

Chapter 22

GUS D. DOROUGH

Livermore, California

July 5th, 1996

VM = Vivian Moses; GD = Gus Dorough; SM = Sheila Moses

VM: This is talking to Gus Dorough in Livermore on July 5th, 1996.

What was your background, before you went to Calvin, and how came you teamed up with him?

GD: I think I should start back in the year 1941 was when I arrived at Berkeley. I had gone to San Diego State College for the first two years and the practice in those days for people interested in science, if they couldn't go to Berkeley originally, went to Berkeley in the last two years; you would take the first two years at a state college or perhaps UCLA or whatever. In any event, I went to San Diego State and then transferred in what was my junior year to Berkeley.

It was a very different time, then. The College of Chemistry was still very much G.N. Lewis' College of Chemistry. The faculty were pretty well along in years. Many of them had come with Lewis, so that was before World War I some time, and Calvin in a sense kind of stood out because he was a young faculty member and very out-going and very easy to approach. It isn't that some of the older ones weren't but they struck a note of awe in the minds of a typical undergraduate of that time. So, I arrived at Berkeley and I think, perhaps that very first year or at least shortly thereafter, I took a course from Calvin; I can't even remember the title of it but it had at least one unique feature. I remember we did literature searches, original literature searches, and developed knowledge about some subject not by taking out a textbook but by actually going to the original literature and writing up a report on that and giving the report verbally in his class. It was then critiqued by Calvin.

VM: So the classes, presumably, were not too big.

GD: That particular one was pretty good size but, as I remember, we broke up in teams. Everybody didn't give a verbal talk but everyone was involved in the literature

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searches. I'm not sure that's all we did in the course. I am sure Calvin lectured as well and he struck me then as a very, very good lecturer; very easy to listen to.

VM: You were going to major in chemistry?

GD: Yes. To back up a little bit: in high school I had a couple of brothers who were physics and chemistry teachers — I still remember their names: Gilbert — and they somewhat steered me to science curricula; I wasn't quite sure whether physics or chemistry was what I wanted. But I was pretty sure I wanted one or the other. And at San Diego State I had again good mentors and good teachers so I sort of got oriented more into chemistry. When I transferred to Berkeley, I was pretty sure chemistry was what I wanted so I entered the College of Chemistry there.

I really didn't see much of Calvin. The war came along, of course.

VM: This had been before the war when you first got there?

HR: I started Berkeley in August of 1941. In December of 1941 the war for the United States started in earnest and everything got quite changed, quite immediately. I recall that the first thing that happened was that we were informed that we could probably continue in school on a student deferment because of our field, that is science, but school would be continuous.

VM: Can I ask a question? What was the position with the draft at that point?

GD: That's why I say a student deferment would allow you to get through to graduation.

VM: You had already registered with some body, had you?

GD: Yes, I was registered. When the war started, I went down, perhaps naively, but I thought "gee, maybe a naval aviator" would be a good thing to be.

VM: Out of San Diego, why not?

GD: I applied but was quickly rejected for childhood asthma and other complaints that they said wouldn't work well for them. So, I was just in the draft and given a student deferment as a science major which was sort of standard at that time. I would have graduated with a bachelor's in June of '43. Because of the continuous classes I got out in February 1943 at which time, again because of the war, the College of Chemistry invited me to stay on as a graduate student. Up until the war that was, I think, quite not the practice. They sent their students away to other schools, got good students in from other schools for the graduate courses.

The College of Chemistry might have been regarded as a little bit ingrown in the sense that all of the faculty members that Lewis appointed starting, I don't know, again way back before World War I sometime, were all UC graduates. I didn't know this until later but Melvin was sort of the first exception since before World War I. I think his thesis professor might have been a Berkeley graduate, I'm not sure of that.

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But he was certainly a graduate of the University of Minnesota and he was appointed to the faculty. He was the first non-UC appointment for a long time. That was kind of interesting. I gather he stood out because of his just general friendly, outgoing characteristics — easy to talk to and he was quite a younger member of the faculty. During my senior year I remember I did work with two of the older members, C.W. Porter and Thomas Dale Stewart, both of whom are long since gone now, I guess. They were so-called organic chemistry professors. I think everybody at the College of Chemistry was really a physical chemist by design and you might sort of dabble in organic chemistry on the side. My recollection was that it was certainly not a strong school for classical synthetic organic chemistry as you found in the big mid-western schools. It was really a school of physical chemistry, or chemistry as a whole, and you could sort of emphasise organic if that was what you wanted.

