicker than we might have done. I am happy to go on chatting, but I can't think of more things which are relevant. I think it might be helpful if we thought it over. Do you?

SM: Whatever you want.

VM: I have actually run out of the immediate things I can say to prompt you (*Calvin*). I could go on talking but it's not going to be particularly relevant.

SM: Perhaps Marilyn can think of some things.

MT: One of the things you might want to think about in the long run, and you might have a separate section, is to describe how this (*the Calvin group*) was the first interdisciplinary lab. in the entire world, I believe.

VM: ORL or the Round House?

MT: ORL. The group; I don't think you can attach it to a building. I am talking about the laboratory in the sense of doing research. If you think of all the things that went on went on — chemical evolution, botany, psychology, planarias, educated mice — if you think of that concept and look at the things that we did, the things that the senior staff and the visitors did, everybody now talks about interdisciplinary labs., it trips off the tongue very easily, everybody has one, they think, but we were the first. I think that's a real interesting thing. I think as you talk to the people you are interviewing with this in mind you're going to get a lot of responses that might be part of the path of carbon in one sense but they are also part of the creation of a laboratory and an environment that was very, very special and unique.

VM: And it really started in those early days.

MT: In those early days. It would be interesting to speculate whether during the entire time of the group if everybody had been together, how it would have been. But (*until* 1963) we were always separate and there was the group over in Donner, there was a group in ORL, there was a group way down in the basement of Chemistry. We were all part of the same thing. But, we were also different.

VM: That's very good. It's down on tape and its accredited to you.

MT: I don't care about that; I think it's an interesting story.

VM: It gives me something actually which I hadn't thought of, an ultimate focus of where the story leads to, is the creation of this interdisciplinary group and indeed an interdisciplinary building to go with it.

MT: It was almost by accident, not accident because Professor Calvin was interested in all these things (*i.e. areas of science*). It took not only his ideas but it took receptive people who would go for this. A lot of people only want to stay in their little box. So he had people around him, or he picked people, who would do that.

JO: Some people who have tried to copy this arrangement haven't been so successful. And I have always thought that the reason is that what they have put together is a multidisciplinary laboratory rather than an interdisciplinary lab. and then they kick the habit.

VM: Those two comments from both of you, I think, are very helpful in providing a very good possible focus for where the thing is going to head. I say "the thing", whatever the thing is by the time I get to it.

MT: If you ask people, particularly people like Alex Wilson, Alice and Al (*Bassham*) and Dick (*Lemmon*) and Bert (*Tolbert*) and Ed (*Bennett*), but Ed wasn't involved, they could give you the nuts and bolts of how the lab. worked. But you ask Alex Wilson and some of the others that who have been gone a long time how unique this atmosphere was and what effect it had on their lives and so forth, I think you'll find interesting answers.

VM: Thank you — very helpful. So shall we give you a rest at this point?

MC: I think that would be helpful.

VM: OK — I'll turn it off...

continued November 30th, 1995

VM: This is an experiment. We haven't altogether decided what this is going to turn out to be, but we were thinking of two possible outcomes. One of them is to write almost a story account of life in the lab., but it depends on how much information we get and what sort looks like, and the other one is try and do something more academic with it. That's one of the things I'd explore with you this morning. Essentially, how this very wide interdisciplinary group grew out of a relatively small beginning, because it started with you and it has fanned out into a group of a hundred people, with lots of through-put. That's something which would be interesting to explore. Maybe we could do that in a formal sort of way, depending on what the information looks like. But it depends what we collect, you know. It's not like a scientific experiment where you know the form of the outcome. We really don't. It's quite new for us.

Perhaps we can start with that, and later on we can talk about some of the comments people made in these letters (*letter collection of 1969 for Calvin's 68th birthday*) and see how you remember them.

MC: I don't remember them

VM: Well, we'll remind you. I just had a quick look at the letters this morning and there are some quite interesting ones that people have sent.

MC: What is that book?

VM: This book is letters sent to you on the occasion of your 68th birthday; that was a party. You said yesterday that you were always having parties; well this is one of the bigger parties. What I would like to explore with you is the way in which from the beginning of your work before the photosynthesis started, you gradually expanded out into so many different sorts of areas.

MC: I suppose; I don't remember.

VM: Do you remember how you started, on the pigments and the phthalocyanines?

MC: Yes, I guess so. Go ahead.

VM: You went from there at the end of the war, you had the availability of C^{14} and somebody told me — maybe you can remember this — that one of the sources was a lot of ammonium nitrate slurry which had been sitting around the reactors for a long time. That's all I know. Can you remember any more about that stuff?

MC: No. Just to get it and you extract the carbon.

VM: What was the ammonium nitrate slurry doing around the reactors? Who put it there? Did you put it there, or was it there for some other reason?

MC: I don't know. I really don't know.

VM: You had access to it?

MC: Yes.

VM: And you did the chemical extraction of the carbon from it? What was it, ammonium carbonate by that stage?

MC: I don't remember. I suppose so.

VM: You had this C^{14} and you started...

(*Short break)*

JO: I was under the impression that the ammonium nitrate was put there for this purpose, to see if this transformation took place with the neutrons.

VM: Do you know who put it there?

JO: No.

MT: Could it be Joe Hamilton, who ran the Crocker Lab. and who was a colleague of Ernest Lawrence? The Crocker was it's own environment. It may be in the book *("Isotopic Carbon".)*

VM: So you had this stuff and you then began the Donner (*Lab*.) development of the isotopic carbon work and the parallel work in photosynthesis, and you saw those very much, did you, as a unitary operation.

MC: What do you mean?

VM: You could see that the two activities were supporting and interacting with one another?

MC: Oh yes, that was me.

VM: That was you. But you didn't stop there. You began to acquire other interests beyond that. I remember chemical evolution which something that was going by the mid-fifties. Had you had a long-term interest in that question?

MC: Not really.

VM: How did you come to that one?

MC: I don't remember. I have no recollection of that. She's (*i.e. Marilyn Taylor*) got something.

MT: When you had your heart attack (*in 1949*) and you were in Kaiser Hospital. You read a book by George Gaylord Simpson on "The Meaning of Evolution" and that sort of twigged your interest in this kind of thing. And then, next door to ORL was the 60 inch cyclotron, so right across the alley was a place where you could go and check some of the ideas that you had. And you followed Oparin's work of the twenties as a general interest. You decided, with Andy (*Benson*) and Joe Hamilton and a couple of other people in Crocker, to do whatever it was you did. (Not being a chemist, I don't remember.) You did something in the cyclotron, a similar experiment to Stanley Miller's, but you didn't get amino acids. You didn't take that next step to get amino acids so that's why everybody thinks Stanley Miller started chemical evolution. Really you started it, but you never did the second experiment to get to the amino acids. You got carbon dioxide, formaldehyde and that kind of thing.

MC: I've no recollection of that.

MT: Andy worked on that, Joe Hamilton and Don Moore(?). This was a preliminary experiment, but you didn't really follow through experimentally for quite a while, until Dick Lemmon got interested in the project and then there was a long period of

time when those kinds of experiments were done, with Dick and Cyril (*Ponnamperuma*).

