

Chapter 6

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Berkeley (California)

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VM = Vivian Moses; DL: Dick Lemmon; SM = Sheila Moses

VM: This is a conversation with Dick Lemmon on the 24th of May, 1996 in Berkeley.

Dick, how did you come to join the group of people that later became the Bio-Organic Group, or maybe they were always called the Bio-Organic Chemistry Group?

DL: I had spent the war years — I'm a little hesitant to say very pleasantly — working on the Cal Tech campus on a war project on the chemistry of rocket propellants. While I was there, I got to know very well a Jack Miller, who was a Berkeley chemistry BS and who was very impressed by a new young member of the faculty at Berkeley, whose name was Melvin Calvin. He said that if I wanted to go on to a PhD that he couldn't give me a higher recommendation. He said, "don't stay at Cal Tech, go to Berkeley, where there not only is there this fantastic young chemist Melvin Calvin", but he had some other reasons, oh yes, his other reason for going to Berkeley was that Berkeley was on the cutting edge of nuclear chemistry, creation of new elements and their use as tracers, and all this kind of thing. For those two reasons he said I ought to go to Berkeley. I followed his advice.

VM: Were you in Pauling's department at Cal Tech?

DL: Pauling was the overall director of this war project that I worked on. A collaborator of his who worked on protein structure with Calvin (*should this read "Pauling"?*) was the immediate director of the group that I worked with. One of the main things we were doing was using column chromatography as an analytical tool. What was in captured German and Japanese rocket propellants was better than what was in ours? The answer to that was "practically everything".

VM: When was this, that you first contacted Calvin?

DL: : It would have been in March of 1946.

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VM: What sort of group and set-up did he have at that time?

DL : Practically nobody, not even Al Bassham then. The group had just been formed and Andy Benson was the main person in the group. I'm not sure about Bert (*Tolbert*) — whether Bert was there or not, it was a month or two earlier or later. But I think Andy and Bert were about it. There was Dorothy Johnson, a young Berkeley BS chemist, who also joined the group about mid-'46. That's all. The group was only three or four people. Oh — Peter Yankwich might have been a member then. Perhaps all of the authors of "Isotopic Carbon" were members of the group at that time. There might have been half a dozen people in that group when I joined as a graduate student.

VM: When you first came was there the clear division of activity between the photosynthesis and the synthetic isotope work that later was so clear?

DL: The group really was formed, E.O. Lawrence's charge to Calvin, was to find ways to utilise carbon-14, the newly-available radioisotope of carbon. I don't think that Calvin had the photosynthesis idea any sooner than he had the idea of the general isotope work. When I talked with him as a prospective graduate student he proposed photosynthesis to me, and I have always regretted that I didn't do it, but photosynthesis was a complicated business, as I saw it then. Another thing he proposed to me was to use the carbon-14 to see how a compound, pyruvic acid or the ester ethyl pyruvate in particular, this compound has two carbonyl groups, adjacent, and one of those carbonyl groups comes off as CO. Which one? It was a very obvious system to work with, it looked like the synthesis would be easy (it wasn't). It turned out to be quite do-able, to put carbon-14 in one of the carbon atoms and see which carbonyl came off. That worked out beautifully and became part of my thesis.

VM: Did you have any biological background?

DL: Not really. I can't remember any course in biology of any sort that I ever took.

VM: So, photosynthesis would have been a bit mysterious for you.

DL: That's perhaps another reason why I didn't take (*that suggestion*).

VM: So, you joined this small group as a graduate student, working on the ethyl pyruvate release of carbon monoxide, and where were you physically?

DL: When I joined the group at that time, its only space was in the Donner Laboratory, on the third floor, and that continued to be the space for research other than photosynthesis. As I recall, the photosynthesis people moved into the Old Radiation Laboratory about mid-1947.

VM: As late as that?

DL: As late as that (it might have been early 1947). They moved over from Donner to take up the photosynthesis in the Old Radiation Lab.

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VM: The first time you met Calvin: can you remember meeting him?

DL: Pretty specifically.

VM: Tell me.

DL: Well, he was obviously very energetic, which I expected, he was very smart, which I also expected, he was very overweight, which I had not expected. But, he was very good, I think, at interviewing a prospective graduate student. He was easy to talk to; although he obviously very bright he was not formidable. I felt at ease talking to him.

VM: Was he very informal then? I always remember him as being informal...,

DL: Yes, he was quite informal. Very easy to talk to.

VM: This was in his office in chemistry?

DL: This was in his office in the old brick chemistry building. Was that still in existence when you came to Berkeley?

VM: Yes, I remember it, the one with the turret (*actually a cupola*) still kept as a memento. I was there before that fell down.

DL: Makes you an old-timer.

VM: Otherwise I wouldn't be doing this!

SM: That's where the fireplace came from (*the one which was in Calvin's office in the Round House*).

DL: That's where he had his fireplace which had been G.N. Lewis' fireplace and that's why he (*Calvin*) was particularly fond of it.

VM: So, you settled in in the Donner Lab. in one of those rooms on the third floor. Were there other graduate students there at the time?

DL: I think Gus Dorough was there, some of the time at least. Other than Gus, I think I was the only graduate student. Al Bassham (*James A. Bassham*) joined the group: I joined it in April and I would say Al perhaps two months later, very close. But I can't specifically remember Al working in Donner. So, I think he joined just as the move (*of the photosynthesis people*) to the Old Radiation Lab. began.

VM: What sort of group feeling was there at that time, because for someone like myself who knew it only much later when it was very much more developed.

DL: We really felt that we were on the ground floor of a very exciting era where isotopically labelled compounds were going to solve all the problems in biology and

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chemistry. If you wanted to know what happened to CO₂ in photosynthesis, you just put in the radioactive stuff, follow the compounds it made. Essentially that idea was quite correct, although it was much more complicated than people knew at the start. We knew that we were on, as I say, the ground floor of an exciting era in chemistry and biology; we knew that we had as director of the group somebody really exceptional. Calvin was already invited to give lectures all over the country, even though he was, what was Melvin then? He was still in his 30's.

VM: He was born in 1911; yes, he would have been...

DL: Yes, in his thirties... and all these invitations and prizes that he was already getting, like the Local Section of the (*American*) Chemical Society had made him their chairman, and a lot of things told us for sure that Calvin was going to be a big name in chemistry.

VM: You were conscious, were you, that you really had a monopoly on carbon-14 at the beginning.

DL: Yes, indeed we did. It was first produced here in the 184-inch cyclotron, right after the war, and the director of the Radiation Laboratory, Ernest Lawrence, had invited Calvin to use it and he obviously was going to hold back on requests from the rest of the country to let Calvin be the first into the use of this important material.