VM: That suited you well, didn't it?

GD: Yes. I was sort of deciding whether I wanted to be a physicist or chemist, anyway. And Melvin, of course, I guess I would initially describe him as a physical chemist but with a strong bent for looking into all kinds of other things. When I graduated, as I say, I did some work, so-called individual honours work; but as a senior you could work with a faculty member and do a little research project. I think I did one with Porter and I know I did one with Stewart and quite enjoyed that. When I graduated and they told me I could stay on as a teaching assistant if I desired, I asked Melvin if I could come with him as a PhD. candidate. He agreed. We didn't really start much in the way of research work because your first year you are still pretty busy taking courses and then lots of other things sort of got in the way. I got quite ill in the fall of '43: I had a lung abscess, pneumonia and all kinds of terrible travails which put me in Cowell Hospital, the hospital on the campus, for some months. I think I got out early in '44. Not too long after that, it was suggested strongly that I should go up The Hill to the Radiation Lab. and go to work there. Which I did and then that was, of course, the Manhattan Project.

VM: And that was not with Calvin?

GD: No, it was not with Calvin. I was now a full-time employee of the Radiation Lab. doing war research work on the atomic bomb and still was registered as a graduate student and was still at Berkeley. I came down in the evenings and did some research work. But it was a very mixed-up time. Melvin was heavily involved in — he had a group of people, I have forgotten all their names — Ferguson...

VM: Was Martell one of them?

GD: Gee, he might have been. They were studying chelates of various kinds.

VM: Yes; Branch. Was Branch one of them too?

GD: No. Branch was, of course, a full professor. He (*Calvin*) had written a text with Branch. I don't recall Branch being involved in that project but he may well have been. Branch had kind of, my impression, was although a very bright man, he had

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sort of “retired”. He spent a lot of time at the Faculty Club playing bridge. I took his course in theoretical organic chemistry, which he and Calvin had written together, from Branch — not as inspiring a lecturer as Melvin. He was certainly a very typical chemist in his own right. In any event, Melvin was heavily involved in these chelate studies, I think to try to find an oxygen-carrier, something they could basically package oxygen up in a solid state rather than putting it in tanks, using chelates for that purpose. He had quite a large group of people working with him. I wasn’t involved directly in that although — I’ve forgotten what it was — he did ask me to look into the synthesis of a chelating material and I did that for some period of time, unsuccessfully: never was able to devise a route to this.

VM: Had you decided what your thesis title was all about?

GD: No, it was still kind of up in the he air. Because of the war and because I was now working full-time on the Manhattan Project which, in due course, sent me to Oak Ridge and then down to Los Alamos; you know, it was a very mixed up period. I wasn’t around Berkeley very much. I did manage to basically complete all the course work I needed to take my prelims. for the doctoral examination and get that out of the way. But then I was off at Los Alamos.

I returned in February of 1946 and at that point I had a National Science Foundation fellowship. As I say, I had really completed my course work and I had my prelims. out of the way, I had financial assistance from the National Science Foundation so I had a year ahead of me at least basically unfettered just to do research.

VM: Can I ask you a question about the earlier period before we move away from it? Did you know Sam Ruben?

GD: Yes. Not well but I was there when he died.

VM: You remember the occasion of the accident?

GD: Very vividly. I don’t know whether he was a close compatriot of Melvin’s or not; I just don’t know that. It was a very tragic event. I think it was sometime probably along in ’43. Sam was working with phosgene of all things, quite a large quantity, and we had a lot of phosgene, I remember, in the storeroom, little glass vials made in Germany, and I think one of those vials broke in a Dewar of liquid nitrogen. Of course it boiled right up in his face. They brought him out on the lawn: he was quite mobile and kept wanting to get up. I remember Latimer came out and just insisted he lay still until they could get an ambulance or something, gurney down from Cowell (*Hospital*). They took him to Cowell and within a couple of days he died. That was a very vivid memory.