MC: I remember the lab. The shakers were in the corner, in ORL, and that's where our algae were. And we kept them going all the time, harvesting new ones all the time. But that's all I can remember of all of that.

VM: Do you remember going to Russia to meet Oparin?

MC: No.

VM: Because I think you did that in about '57, was it?

MT: '57.

MC: It could easily be. But it didn't impress me very much.

MT: That was the first conference (*in 1957 in Moscow*) on the study of origin of life (I think we're now up to thirteen or something), but that was the first time that all you people like Sid Fox, Oparin, Stanley Miller and yourself and Oparin and all the other people in the world that were working on this (*i.e. chemical evolution*) got together. It was very special because it was in the Soviet Union. That was a difficult place to go in 1957, lots of paperwork, the Russians were very hospitable and so forth. When you came back you wrote a piece for Chemical Engineering News called "Diary of a Meeting in Moscow"...

MC: I did? Well it's in there someplace.

MT: Yes. ...it told about the whole trip, the institutes you visited the whole discussion on the origin of life.

MC: Well, that's in there somewhere.

MT: It's in your reprints.

VM: Do you have any memory of what followed and the people involved? I remember there was Cyril Ponnamperuma — you remember him?

MC: Yes.

VM: He was quite a guy, wasn't he? Do you remember where he came from, how he came here? I've know him...

MC: She knows.

MT: He was a graduate student (*from Ceylon*).

VM: I knew him in London, before he came here (*to Berkeley*).

MT: He came here as a graduate student. He was an older graduate student because he'd worked before. He got into the Donner and he hooked up with Dick Lemmon and worked on this chemical evolution thing and that was his whole life, professionally and everything. And afterwards he kept going on that until the day he died (*December 1994*: *his laboratory at the University of Maryland was called "Laboratory of Chemical Evolution". He served for many years as a special adviser on scientific affairs to the Sri Lanka government*).

VM: There were some other people. I can only remember some of them. One of them was Geoff Eglinton; do you remember him? He worked in that area as well.

MT: I don't know whether he did or not, but I remember Geoff.

VM: I saw him recently. He is alive and kicking.

MC: Where is he?

VM: He's in Bristol, retired, but still working. He was a Professor of Geochemistry, I guess. Who else was there?

MT: Geoff actually worked on the organic geochemistry project (*and the analysis of lunar samples; this work was funded by NASA*). (*In the chemical evolution area*) Christof Palm work with Dick Lemmon. There was Leonard Spicer — I'll have to go back into the literature. There were quite a few people who worked on it: Anneliese Schimpl. If you look at the list of reprints on that subject you can pick up the various names. And there was another different group that worked on the organic geochemistry, on the Apollo program, through the Space Sciences Lab.: Al Burlingame, (*William Van Hoeven, Jerry Han*) people like that.

VM: The next thing I can remember (the chemical evolution work was in the fifties) was in the sixties you began to become interested in genetic control. I remember you asking me a question which you thought I ought to know (and I should have done, but I didn't) about how bacteria control enzyme synthesis. I remember saying to you at the time, this was one of these conversations in the lab., and I said I would look it up and tell you. And about three weeks later I admitted I couldn't find it and I would do it. And that set me on ten years work, doing it myself, so that was another thing got that added to the lab. But it really started because you asked that question. Had you not asked that question my life would have been quite different. You remember that?

MC: No.

VM: We published some papers on that in the sixties.

MC: I can believe it.

VM: And then what happened next? Then there was the planaria: do you remember the planaria?

MC: Yes.

VM: What turned you on to planaria?

MC: They were simple organisms, animals, as opposed to plants. That's all I can tell you.

VM: You don't remember what you were hoping to get out of them?

MC: No.

MT: Ed (*Bennett*) could tell you that.

VM: There was a guy who came to do it. There was a lot of trouble. Do you remember the story how the planaria were learning, you could teach them, and then you chopped them in half and each bit remembered? Do you remember that?

MC: No, but that's interesting.

MT: There was a sister-brother or man-wife or something (I can't remember their names).

VM: There was a guy who came...

MT: ...from Indiana somewhere?

VM: ...from Duke? I can't remember what his name was (*McConnell*) but he came to work on this stuff here because you couldn't repeat their results, and when they came they couldn't reproduce it either. There was a guy: ginger-haired, losing his hair — Alan somebody or other (*Jacobson?*).

MC: I would have to look at the list. You know they published all this stuff in "The Worm Runners' Digest".

VM: Do you remember "The Worm Runners' Digest"? Those were great times with these very excited conversations about whether they would or whether they wouldn't.

MT: Jan Alvarez worked on that, too, Luis Alvarez's wife.

VM: In here? In the lab.?

MT: In Donner and also in the round building. Ed (*Bennett*) is the person who knows all about planaria.

VM: The next thing that I remember, this is all fitted together in the sense that you were sitting in the centre co-ordinating everything, and, secondly, the people involved were interacting with one another, because it was that type of building (*now referring to the, not ORL*).

MC: Was that in ORL?

VM: That was in the Round House.

MT: It started in Donner and then came to the round building; the planaria work.

MC: Never in ORL?

MT: No — ORL was gone by that time.

VM: The last thing that I remember you getting involved with, but by that time I was on the verge of leaving and there may have been things later, was the cancer work, which was the late sixties and early seventies. I remember that you went to a cancer conference, I don't know why you decided to go to this cancer conference, in '69 or '70, and you came back very excited, full of ideas about what you wanted to do. And that, I think, started it.

MT: Then people like Ercole Cavalieri came on a Damon Runyon Fellowship and he had to work on cancer. He was an organic chemist and so they got into the whole business of photochemistry. I think that was pointed out in the Norberg volume. And then you had grad. students like Joe Landolph, and Dave Warshawsky. There was a whole group of ten or fifteen people who came to the lab. to work on various aspects of the cancer problem.

MC: I didn't remember that.

MT: It all started from that meeting and also from the fact that when Ercole came he had to work on something to do with cancer; there was no way he could use that money without working on cancer.

VM: Then I really lost track because I was only an occasional visitor after that. Was there anything major that developed after the cancer initiative?

MT: Part of the cancer project was when they built the Cell Culture Lab. on the roof (*of the Round House*) and employed two new people for the senior staff — Jim (*Bartholomew*) and Mina (*Bissell*) — that activity was quite central for some years, until they all moved up on The Hill (*with its own building*). They were on the roof of the Round House for some years, ten years maybe.

VM: There weren't any other radical departures into new areas after the cancer, were there?

MT: I don't think so. Dick Lemmon always working in the area of hot atom chemistry and isotope effects; there was the work done by Mel Klein and Ken Sauer and Al was working on his metabolic metabolism studies, Al Bassham.

VM: I can understand, you know, how you got involved in the chemical things, because you are a chemist, but NMR: wasn't that a bit unfamiliar to you?

MC: Well, I learned it.

VM: Was it difficult?

MC: No.

VM: Really? Because that was a major activity. Wasn't Power Sogo the first guy who came into the lab to do that kind of work?

MC: Yes.

VM: I have no idea how you got into that. Do you remember how you became involved?

MC: It was a matter of structure determination. That's all I can tell you.

VM: It was a developing technology which was coming into chemistry and you recognised the value of it?

MC: I guess so.

