

Chapter 25

RODERIC B. PARK

Berkeley, California

July 9th, 1996

VM = Vivian Moses; RP = Rod Park; SM = Sheila Moses

VM: This is a conversation with Rod Park on July 9th, 1996 in Berkeley, California. OK — all yours, Rod.

RP: I got my PhD from the California Institute of Technology, CalTech, in the spring of 1958. I served as a postdoc. there for a short time in geochemistry. I visited the Calvin lab. and Calvin had pointed out to me that Ozzie Holm-Hansen would be leaving the lab. that summer and he needed someone to take care of his algae lab., probably more correctly called algal lab. (if it's an adjective). But it was called the algae lab. in the Calvin days and located in the upstairs of the Old Radiation Laboratory. I arrived about July 1 to start work in ORL and my wife and kids and I lived for a while in a motel down on University Avenue while we were searching out a house and I used to take the bus up to ORL.

I saw Ozzie Holm-Hansen in his last few days who he seemed to be particularly happy and somewhat blithe about leaving and was rather inexact about all the problems and all the things I was going to face. So, I got started and one of the first things that happened was it seemed that some of the cultures might actually be contaminated with bacteria and were not actually pure cultures. I went back to single cell isolation and one of the things that had been mistaken for contamination was that the cell walls of *Scenedesmus* stained like bacteria and people had mistaken these. I think they were contaminated. We went back to single cell isolation and worked up from cultures and eventually got both the shake cultures and the continuous cultures. Operating this took a considerable amount of time. We also set up a pump that had parallel pistons for air and for CO₂ to generate a large CO₂ supply so we weren't always twiddling the dials. We gradually got that under control.

VM: Can I ask you two questions. One of them: was this the older shake flask arrangement or the vertical?

RP: We had both the old shake flasks as well as the continuous culture, the vertical ones.

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VM: And who was the other half of “we”? You said “we”.

RP: Well, Pat — what was Pat’s last name?

VM: Pat Smith?

RP: Pat Smith was there...

VM: ...the redhead.

RP: ...the redhead was there and I inherited a staff member who had very good intentions but, I think, a lot of instability psychologically in various ways. That created its own dynamic of certain difficulties in getting this whole operation going. We finally got it under control. My office was right around the corner, just to the north-west of the lab. beyond the stairway, which turned out to be E O. Lawrence’s former office in the Old Radiation Laboratory. He actually had an office through a door just to the east that went out under the roof from my office, where he had a desk, and he actually came in once during that fall and I had an opportunity to meet him and went to his desk in the other room. Later that fall he died. But I did have an opportunity to meet him a month or so before he passed away.

The science was interesting. I saw Calvin and he said for me to get the lab. started and see Vivian, and see others, and Ning Pon, run some chromatograms and find out how to do the carbon cycle work.

VM: I can’t remember whether you actually arrived before I left, but you might remember.

RP: Yes, I arrived before Vivian (*sic!*) left by a couple of months. I only spent about six months in ORL — all of us did — and then we moved down to the Life Sciences Building as an interim move before the Round House was built. I had a remarkable industrial strength experience with Melvin about three weeks after I got there. I had not run chromatograms; I had been isolating chloroplasts. I was interested in some geochemical work on what the fractionation factor would be — we never got this completed — but the idea was I wanted to find out what the fractionation factor was in photosynthesis for the O^{18}/O^{16} ratios of oxygen produced as compared to the water source of the oxygen, because it wouldn’t be exactly the same; it would be a fractionation factor.

I started working on this and started working with chloroplasts and started working with Ning Pon on CO_2 fixation by these chloroplasts also. We had done some experiments that looked kind of interesting in terms of what the membranes did and what the supernatants did from these chloroplasts, and which activities the carbon cycle were held and which ones, which, of course, is in the supernatant was the membranes being responsible for electron transport and phosphorylation. We had done some preliminary experiments and Calvin came up at the end of the day, it must have been about 4:30 one afternoon. I was alone up in the algal lab. and he said to me, “well, have you been running any chromatograms? What have you been doing?”

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I said “no, I haven’t been running any chromatograms, but let me show you what I have been doing.” He got quite angry, and started asking me all sorts of questions because obviously I hadn’t done what he’d asked. I kept holding my ground and showing why I thought this was interesting and why I thought it would be an interesting direction to start working in. He was really roaring by the end. I sort of felt well, I’ve been here about a month or so and maybe I’ll be leaving next week! I thought maybe I was out of there. I went down and my wife had come to pick me up in the car and I said “I’m not sure how much longer we’re going to be here.” The interesting thing was, he never did that again. As a matter of fact, he was very supportive of what I was doing. Apparently, I don’t know whether consciously on his part, it had been a test to find out what I was made out of. It was rather remarkable to be a wet-behind-the-ears PhD recipient challenged this way by a Nobel laureate. It was something I will never forget. On the other hand, I was sure that I was doing good work, I wasn’t about to let him roar over me without listening. Apparently, he did listen because after that whatever I was doing he was very supportive. He gave me my own head and let me go. I must say that was a remarkable experience. It’s the one that most sticks in my mind from that whole lab. experience as being one of those forks in the road that happen in your life.