VM: Were you aware of the photosynthesis work that he had been doing before he was diverted onto war work?

GD: No, I was not familiar with that.

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VM: Or his discovery of C^{14} ?

GD: I knew of that but I didn't really know Ruben well except to know that he was very well regarded. He was quite an up and coming young faculty member.

VM: How about Kamen? Did you know him?

GD: Yes, I did get to know Kamen, quite well as a matter of fact. Kamen was on the project at Berkeley and I got to know him there. He ran into all kinds of security problems which you probably know about. To jump ahead a minute, when I graduated with my doctorate I went to Washington University in St. Louis and taught chemistry there for some years. Kamen also went to Washington U. in the Medical School. In the forties and fifties I got to know Martin very well, just as far as a friend; we never collaborated. He was a very nice gentleman.

VM: Anyway; back to your return to Berkeley.

GD: Finally, after '46, the war ended, of course, in late '45, August '45 and I stayed on at Los Alamos cleaning things up and left in '46. Melvin said, "OK, let's go to work". I was basically free to do that. It was a most exciting year. I probably will remember it as one of the highlights of my life, if you will. I did do some voluntary teaching, just as a teaching assistant but not with pay, just to sort of do that. Other than that, I worked in the lab. I remember Melvin saying, "well, we won't try to describe a detailed route to your ultimate thesis but we have some areas here to explore. Let's explore them, let's see what kind of develops and we won't try to pick a specific topic or a specific thing now". That seemed reasonable because we had some interesting areas to work in.

The area that I sort of eventually concentrated on was dealing with the system of porphyrins which is the basic ring structure of chlorophyll and the iron haemin and haemoglobin. I guess, I don't know — Calvin had several graduate students; Sam Aronoff was one; Russ Ball was a master's candidate; and there was a man at Antioch College, in Silver Springs, Ohio, I believe, that had explored the synthesis of tetraphenylporphyrin; the phenyls are hooked on the carbons that hook the pyrrole rings together. The reaction of benzaldehyde and pyrrole leads directly to this porphyrin and in fairly good yields. Rothman had found two major porphyrin-like species and he thought they were the NH-isomers. If you have two hydrogens in the middle they could be either of two configurations which, on theoretical grounds doesn't seem to make sense. They should exist easily and separably. I think that intrigued Calvin some and he and Aronoff looked into it and decided that really it was a porphyrin and a chlorin: one of them was the porphyrin tetraphenylporphyrin and the other material that Rothman was thought was an NH-isomer was, in fact, the chlorin molecule which is the one with two extra hydrogens. It's the basic ring structure of chlorophyll. Here are these two very clean porphyrin structures, one the porphyrin ring and then one with two extra hydrogens like chlorophyll, fairly easily made and with lots of interesting spectral and other properties to study. They kind of represent the basic ring structures of important biological materials. Obviously,

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Melvin was interested in photosynthesis; it kept coming up in conversations. But he had not started the carbon work then. This came, I guess, shortly thereafter.

The short of it is: we discovered a reaction that would transform the chlorin quantitatively to the porphyrin, take the two hydrogens off. The reaction was the chlorin molecule with some quinone — naphthaquinone, I believe it was o-naphthaquinone — and quinone gets converted, the hydroquinone, picking up those two hydrogens off the back of the pyrrole ring of the chlorin molecule. The reaction, which occurred only in light and turned out to be independent of the naphthaquinone concentrations. It depended only upon the light intensity and the concentration of the chlorin, which was kind of puzzling. How could it be independent of the quinone?

It turned out eventually what we discovered was lots of evidence that the light excitation which was in the furthest red band of the chlorin (nothing else absorbing light), that transition takes you from a ground singlet to an excited singlet for the chlorin molecule. That excited singlet fluoresces, comes back down to the ground state. Or, some of it can get diverted to a triplet state which G.N. Lewis had studied. He was the first one to propose that phosphorescence really results of a triplet-singlet transition which is basically a forbidden transition, so of low probability. If some of the excited singlet can get over by just non-radiative processes (collisions, or whatever), over into this triplet state which lies normally below the singlet state, excited singlet state, that you can then kind of trap this molecule in that triplet state. This trapped molecule either can get brought back to the ground state by non-radiation processes (collisions, or whatever) or, if you put it at very cold temperatures which is what Lewis did where the collisions are much less, it just sits there until it decays by radiation transfer. So, a light comes out and that is phosphorescence. This was Lewis' proposal for phosphorescence which I think is presumably widely accepted now.