VM: Somebody mentioned, but I can't remember who, that one of the earliest sources of C¹⁴ was from bags of ammonium nitrate which were placed around the cyclotron. You must know that story.

DL: Yes, that's right. It was a reaction of protons bombarding the nitrogen atoms which then became carbon-14. That's why the ammonium nitrate, a heavily nitrogen-containing compound.

VM: Was it actually placed there as a shield as a shield? Is that why it was there?

DL: No, it was there as the reactant to get carbon-14. The nitrogen in the ammonium nitrate (nitrogen -14), proton in, neutron out, the same way essentially, gave you carbon-14.

VM: So it was put there deliberately for that purpose?

DL: Deliberately as a reactant. They had shielding, of course, but that was lead bricks.

VM: You guys worked it (*the carbon-14*) up from that?

DL: You know, I'm not sure about this. All I remember was getting the carbon-14 as barium carbonate. After the irradiation of the ammonium nitrate and the formation of the carbon-14 it was recovered as CO₂ which bubbled into barium hydroxide which became barium carbonate. This is pretty simple chemistry. Some technician up on The Hill was doing this and we first got it as barium carbonate.

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VM: But later on, presumably, you got it from the nuclear reactors, not from the cyclotron.

DL: This was in 1946-47 and, by — I would say by the early fifties, it was commercially available. We did supply a few people, I guess were friends of Calvin, and helped them out with occasional small samples of our own carbon-14.

VM: It must have been five-seven years before there really began to be a commercial supply.

DL: Yes, I would say the early fifties.

VM: What sort of group activity did you have internally? When did the seminars start?

DL: The seminars, I think, were already an ongoing institution when I joined and they were held in the Donner Laboratory's library which was a floor just below our main laboratory.

Which brings up — maybe I can diverge a little bit, with a story about that. Up in our laboratory we had the job, always, of removing the last traces of carbon-14 from any glass vessel that had been used. The way we did it in those days was to have a bath of warm dichromic acid-sulphuric acid, concentrated. It was a wicked, wicked brew. There were a lot of precautions taken to try to be sure that this concentrated dichromic acid stayed in its place in this bath. One night it began to drip down onto the library below us. Fortunately, the drip was small in amount and no books were damaged down below. The flooring was damaged, that kind of thing, but we had daytime nightmares thinking of what could have been the case if that whole big pot of stuff had failed. It was a stainless steel pot. Even so, the dichromic acid somehow managed to eat through that...

VM: Actually corroded it? The pot was leaking?

DL: Yes, the pot was leaking.

VM: Not only was it this strong acid, oxidising acid, but it also had the residues of your C^{14} in it?

DL: That would have been, from the standpoint of the health hazard, very, very minor because in those days we had "tut, tut, tut, tut, tut, tut, tut" (*i.e. very little*) radioactivity, and that was enough for your experiment and what you had to do. What it took to damage a living organism was thousands, if not millions of times that.

VM: So the acid rather than the radioactivity...

DL: The acid; the radioactivity was of no consequence. Today, it would be. If say there's radioactivity in anything, 95% of the American public wants to run in the opposite direction. When you tell them that they have carbon-14 in their own bodies, they don't believe it.

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DL: So were you sitting there, one day, having a seminar, with this stuff coming through the ceiling?

DL: No. This happened in the early morning hours, something like 3 or 4 or 5 a.m. The first person who got to work that day found a slow drip of this awful stuff down in the library below.

VM: Were the seminars always at eight o'clock in the morning on Fridays?

DL: Yes. In the early years they were., I don't think it was you, Vivian, but I remember somebody saying that in England this would be regarded as totally uncivilised.

VM: I think all of us would have said it.

DL: I was one who thought that was a good time for the seminar.

VM: I mean all the English would have said it.

DL: The English; yes, I dare say.

VM: How quickly did the group grow?

DL: Let us say there were six, roughly half a dozen in 1946. I would say by 1950, another four years, it had grown to be 20-25. I have no firm handle on this; this is just a general recollection of how it was I don't think you'd find that seriously wrong. Something like 20 or 25. By 1960 we were maybe 40...

VM: ...or even more than that.

DL: Even more than that.

VM: I remember that by the time we got into the Round House...

DL: Maybe you're right.

VM: ...there were actually 90 people or so, living in that building. Some of them were really attached more to Rapoport and other faculty. Nevertheless, in the sense of being members of the building community, there were about 90 people.

DL: I still don't think I'm too wrong about saying 20 in 1950.

VM: How did they begin to come? What drew them? Was Calvin's work becoming publicised.

DL: The main thing was people who were interested in photosynthesis saw paper after paper and could see that the head of the laboratory doing this was one Melvin Calvin. Perhaps even more came from their professors of whatever institution they came

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from. They knew Calvin personally, or knew him very well from the literature, and knew he was a very important guy. So, if you wanted to work for your PhD with somebody who is a real live wire, I recommend Melvin Calvin.

VM: Did they begin come from abroad as well as from inside the US?

DL: Oh sure.

VM: Quickly? Early in the day?

DL: I remember a man by the name of Hirschberg who came to us from Israel. He was here in the early 1950's.

VM: We met him here in 1956, when we first came. But I don't know whether he hadn't perhaps been here earlier, before then.

DL: Well, I can't tell you. I said early fifties: so if he came he came '52 to '56; something close to that. There were, let's see: I'm, sure there were others, other than Hirschberg. I remember a man from Brazil, though at least for the moment I have forgotten his name (*Henrich Hauptmann*); he was a very early member of the group. He was a professor of chemistry at one of the major universities in Brazil. As I recall, he was a very good chemist. He did some work in mechanisms, it wasn't in photosynthesis.

VM: Did people come because of the development of C¹⁴ technology, did they come to learn that?

DL: That was a major reason. That reminds me of one of our earliest foreign visitors, Professor Hans Schmid from the University of Zürich in Switzerland. He was attracted to Calvin's group specifically by the availability of carbon-14. He was a very good organic chemist and not so interested in things biological. I remember his saying — he wondered if the customs people at the airport would catch him as he brought the first carbon-14, research carbon-14, into Switzerland and he did not declare it! (*Laughter*)

VM: They wouldn't have known what to do with it!

DL: That is the second foreigner that I remember. Hans came in 1948 or 1949.

VM: So people began to come for a period, either as graduate students or as postdocs. for a year or two, quite early on in the life of the group?

DL: Quite early on, definitely. I am sure there's another one or two that I am not thinking of at the moment but who were here, let's say, in the forties.