VM: I’m not sure that I can recall, maybe you it said already, how did you first come to meet him?

RP: I came to meet him on a trip up here because, as a graduate student, I had started working with James Bonner (*at CalTech*) on terpene biosynthesis and eventually in the rubber plant. Of course, we were doing me-tooism in plants because it had been worked out in liver. We started out feeding acetate labelled and that worked fine, we could degrade the rubber, it went in the right way, but all the intermediates he proposed didn’t work because it was laevulinic acid and we were trying β -methylcrotonic acid and various other things which gave randomly-labelled rubber. Bonner sort of took this view that you were against me when it didn’t turn out right. It was kind of upsetting!

So I went over at that time at CalTech and started working with the geochemists (I had done a minor in geochemistry there) particularly on carbon isotope fractionation. We did some control experiments on carbon isotope fractionation in photosynthesis and that got me interested in photosynthesis. And, if there was a place to work on it, it seemed to be, particularly with respect to carbon, it was Berkeley so I took a trip up here and met Calvin, told him what I was doing and told him I was interested in coming up. He called around to a few people, I guess, and offered me a job as a Chemist P1 which was the lowest level that you could possibly get in the Lawrence Berkeley Lab. at the time.

VM: But that was as a regular employee?

RP: Regular employee.

VM: And it was an open-ended thing, it wasn’t time-limited?

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RP: It wasn't time-limited but we really didn't discuss it. We assumed that it was a year or two, something like that.

VM: What impression did he make when you first saw him? Presumably you knew about him and knew what he was doing?

RP: My impression was...I met with him in his old brick office — I guess it was G.N. Lewis' office at one point in the Old Chemistry Building and that, of course, had its own aura of tradition, of vast glass-fronted bookcases and fireplace and all these other matters in it. I found Calvin himself kind of hyperexcited about things, a man who was so intense he obviously had at least at that point in dealing with me, no sense of humour whatsoever. It was all extremely...well, I guess the word is intense and excited and petulant. At times he could be very petulant, didn't like you to disagree with him, although he would immediately create arguments, but he got a little petulant...

VM: And if you were winning the argument?

RP: I had that time, once. I think I won it, but he didn't acknowledge it until about a day or two later. I think he thought things through pretty well and had his mind kind of made up and it took a lot of thinking on his part to change it. That's the way it should be. One shouldn't be ephemeral about these things. That's certainly the way he was.

I was with those first six months. I started very satisfactory cooperation with Ning Pon, which was a lot of fun, we had a good time together and did some interesting work. We moved after six months to the Life Sciences Building, the old Greenberg space. You weren't there then.

VM: No, I'd gone. What was it like in LSB? We haven't actually heard from anybody what the move was like, what the mood was like, what the mood in ORL was like as it came to an end and things like that.

RP: The move went off, I think, smoothly. The space was behind time down there. I remember they had a budget which sounds ridiculously low now, it was \$75,000 to remodel the space. It was in lower floor, the north-east and along the northern side of LSB, which is now occupied by the Jepson Herbarium, down in the basement there. I had what had been an old teaching lab. as part of the medical school, before that moved to San Francisco, for the algal lab., with benches taken out on one end, with all the algal laboratory itself for the growing facilities, and then several lab. benches for us to do our work on. There were other labs. around the way, some of them were Rapoport's, various other facilities. The next lab. to the west, the big double lab., was made into one large room, at least it was connected at the end, to try and at least get some of the large lab. interaction and experience that was done in ORL and was again done in the new building. Obviously, with walk-through corridors and all the rest it did not have the same sense of family that ORL did, or that the new building does. It was an interim solution. I can't remember how long we were there, a couple of years.

VM: Oh. actually from the end of '58, I think, until '63.

RP: '63; five years, yeah. I was involved in the design and construction of the new building as part of the building committee. Some interesting things happened there. The architect was Michael Goodman, who was on the faculty here, a typical inside job (I don't think they are done that way any more) but you sort of take care of your own in architecture and so forth. Calvin had a distinct idea of what he liked. He wanted a building where people were forced to interact with each other, interdisciplinary building, and that's why it eventually ended up round. It kind of focused everyone at the coffee table or something with special facility rooms in the back. We had our first meetings and (*Calvin*) said what he wanted. Louis DeMonte was the campus architect at the time and Michael Goodman understood what Calvin wanted. He first started drawing a square building, which sort of forced people to interact, and then he drew a round building which had interesting design characteristics. If it's smaller than a certain size, you can't have interior corridors; if it's larger than a certain size, you run into all sorts of problems also. It turns out that there are certain quantum sizes of round buildings that make them work well for science. So, Calvin's building — Michael Goodman came in with this round design.