What we found was that this actual chemical reaction was taking place through the triplet state. That was a reactive species. It was independent of the quinone, as long as there was enough quinone around, and it could snag off one of these triplets before it was knocked off by some other process; didn't matter how much quinone was there. The whole key was simply to excite this molecule and get the triplet that reacted quantitatively with the quinone and made the hydroquinone. That was really a fun piece of research I got to because of its closeness to the work that Lewis did. I had several long conversations with Lewis. He was always held in such awe, and considered to be a little bit of a curmudgeon if you made dumb comments to him, but I found him extremely kindly and supportive and very interested in what I was doing, suggestions of what I might try...

VM: So he was aware of research going on in the department outside his own immediate area.

GD: Oh, yes. I think so. Partly. you've heard of the famous colloquia which he chaired right up to his death. Those were held once a week, I've forgotten the afternoon now but many people can tell you. Lewis sat in the same chair, all the time, up in the front of the room, facing the blackboard, smoking his cigars. They always had two talks:

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one given by usually a graduate student reporting on perhaps his own research or perhaps even something from the literature, and then one of the faculty members reported. They were pretty awesome. I gave several talks before that (*group*) as a young graduate student. I still remember it, that's something!

VM: Did Lewis interrupt you to ask questions?

GD: Not frequently but he would often ask questions. You started this by saying was he (*Lewis*) knowledgeable about what was going on in the department. That was one mechanism. He had such a broad interest in things, no matter who was talking or what they were talking about, he usually had cogent remarks to make.

VM: I have two questions. One of them: were you still in Calvin's group when his own group began to have their Friday morning seminar series?

GD: No. I had left by then. I left in February '47.

VM: I see, OK. And the second thing was: by the time left you, you would no doubt have seen this — Calvin's group by 1947 was already becoming a distinct entity. Was Lewis interested? Did Lewis interact with that group?

GD: Now, he died in 1946.

VM: I see; OK.

GD: Yes; he died in March of...That's another thing I remember but not terribly well. I came back in February of '46 and a fellow graduate student, Paul Gilles I remember, came to my lab. late at night (I don't know: eight or nine in the evening, or something) and said did I know that Lewis had died that afternoon? I didn't. He died in his lab. of a heart attack.

VM: When was it that you had seen him (*Lewis*) in these seminars?

GD: My earlier graduate years. I started graduate work in '43 and so I saw him then. I think I actually might have given a research seminar on some of the honours work I did with Stewart or with Porter. I had several times in the barrel.

VM: I would just like to reconstruct the scene in Lewis' seminars because it's very reminiscent of Calvin's own seminars. You say Lewis sat at the same position at the table...

GD: Always, yes.

VM: ...this was a long table running the length of the room?

GD: It was a somewhat shorter table because it was a room in Gilman Hall (it still must be there). The faculty sort of sat up in the front chairs — can't remember; they sat around the table; I think they just sat in the front chairs — and behind were all the

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graduate students and younger faculty. People were free to ask questions. Sometimes somebody would ask a rather foolish question. That was very dangerous with Lewis there. He might make some rather pungent remark about it and I guess that's where he got the reputation of being a little bit of a curmudgeon. I don't know this first hand. His reputation was he didn't suffer fools very gladly.

VM: Nor did Calvin!

GD: Nor did Calvin. In fact, I want to talk about that. That's also his (*Calvin's*) reputation and I worked with him closely for a year, I made some dumb mistakes, did some foolish things. Never was chastised in any mean way. If it was clear he wasn't happy with what I had done, he would sort of get a serious mien on his face that was clear that this was not going the way he wanted it to. But there were never any unkind words. In fact, it was always the reverse. There was encouragement and sort of recognition that humans are frail and you do blow things now and then, but let's get on with it. It was strong encouragement to do better and not anything that was mean or cutting. That year was just a fun year. I would describe it as lots of humour. I wish I could remember some anecdotes but I can't remember a single one but I remember we had a fun time together.