VM: And there were other things: there was ESR, I remember at the time as well. Did that develop very much? I don't know what happened to that.

MC: No.

VM: NMR turned out to be the more powerful technique.

MC: Well it did, yes.

VM: What else was there at the time? There were other sorts of novel pieces of equipment around, but I am getting out of the depth of my memory.

MT: Some of the older equipment was automated, the chromatography equipment for example, and the algae culture became much more automated than it was originally. When we moved into the building (*i.e. the Round House*) all the equipment was updated so there were much more modern facilities.

VM: That activity of Dick Lemmon's, with hot atom chemistry, you remember he used to fire carbon ions into compounds and got interaction between them.

MC: I don't remember.

VM: You don't know how that came about, whether that one of your ideas or whether it was separate.

MC: I have no idea.

MT: Wasn't it kind of problem with the degradation of radioactive choline chloride? It started over in Donner with Dick, and when these compounds, which were made in Donner, were sent away, there were problems with degradation and there was some kind of isotope effect.

VM: Choline chloride, I remember, was one of these very radiosensitive compounds and that became an object of study as to why it should be radiosensitive. That was part of the whole isotope philosophy of looking at these things.

MT: When we got into the new building had a whole room with nothing but hot atom equipment— you remember that great big thing that Dick (*Lemmon*) and Wally (*Erwin*) and Irv Whittamore and Ben Gordon and Wally Erwin worked on. That's later.

MC: Over there?

MT: Yes — the Round House. They'd wanted that equipment before, but there had been no place to put it in Donner so when the building was built, that was part of the building and it went in there to continue that research. It was there for ever until it got taken out about five years ago.

VM: One of the things that which was unusual, to say the least, about that building (*ORL*) and the way the whole group developed in later years, was that it seemed perfectly natural to you and to the people in it that these (*interdisciplinary*) activities arose within the context of the whole envelope of the group. But I think for anybody looking from outside, this would be a totally unusual phenomenon. Each of these activities would have merited an group/institution of its own. And yet here they all were, welded together under one thing, including the name of the building. Remember the name that you gave to the Round House, to the laboratory?

MC: LCB.

VM: But do you know what the "CB" stands for?

MT: Laboratory of Chemical Biodynamics.

VM: You had dreamed up that name. I don't think that anyone had heard of a name like that before. It was a good way, in only three words, of describing a very wide thing. In fact I recall once calling someone and talking to them and giving them my address as the Laboratory of Chemical Biodynamics and he said, "what, no astronomy?"

JO: You have come up to a point where we haven't said anything yet about artificial photosynthesis.

VM: Oh yes, I'm sorry — go ahead.

JO: That started somehow when Helmut Tributsch came here and a paper was published about photosensitised photocurrents in semiconductors and their solutions. That grew

and that's when I started, in 1976. Tributsch's experiment was done in 1970 and things were going along rather slowly, and I was told that I was brought here partly just to co-ordinate that particular subject and that's just ending now. We have had 25 years of that.

VM: Are you there?

JO: No. We have lots of interesting things to follow up..

VM: So it's good science.

JO: Yes.

VM: That reminds me that you were also involved comparatively recently with diesel trees.

MC: Yes, there are pictures of them there.

VM: I heard stores when I came through —

MC: That's one back there.

VM: Is that a diesel tree?

MT: (*Indecipherable*.)

VM: What happened? I gather the stuff worked all right.

MC: You got latex out of it, but that's all. I didn't pursue it beyond that.

VM: Could you use it in diesel engines?

MC: No. It had to be purified.

VM: Expensive?

MC: No. But there wasn't enough of it to make a significant impact.

MT: But the purification was expensive.

VM: I guess there's just too much oil around at the present time.

MT: Too much cheap oil. It got to the point with this research, where Dr. Calvin was involved and they were doing field studies and harvesting the stuff there were plantations of the hydrocarbon-producing plants which were being harvested and (*indecipherable*) and they got out a material from these plants which could have been used as a chemical feedstock. But the price of oil tumbled again and there was no

economic justification. All that knowledge that was built up by a lot people over a 10-15 year period just disappeared.

VM: Well, it hasn't disappeared; it's there. It'll wake up again some time.

MT: It will wake up again sometime. They had the California Energy Commission interested, people all over the world (*India, Japan, African countries, Spain, etc.)* wrote and asked about it. Developing countries: there's not a problem with the cost the labour was so cheap.

VM: You see relics of these initiatives. When we drove in the other day there were all the wind farms (*out near Livermore*); these propellers.. There are a bit redundant these days. They're not very pretty anyway; they are ecologically not terribly desirable. And I believe they have turned out to be much too expensive.

MT: PG&E is still forced to buy power from them. That's one of the reason our power is 50% higher than the state of Oregon. The windfarms were set up here and in Northern and Southern California and the utility companies have to purchase the power. When they (*the windmills*) went in, everyone said "we need power, we need power", so the government agreements were set. I was reading about this in the *Wall Street Journal* the other day and the discussion in California about the deregulation of electricity costs, so they (*utility companies*) are forced to buy the power (*from the windfarms*) even if they don't really need and of course that affects the price which is not (*indecipherable*) in the market. We could have another oil embargo and the power (*from the windfarms*) would look pretty good again.

VM: Another thing that we might talk about. In the sixties you joined Kennedy's Presidential Science Advisory Committee (*PSAC*). You must have been in a very interesting position on that committee because of the wide range of activities back here in Berkeley.

MC: I don't know. I suppose so.

VM: Did you feel that you were able to draw on many types of experience?

MC: I never thought about it that way. I just did what I did.

VM: I presume that the Committee didn't discuss the details of photosynthesis very much, broader issues than that.

MC: Yes, that's right.

VM: You by then had a fair experience of broad issues in this range of things. Your wartime activities as well, on the chelates. Are you in a position at this stage to almost philosophise about what it all adds up to? Do you want to hazard a guess?

MC: No — it didn't get very far.

VM: This is part of the exercise I'm doing now, it's such an interesting phenomenon that I think one must make sure that...

MC: ...it doesn't get lost.

VM: ... doesn't get lost. As the initiator of it...

MC: That's just the way I worked.

VM: So you had little idea at the beginning that it was going to develop in the way it did?

MC: No. I didn't think about things like that.

VM: You just went ahead and did it. But you thought, I'm sure you didn't think just of today and tomorrow. You must have been thinking of what's going to happen next week and next month.

MC: Not very much. I was more concerned about the algae and the shaker, would they live and would they produce what I wanted them to do, and so on, than I did about the long-term outcome of it.

VM: You mentioned yesterday that when you started the photosynthesis thing you really had no concept of how long it would take to solve the problem.

MC: No, of course not.

VM: You thought — once possibility was that it might be pretty quick.

MC: I didn't think about it that way. I never thought about time, how long it would take or anything like that. I don't recall ever thinking about things like that.

VM: After a little while it must have been pretty clear that it was not going to be a very easy problem, because as the problems developed and puzzles showed up and you were beginning to collect a lot of people to do the work. So that's an indication that it wasn't going to be over quickly, at that stage. I guess by the time, by about the end of the forties or so (*to Marilyn Taylor* — and you were already here by the end of the forties), the group was already fairly big.