The campus architect, Louis DeMonte, was absolutely shocked. He said, “Michael, when that goes to Sacramento we could lose every building in the building programme of the University”; he got really angry at Michael Goodman. Calvin kind of sat there and listened to it, didn’t say anything. We had a meeting again about ten days later. Calvin had obviously gotten to the important regents at that point. Louis DeMonte said “well, I think a round building might be rather attractive statement right at this point on the campus.” I saw University politics operating! I think (*Donald*) McLaughlin was probably the regent who pulled a turn on what kind of building Calvin could have.

VM: In the early days of the design, from what you say, it seems that Calvin had been able to impress upon Michael Goodman very much the sort of building he wanted, or at least the properties he wanted the building to have.

RP: He impressed on him the programme, before the design, of how he wanted people to interact. That was the *(most important thing)*.

VM: The design of the building came from Goodman and from the (*campus*) architect and not from Calvin himself.

RP: That's correct. Calvin thought the round building might work...

VM: Had he?

RP: Yes...and in Calvin's mind it was a replacement for the old Bacon Hall which was a museum which used to exist down right to the south-east of the Campanile — that would have been torn down for the new physics building — and Calvin also thought of it as a replacement for Bacon Hall in terms of that kind of architectural statement on campus. (*Note: Bacon Hall was a semicircular old red brick building, one of the original campus buildings.*)

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VM: Bacon Hall was geology, was it?

RP: I think it was geology. It had a geological museum in it. I just went in there to look at the rocks, I never looked around at the building. You notice, one other thing that happened was that Calvin kept saying, “well how do we connect the two floors?” And he put in that little spiral staircase so that you didn’t have to go to the other staircases. That was a hell of a fight with the fire marshal and everybody else as to how you connect the two without having everything locked-off through doorways. But somehow they got it done.

VM: At which stage do you remember did they start looking for money? Because clearly, they weren’t going to build a building unless they had cash for it. They had to go out with some building proposals.

RP: The building as I recall — again it seems incredibly cheap by today’s standards — I think was two million dollars. Calvin had raised quite a bit of money, I think, from the Health and Human Services people, which was HEW at that point — they had given a big grant and he had gotten several other grants and private gifts. I don’t really recall the details of the financing but I know that he was active in fund-raising. I was not involved in that in any way but he would speak about it at his lunches over in the Faculty Club, in the Directors’ Room where we would meet every Thursday noon for lunch.

VM: Had he got the money before the building design was finalised?

RP: I think they were going on simultaneously. He had enough in-hand to get going and I guess The Regents felt that there was a good possibility he was going to raise the rest of it. So it was not totally in-hand. He was still raising it while the whole building planning and design was going on. He certainly had enough to cover the architects’ fees and the 10% at that front end, and then it just kept on coming in.

VM: Was there discussion in the lab. generally about what the building was going to be like or was it confined to the Thursday lunch people?

RP: My interaction with the design was primarily through the Thursday lunch people and Calvin and the architect and the building committee. There was discussion with others but I don’t remember there being strong opinions about it from the transitory people who were there for a year or two. It was really kind of Calvin’s show and the more permanent staff’s show — people like myself.

VM: What’s your own view? Do you think that ORL was the glory building in which regard that some people hold it?

RP: I think it certainly had some great science going on in it. I think the architecture sort of by accident contributed to that. It’s not a building that you would design and reconstruct. On the other hand, it had this fortuitous configuration. I think the...Calvin obviously wanted to recapture that. I think part of the sense of morale that existed,

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which seemed to be pretty good in ORL, had something to do with the fact that it wasn't such a hot building. I think when you get a building that is sterile and artistic and totally finished, sometimes it's — maybe I'm speaking about myself now; I think I'm speaking about myself — it's harder to focus in those surroundings, sometimes, than it is in surroundings that are a bit more demanding. Sometimes surroundings that are a bit more demanding for me generate a greater sense of creativity than in the building that seems totally finished and perfect.

VM: Well, you grow into the building; the building is built around you, grows around you.

RP: Yes, it grows around you and maybe it's a building that's not finished, that needs improvement, to me leads to greater creative energy on my part than one that's absolutely finished and perfect where you don't have that sense that things aren't right, let's make them better, that may just infect you.

VM: I think the personalisation of a building, the fact that you have the stain on the ceiling, you remember the time when somebody shot something up there, it makes it real for the people that live in it. But, for people that didn't experience that, it's historic.

RP: There were some parts of the ORL which were really a pit. The place where the counting was done down under the building — it was a pit. I don't remember exactly what it was but...There were some pretty crummy parts of it. But the labs. I worked in upstairs were really kind of fun. I got a little hot under that tin roof up there...