VM: The style, presumably, gradually progressed to having a central core of people who were permanent scientific residents of the group and the floating population...

DL: ...sure, of graduate students and postdocs and visiting faculty.

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VM: And you became one of the permanent people when you completed your PhD?

DL: No, I spent a year in Zürich as a postdoctoral fellow. Toward the middle of that year, I began to think what am I going to do when the year's up? I wrote Melvin a letter saying that if he had something worthwhile for me to do in Berkeley while I was seeking a permanent job, and he wrote and said "yes". I came back and nothing was said from that day to this about when I was leaving. (*Laughter*) I did a little looking around, for some kind of a job in industry or academia, and I don't remember making any particular progress whatever. But nothing was said about my leaving, so I just didn't leave.

VM: When was the year that you spent in Zürich, so I can reconstruct (*the sequence*).

DL: I was there in the academic year '50-'51.

VM: By that time, what was the position of people like Yankwich and Charlie Heidelberger; was Kritchevsky there?

DL: Kritchevsky was here in '49 — no '50. He came here while I was in Zürich because he had come from Zürich.

VM: Was he a temporary visitor?

DL: He was in his second postdoc. assignment; the first was in Zürich.

VM: Who were the permanent people at that time?

DL: The authors of "Isotopic Carbon": Calvin, Reid, Tolbert, Yankwich, Heidelberger. So they were all here as permanent people.

VM: So they were AEC employees except for Calvin who was faculty?

DL: That's right. Which reminds me, I wanted to say sometime here — although it's obvious to you, it might not be to some others — that when the new space became available to Calvin in the Old Radiation Lab. and everybody but Calvin thought it looked like a cow barn and you couldn't make a lab out of it, he was right, you could make a good lab out of it. Then all the photosynthesis people moved into that lab. All the rest of the group, who were doing things other than photosynthesis, were in Donner. There was a connection, however. It came about this way: one of the main lines of research in our Donner section became radiation chemistry because it turned out, quite unexpectedly, that one of the compounds that we had synthesised for somebody's tracer use some place else was a compound called choline chloride which was decomposing under the effects of its own carbon-14 β -particle emission. That led to a lot of work on choline chloride — why was this compound so unstable? A lot of the mechanism was worked out that the real reason for that instability is still not known. But this got us into the area of radiation chemistry. One of the things that Cyril Ponnampuruma (*one of Calvin's graduate students*) was doing when he came

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was the radiation chemistry of adenine. So, in addition to putting carbon-14 into compounds of (*biological*) interest, there was this radiation chemistry. There was still organic chemistry mechanism work going on. For instance, Pete Yankwich's work on the decarboxylation of malonic acid which first showed up with the rates of reaction of the carbon-14 compounds were not just the same as the carbon-12. Later, it was found in the photosynthesis work that indeed the algae or plants prefer carbon-12 compounds to carbon-14 compounds.

VM: I remember that. That was me and Ozzie (*Holm-Hansen*) and Chris Van Sumere.

DL: How about one John Weigl?

VM: John Weigl is dead now.

DL: He's dead now but I'm talking about when you were saying... He got his PhD in '49.

VM: I never knew him.

DL: W-E-I-G-L.

VM: I know who he is, yes.

DL: He was here...he left after he got his PhD. But as I remember, I thought that John was the first one to show this (*isotope effect*) in algae; I could be mistaken.

VM: Maybe we just repeated what he did and didn't even know about it. That's happened before in history!

DL: I would have thought that Calvin would have put you in contact with him — he could have called up on the phone...

VM: We certainly did a paper like that in which we showed that (*effect*)...as you happened to mention it.

DL: Charlie Heidelberger, in the early fifties, was working on a rather complex synthesis, I think it was (*carbon-14 labelled*) benzantracene (*Editor's note: It was in the late forties; Charlie Heidelberger left the group in 1948*) which had a very theoretical role in cancer and Heidelberger went on to a career in cancer research (*at the University of Wisconsin and later*) at USC (*University of Southern California*). You know that he's gone: he's dead.

VM: When the photoyntesisers moved into ORL, what was the feeling among those who didn't move? Were you glad not to go into this shack or did you feel that you were missing out? Was there a feeling of loss?

DL: I think that those of us who were not doing photosynthesis, we all had something else going and wanted to see it through. We might have said when I'm through with ethyl

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pyruvate, or whatever, I'll maybe do some work in photosynthesis, which, incidentally, I did. Part of my thesis was the role of pyruvate in photosynthesis.

VM: Was it? Was that published?

DL: I don't think it was and I don't know why it wasn't. I think it's because Melvin got very interested in something else and so did I in something else. We just never pursued it. The pyruvate became acetate mostly and it wasn't very profound work.

VM: But pyruvate is the very centre of metabolic biochemistry; you can't get a more central compound than that.

Was there a feeling that the group was splitting?

DL: No. We were all together and we'd meet once a week in the seminars. The people doing photosynthesis contributed ideas to the people over in Donner and vice versa. It's an interesting question that you ask and I have never thought of this before. My impression was that there wasn't any feeling that we had been split apart. After all, we had the same director as well as the same seminars.

VM: That's true. And the distance between you was only 150 yards.

DL: Yes, good exercise.

VM: Was there a lot of toing and froing, people moving between the two labs.?

DL: I'm sure, using pieces of equipment that were available one place and not the other. There was a lot of such.

VM: There was no sense of estrangement between the two groups.

DL: No and there was a very good reason for that. Melvin himself didn't reflect a dichotomy. He seemingly was just about as interested, maybe not quite, but still very interested in the work in Donner and by the mid-1950s I think he was smelling a Nobel Prize in photosynthesis so he must have been more interested in photosynthesis, but he did not neglect Donner.

VM: I remember that when I first came in 1956 it was very noticeable how much time he spent in ORL and how close he got to the people there. He would sit down with people at their desks and want to see the raw data and talk about it. Did he do that in Donner too?

DL: Not that much.

VM: So he did some?

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DL: Yes. He showed up often enough that people would say “Oh, my God. I was supposed to have this done and there he is! Asking me what I’ve done and I haven’t done it yet.”

VM: Where were the secretaries?

DL: They were also in Donner, in a room just down the hall. We had about four laboratory rooms, all in the same group together, and at the end of that hall was the administrative of the lab. which was Bert Tolbert’s office and Marilyn Taylor, and, later on, about 1960, we had yet a second secretary. (Marilyn shouldn’t be called a secretary, she’s an administrative assistant and, of course, a damn good one.) Those two or three were in this administrative office in the same hall as the research for the group went on in Donner.