MT: Probably 35-40 people.

MC: Really?

MT: There were two groups.

VM: It must have been one of the largest academic science groups here except, maybe, for The Hill itself. Did other people have groups of that size then?

MT: All the professors in Chemistry had their own groups, but they weren't this big.

VM: Even then, after just a few years, yours must have been one of the largest groups on the campus.

MC: I never thought of it that way; maybe it was.

MT: The other big group in Chemistry was up on The Hill with Glenn Seaborg. That group, of course, was never on the campus; it was always up on The Hill.

VM: Were there pressures to move your group up to The Hill?

MC: Not that I can remember. If there were, I didn't do anything about it.

MT: At the time when it became obvious that ORL was gone and you had to have a new building, you were offered space up on The Hill, somewhat near where Mina (*Bissel*) now is.

MC: Yes, but I wouldn't take it.

MT: You said you would not take it because you wanted to be on the campus. But the AEC at that time would have been perfectly willing to put building for you up there.

MC: But they put it up here.

MT: The AEC did not put any money...

MC: No, Kettering did that.

MT: I know but I'm saying the AEC would have given you the money for a building on The Hill somewhere near where Mina is. That was an option you didn't choose to exercise. Therefore, you had to go and get the money for the building.

MC: That's when I went to Kettering.

MT: No, Kettering was the end, the last $300,000. The State of California, the National Science Foundation and the National Institutes of Health and then Kettering. Kettering was the last little bit.

MC: That was the last $300,00.

MT: Yes, that was the last little shot. You had gotten all this other money before and then you were able to get money from Kettering.

VM: Why didn't you want to go up on The Hill?

MC: It was away from the campus.

VM: That was an important factor for you?

MC: It was to me, yes.

MT: you wouldn't have been able to interact with the Psychology Department, the Anatomy Department, the Botany Department, the Heaven knows what else you would have worked with near as easily on The Hill. Even though The Hill is most wonderful, it's isolated from the campus. I think being on the campus has a certain advantage scientifically for the interaction you are talking about.

VM: So your own ability to migrate easily from your office in Chemistry to your office in the round building was partly...

MC: Mostly it was from my office in (*Old*) Chemistry to ORL, the old wooden building next door where this building (*Latimer Hall*) is now.

VM: But that was destroyed and something had to happen.

MC: Then I went to the Round House.

VM: You went to LSB (*Life Science Building*)...

MT: That was five years later. *Then* you had to go between your office in Old Chemistry and the Life Sciences Building.

MC: Oh yes, I remember that — that was a terrible thing.

MT: You had that (*electric*) cart; that car was always where you weren't.

VM: That really must have been a severe jolt to your scientific lifestyle.

MC: Well, it was but it didn't last very long.

MT: Five years is a long time.

VM: I think that had a serious effect on the relationship between the two groups of people...

MT: It was very serious effect on the relationship between the group in Donner, up here, at this end of the campus, and then the people in LSB. It was very difficult to have communication: it was a long way up and down the campus and between the Old Chemistry Building: it was like a triangle.

VM: And everybody was getting a bit older and that hill was getting steeper every year. That's why you had a cart.

MT: The idea of the cart was wonderful. It was very practical, but he (*Calvin*) would drive the cart down to LSB...

VM: Oh, he drove the cart himself...

MT: Yes, he drove the cart down. And then his wife would call and she would pick him up in LSB and then the cart was at LSB. The next day, you would be up in your Old Chemistry office, and where's the cart? It was down there where you left it. There was one of the graduate students and one of the things he volunteered to do was to keep the cart in the proper place; it was John Eastman.

VM: Do you remember this car?

MC: No, not really.

MT: But it was so funny: John Eastman, when we had that thing (*i.e. symposium*) for Dr. Calvin in 1989. his little a speech (I don't know whether John remembers that or not) was about his responsibility for the cart. He was the cart man.

VM: I remember you in the cart, actually going up and down the hill. But it was a real pain having to commute .

MC: It didn't last very long.

MT: Five years!!! That's a long time. I remember it vividly. I had to walk from the Donner Office to the Life Sciences Office to your office (*in Old Chemistry*). I spent a lot of time walking. It was good for me, but it wasn't easy to keep up any kind of communication or relationship between the two groups under the circumstances.

MC: I hear you: I hadn't realised that.

JO: When was his heart attack, in sequence of all these things of moving...?

MT: In 1949, very early in my career at the lab.

MC: It only laid me out for a few days.

MT: I bow to differ with that. You were home for a long time...

MC: ...how long?

MT: At least two-three months. I drove up to your house every day, bringing the mail, taking dictation, for a long time.

MC: Really! What do you mean by a long time?

MT: At least two to three months. You would have people come up to the house, come up to see you. You would call up Al and tell him you wanted to see him about this and that and he would go up to the house and you would call whatever and there was a constant stream of people up there. But I was up in the house almost every day for quite a long time.

MC: I believe you but I just don't recall.

MT: Not only did I take dictation, I made coffee!!

VM: Down in LSB the accommodation was much less open, of course, than it had been in ORL and it was later in the Round House, but it wasn't totally enclosed. There were a couple of big labs, there, I remember, one of them roughly in the middle of the long corridor and then there were individual rooms — you were right: they were not very nice down there. In the big lab in LSB it was possible to recreate something of the atmosphere of (*ORL*).

MT: It was actually. I have a picture of Martha (*Kirk*) cutting hair in the big lab and that sort of thing. The parties around the table — there was that one big lab. where a lot of work went on and there were individual rooms...

VM: I think it was a great relief when we came up to this round building. And I remember the discussions that went out about what sort of building one ought to have and at that time there was overt discussion about the philosophy behind the group.

MC: Who built it, what was his name?

MT: Michael Goodman.

MC: He did what I asked him to do.

MT: Well, after discussion.

VM: I remember on many occasions, and I think Al must have been one of the prime people and Dick...

MT: And Paul Hayes, of course.

VM: ...discussing, since it was a clean sheet building, you know, at the beginning there was a possibility of suggesting anything, one didn't know how far it would get, but at least *suggesting* anything. And trying to recreate in the new building what people felt had been the best, particularly of ORL, I don't think anyone wanted to create what LSB was like, but recreating ORL in a modern building. The philosophy of putting all the individual work space in a big lab., in one half of the building, and all the service facilities which had to be in rooms because of noise, etc. in another part was excellent.

MC: That was the way it was in ORL.

VM: Yes, but it required an analytical understanding of ORL in order to be able to design the new building and that's what came out of it. That building (*the Round House*) was almost perfect: one or two mistakes, we made.

MT: ORL?

VM: No, the Round House. I remember that we forgot to put floor drains in the freezer room but apart from a few minor things like that it really worked very well indeed.

MC: It's still working.

VM: It's still working. There was a great fear at the beginning that it would be very noisy because everybody would be in the same lab. But it turned out not to be. The rooms were big enough so that the noise didn't carry and people just weren't that noisy so it was OK.

MC: The big labs were half-labs. (*half of the building*). The benches were on radii; I guess they still are. There was a sort of centre (*on the second floor*), where the blackboards were, and that's all there was to it.

MT: And then the staff offices all the way around the edge. And the rooms which had to be for themselves, like the isotope lab. and that kind of thing, had their own spot.