VM: Did you notice a change, then, in attitude among people when you moved down to LSB?

RP: I'd only been six months, I think, in ORL so it's a little hard for me to say. I think we were more isolated from each other. In other words, (*in LSB*) for me to walk to the main lab. I had to walk 100 feet or so and around two corners and down this wide corridor and open a door and go into other labs. That made it different.

SM: May I ask a question? I do remember. Was the planning for the new building, had it begun, and had the application for funding and so on begun, before the Nobel Prize excitement or after.

RP: I think the new building was in Calvin's mind and a lot of minds — it wasn't shared with me — because they knew that ORL was going to go, and that Crocker (*Lab.*) was going to go, and that they knew LSB was not permanent. Before I got there, there was already in everyone's mind the notion that he would get a new building. Now the Nobel Prize came along — I can't remember — about six months after I got there...no a year and a half.

VM: No much later than that; the Nobel Prize was '61

RP: '61? Well, I can't remember: two and a half years. But people sort of thought that was going to happen. One of the things that occurred, as long as we're on the Nobel Prize, was, as you know, there was this "competition", I guess is the best way to call

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it, between Arnon and Calvin. Roger Stanier thought he'd be the great mediator and he organised this photosynthesis seminar which was about a year or so after I got there. All of Stanier's people came, Arnon's people came, Calvin's people came who were working in photosynthesis. Stanier was sort of going to get these great minds together and have a synergy occur between something where one and one had been less than two; he (Stanier) was going to make it three or four and sort of bring all this creative energy together.

Well, they decided who would give the first seminar. It turned out to be me, to be I rather; I would be the first one. I pulled my material together, I looked out at the audience. I remember it was in the old Botany seminar room on the south side of LSB there on the first floor. I started and Calvin's people were on the one side and Arnon's people were on the other, and they (*Calvin and Arnon*) were sitting in the front row with their people behind them. We got into some sort of rate discussion about something I was doing, and Calvin and Arnon started at each other. Roger was sort of standing there with a blank(?) (*indecipherable*). Calvin was sort of saying "well, the problem was such and such" and then Arnon would say "well, I regard this very much as an athlete who is running and in training and the question is, does he have a large heart or a small heart?" He would start along with these analogies and Calvin was just beside himself. He said "to hell with the athletic hearts! Let's get back to photosynthesis!" Anyway, this thing was a disaster. There I was, standing up in front, this wet-behind-the-ears postdoc. with kind of these missiles flying in every direction, not at me, it had nothing to do with what I was saying. It was totally out of control and Stanier was trying to handle it. I remember those days well.

VM: So nothing happened? There was no reconciliation?

RP: (Great laughter!) There was no reconciliation at all. It was trains passing in the night!

VM: Do you know what the original antagonism between Calvin and Arnon was? Nobody seems to remember it or...

RP: I don't really know. Arnon used to say, "look, everything in the Calvin cycle occurs in animals also, the unique thing is photosynthetic phosphorylation". He obviously had the view that he should get the Nobel Prize because he was doing the only thing that was uniquely plant-like, which was cyclic phosphorylation, and if you took what Hill did, electron plant transport that was one, you added phosphorylation, but all this carbon cycle stuff occurred in animals. That was sort of his (*Arnon's*) argument. Of course, he had started as someone in plant nutrition and had moved over and started in this area with Bob Whatley. I think it was direct competition for recognition, ego gratification.

VM: They were a couple of powerful characters.

RP: Very much so.

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VM: Another thing in which you were almost the only one for this to happen, is that you had a faculty position during the time that you were a full member of the Calvin group. I think only one other person (*Ken Sauer*) was in that position.

RP: Dan Mazia and some others apparently went to the dean and said that they felt that there should be some positions opened up, since the place was expanding and new faculty, additional faculty being hired, for people they could just identify as targets of opportunity. I was asked by — I think it was Sandy Elberg (*Sanford S. Elberg*) and Dan Mazia and some others who were on this committee — if I would be interested in an appointment. They didn't even say exactly where it would be. I said I thought my background was such that it should be in Botany and Plant Physiology, though Chemistry was not out of the question if, in fact, that's what I wanted to do and the chemists went along with it. I should have thought, deep in my heart at that point, what I would be best at and I felt I would be best in a department that did everything from looking at fields of plants and doing ecology all the way down to detailed analyses of how certain reactions work. I would be happier there than I would in a chemistry department because my background was more like that.

So I decided to go with the Botany Department which was a bit strange in many people's minds. I think it was the right thing for me to do. This was apparently an initiative by Dan Mazia and Sandy Elberg and a few others to make a recommendation to the dean. What I got was half-time appointment and then it went full-time in '64.

VM: With the half-time, you were regular faculty, on a half-time basis.