VM: When we first arrived there was a secretary called Dea Lee Harrison.

DL: Oh, there was one in ORL.

VM: I couldn’t remember — I thought she was in ORL.

DL: You’re quite right.

VM: She was a sort of local secretary, was she, for the people to work with in ORL?

DL: Yes. The budget was kept in Donner, the most important documents of all were in Donner. The personnel files were in Donner.

VM: So the whole administrative centre of the group...

DL: ...was in Donner right up to the time of the occupancy of the Round House.

VM: Was Bert (*Tolbert*) the chief administrator in the early days?

DL: Yes, he was.

VM: And then you took over from him later?

DL: I took over that job when Bert left.

VM: Was there actually a lot of administration because those of us who worked in the scientific end were really not very conscious of it, because we didn’t do it? There was no grant getting, it all seemed to resolve itself around Thursday lunchtime meetings.

DL: I would say that I spent for many years maybe a third of my time on administration and two-thirds on research. I think that is about what it was for Bert, but you’ll have to ask him.

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VM: The funding for the group in those early days came overwhelmingly from The Hill, presumably, from the Radiation Lab.?

DL: That was a way point: it came from the AEC.

VM: But through The Hill?

DL: Yes, through The Hill.

VM: The group was answerable to people on The Hill?

DL: It's curious to reflect back on those days and think how it is now. Once a year we had to write up our research proposals and nobody suspected that some of it, or any of it, wouldn't be funded. It all got funded, year after year. The AEC staff in Washington regarded their primary job as to explain to the Congress why this was important work and should be supported. Nobody in Washington ever thought of saying "oh you shouldn't do it that way, you should do it this way". By and large, the people on the AEC staff in Washington were not so gifted at research work as they were at administration. Now, of course, it's very hard to fight for your grants, very difficult. Of course, the names of Lawrence and Calvin had a big part of this. Anything those two men wanted, I wouldn't say that the AEC necessarily got it for them, but they went to bat immediately without any question.

VM: Was there much debate about the size of the annual budget or was it something that you put a figure forward and they accepted it?

DL: It was based very much on recent past experience. We knew that per person of a given category we needed approximately so much money and we saw last year that it had taken x-dollars to run the lab. and buy the equipment. So, for this year, let's make it $x + 25\%$. It was informal as that.

VM: Year after year, x plus 25%?

DL: Yes — no, I don't say it was every year. The one year we might say that we have two new full-time PhDs and we had better make it 30%. Or, we didn't hire anybody new this year, but all we need is for inflation, so add another 5% and send it through.

VM: Out of curiosity, when did that begin to change?

DL: I would say it began to change about 1965-67. I guess the reason I say that...I remember that Melvin spent a year at Oxford in '69.

VM: No, 1967-68; we were there the same year.

DL: You must know that. It was about the time he wrote his book "Chemical Evolution", which I should mention is another major research effort that went on in the Donner Laboratory simultaneously with the photosynthesis in the old building. Now, I've forgotten what I was going to say.

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VM: About the change in the budgetary mood.

DL: Oh, yes. I remember that when Melvin came back from his year at Oxford, I don't know how the subject came up, but he mentioned some press conference that he had and some British newspaper man said "Well, I suppose to fellow scientists this business on photosynthesis is interesting to you guys, but why should I want to have my tax moneys going to you having a pleasant time in the laboratory/" I think Melvin's reaction to this was a bit of a surprise because until that moment, at least, science was so much looked up to by the public on both sides of the Atlantic, particularly in the United States, that they got their budgets with the greatest of ease., Now, suddenly, here's a voice saying "Well, is the public really getting its money's worth for all this work that you are doing?" Then, after that, we began to hear that from American voices in Congress and elsewhere. So, for me, the awareness of this new reaction toward science (*began in*) the late sixties.

VM: Interesting to think how differently the group might have developed had that mood been prevalent much earlier.

DL: If we had had really to fight for our budget, it would have been a devil of a lot harder to get that budget for photosynthesis (*that we did up until the mid-sixties*), let alone for organic reaction mechanisms. Radiation chemistry might not have been so bad because that was tied into radiobiology which was the accumulation of knowledge that might help us survive when the bomb goes off. Then you need to know what's happening to all these metabolic compounds. That might not have been stopped. But photosynthesis would have been harder, I'm almost certain.

VM: I think one of the great benefits of photosynthesis research in those early years was that there were the resources, in effect, one way and another, to chase all the leads that came up. One didn't have to pick and choose and the horizon, I remember this from the photosynthesis point of view and no doubt it was in parallel on the isotopic work, any good idea somehow would be followed. Maybe people couldn't do two things at once, so they would have to make a personal decision about what to do. If it was good, the resources would be made available somehow and you didn't have agonising appraisals about which way to do.

DL: I don't know why it has occurred to me, but I would like to mention a German postdoc. that we had here many years ago, I have forgotten who it was at the moment, but I remember very well his praise of the Berkeley Campus. He said if you have a question about **anything** in biology, physics, chemistry, you name it, someone would say go talk to professor (*so and so*), he's an expert. There's an expert on this campus on every subject that he could think of. I hadn't thought of it that way before. I knew that Calvin was a big expert but the rest of the faculty, so what? The rest of the faculty had a lot of very talented people.

VM: It's a big institution and there are a lot of people so that there are enormous numbers of specialities around this campus, more than you would find in many others. It has, for a long time been a big campus, so people felt like that.

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Coming back to Donner, in particular, by the time the group recombined in the round building in '63, there was already a fair amount of biological work in Donner.

DL: Are you talking about in the Calvin group?

VM: In the Calvin group, yes, with Martha Kirk and Ann Hughes, in particular, and I think Karl Lonberg started there.

DL: And there was Ed Bennett's brain biochemistry, which I am sure Ed will talk about.

VM: How did that happen, that that sort of work arose in Donner as yet another activity.

DL: My impression was that again it was Melvin Calvin reading all sorts of scientific literature, and he read a paper by a guy back at the University of Michigan who claimed to have trained worms to grow in either dark passages or light passages, whatever you train them to do. Then when the worms had been trained to prefer a light passage rather than a dark passage, you ground them up and fed them to untrained worms.

SM: That was planaria, wasn't it?

DL: That was?

SM: Planaria.

DL: Planaria, you're right. What was the name of the man who...?

VM: The man's name, I think, was McConnell; can't remember his first name. There was another guy called Allan Jacobson who was his associate who actually came here and spent some time in the lab., together with a man named Bill something from Duke (*Bill Byrne*). There was a lot of hassle and they could never repeat it (*their experiment*), do you remember? Not here, anyway.