VM: Where was it? Where were you working?

GD: I worked in the Old Chemistry, a little room right on the south-west corner of the old buildings (it's long since gone now) and on the lower floor. My recollection is that Melvin would come by, if not every day, certainly every other, every third day at the most. We would have a good conversation. He just wouldn't dash in and dash out. He'd come in. He'd want to look at results, he'd want to talk about what I was doing now, he would make a ton of suggestions — if I did them all I would never get any sleep ever. You had to kind of pick and choose, you know. If you picked the right ones and they worked out, that would obviate having to do any of the others anyway. It was a good exercise to try and figure out, of these many things he might tell me about next time, which one should I zero-in on?

I would describe him as just a great mentor. He was supportive, he was important to you, he had a great curiosity about physical things, about chemistry. We'd also...you'd digress and talk about all kinds of other subjects as well. He was just a friendly, outgoing guy, had a great smile, as you know, a dimple. He would sit in my office and do this gesture which I guess he has done all his life, he would sit in seminars and do this, maybe in the seminar you described, that's probably what he was doing.

VM: playing with his hands.

GD: That was a very familiar gesture. I guess what I am trying to say: I remember that year with Calvin, that's really the only time I worked closely with him. It was just a great year. Of course, he had a lot of other students and a lot of other activities going on, and he had probably begun the move over to the Old Radiation Lab. where his

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group got started and ensconced before I left, but that's 50 years ago and I just can't remember.

VM: I should have come around 20 years earlier and done this, but it wasn't possible at that stage. You really don't recall any contact with the people in ORL.

GD: I knew Dick Lemmon and Bassham, Jim Bassham, they were fellow graduate students. I think they graduated somewhat behind me. I graduated in February '43, I think they were a little later. I knew that he (*Calvin*) had got on this idea of using carbon-14 to track the path of carbon in photosynthesis but I was just happy as a clam with my little porphyrin problem, which was very peripheral to photosynthesis and yet it was studying some very basic physical chemistry of a very fundamental ring structure.

VM: As a resident in the Old Chemistry Building you were not drawn into activities with the people in ORL.

GD: I'm not so sure he had moved into ORL. Do you know when that occurred?

VM: It was sometime in '46, I think, maybe later. There was also a group in Donner, of course; in the Donner Lab. He group started in Donner. Lemmon certainly and Bennett. Do you remember Bennett?

GD: Yes, Bennett.

VM: They certainly started in Donner and indeed stayed there until the round building was opened. The photosynthesizers, the carbon people, moved into the Old Radiation Lab. which Andy Benson set up for them and that must have been — Andy's told us...

GD: Benson was one of those who worked on the chelates too, I think, during the war. (*Note: Benson did not work on the chelate programme during the war. He was away from Berkeley as he was a conscientious objector.*) It was a Navy-sponsored research programme or something. He had several...have you heard of Lloyd Ferguson?

VM: No.

GD: (*Indecipherable*) He was a graduate student of Calvins, about my vintage, a black man: this was in a time when the US was much more racially divided than it is now. After graduation he had a terrible time, Lloyd Ferguson did: he had a PhD, very well qualified, getting a job somewhere. He wanted to teach; he finally got a teaching job in a small southern university which I think was very difficult for him. I think he ended up going to Washington, DC. There's a very well known black school there, name I can't remember (*probably Howard University*). I think he eventually came back to the west coast. He wrote a theoretical organic text, somewhat patterned after Branch and Calvin, but, I guess, had updated with newer things. I have lost total track of Lloyd, but I remember Lloyd as being one of the people in that chelate group. Benson was another (*no; this is in error*). Sam Aronoff, I don't know whether he stayed around to work in that area or not; he was another early graduate.

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VM: He was contemporary with you, more or less.

GD: Yes, well, I followed in his footsteps in a sense because Aronoff did some of the early work trying to ferret out what this porphyrin and chlorin were and, as I mentioned, Ball and Aronoff elucidated the porphyrin and chlorin structures. I don't know whether that was published or not. I remember trying to get enough of the porphyrin and chlorin that we could actually run carbon-hydrogen analyses. They're not very different because in that big ring structure, adding two hydrogens doesn't change things very much. We did do that and, as I recall, the results were quite consistent with the fact that chlorin did have two extra hydrogens, a hydrogen count that was higher. But I don't recall that we ever published that.