RP: Yes, on a half-time basis, teaching and...That worked out pretty well because we were down in Life Science Building over that period. As they (*the Calvin group*) went back into the new building it was a little too much of a split to handle well so I went out and got some grants and told Calvin I wanted to go full-time in the Botany Department, which I think upset him a little bit. I went to see Henry Rapoport before I went to see Calvin and said "what do you think I should do? I sort of find myself trying to work on my own here and get a lab. going at the same time doing work in Calvin's lab. which is fine, but it's not satisfactory in terms of the totally focusing of my effort." Henry said "well" — the way Henry would — "it's pretty important to be able to call your own shots". So I thought about that a bit, that conversation, and I finally if I decided if I was going to do this right I would have to fly on my own and so decided to go full-time in the University .

VM: But you retained an association with Calvin's lab.

RP: Yeah, I did retain an association over time and could still use the equipment up there and help his graduate students and do other things. But I had my own lab. and my own grants, and so forth that I ran down in the Life Science Building. It turned out to be in exactly the same lab. that I had been in, on the first floor, that the algal lab. had been in.

VM: You'd thought you had got out of that building!

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RP: Well, you know, it was kind of a crummy building. When they emptied the dishwasher upstairs all the foam and all the crap would start coming up through the floor and that kind of thing. It was awful. It was a terrible building. It's a good thing they gutted it and rebuilt it.

VM: Coming back to the round building, do you think the effort was satisfactory? Do you think that they did, in fact, recreate the atmosphere of ORL as well as one could in a new building?

RP: No, I don't think so. I think...perhaps I should go back to the comment that I made earlier that when buildings are complete and beautifully finished they become somewhat sterile. I never found the same kind of interaction that might have occurred. Part of it had to do with the fact that Mel Klein's group, for example, was downstairs again, through the closed doors, not via the spiral staircase. You had to go through another door, to make an intentional trip down there to talk to him. I think losing the physicists, which kind of got lost, detracted somewhat. I think the chemical part, the biological part on the second and third floors worked pretty well, but there was something about the segregation of the physical chemistry, physical side, that you didn't walk through and didn't see and didn't experience in the way that we used to in ORL.

VM: They chose it like that, as I remember. They didn't want to be in big open labs. for their own reasons and that was one of the reasons why the ground floor was separately configured.

RP: Thinking back on it, there should have been some more spiral staircases or something. There was something about that that in my view didn't work as well.

VM: Was there also, do you feel, two other considerations. One of them was the size. It was obviously much bigger than ORL and more people, and then there was the bringing together of the Donner group which did not have the same intensity of focus as the ORL people had had.

RP : That's true. The Donner people, there was — let's see — Dick Lemmon had his isotope bombardment (*equipment*); that was again behind a closed door down on the first floor. There was Ed Bennett with the brain biochemistry. But all of those didn't really get integrated in the sense that they were on that first floor. Actually to leave the second floor you went through a fire door at the top, a fire door at the bottom, down the hall and through another door to get to somebody. I think that was enough to make it more isolated.

VM: There was also, I suppose, the fact that the photosynthesis activity became very different by the time the new building was open. There was no longer the focus on solving the carbon cycle and what immediately followed it. It began to spread into all sorts of areas.

RP: There was Al Bassham doing his work and...

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VM: Your work was quite separate.

RP: ...quite separate. A lot of work on electron transport and structure and certain physical-chemical measurements and others. I think the techniques became themselves more isolated than everyone working on the segments of the carbon cycle. It was different. You can't really recapture things. Calvin's view was to have interdisciplinary synergy in that lab. and, as I say, I think it worked pretty well on the second and third floors but I think somehow the physical part on the first floor got removed by three doors. That was enough to make it separate.

VM: But I am sure you value the whole thing as being an interesting and pleasant time of your life.

RP: It was a very formative time and a remarkable opportunity to participate in a grand adventure and one is indelibly inscribed by those experiences and I think they were very positive for me, particularly the opportunity to talk to and work with the people — Mel Klein and others — on the physical side whose way of looking at things was as a physicist, which really is a different way. This was extraordinarily valuable to me. Ken Sauer is the same way, looking at things as a physical chemist and the kinds of questions he would ask were not the ones I necessarily would have asked, but influenced me because these were the kinds of questions I subsequently did ask.

It's one of the great difficulties in our disciplinary universities. As our fields became more and more specialised, one worked with a smaller and smaller select group of colleagues on more and more specialised areas and one didn't have this interaction. Calvin may have foreseen this over-specialisation that occurs. What has happened now is that people don't even work with their colleagues within the university. The work on the Internet with colleagues around the world who are specialised! We lose community and we lose the synergy that comes from trying to understand the broader dimensions of our science and our problems.

VM: How do you see him (*Calvin*) as a leader and an inspirer and an innovator?

RP: See whom?

VM: Melvin.