DL: I remember when this McConnell gave a seminar here in Berkeley. I resented it as he was talking about: "Here, you spend multimillions of dollars on these huge pieces of equipment and my research", he said, "this flask only costs 15 cents" and the worms were free!. He was emphasising how he was doing this great research, at least as important, as the work up on The Hill, or any place else on the Berkeley Campus, for a budget of nothing.

VM: But I remember that long before then, must be by the mid-fifties, people were working on isotopes in animals. Wasn't Ann Hughes working on deuterium effects in mice, for example?

DL: Yes, she was indeed.

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VM: Was that a deliberate decision in order to expand the isotope work into biology or did it just arise one day?

DL: I think it was an outgrowth— we did a lot of work with carbon-14 and somebody seeing that there were opportunities with tritium and with phosphorus-32 in biological work. As I recall, it sort of flowed naturally out of that.

VM: We'll be talking to Ann so I'll ask her how she got moving on that.

DL: I'm sorry that Martha isn't still here to be interviewed.

VM: That's right. For the record, that's Martha Kirk who must have died 10, 15 years ago.

DL: It's more than a decade now: it's 12, 14 years ago.

VM: Can we talk a bit about the social scene in the lab. in those early days? By the time I came, the party season was very well established. Was it always like that?

DL: Yes. I can remember a Christmas party when there were only half a dozen of us around the Christmas tree. In the next year or two we started exchanging presents, as long as the presents did not cost over twenty-five cents. Dinners at each others homes were quite frequent. There were very few exceptions by our feeling (by "our" I mean the group in general) that their co-workers were interesting and pleasant people to be with. The exceptions, I can only think of a couple and I will not say who they were, were so kind of gross that it was just normal whatever.

VM: We remember that part of the social thing was the fact that the lab was populated almost 24 hours a day. There were people around; I guess they were graduate students rather than older people with family commitments, but that was always the case, was it?

DL: Yes. It is not the case, now, for several reasons. One is, I guess, the principle one is the safety factor. This reminds me, I want to digress just briefly.

VM: Please, digress as much as you like.

DL: This is a safety story out of the Donner Laboratory. Whoever was in charge of safety up on The Hill used to give plaques, framed glass-enclosed certification, that the laboratory had gone for the last 24, 36 months without a single reportable accident. One of those plaques was put up on the wall of the laboratory and Toni Phipps, who in those days did the laboratory glass cleaning — later on she was the stockroom manager — was reaching to get something from a shelf and this plaque fell down and missed her head by a matter of inches. We all congratulated Toni on her near-miss. We got to thinking about this and Toni also was saying, she said she wished she had been hit on the head by this as it would have been a wonderful safety report to have put in — here we had an accident because we had been issued a safe-work plaque. That's the story.

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VM: Were there really no accidents at all, apart from your story of the chromic acid coming through the ceiling? No serious mishaps at all that you recall throughout the history of the group?

DL: I remember the most serious accident that I remember was that one of our graduate students; he worked on chemical evolution with me and I should remember his name but I don't at the moment. He was doing a synthesis at the hood nearest my office and suddenly there was a bang, an explosion. This man looked around, and his glasses were encased with chemicals which had blown into his face; he was wearing safety glasses.

I am reminded of another story from the Donner Laboratory, with respect to Marilyn Taylor, who, as you all know, was the group secretary and/or administrative assistant for an incredible number of years and recently, incidentally, was awarded a Berkeley Campus prize or award for her tremendously effective and long service.

VM: Can we interrupt you there? We talked to her and she said nothing about this prize. What was the prize?

DL: This was given to Marilyn— I guess it was just a year ago at commencement time in the Chemistry Department. It's a very high honour. The highest honour the campus gives to the staff, the highest staff award. "To Marilyn Taylor for...."; I never did see the thing, so I can't give you the direct wording. For very long, very devoted and very effective service to the Berkeley Campus.

VM: Very appropriate. I'm very glad; that's very good.

SM: She's a living archive.

DL: I have not been able to remember names which she is going to come up with like that.

VM: I interrupted you; you were about to tell a story.

(Brief technical discussion about the recording.)

DL: This story involves Marilyn Taylor. One day she was in the Donner office; there were three of us also in there at the time. That was important because we had to say to each other: "Are you sure that she said it, the way I heard it" And everybody agrees it was right.

She was involved in getting a book that Professor Calvin wanted. She wanted to "borrow" that book. So she dialled the library number and when the phone was picked up at the other end we distinctly heard Marilyn say "Is this the University bar room service?" (*Laughter*). We all cracked up. Marilyn had to put down the phone and dial again later because there was so much laughter in the office. End of story.

VM: I noticed just now, for the first time since we began talking, or perhaps for the first time, that you refer to *Professor* Calvin. It reminds me that in this entirely informal

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group the only bit of formality was the way we all addressed him. It was not until very late in the day that any of the scientists called him by his first name.

DL: I think that was generally the situation. Here's John Lawrence, who was the director of the division in the Donner Laboratory, I think everybody addressed him as Dr. or Professor Lawrence. The reason I bring up this name is that even though he had an extremely famous brother (Ernest Lawrence), John Lawrence was not that famous and still I think those were the titles that were used. After all, the graduate students more or less had to do that.

VM: Well they did. But the staff members who, after all, had been working with him for many years, continued to do that. I remember talking to Mel Klein about this in the sixties. Calvin's wife would always refer to him as Melvin.

DL: Yes.

VM: Always — maybe not to the graduate students, but to us, certainly. And yet we never called him Melvin. One day Mel and I decided that we would. From then on, we called him Melvin. He never batted an eyelid. It wasn't as if he noticed!! Maybe he never had noticed!!

DL: I don't know when I started saying Melvin, but it was some time more or less way back. But I remember Joel Hildebrand, who we got to know very late in his life, an extremely distinguished chemist, I remember his saying to me one day "Oh, call me Joel", like that; he was almost annoyed to be called Dr. Hildebrand.

VM: Did you call him Joel?

DL: Yes, from then on I never said anything else. He obviously meant it. You and I have had this same kind of a problem. A graduate student who would say "Dr. Lemmon" for a while, and I would say "Oh, call me Dick". Some of the graduate students, at least, were reluctant to abandon the formality, thinking it wasn't quite proper.

SM: A lot would depend where they came from because formality...

DL: The Europeans are far more formal than the Americans.

SM: Or they were.