VM: In addition to the people that Melvin had in Donner and ORL, whether or not that coincided with your tenure, about how many students did he have with him in Chemistry?

GD: I would think during my career there were very few but it began to build up because of this carbon work.

VM: For or five: that sort...

GD: Half a dozen people.

VM: Were you a coherent group among yourselves? Were you socially friendly?

GD: We were socially quite friendly and we would sort of talk about our work with each other but we didn't have collective seminars just of his group at that point. I think it became much more focused, you know, when it became this path of carbon activity with quite a few people involved, really a focused activity with a very clear aim of where you are going. It wasn't a sort of individual PhD thesis type of research.

VM: Before that, in your period, it was much more like the usual arrangement between the professor and a group of students each one of which is partly independent...

GD: ...each one which is sort of working a somewhat separate area but all along Melvin's interests have always been very broad, that has been my impression. He had Aronoff and Ball working on porphyrins and chlorins and I kind of followed. I think after I graduated he might have had some other graduate students looking at that same system because it is an interesting system, if you will. He had these folks doing chelate work. I don't know what Benson's thesis was; I presume on the chelates.

VM: I don't remember whether he told us or not but clearly we could ask him. (*It was with Carl Nieman on the structure of sphingosine.*)

GD: He (*Benson*) was a sort of transition. As I recall he was working on the chelates, the oxygen-carriers, and then he was involved, as you say, in the early set up of the lab.

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VM: He was also involved with Ruben and Kamen in the earlier period before Ruben died. He was involved in some of that work.

GD: Where did Benson go? I sort of lost track of him.

VM: After he left Berkeley?

GD: Yes.

VM: I can't remember the exact sequence: he went to Penn State and then he went to UCLA. But for 33 years now, or so, he has been at the Scripps Institution of Oceanography in San Diego. That's where we saw him, hale and hearty and very lively still!

GD: I just remember him mostly socially, a very nice helpful fellow. I think he was a year or so ahead of me. It was still, I think, up through the time I left it hadn't quite ceased to be G. N. Lewis' College of Chemistry. The students there, even as undergraduates in the College of Chemistry, were unique. They didn't take the same courses as those in the College of Liberal Arts did (*Dorough might here be referring actually to the College of Letters and Science*). In fact, you could actually get a degree, a BS in chemistry, and take practically nothing else but chemistry, physics and mathematics, which was kind of unique. Some people think that might not have been a good idea.

VM: The College of Chemistry wrote its own rules for degrees?

GD: Yes. It was a totally separate college. If you wanted to, you could take a chemistry degree, a BA degree in chemistry, in (*actually the College of Letters and Science*) but then you had other requirements which the College of Liberal Arts laid down — history, English and a much broader spectrum of courses. You came out from the College of Chemistry a very well educated chemist but you might not have had a very broad education.

VM: (*Noise of aeroplane flying overhead — this conversation was being recorded outdoors.*) I think you might have to repeat that when this thing has gone away! So you were saying you came out of the Chemistry...

GD: Where's it gone?

VM: It's gone behind the house.

GD: Horribly loud!

VM: You were saying, you came out of the College of Chemistry...

GD: I guess it was a criticism of the time that you might graduate from the College of Chemistry and be a very well educated chemist because you've had basically chemistry, physics and mathematics for four years, very well prepared for any

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graduate school in the country. But you might not be a very well-rounded individual in the sense that your academic training was somewhat narrow.

VM: That's exactly the system that I grew up under in Britain, it's exactly like that, very concentrated and very focused.

GD: You gain your appreciation of other fields and other activities by some other osmotic process.

VM: By talking to students in other areas, that's part of being at university. You don't just derive...lectures.

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GD: In that regard, as an undergraduate at Berkeley, if you were in college in the College of Chemistry you got to know your fellow students pretty well, it wasn't such a huge mob. Particularly in the junior and senior years the classes were smaller, you got to know those awesome professors pretty well. Although they were quite a bit older in years and had these big reputations they were friendly people.

VM: Where you worked as a graduate student in Old Chemistry, were you in a room by yourself?