RP: Melvin...I saw sort of at the end of his carbon cycle career so I never saw him at the really creative part when he and Benson were initially doing this work and putting this excitement together. At the end of his career, I saw him more trying to regain the excitement and the prominence of what had occurred during those exciting days of the carbon cycle. He is very inventive and very thoughtful about a lot of things. Let's face it. You don't very often create one great kind of discovery after another. So I think, in my experience with him, I found him a bit more frustrated and slightly desperate to regain what had been than what he must have been when he was right on top of the wave. He slid off the wave, everybody does. You never stay on the crest of the wave forever. I didn't see him at probably at his creative peak.

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If there is anything I would say, that is characteristic of him, is that he didn't have the psychological resource to enjoy his success and to be satisfied with his success. Insecurity followed him forever if he wasn't on the crest of the wave. He felt somewhat insecure. I suppose as you get older you get over that but if you get more experience you get over that. Maybe if you have his creative desire and ambition you never get over it. You've got the monkey on your back your whole life in terms of trying to regain that sense of prominence as the symposium speaker that everyone comes to hear who's made the latest great discovery. I saw him in the years when he had drawn away from that or had gone beyond that. He was having, I guess the term to use is that he was having withdrawing symptoms.

VM: The other people in his lab., the permanent people: there were some like yourself and like Ken (*Sauer*) who moved to faculty positions either in Berkeley and then people like Andy (*Benson*) and I moved away for faculty positions. There were some people who stayed there. Do you think they were a good group of people, high quality people throughout? Had he been successful in gathering the right people around him?

RP: I think there's a fundamental problem: that really excellent people are not going to stay working for Calvin. What he would have to do to create a group was to find people who were very good but who could endure always being a little lower down in the organisation and not kind of their own boss. Now, you can get a lot of good people that way. One of the problems you run into is that they tend not to be as versatile and you tend to have more trouble picking up exciting new initiatives with those kinds of people because they have been selected around certain programmes. So in a way, the best people he could attract aren't going to stay. The people that do stay are never going to be necessarily the people you'd want. The best thing to have in those situations is what he had with his postdocs. and others coming in and turning over. That makes it by far the best way to do it. It's the same thing in any lab. When you hire a technician, at a rather low level, you'd rather hire a really young bright graduate student who is going on to other things, that you have to train and then they are gone after two years, than someone who's going to be there thirty years and be really dull and never learn anything new. That would be an extreme example. So in a way, to keep a lab. going with a permanent staff almost guarantees that the place is going to become a little slower and less interesting over time.

VM: There is one saving grace which is worth reminding ourselves about. And that is that over the course of time, and certainly by the sixties, there were several faculty members who were pretty independent who were still associated: you, Ken, Rapoport, Tinoco, John Hearst. These were all people who were not totally beholden to Calvin.

RP: That's right. I think that was difficult for him but I think he recognised that and it did keep the place vital. Of course, one of John Hearst's students, Tom Cech, went on to become a Nobel Laureate himself with the RNA enzymes, and so forth. There certainly was a continuity of excellence there. And Calvin himself? I don't know what his health has been over time..

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VM: Unfortunately now he has deteriorated quite badly.

RP: I think when his wife died that had a rather severe impact on him. She had done so much for him. I had always thought that she was keeping him alive and then suddenly she was gone.

VM: Well, she had been healthy but she incurred a disease which is unpredictable.

RP: I guess we are down to discussing what Melvin Calvin is like. He has certainly been a creative kind of a giant in the field and a remarkably clear thinker, perceptive. I wouldn't say his strongest characteristic was leadership the way I think of leadership. I think when he was actively engaged in something really exciting people wanted to join him and work on it. I can't exactly imagine him as captain of a ship, successfully, if you put leadership in those terms. It's more a ...

VM: It's the field marshal, not the general. He's the guy who sets objectives and gives directions. He doesn't order the troops around.

RP: That's right.

VM: Just a few specific points before we wind up.

SM: My interest generally is people, the kind of thing I make notes on. I was wondering how — you talked about your settling down to work with Melvin to start with and your early reaction to him. How do you feel that he got along with other people? Do you think he was an encouraging sort of person or was he harsh; how to think about that?

RP: I didn't see a lot of his personal interactions with the people in his office. What I saw were the interactions at the Friday morning seminar, perhaps was the...I didn't see him go around the lab. too much. Once in a while he would come around and talk to people. I wasn't there when he was talking to them. What I am saying is coming primarily from the Friday morning seminars. He was very interested in what people were doing, he was extraordinarily — and maybe rightfully — impatient with people who could not present their material in a logical, understandable way, and he would get quite angry and lose his temper. That for some people was not traumatic; for some it was extraordinarily traumatic. A number, I think particularly the graduate students, were afraid of him; some of the graduate students were.