MC: Well, it's still that way, isn't it?

MT: The uses of some of the rooms have certainly. If you go through there now, if you look at rooms that used to have the isotope lab., where you did radioactive synthesis, that's used for something else. The function for some of these rooms has changed as the research has changed.

JO: I thing also one of the reasons that the sound problem didn't develop was because there are no two walls in that building that are parallel. There is no way that a sound wave can hit a wall twice and it just gets disbursed.

MT: Even the offices.

VM: I'm sure that's a factor. But also internally the rooms, soundwise, are very broken up with pieces of equipment so that you wouldn't get flat reflecting walls. But you said the idea was to have the benches on the radii and then an inner radius would have the common equipment that people were using, and then you get to the discussion centre in the middle. It worked pretty well.

MC: That was my design, with Mr. Goodman.

VM: I think, as I recall, they insisted on putting a red tile roof on it so that airline pilots flying over could make sure where they were.

MT: If you look around the University almost every building, including the new one right here next to us (*in Latimer Hall*), has a red tile roof. There must be a Regent some place...

VM: ...someone who owns a tile company.

MT: Cory Hall over there is one of the few that doesn't have a red tile roof.

VM: But I think the Round House was designed well.

MT: It's like a little cake, a cake with a (*red roof*) frosting.

VM: In fact, when you won the Nobel Prize, there was a party, and there was a cake (*in the shape of the building*). Do you remember the cake?

MT: I have a wonderful picture of that cake.

MC: Where was it, at my house?

MT: At your house.

MC: Not the Round House?

MT: No — the Round House wasn't even started at the time you got your Nobel Prize.

VM: There was a cake, in the shape of this Round House to be, and there were napkins (*with a picture of the building on them*), weren't there?

MT: That's right.

MC: There were what?

VM: Napkins.

MT: The cake was nice. I have a few of those. The cake was wonderful. Terry (*Taylor, her husband*) was taking pictures and I have a marvellous picture of the cake before it was cut into.

MC: Did it have candles on it?

MT: No — not the building, not for the Nobel Prize. It was in the shape of the building.

MC: Candles sticking in it, though.

MT: I don't remember that.

VM: I remember also, on the occasion when you got your Nobel Prize, there was the problem of your speech.

MC: What speech?

VM: That you had to make in Stockholm. I don't know whether you remember the way you used to make speeches. But, you had no script, and you used to sit on the edge of

the table and talk, and then at some point you would realise the time was nearly out and you had hardly started, and so you would rush through the things (*which was usually the newer research results*)...

MC: Really?

VM: Oh yes, that was quite common ...and you knew that this had to be different for the Nobel Prize speech. So you did the speech and I remember that Paul Hayes and I were sitting with a stopwatch. You had to finish in 40 minutes otherwise all hell breaks loose. We got it for you and we put clues in the thing and I think Gen (*Calvin*) was going to sit in the projection office with a script and know when to tell the projectionist to change the slides so you didn't have to say the words and you would save a few seconds there. I wasn't there but I gather you finished in time, which was rare for your speeches.

MT: It was all organised and all the hot stuff didn't come at the end, boom, like that.

VM: Yes, you did tend to do that. The introductions to your speeches were very good but they went on a bit.

MT: They want on for a long time; I almost knew some of them by heart!

VM: People wanted the "meat" at the end and they got it very fast.

JO: I had some input later on. I programmed one of these little calculators to flash the digits progressively along the speech. I put it into a cycling pattern: the first two would flash, and then the next two. When he got within 15 minutes of the end of the speech, he could see like a thermometer when he was getting to the end. This was the old type that had a little paper in it, so it could print out, and part of the program was that when it got to be one minute from the end, the paper started to come rolling out of the calculator to catch his attention.

VM: Did it work?

JO: I don't think he used it very much. That wasn't his style.

VM: I can understand. It takes away the spontaneity, doesn't it, if somebody tells you how to talk and it's not your style.

MT: The most interesting experimental stuff (*i.e. information*) always came at the end and people didn't have time even to absorb it before it was all over. I think that was frustrating for them sometimes.

VM: They had to go away and read the papers after that.

MT: Or have a question and answer session.

VM: That reminds me of something else. All the papers, lots of people wrote papers, and I remember through the period when I was publishing with you, you read everything pretty carefully, didn't you...

MC: I tried to.

VM: ...and you would make your comments and corrections. There was quite a lot of debate, argument one even might call it, from time to time about what should go in and what should come out. That must have taken a lot of time, reading all that stuff.

MC: Well, that was my business. I didn't worry about the time.

JO: But there was a system to it. He didn't like to look at papers until they were practically perfect. So there was a time we came in and said "this is the last session" and the authors are here. And then he went through the paper word by word. When that was over, that was it! He never saw it again and it went out.

VM: There was a marvellous thing happened in our lives. The most important thing that happened to any of us was the development of these typewriters that you can alter things with. I remember that first one we had was the memory typewriter? The secretaries had a hell of a job, you can imagine, every time we made a correction they would have to start retyping the manuscript...

MT: ...and making more mistakes.

VM: Do you remember this typewriter?

MT: He wouldn't remember.

VM: It was a big thing.

MT: It took up one whole corner of my office. It was huge. It had two tapes, one on each side; oh dear.

VM: Now we have these little desktop things. They are very clever.

MT: I had one of the first electric typewriters at the Lawrence Berkeley Laboratory, the Lawrence Radiation Lab. When I came to work for Professor Calvin I had an old beater and I had been there about one year and I was upset with the typewriter. So Bert (*Tolbert*) put in an order for an electric typewriter and I had the second. And I have gone through every single thing since.

VM: That was pre-golfball?

MT: Oh yes.

MC: Pre what?

VM: These were little balls that bounced around, balls with the letters in typeface, and they bounced around in the right position.

MT: There were many different kinds of typefaces and symbols and that kind of thing.

VM: That was high technology, 30 years ago, 35 years ago

MT: I had one of those for about 20 years.

VM: (*To Calvin*) Did you ever learn to use a computer?

MC: No.

VM: Let me recommend it. It's a good way of wasting your time.

MC: It's too late now.

VM: Let me comment on one or two other things as we are getting on here. Let me tell you about some of the letters (*that were written to you in 1979*)There is a letter here from Kent State University written by Ed Gould. I don't remember Ed Gould (*to Marilyn Taylor*): could you remind us about him?

MT: He was a tall, dark-haired...

MC: He was here for a while, wasn't he?

MT: He was here for a year. Professor of Organic Chemist (*from Kent State University in Ohio*)...He came on his sabbatical and worked on the third floor (*of the Round House*). He was strictly an organic chemist; I really don't know exactly what problem he was working on.

VM: What he says in the letter is. I'll read you his birthday greeting. This was for your 68th birthday: Some of them are obvious and don't need any comment. He says: "Birthday greetings to Melvin Calvin: "Professor, scientist, scholar, institute director, author, editor (that's all plain sailing) — molecular architect". What do you suppose he means by "molecular architect"?

MC: Designing new molecules.

VM: You designed new molecules?

MC: Sure; that's what I was doing most of the time.

VM: (*continuing Ed Gould's message*): "Radical prober".