VM: I don't want to mention names at this point, but in the course of our stay here there were a couple of German postdocs. and, although we were all on first name bases, the Germans always called one another "Herr Doktor", or at least "Doktor", and we raised an eyebrow at this. We said to the one with whom we were more friendly: "Why do you call your German colleague in this way?" He said, "he's older than I am and I can't refer to him in a familiar way unless he does it first to me". We fortunately don't suffer from that.

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DL: I remember our Swiss friends, the Schmidts (*Hans and Kathi*), once got into quite an argument in front of Marguerite (*Dick Lemmon's wife*) and me about which of the two of them first used “du”, the informal form. Particularly, Kathi was very annoyed at her husband who said that you (*Kathi*) first said “du” to me, and she swore up and down that she did not. The same man, Hans Schmid, a professor of chemistry at Zürich, he like all able bodied Swiss men had to serve two weeks, three weeks, something like that, in the army every year. When they went on their military manoeuvres, Hans said here one day along came a graduate student of mine who outranked me militarily. The poor graduate student just didn't know what to do. If it had been the two of us by ourselves no doubt he would have said “Professor Schmid”, but with other military colleagues around him, he was supposed to use an informal form — you do this or you do that. The Europeans can really have a problem and it is exacerbated. I remember the story about the young boy who comes to work as a minor-grade technician, you say “du” to him, informal; then a couple of years goes by and he becomes a graduate student, and to a graduate student you say “Sie”; then later on, when he becomes a close research colleague, you are back to “du” again. There's always a little uneasiness the first person who starts the new title.

Are we recording all this?

VM: Of course.

DL: Nothing to do with the lab.

SM: What it pertains to is the lack of at a personal level in the Calvin group. This is how it started because Melvin was certainly not somebody who stood on ceremony.

DL: No he didn't. I remember one time at a seminar (I don't know if you were there). It was being given by a young lady, I think her name was Doris Chin, an organic chemist (*Editor: Actually it was Peggy Kwong*) working on some problem in organic chemistry. Anyway, Melvin kept interrupting her as she would say something and, after one of these interruptions, she suddenly looked right at him and said “Professor Calvin, will you let me finish what I am trying to say and then you can ask questions”. He said, “Oh, all right”. We were all surprised that he took a direct reproach very well. There was no sign in subsequent weeks or months that he was angry at her for saying, in effect, “shut up”. It wasn't quite that blunt but it was close. She pointed her finger at him and said “Professor Calvin, let me finish”.

VM: Was he always like that? Was he always attacking the seminarist immediately?

DL: Yes, absolutely. This wasn't supposed to be a formal occasion where you wait until the speaker was all though and then ask questions. We all understood that we could interrupt the speaker. I remember somebody, perhaps it was Melvin himself, saying that “when you get up to talk about your research, pretend that there's no audience but an old friend of yours who graduated in chemistry the same year you did, and he came in the lab. and said Hey, John, what are you doing?”

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VM: People used to get very worked up, didn't they, about the prospect of having to give a Friday morning seminar, especially the younger members of the group.

DL: Yes and that's why we had to abandon the original system where we just gathered, and Melvin would look around, and he would say, "Hey, Vivian, tell us what you have been doing recently". He had no more notice than that. The young people kept saying: "Yes, I can tell you, but I need to have my paper chromatograms; let me run upstairs and get them". So we would sit there for three or four minutes while somebody ran upstairs to get their data. Because of that awkwardness it was agreed that we would give the speaker notice a week in advance or a day in advance. It got to be more and more that way that the big lead time had to be given.

VM: When I first came in '56 people were getting a day's notice. The seminarist was decided at the Thursday lunch (*by the senior staff*).

DL: OK. Then it went to longer.

VM: Then it went longer because people were going into purdah as soon as somebody felt the finger pointing, they immediately stopped doing anything except crash (*preparing for the seminar*). They often stayed up all night getting this stuff done and they were all done in the morning.

DL: They couldn't have had very good command of what they were doing if they had to stay up all night. Again, as somebody said, if an old friend of yours walks into the lab. and says "Hey, what's your research? You aren't going to say "Well, come back in a week and I'll tell you". You know what you're doing, for God sake.

VM: In practice, Melvin wanted a little more detail than this.

DL: He wanted a little more detail, of course; that was the sticking point.

VM: I wonder if we can close this session by talking a bit about the way in which the new building came to be there, the Round House, and what it meant for all of you to move over. I guess the whole thing was precipitated by the approaching demolition of ORL when they built the chemistry building.

DL: Indeed it was. We sent in applications (*for building money*) to the National Science Foundation, the AEC, of course, and there was one other, might have been the — I think it was NIH. There were three granting institutions combined in this which meant we had goings over by committees from all three of them.

One of the things I remember about this planning for the new building was Calvin's ideas of this big laboratory where people could see what other people are doing and all that sort of thing. When we first drew a Round House with half of that circle the open lab. and the other half with the things that had to be enclosed (like isotopic work, various instruments, the darkened room, whatever, administrative offices) — when we first put this in front of the University architect, Louis DeMonte was his name, he said like this: "Don't be ridiculous — a **round** building? Do you know what

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you have to do to build a round building? You run a pipe a couple of yards and you have to make a bend or an angle of 15° and then another angle?” It’s impossible!” Well, all of us on the committee planning the building, Melvin, of course, was in charge things, we all said that we’ve got to work in this building and even if it costs a little more we want it done this way, a round building. So, finally, after a second or third meeting, this architect finally threw up his hands and let us go ahead on that basis. But it was quite a fight. Even though there is at least one partly-round building on the campus, one of the agricultural buildings (*Wellman Hall*) down at the other end of the campus is that way. What I particularly remember was the horror on the face of the University architect when that plan was first presented to him.

VM: What was the relation between him and Michael Goodman? Was he the actual architect?

DL: Michael Goodman was **the** architect for the building. Above him was Louis DeMonte who was the campus-wide architect. Anybody who was planning a building had to report to DeMonte and had to satisfy him. The Regents had given him the charge to have some kind of coherence on the campus. It was such a charge that never worked, obviously!

VM: I remember at the time of this planning that there were various ideas. I think it was Al who suggested at one point a semi-circle on a block and I think Michael Goodman didn’t like that.

DL: That’s right; it wasn’t a round building at first, it was a semicircle on a block. That was even worse from DeMonte’s standpoint than a totally round building.

VM: One of the problems that I seem to remember, but I may have this wrong, was the difficulty of sealing the semicircle to the block. I don’t see why it should have been a difficulty but I think it was.

DL: I don’t remember DeMonte bringing that up, but he may well have. It sounds reasonable.