GD: I shared it with Russ Ball until he graduated and then it was mine, just a little cubby-hole down there. The things we did in a lab. — there were no hoods and I had all these organic solvents I was dealing with the whole time — benzene...I don't know; God took care of us! We survived just fine. Occasionally terrible accidents like Sam Ruben's happened but you were kind of responsible for your own safety. You knew that. The things you were dealing with were generally toxic and had to be treated them with respect.

VM: Were the other members of Calvin's group in Chemistry close by in adjacent rooms?

GD: We were scattered up and down the bottom, the first floor — it was kind of a basement floor of the Old Chemistry Building. His office was up on the first floor where there were at that time organic chemistry labs. So, we had a stairway to climb to go up to his office.

VM: Were you equally in communication with graduate students working for other professors or was there any particular sense of cohesion in Calvin's group at that time?

GD: Yeah; you knew your fellow graduate students. You went to the colloquia with them, you went to courses with them, you got to know them socially in various ways. I got to know Mike Kasha pretty well. He was G.N. Lewis' last graduate student.

VM: So the graduate students were a body among themselves and were not divided up into professorial groups.

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GD: No, we were certainly encouraged. I got a lot of help from Mike Kasha and from, I think, McClure (*spelling?*).

VM: How do you spell Kasha?

GD: K-A-S-H-A. He's a professor at Florida or Florida State, I forget now. He came...I'm digressing all over the place.

VM: That's fine.

GD: There's a G.N. Lewis annual luncheon, supposedly for compatriots of Lewis or people who were in the College of Chemistry in Lewis' time, so that rules out pretty much any students after 1945. It's kind of an "old mens' club" now but we have a luncheon every year, slightly dwindling group. Melvin always comes. I have noticed, unfortunately, a considerable decline in his health since he's come to those. Actually, it's very sad to watch. Back to the point then: Mike Kasha was the featured speaker at the last meeting and he just gave a talk about his remembrances of Lewis and the late times. He also wanted to clarify — there's a lot of rumours about G.N. Lewis' death, that he committed suicide or some crazy thing. Kasha is firmly in the belief, and I think all the evidence at the time would certainly support him, that Lewis died of a heart attack. He happened to be in the lab. at the time.

So, then I guess, what I have tried to say that I had this marvellous year with Melvin as a sort of mentor and friend, compatriot and colleague and PhD advisor, etc. Got to know his wife, Gen, a little bit; she was a most charming lady. They had just been married rather recently, I think.

VM: I must stop you there; it (*the tape*) is just about to run (*out*).

(*Tape turned over*)

What sort of contact have you maintained with him and with the other people since those days?

GD: Well, as I guess I indicated earlier, I went off to Washington University to teach chemistry in 1947. I maintained moderately close contact. I came back to Berkeley and worked in the Old Radiation Lab. I did a series of experiments with oxygen-18...

VM: When was that?

GD: About 1949, perhaps; something like that.

VM: How long were you there for?

GD: Just a summer leave, from Washington U.

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VM: Oh; we'll have to explore what you remember of *that* period in a few minutes, at any rate.

GD: I don't remember too much about it except that I worked in the Old Rad. Lab. and Bassham and all those fellows were there. Everybody was busy chromatographing on paper, two-dimensional paper chromatography. We had a little sample of water, of O¹⁸ water, didn't have much O¹⁸ in it, it was not highly enriched. We did some experiments, I guess mixing algae with water and we were going to try and find where the O¹⁸ went. We suspected that it ended up in some carotenoid-type, I mean carotene-type product. We isolated the carotenes and looked for oxygen-18 in them. It appeared enhanced in both dark and light, so the experiments weren't very conclusive and the oxygen-18 sample wasn't really rich enough to do very definitive experiments. We wrote up a little, tiny short note, kind of saying this is a teaser, probably could be some fruitful work done here but this is what we did and it was inconclusive because we didn't really have the right tools. It might be something that someone might want to explore at some future.

VM: You worked in the main lab., in the big lab., in ORL?

GD: In the Old radiation Lab. I think I came back one other time but I can't remember what the heck I did! It seems awful but I must not have led anything very specific.

SM: I just wanted to check on which time you were working...