MC: I don't know what that means.

VM: Did you work with radicals?

MC: Oh yeah. That happens.

VM: "Mechanistic detective".

MC: (Laughs) Who wrote all that?

VM: Ed Gould. The next thing he calls you "A virtuoso of the isotopic trail...

MC: He's a poet.

VM: "Unraveller of the greening enigma, Nobel Laureate".

MC: What book is that?

VM: This book is the letters which were sent to you on your birthday. I just had a quick look that morning and I wanted one or two to talk to you about, and this one jumped out at me. "Savant". Are you a wise man, you reckon?

MC: No.

VM: "Master of porphyrin excitation, Carousel ringmaster, Critic" (you're certainly a critic), "Friendly but insistent inquisitor" How does that grab you? Do you see yourself as an inquisitor? "Sceptic, Quizmarshal".

MC: What does that mean?

MT: Seminars. You're sitting and quizzing.

VM: "Gadfly".

MC: That was my job.

VM: "Scientific statesman" (now we're getting up market a bit), "Interdisciplinary catalyst, Fountainhead of new ideas". And then he says essentially that it was a pleasure to know you.

MC: Who is this?

VM: Ed Gould, writing from Kent State University the Chemistry Department. Then there's another letter here from Peter Hammond, do you remember him?

MC: He's in England now.

MT: No, he's at Livermore; he never went back to England

VM: He (*Peter Hammond*) says "the multidisciplinary group (that's your group here) was an excellent tonic after the strict organic synthesis school of the University Chemical Laboratory, Cambridge". I guess that was Todd, wasn't it?

MC: He was a professor there.

VM: What sort of a guy was Todd?

MC: Oh, my. What do you mean what sort of a guy was he? He was an autocrat.

VM: He was not an interdisciplinarian?

MC: No.

VM: He was a straight disciplinarian! Was he an autocrat? But he was good, wasn't he?

MC: You did what he told you to do.

VM: But he was a good chemist?

MC: Yes. He got a Nobel Prize.

VM: But he never branched out into lots of things, the way you do.

MC: Not that I can recall, but that doesn't mean much.

VM: We had a guy called Neal something, who came from Todd's lab. in the fifties, who worked on pyridine nucleotides, nicotinamide adenine dinucleotide. You remember this guy, Neal somebody, a chemist, a young man? He went back to England, as we did. I was a postdoc. here and went back to England and you suggested it might be nice for me to come back. I wasn't sure at that stage, but six months in England in 1959 convinced me. The same thing, apparently, happened to Peter Hammond. He went back to England and his wife apparently jolted him into coming back and he sent a letter to you saying that if there was a job going in Berkeley he would give his right arm for it. Is he one-armed now? Was there a job in Berkeley?

MT: No, but he got to Livermore and he stayed there the rest of his professional life. He's now retired.

VM: And then there was Barrie Hesp? Do you remember Barrie?

MC: Oh, yeah. Where did you dig up all those names?

VM: He came from John Barltrop, he was one of John's students.

MC: I remember John. Barltrop was in Cambridge.

VM: Oxford. He (*Barrie Hesp*) came here in the early sixties and he worked, I think, with a guy called John Turner, who came here from Liverpool and worked on porphyrin something; he was a chemist. He was very impressed with the interdisciplinary activity of (*indecipherable*) and how much his stay in the Round House influenced his later life. I think you will find many people were in that position Then he joined the pharmaceutical industry. He's in the States now. This (letter) was still written from ICI in England but later on he came to the States, I think also with ICI. Then there's another letter here, this isn't exactly a letter. This is from Lorel (*Daus*) Kay.

MC: I remember her.

VM: She talks about the pleasant memories, and then she summarises her reactions. This is the time you were approaching retirement as director of the Round House. She says "They're shelvin' Melvin!!!, No way!! I'll bet a rare isotope, He's just as active in the 1980s as he was in the 1950s; here are C-14 reasons why, she says:

C1, Ceaseless energy;

C2, catalytic activation of everybody else. That was absolutely true. You used to say things to people which got them moving in ways they hadn't necessarily thought of.

C3, consideration for his group — now, that's true.

C4, The Calvin Cycle, in every biology text now, including the one she teaches from.

C5, Chromatography.

C6, Counts/min.

C7, Chlorophyll;

C8, was Chlorella;

C9, was sedoheptulose and ribulose phosphates and PGA;

(*Tape turned over)*

C10, Curiosity and capacity for new ideas. I don't think you ever felt, did you, that there was something which was not in your bailiwick?

MC: Well, I did whatever I had to do.

VM: Whichever direction it went?

MC: Yes.

VM: C11, candour.

MC: What does that mean?

VM: It means you always spoke your mind. Did you?

MC: I think so.

VM: I think so, too.

C12, Creativity concerning creation of life.

MC: Well that was evolution.

VM: C13, she says, Calvin

Then, she says, C-14 reasons why you'll still be as much a Contributor as ever to the Chemical Biodynamics Lab. And she sends you happy birthday greetings.

The last one I have here is from Hans Kornberg; Marilyn put a little sticker: now "Sir" Hans Kornberg. Did you know he's "Sir Hand Kornberg"?

MC: No, I didn't.

MT: We have a lot of "Sirs" (*as alumni of the group*).

VM: There's Rod Quayle and George Radda and Hans Kornberg...

MT: ...and Ted Abraham.

MC: What does that mean?

MT: They have been knighted by the Queen.

MC: So what!!!

MT: It's nice to be called "Sir".

VM: It depends on your taste. Some people like it; I think Hans quite likes it. And Geoff, I think, Geoff Wilkinson. Do you remember Lise Schou? She is married to Geoff Wilkinson, who is a Nobel Laureate in chemistry. We had dinner at their house a few months ago. I contacted her about this (*project*) and she said we must get together. She is very helpful. I haven't talked to her yet in detail, but I will. I will remind you of something else before coming back to Hans: when you went on sabbatical in Oxford as Eastman Professor and you lived in Eastman House, which is still there, Bob Rabin and I arranged a dinner in London for all the ex-members of your group, and that's the first time I met Lise Schou. It was in University College.

MT: I remember the house in Oxford. It was next to the playing field.

VM: That's right and I remember that Gen was very impressed with that book which was written by (*George*) Beadle's wife called "These Ruins are Inhabited". I have never read the book but she obviously read it. Did you ever read it?

MT: It book was written from a different point of view!!

SM: I remember that Gen wasn't very happy with Eastman House because it didn't have a fence around it. She felt it was like living in a goldfish bowl.

MC: That's right. It was.

VM: Subsequently, I think it has a hedge round it; I think we saw it not too long ago. Anyhow, to get back to Hans (*Kornberg*). He says: "My reluctance to discuss experimental findings, and even have them announced in seminars given by other members of the group elsewhere in the United States, before I could be reasonably certain of their accuracy, led Clint Fuller to stamp my notebook with an imposing purple SECRET stamp. Unbeknown to me, this now desecrated notebook was left on the bench and to my dismay, I was handed a security violation citation by an armed policeman the next day.

MC: Who was that?

VM: Hans Kornberg. So, were there armed policemen walking through the building?