VM: At the time, the thinking was, as I recall, that the big labs., as they finally evolved, would have this spoke-like structure with the work benches radiating at the edges and people would turn away from their benches, or whatever they did, and would face the middle. And the middle would be the discussion area where people would congregate.

DL: There was a big white space: you could put down your paper chromatograms.

VM: The big white table and the paper chromatograms was obviously a very strong focal point in the thinking of people. In Donner, you didn’t quite have a big white table equivalent, did you?

DL: In Donner? No, we didn’t; we didn’t at all. Nothing like that.

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VM: Was there a meeting room for you?

DL: We had the library at eight o'clock in the mornings — the library opened for business at nine. But that was all in Donner.

VM: It was not so cohesive, was it, as the (*ORL space*)?

DL: No. It was much better, of course, when we moved into the Round House.

VM: As far as I remember, we all contributed variously to the design of the building and the architects and the construction people actually did very well for us.

DL: Yes, I think they did. When we finally got over this major sticking point about the overall shape of the building. One of the problems that I think DeMonte had pointed out was, I guess, (*indecipherable*) the tile roof: the tiles had to be somehow wider at the bottom than at the top, they had to slope. If you haven't looked at the building with this in mind, you might do it some day and see how it was something of a problem to do that. It's the sort of thing that a non-architect would never think of.

VM: So did they have to have specially made tiles for the roof?

DL: I think so; I think they were specially built.

VM: Have they got some spares?

SM: Yes, they would have to be fan-shaped.

VM: That's right. What's your view of the success of the building as it was built? Do you think it fulfilled the hopes of the people who built it?

DL: Yes, I do. I think it was certainly better than what followed it because when George Pimentel became the director and saw all this space, this wide-open space, he started putting little cubby-holes (desk with a partition around it) (*everywhere*), going back to the idea of everybody in his little niche and people not interacting so much as they inevitably did in those big labs. I think the building was successful. We didn't expect that it would lead to six other Nobel Prizes in addition to Melvin's but I think it worked well.

VM: I would agree. I think that bearing in mind that you could never recreate ORL because it wasn't just the space, it was also the time that was characteristic of ORL, it was the place where the early group lived, and the group wasn't early any more in 1963. The whole character was beginning to change.

DL: I remember a specific example of that system being very good and that involves Karl Erismann from Bern, Switzerland, a professor of agricultural chemistry, something like that, a biologically-related chemist. He was the most repressed, introverted person who ever joined the Calvin Lab., the most introverted one. It was so hard to get a word out of that man. Somebody from the third floor, perhaps it was Al

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Bassham, got Karl to come down to a cup of coffee occasionally and people would ask him questions about Switzerland and about his research, etc. We visited Karl a year and half ago in Switzerland, in the Engadine where they were on vacation. Karl is not an inhibited guy any more, he's a completely different individual from this very repressed, very formal Swiss professor who joined our group many, many years ago. I am sure there were cases not so dramatic as with Erismann but I would cite that as evidence of that we brought him out of his shell, which made him a better scientist. He was brought out of his shell in a way that he could now more easily go to somebody and say "Hey, John, I know you're working with this infrared spectrometer; would you mind helping me, I have a little problem on it". Before, he wouldn't have thought of doing that.

VM: I think that was true, certainly at that period, for all the Europeans because all of them, even the English who are perhaps less uptight than others...

DL: ...but none of them so dramatically as Karl Erismann, in my view.

VM: The question that I have for you is: whereas the Europeans (and I presume the Japanese) found it a remarkable place in terms of social ease and interaction, what about incoming Americans: was it a novelty for them or was all American science like that?

DL: I would say not nearly so much. The wide open spaces, I can remember at least one American objecting to it for what I thought were very valid reasons. He said, here I'm doing something that's very touchy, and I have to have the right number of micrometers, or something, and somebody comes along and says "Hey, how did you enjoy your hike last weekend?" He would be interrupted at just the wrong time. I remember suggesting to him, and he did follow this, put one of these (*lab.*) stools out by the sink, where your passageway is, that says "Please don't bother me until noon time", or whatever. I think that at least in part worked. That's the only objection I can remember to that system (*i.e., the open laboratories*).

VM: Clearly, there are pros and cons in any systems.

SM: When you came in, Dick, you quoted us from a magazine you were looking at where somebody had written about the kind of atmosphere, the kind of ethos in a new building which you thought that this was the first time that things had been expressed in this way. Do tell us, again, what it was that you told us then.

DL: The article in C&E News about a new laboratory building, I think it's called the Beckman Chemical Sciences Building, at UCSD (La Jolla). In this new building, somebody was praising its architecture in having wide open spaces which would provide interaction between scientists of different disciplines so they could learn from each other what each other was doing. To my own recollection, Calvin was the first one who had that idea. Even Calvin, I can't give him too much credit, because the Old Radiation Laboratory, which the group moved into, I described, and I think not unfairly, as looking like a cow barn. It was a big wide open spaces and the laboratory benches were put in there and, I think, maybe it was only after the fact that Calvin

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decided “Gee; this wasn’t such a bad idea after all, to have the lab. benches in wide open spaces”.

VM: I think all of us who lived in that building recognised the value of that sort of structure, fortuitous as it happened with ORL.

DL: If you wanted some tubing to go through the wall into the next room, you just drilled yourself a hole. You couldn’t possibly do that in a normal building.

VM: As far as the name of that building, the first name of that thing, Chemical Biodynamics...

DL: Well, the “Bio-Organic Group” was the first name.

VM: How did that happen?

DL: That was the name that Melvin picked. Bio-Organic — he wanted to emphasise the applications of organic chemistry in particular to biological problems.

VM: When the lab. itself, the round building, was called the Laboratory of Chemical Biodynamics.

DL: That happened in part because the completion of that building our group got cohesive and put back together again in one place. Ernest Lawrence wanted to designate us, now, as a division of the Lawrence Laboratory. Am I right? I think it was. Ernest Lawrence died in 1958 and the (*Calvin*) laboratory was not yet under construction. It was under (*Dr. Edwin*) McMillan that the formal division came about. We were supposed to call ourselves a division so we picked the (*name*) Chemical Biodynamics, Melvin again picked it. Why we were not called the Bio-Organic Division, I don’t know. Somehow Calvin thought Chemical Biodynamics sounded more like photosynthesis, which was the main thrust of the lab.

VM: He was interested in the dynamic view of biology. So that’s fair enough.

DL: I guess it was a superior name and that was the new division that got that name.