GD: My guess is that it was about 1949 but I'll have to...I think I can find the paper. (*It was The Path of Oxygen in Photosynthesis in the Journal of the American Chemical Society, 73, 2362 [1951]*).

VM: I've seen the paper. That's actually how I found your name. I hadn't heard of you before until quite recently when I came across that paper and asked "who is this guy"? He's writing from this lab. and I didn't know him.

GD: Well, that was just a very short contact; I was there two or three months during the summer. When I came out on things like that we tried to pick some, you know, rather narrow little topic and in a couple of months you might be able actually to accomplish something. That was a neat little project. The oxygen-18 sample was too small and not enriched enough to do anything definitive.

VM: How long were you in St. Louis?

GD: I was there about seven years or so. Like many small privately-endowed colleges, the support of chemistry and physics became an increasing problem. I was a pre-World War person, if you will; my idea of academic research was you taught at a university and you did research of your own choosing, let your mind explore what you will. That was the great tradition. When chemistry and physics departments cost a lot of money, it gets harder in a small liberal arts college to support that. The short of it is that contract research began to appear in ever-increasing amounts. The physics department began with heavy Navy contracts. My department said we've just got to

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have some support. I ended up doing basically contract research with grants. I was supporting all but \$1,000 of my salary, all my graduate students, supplies, equipment; I was a little business ensconced in a university. I became, frankly, disenchanted with that. If I want to do contract research, maybe I just go do contract research. I had worked on the Manhattan Project during the war. What became the Lawrence Livermore National Laboratory was starting up out here; I had invitations to come. So I came and looked and decided to return to California.

VM: So you've been here for about 40 years.

GD: I came back in '54. The last part of my Washington U. career I wasn't doing anything similar to what Calvin was doing. We had some discussions, I remember, on the structure of bacteriochlorophyll which has two more hydrogens in it. But I can't remember: I think he had a graduate student who worked on the spectrum of such materials or something. Seely (*Gilbert N. Seely*)?

VM: I don't know. There were periods, of course, when I wasn't there.

GD: This was back more in the early fifties.

VM: That would have been before my time. I don't remember any such person. There's a lot of stuff about his work that I didn't know about.

GD: He maintained an interest in this porphyrin... Initially when I went to Washington U. I continued some work on these basic porphyrin structures. We looked at the N-H isomerism, we looked at the spectra, we looked at chelating — they are great chelators, they add a base like pyridine; drop, you know, make a strong pyridinate complex. We made tetrahydroporphyrins, did a lot of interesting things.

VM: Since coming to Livermore you haven't been involved with that sort of thing at all?

GD: No. I was strictly involved in mostly high explosives research.

VM: Do you get to Berkeley to visit the group there often?

GD: No, I never returned really after my couple of visits in the late forties or early fifties. I never went back.

VM: You've never seen the round building?

GD: I've never been inside the round building.

VM: I'm afraid it's beyond its heyday because it's no longer a building occupied by a single group. But, it's an interesting concept and, as someone who once lived in ORL...The idea, of course, was to try somehow to recreate the atmosphere and the climate in a modern building. That building is now 33 years old but you ought to go sometime and see it.

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GD: I've heard of it, the structure and design so that people almost have to interact.

VM: That was the theory. It start out...and people really feel it did that at the beginning. You are still in touch with people like Marilyn Taylor, are you? Do you know Marilyn Taylor?

GD: No.

VM: Calvin's secretary. No — perhaps; she was '48. I think sometime when you are going to be in Berkeley you should ring up his secretary, she certainly knows who you are, and say you'd like to have a look at the building, just for the hell of it.

GD: I haven't even seen the new big chemistry building. At the G.N. Lewis luncheon, which is held in the old Faculty Club...

VM: Well, that's very close.

GD: ...they usually offer a tour and, unfortunately, I had another engagement this last time and had to dash out.

VM: Next time perhaps you will go.

GD: Perhaps we should go now (*to lunch*); we can easily talk some more, transcribe some more.

VM: That's very helpful and very interesting what you have said and I'm very glad we could get together and talk in this way.

GD: It kind of predates your main line of endeavour here.

VM: The whole project is really...

GD: It's part and parcel of a whole.

SM: Of a history...

VM: Of course, Melvin is the focal point. OK; let's leave it there. Thank you very much.