MT: Yes. When we had classified material, in Donner the classified material was there for a long time — I forget when it finally left — there were policemen coming through and one of the things they checked for was whether the safe was closed, whether there were any documents on the desk and so forth. (*Policemen also walked through ORL*.) This was just a joke document, but the policeman wouldn't know that.

VM: There wasn't any classified material in ORL was there?

MT: No, but there had been (*in the early forties*). There was quite a bit in Crocker and actually a lot of the things we did in Donner when I first came to work we had to use sealing wax, and that kind of thing. So there was classified work going on there.

VM: This is not the end of the story. He says: "Luckily Melvin was not in Berkeley at the time".

MC: Who says that?

VM: Hans Kornberg. "And the inexorable progress of my file from security office to security office was neatly aborted by Bert Tolbert who tore out the offending page and with a disarming smile said that he saw no page marked SECRET in the book. Although the matter was thus ended, I also remember the chill horror that I felt when on my way back to England I opened a marvellous book on the beauties of California to find that the title page was stamped THIS DOCUMENT CONTAINS INFORMATION VITAL TO THE SECURITY OF THE UNITED STATES. I had

visions of spending the remainder of my fellowship in jail." But he didn't. And, instead you know, he became Master of Christ's College in Cambridge and I visited him there about 18 months ago when a microbiologist in London died who happened to be his cousin and there was a memorial meeting there. Hans was a very good Master of Christ's, I think, but now he has retired from that and from Cambridge and has gone to Boston University, whether permanently or on a temporary basis, I don't know.

JO: This is a little out of order, but can be fixed up. When you were talking about the fact that Professor Calvin always did what he wanted to do and never went from one thing to the other reminded me when he retired, formally, and became emeritus it was in the newspapers and the Daily Cal sent a reporter to interview him to get some impressions. She was a young girl, a freshman or sophomore, and she sat here (*in Calvin's office*), and I was in here, too, and the first thing she asked him was "Well, Professor Calvin, now that you have retired I suppose you'll now be able to do all those things that you always wanted to do!" He almost threw her out of the office.

VM: Do you ever remember doing anything you didn't want to do?

MC: Sometimes I had to, it was forced on me by the circumstance. I don't remember what they were, but I am sure there were occasions like that.

VM: But you never had onerous administrative jobs in the Chemistry Department. Were you ever Chairman of the department?

MC: No, no.

VM: You never want to be?

MC: No, no, no.

VM: So you avoided committees, did you?

MC: Yes.

VM: What about things like the Academic Senate, did you play much of a role in that?

MC: No, not much.

MT: You were on the Educational Policy Committee for some years, which was one of the more important committees on the campus; I don't remember whether or not you were chairman, but you were on that committee for at least three-five years. And that was quite a time-consuming thing. The committee meet frequently and it was an important committee for setting up (*educational*) policy for various types of things. And then, of course, you were always on promotional committees — that kind of thing.

MC: That was departmental.

MT: University-wide sometimes because sometimes Chemistry had input into Biochemistry and things like that. I remember the Educational Policy Committee as being a fairly onerous type of task, you can't slough off something like that: you have to go to the meetings, pay attention, write reports and that went on, I think, for at least three years.

MC: I don't remember that, but it's probably true.

VM: What about the directoral obligations on The Hill? I remember you were one of a panel of directors.

MC: As director of the Round House.

MT: You were an Associate Director of Lawrence Berkeley Lab. also.

MC: That was because (the group was a division of which was a division of Lawrence Berkeley Lab.). You had to go along to meetings and take part in all the discussion.

MC: I guess I did bit I don't recall very much about that. I didn't impress me very much.

VM: I can see why. Because you often ask one of "us" to go and replace you (*at the meetings*) when you were away somewhere and they were rather boring meetings so I can see why you wouldn't have been terribly interested. But I guess they all took time. You sought to avoid them.

MC: Of course.

VM: But you did like to travel around and meet other people and go to meetings.

MC: I did a lot of that.

VM: You enjoyed that?

MC: I don't know whether or not I enjoyed it, but I did a lot of it.

VM: You learned a lot of stuff from those places?

MC: Well, I was invited and I went. I didn't ever initiate the action that I can recall.

VM: I'm sorry to keep remembering these things, but I remember one occasion in the early sixties, about '62 or so, when it followed this thing that I mentioned earlier on about you asking me on enzyme control mechanisms. When I'd looked into this a bit, I realised that many of the interesting people who were working in this area were in Europe. I said why don't we get them here? And you said: "We'll see about getting some money". And you got some money — from NSF?

MT: From NSF. We had a group of seminars for two separate years.

VM: We had some discussion about who to invite. And we invited people mostly from Europe — there was an odd Russian and there was someone from Israel, Michael Sela from Israel. There were 12 of them and I was their host.

MC: All at once?

VM: No, they came successively — over two years?

MT: Three years?

VM: Something like that — and each one spent a week in Berkeley and they were guests of "us", of our group, but we also, of course, shared them with other interested people on the campus and they gave a scientific seminar and a public lecture and they communed around in the place generally, and we took them about. I remember the question arose about whether we should offer them any money. We paid all their expenses, but do we also offer them an honorarium? There was some discussion about how much you offer people like this, prominent people.

MC: I don't remember it.

VM: I remember how we resolved it. I said to you: "Suppose somebody invites you to Paris for a week and pays all your expenses and offers you $100...

MC: $100!!!

VM: This was the 1960s...would you go?" And you said "no"; "$200", "no"; "$500", signs of wavering. (Laughter) So we settled on $1000.

MC: Did I get it?

VM: You didn't get it, you didn't go; it was for these other people. We gave them $1000. And most of them came, not all of them. We invited Hinshelwood, but he didn't come. He was one of the few who turned us down. We had a good group of visitors on that occasion.

MT: I have all of those archives, too.

VM: Yes, I bet you do! We felt that the Americans were relatively easy to get here because the distances were not great and they travelled around all the time anyway. But the Europeans were more of a problem because they travelled less, especially in those days. You don't remember them coming?

MT: That was part of the (*indecipherable*) with the building opening. Remember (*on April 1st, 1964*) we had the dedication of the building when it was brand new...

MC: The Round House?

MT: The Round House...and the proposal was made, here we had this new institute and one of the nice things to do would be to have a series of special visitors presenting seminars. You applied to the National Science Foundation and convinced them that that would be a good idea and the invitations went out. It was initially for only one year but it was so successful you got a second year. So, for the two years after the building was opened we had these seminars, I think it was in the spring; they all came in the winter before and did their thing. It was very successful, I think. It did two things. It first of all brought people here to meet with others (their colleagues on the Berkeley campus) and also put the laboratory on the map as a scientific institute. If you create something, a new building with a lot of people in it, but nobody knows who it is or what it is. So, we got a lot of publicity through the university to have these people come.

VM: As Marilyn said, it put the Round House more on the map than it otherwise would have been and also faster. And it also, I think, emphasised this interdisciplinary attitude, because these people were over a fairly wide range of biological and chemical interests, and they were all guests of this group.

MC: You remember it, and I don't.

MT: You haven't asked this question yet, but you might in discussing the laboratory, its dual function as an Organised Research Unit of the University of California and its function as a division of Lawrence Berkeley Laboratory. We are a dual-functioning institute and that is very unusual.