VM: Soon after we moved in there I had occasion to call someone and ask them to send me something and was giving them the address over the phone, and gave them this word, this “Laboratory of Chemical Biodynamics”. I heard a sort of gasp at the other end and they said “You have forgotten the ‘astro’”. They thought we’d got the chemical and the bio and the physics (as represented by the dynamics) but we left out the astronomy. One last question, because I know that you have to leave soon. When ORL was going to be demolished, how was the space found in Life Sciences to accommodate the (*ORL*) people?

DL: Vivian, this is a question that you are going to have to ask Al Bassham. However, there is a campus-wide faculty/staff committee that was charged with this business of finding space if a new department was formed; there’s a cancer research laboratory

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now at that end of the campus. It was obviously a very legitimate request on the part of Calvin to be provided with some suitable temporary space until the new building was built. That was the “suitable” space that they found.

VM: That must have reacted very badly on the relationship between that (*the ORL*) group and (*the*) Donner (*people*) because they were now much further apart.

DL: There was still going back and forth but I have no doubt it was less intense than when the other group was in the Old Radiation Lab.

VM: And Calvin had his little electric cart, you remember.

DL: I would have thought it was a good idea — Melvin had already had his severe heart attack — should have a little electric cart (*to go between his office in Old Chemistry and the Life Sciences Building*). It turned out that he didn’t use it very much and just quit using it which must have meant that he got down to LSB less frequently than he used to when they were in the Old Radiation Lab.

VM: There was a very marked difference. The whole character (*of the group*) changed and it was only partly recovered when we went back into the Round House.,

DL: It sort of mystified me because I thought it was fun to ride in that little electric cart. I don’t know what Melvin objected to.

SM: I know you’re in a hurry and perhaps the answer will need to come some other time. But we are talking about the interaction of personalities in the group, particularly that of Melvin, of course, and how his personality changed the ethos — or originated it. Since he has not been someone who’s very easy at relationships on a personal level, as opposed to the professional personal level, how much influence do you think that Gen might have had in the cohesion of the group socially?

DL: I think she was a very important effect on Melvin and on the group in general. I have heard it said by more than one person that Melvin was extremely lucky, both in his wife and his secretary. I never heard anybody say that that wasn’t the case. They were both just magnificent supporters of Melvin’s work. I don’t suggest that he would not have gotten the Nobel Prize without these ladies, but certainly they contributed a great deal to his professional successes. With respect to social things, like having dinner parties, without any doubt it was Gen that was always proposing these things and was sort of the life of the dinner parties at the Calvin’s home while Melvin tended to sort of drift off in the corner by himself.

VM: Melvin on these occasions would go so far and then something would click and he would get fed up with the occasion and drift off.

DL: I often thought that I think Melvin would have remained a better director (*of the lab.*) if he had never gotten the Nobel Prize. Because he became somewhat stiffer, more formal and harder to communicate with, at least it seemed so to me, after that Nobel Prize than before. He tended to annoy people by seeming to be an expert on any

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subject that came up, however remote from science. If something came up about who's the better man, Dole or Clinton, if somebody said "oh, Dole", Melvin, if he disagreed, would just jump on this person very hard. He wasn't any more an expert on other matters than the rest of us, granting his tremendous superiority as a scientist. I think in that respect the Nobel Prize was not good for his group, however good it was for him.

SM: Do you think this was because he saw himself differently, or because, since he had to be absent a lot after that...

DL: I think he saw himself differently. He was in a rare, very select group of people now, whereas before he was a professor of chemistry. There are a few thousands of them but Nobel Laureates aren't so many.

VM: He was also mixing with a different crowd of people. He wasn't mixing almost entirely with scientists as he had been previously.

DL: That's quite right. He became a member of the Bohemian Club, and all that.

VM: And the President's Science Advisory Committee and he had a role...

DL: ...in all these assignments in Washington.

SM: He took himself seriously in the sense that he had to perform.

DL: It's just an impression. I don't know how true that really is. I remember somebody over in the Medical Center (*in San Francisco*), about a year or two, let's say '62-'63, a year or two after Calvin got his award and I don't know that he had Calvin in mind; he was a professor over there and I don't remember his name at the moment. He wrote an article in *Science* about how he felt the Nobel Prize was really hurting science in general, not helping it. There was this fierce, intense rivalry and people tried to politick, how about sending a letter on my behalf and all this kind of thing that was going on. After the person gained his award, a new building was built. Calvin's came before the award so it doesn't apply to him. But a new building is built and the person is a new director of this grand new research institute and he tended to disappear into his fancy office and was not so available any more to his students and research collaborators as he used to be.

VM: Not only was he (*Calvin*) not so available, he was almost unavailable. In the latter days, he was rarely in the working part of the lab. at all. If he was there, I have to say you usually had to look for the television cameras that were behind him.

DL: You might agree with me that maybe it was not for the best for his lab. and his group that he got a Nobel Prize.

VM: It's difficult to come to a decision either way. It's the passage of time, people mature, that's what happens to them. The group would have developed in some way or other,

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it couldn't have remained the way it had been in the glory days of the fifties. It must have changed in some way and this was one of them, one of the possibles.

DL: By the way, we must all remember, as you two I'm sure know, that the Round House, now the Calvin Laboratory, was not a result of the Nobel Prize. It was all funded and done and in the works when the announcement came from Stockholm.

VM: While we are talking about this new building, and I don't know whether I may have mentioned this in other interviews that we have already done, that Al Bassham had a glassed-in office, you may remember in ORL, in one corner of one of the labs., one of the few partitions in the building I'm bound to say. On the wall of this room he had a little story about the three stages in the life of a great man, the anonymous great man, not any particular one. The first stage where he does his great work under poor conditions (pink string and sealing wax); the second stage is where he is designing his new building; and the third stage is when he is showing the visitors around the new building. That turned out to be not totally untrue in this case.

So anyway, I wonder perhaps we ought not to leave it at this point.

DL: Yes; I would like to leave about now.

VM: Perhaps we can pick it up again when we have talked to some more people.

DL: You've exhausted me, Vivian.

VM: No, no. There's a lot more where that came from.

SM: It occurred to me, that when Ann (*Hughes*) mentioned that we were going to have dinner together at her house, with Ed and Millie Bennett, she was under the impression that we were going to do this sort of thing then. Then she learned that it would be a one-on-one interview. I am wondering whether in addition to the one-on-one interview, one might not see what sort of patterns, as it were.

VM: Let's try it. The worst that can happen is that it becomes unintelligible and we can't use it.

DL: The most important interviewee will be Marilyn Taylor. She knows everything about everything.

VM: We've already talked to Marilyn extensively already and I'm sure we'll come back to her as well. For the moment, let's call this a day then until the next time.

DL: OK.