On the other hand, if you knew your material and knew what you were trying to do, knew what you had shown and hadn't shown, and could anticipate the weakness in your argument and say "so far we don't know this, it's something we have to do, let me show what we have done", Calvin was terrific. He would go right along and he would contribute and have all sorts of ideas and he would very often see relationships with other kinds of science that weren't so obvious. I think he had, and this is not an unusual characteristic, a lot of trouble — and this was not conscious, this was unconscious — dealing with people that he regarded as not very smart, or not as smart as he was. Let's put it that way, which meant that you had to have pretty high

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standards! His interaction was very good with people that he felt comfortable with scientifically. On the other hand, with people who should have known better, he was merciless. With the graduate students, he realised that they weren't that experienced, but I think sometimes he was not exactly parental in his guidance and encouragement. I don't see him turning a C person into a B+ person very well. I can see him turning a B person into a D person pretty rapidly! He was a very intelligent, quick, demanding taskmaster who didn't suffer fools gladly. He could be very impatient and that "Hell with the athletic hearts. Let's get back to photosynthesis!" ; that's exactly the way he treated Arnon.

(Tape turned over. Opening question apparently missed)

SM: ...That's important too; what do you remember?

RP: About the social life in the lab.?

SM: Well, the very first Christmas I was there I was Santa Claus at the Christmas party, I remember that. I remember a party up at the Calvin ranch up in...where was it?

VM: Healdsville.

RP: Healdsburg and having a lot of fun at that. I remember some parties at his house, which were graciously and beautifully done, we went to. I did not have a real social life with the people of the lab. outside the lab. as such. I mean, we'd have people to dinner on occasion but social life got built around other friends, some of whom had nothing to do with the University. I got to know the Evans family, Evan C. Evans III and his family, and various other groups, and those were the people my wife liked.

VM: You were already married when you joined the group?

RP: Yes.

VM: So that in a sense separates you from the transient postdoc. and students...

RP: Somewhat; that's right.

VM: And, as I remember, you also lived across the hill, in Orinda?

RP: Yes, we bought a house in Orinda because it was cheaper. I could even tell you how much it was: it was \$17,500 with a \$14,000 GI loan on it and 4-1/4% or something like that. I mean it was so cheap in those days: £135 a month paid for everything.

VM: But \$135 a month was more, of course, then than it is now.

RP: It was more then; true. So we lived...that was also true; we didn't live there. So my social life was not built around the people in the Calvin lab.

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VM: Did that mean living in Orinda you were really a bit too far to pop back in in the evening after dinner, or not?

RP: No, I used to come back or stay in. I worked long hours. I worked on Saturdays. It wasn't too far, it was about 20 minutes or something. There was less traffic in those days. I got used to that. It was better to work long days, get there earlier, leave late, see the children in the evening, than come back.

VM: The last thing is: what happened to you post-Calvin, post Calvin lab.?

RP: Post-Calvin I went on and I worked in my own lab. down in the Life Science Building. I probably had one of my best scientific years about 1970 when we separated the grana and stroma lamellae out of chloroplasts and showed that one had only system I and the other had system I and II, and came up with some interesting models for how one could explain certain photosynthetic phenomena on that basis. That was with David Goodchild and Raj Sani that year. We published a review that was one of the most widely cited papers and reviews that year. It was also a year ('69/'70) in which we had riots on the campus and people marching through, painting the walls and spray painting and tear gas coming in through the ducts and people coming around telling us at one o'clock the building was going to be bombed and we had to get out. We never went out because we had spent all morning, since eight o'clock, making some biochemical preparation that we were going to use and we weren't about to leave it. And no bomb ever went off.

VM: This was Life Science Building?

RP: Yes. So actually, that again, was the fact that some of the best work is done in the crummier buildings under crummier circumstances, was another example of that. I went on and became, at that point, a chair of a new department, which was the Department of Instruction in Biology, and did a lot of work in setting up the new curriculum, new biology labs., working with Dave Hackett and Gunther Stent and a lot of really fine people — Oscar Paris and Dan Branton and others — setting up these courses.

VM: By this time, of course, you were a full professor?

RP: I had become a full professor by then. I was a Miller Professor in 1970. In 1972 I was asked to become the Dean of the College of Letters and Science. I thought long and hard about that and I realised that we had some 900 faculty members in the College and that only four, or three or four, would ever be at the right stage in their career, the right time, to become a Dean and maybe I should taste this dish in the buffet of life. I wasn't sure I could handle it. Everything I have taken like that I wasn't sure I could handle it, that's the reason it became interesting. So I took that over; we were recovering from Cambodia, getting the curriculum back in shape. I did that for probably too long, for eight years. I probably should have quit after six or seven; I think that's too long to be a Dean.

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At that point my marriage was falling apart so I wanted to find out whether I was OK or not. So I went on a single-handed sailboat race out to Hawaii and back. By the time I hit the dock, I felt I could do anything. I never felt so strong in my life; just an amazing experience. It turned out that the trip, during the time over there and the time back, was one hour short of forty days so it had a biblical roundness to it. A very important experience in my life in terms of knowing what I was made out of. I was stronger than I ever realised. We all need social support. The question is whether we need it all the time or can we get along some period without it.