VM: Did this dual-functionality of the group influence the way it developed, do you think?

MC: I don't know. I have no idea.

MT: The dual-functionality did not come into existence until 1960, which was when the building plans had gelled and the money for the building was available. When we were ORL and Donner, or Donner and LSB, we were only a division of Lawrence Berkeley Lab., the Rad. Lab...

VM: But always with a teaching function.

MT: With a teaching function but we were not a separate University entity. This University entity was created in 1960. Part of the reason for that was to get the money. People like Kettering could give money to the University but they couldn't give it to the Lawrence Berkeley Laboratory because that was AEC or whatever it was at the time *(i.e. a government laboratory*). We had a dual thing: it was an administrative and also a financial avenue. The financial avenue continued for a long time. When people wanted to give money they could give it to the Laboratory of Chemical Biodynamics as opposed to trying to do anything through The Hill. That still continues: money still comes into the laboratory as an Organized Research Unit (*and is administered by the laboratory as a University institute*). You always wore hat you wanted to wear: sometimes it was better to be campus institute and other

times a Hill-connected group. So you could whichever was best at the moment. It was very handy.

VM: You were always able to place graduate students into these activities.

MC: What do you mean?

VM: The graduate students that came to you essentially from chemistry.

MC: I would persuade them to do whatever they wanted to do, so to speak.

VM: There was no difficulty with putting them into the units which were all AEC units.

MC: No.

MT: That was where the support came from. We also had graduate students from biophysics, botany, psychology and biochemistry, so it wasn't just chemistry students.

VM: That means that you had...

MT: ...faculty ties to these other groups.

VM: I remember that you became Professor of Molecular Biology at one stage...

MC: I guess so.

VM: ...in addition to Chemistry and you gave a course in chemical evolution...

MC: I don't remember but maybe so...

VM: ...and I think it was the course either based on your book or on which you later wrote the book (the book *Chemical* Evolution which Calvin wrote at Oxford while Eastman Professor).

Why don't we take a break at this point, take a deep breath and think whether there are any other topics that we should bring up.

MC: Good, good.

After a break:

VM: Could you talk a little bit about all the support staff in the lab., the technicians and dishwashers and the secretaries and the carpenters and all the people who kept the thing actually going.

MC: Well, "she" (*i.e. Marilyn Taylor*) did.

VM: She was one of them, but there were lots of them.

MC: But she did everything.

VM: What does he mean?

MT: I don't know what he means.

VM: What do you mean that she did everything?

MC: I didn't do anything.

VM: We had all these people in the place, they must have come from somewhere.

MC: Well, she did it.

MT: No, no, I didn't do it. The senior staff in the lab. was responsible basically for the support staff. In other words, they took care of hiring them, and nurturing them, showing what they wanted to have done. The senior staff, really, under you, who set the tone for the laboratory. They were the ones who together would interview people for jobs. It wasn't any one person who would interview you for a position; for example, Martha Kirk was interviewed by several people, Ann Hughes was interviewed by several people. We got the carpenter from ORL, he just moved over, also the glassblower. And then we had a dishwasher.

VM: Yes, Alice.

SM: We were talking about this and the point about it was the fact that there were all these remarkable people who stayed here virtually forever and it was very much part of your group and these people were happy to be in it and felt that it was a home to them. This was the point.

MT: We felt like a family. Coming to work was like coming home. I think most people performed in that kind of a situation. When necessary, you did 150% or 200% or whatever because of the situation that we had in ORL, in Donner and, of course, eventually, in the Round House. Particularly for the office staff, it was very interesting: there were four of us that were there for 20 years, that's a very unusual thing in an office staff. There was myself, Lois Soule (you didn't know here), Beth Klingel and Gloria Goldberg. We all worked together as a group. What had to be done, was done. There was no "boss" or people working underneath someone. Having been over here in Chemistry now for almost 15 years, I can see that that was an unusual situation. I am sure John (Otvos) will agree with that, too. Very cohesive situation. The same thing existed with the people who assisted in the laboratory, people like Martha (*Kirk*) and Ann Hughes — we all worked as a team and felt that what we did was important.

VM: And there were all the people who were offering various sorts of technical support directly to the scientists: the carpenters, electronics people, those sorts of guys, whom

you could absolutely rely on all the time. They were very helpful and they were very good and competent.

MC: I didn't hire them.

VM: But you knew them all, didn't you?

MC: Yes.

VM: And you learned some most amazing things. I remember from Mr. Norman, the carpenter (*from ORL*), he taught me how to carry a full cup of coffee along the corridor because it tends to spill. He said what you have to do is keep moving it up and down as you walk. (*Laughter*)

MT: I remember the glassblower (*Bill Hart*). If you needed some little thing personally, you could always go down to the glassblower for it (*first in ORL and then in the Round House*). To be able to have our own building, with these services — a machine shop, a glassblower...

MC: You're talking about ORL?

MT: no the Round House — was very unusual. We had our own "goodies" — electronics shop, computer support. That was very unusual, it made working there for the scientists much easier than if they had to go through the ordinary bureaucratic situation (*which existed elsewhere on campus*.)

VM: The secretarial help — Marilyn is being modest — in those days was even more important than it is now when everybody has their own computer. In those days, they didn't and the secretaries did the typing, did the drawings for the figures. Nowadays you do all this kind of thing on the keyboard. But then, you didn't. You needed to have the skills, reliability and a degree of imagination, particularly when you came to the drawings the scientists would only give approximate indications what they wanted and they expected other people to make a good job of it. And the journals always wanted the drawings to be very well. You my remember that we walked yesterday about Alex Wilson and his diagram with the fisherman on the tank. Do you remember that?

MC: I remember that, but I don't remember how it came about.

VM: It came about because...

MT: Alice Holtham drew it.

VM: ...the journal (*Journal of the American Chemical Society*) wanted to compress the picture into a very small space and these guys got their revenge on the journal by putting the little picture of a fisherman which was then was so small you could hardly see it. In fact, Sheila wanted to have a look at the fisherman on the edge of the tank.

(*The drawings*) were all very skilfully done by the secretaries who drew (*the figures for the technical papers)*.

MT: The fact that these people had been going on for long periods of time meant that had expertise so the scientists did haven't to be so specific, particularly with the drawings. We had a wonderful girl who did our drawings for a long time, maybe more than over ten years (Evie Litton) who knew more about how to present these things for the journals, when it had to be drawn up, than the men did. She could interpret everything that they did and that wasn't easy. We always had one person who did that, even from the early days in ORL we had that kind of person who did the drawing and some of the typing. These people stayed a long time.

SM: I remember, when I did my homework in preparation for these sessions, reading that very early in your career, you (*Calvin*) did your own glassblowing, you made your own equipment. I think that was in 19?? You did that yourself.

MT: Maybe.

SM: But you probably have a particular appreciation for the way that other people do things because you had done it yourself.

MT: I suppose; I never thought about it that way, but that's probably true. I'm tired, I'm beginning to get tired.

VM: I think that's about all I can think of. Any more things you'd like to say at this point? Need a rest?

This is the first interview in our series. When we come back next spring and summer, we'll collect some more information and perhaps we will talk again. We are signing off for this one.