So, I got back. I found out the search had been going on to replace Mike Heyman who had become the Chancellor of the campus, replacing Albert Bowker. So they needed a new Vice Chancellor, which is sort of like the Provost here, the executive Vice Chancellor. George Pimentel and I were the finalists in that. Heyman interviewed both of us and then he selected me rather than George. Then I spent the next ten years as the Vice Chancellor, working with Mike as sort of Mr. Inside and he was Mr. Outside, in terms of what we did. I was sort of the architect behind the total biology reorganisation and the construction of the new buildings.

SM: This was around 1980?

RP: 1980 through '90. But the buildings were not finally completed until '92 or '93. But the project had started when I was Dean of Letters and Science, and Fred Carpenter was the chair of the Division of Biology, or the Divisional Dean for Biology. We started an inventory of all the faculty members who did biology on the campus, regardless of departmental orientation, and what they did and grouped them by what they did to find out what we were good at and not so good at. And then went on with this for the reorganisation and built the buildings around the reorganisation. That was a 200 million dollar project and a lot of fund-raising. That was my major contribution, I think, over those years. There were a lot of other things we did but that's the one that one sees, left behind as a kind of a monument.

VM: What happened to your science during this period?

RP: I published all through the period with a man named Fernando Henrique. Not so much and my very last paper came out just a year ago this month (*i.e. in July 1995*), which was starting to do some molecular biology and sequencing of genes. This was just to learn how to do it with ubiquitin in conifers (in *Pinus sebiniana*) which is different from other ubiquitin. It has a kind of very highly conserved gene, I know, but we published some things.

I was going to go on with this but then the University offered me this opportunity to make a lot more money in retirement than I would as a professor and I decided I would go on and do other things. I had started working with ConAgra on a potato project in Russia and I spent some time on a farm outside of Moscow where they were thinking of doing a potato seed operation and learning a little bit. We planted, along with a fellow from ConAgra, about 8 tons of the first several varieties of the first US seed potatoes ever to go into Russia.

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SM: How long were you there?

RP: I was there about three weeks, three or four weeks. We had a hell of a time, getting everything to work, getting the potatoes there without being stolen, just one thing after another. This was in Krasnygorsk which is north-west of Moscow. To give you some idea of how it worked: the collective farms were getting about 8 metric tons/hectare; the privatised farm we were on, which had about ten farmers, was getting about 10 metric tons/hectare. Our seed got about 30 metric tons/hectare because theirs (*i.e. their seeds*) were so badly virused. What they did every year was to take the smallest, scaliest potatoes and use them as the seed for next year — a good Lysenko approach! (*Laughter*)

Anyway, of course, the farmers were interesting; they were highly educated in mathematics and various other things but they absolutely had no idea of investment. You could not even ask the question for the interpreter “how did you finance your crop?” It wouldn’t mean anything. Or “how did you sell your crop?” The stuff was just given to them, somebody else came and took it away and they might be given a tractor at some point. It was totally different. I found out one interesting thing. I took a Powerbook with me. They were extraordinarily interested and good at gambling. Because they had these casino card games in the (*Power*)book that could be played and they were really interested in that. Obviously, you could see how to teach them. You could start with gambling and about investment and odds. This was certainly a way to do it.

And then I was asked to be a consultant with all this blow-up at CU (*University of Colorado*) Boulder,. Joe Cerny put me onto that. Apparently he was called by his colleague Chris Zaffartis who was the Vice-President for Academic Affairs, worked in the President’s office; they both were nuclear chemists and recommended my name. I went back there and worked in the spring of ’94 with two other people, Joe Kaufman, who had been the System Vice-President for Administration at (*the University of*) Wisconsin and John Schaeffer who had been a very successful President of the University of Arizona.

We came up with a really tough report about what was wrong on the campus, what was wrong in the President’s office, what was wrong with the regents about the fact that they had a democratic tyranny and the structure didn’t make any difference, everybody ran to everyone else and did end runs. It was really just a mess, a disaster. So, we said a lot of tough things. I remember when the plane took off that May from Colorado, having the same sense of relief when a plane took off from Russia. You know: “Who the hell is going to fix that mess?” About a month later I got a call saying “well we didn’t get our chancellor (Tim Mosely from Oregon), would you be interested?” I said “let me think about it”. I was very surprised to get the call because I had been so negative, not insulting but very critical of what people had been doing. I thought about it for a while and I thought “well, gee, it sounds so difficult, it’s sort of interesting; that might take everything that I ever learned and every bit of intuition I had to begin to straighten that one out and you really can’t fail, because it’s a failure already!. And so it might be interesting to try that for a while.” So...and I said I would do it no more than two years. Well, the two years is up and they wanted me to stay

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longer and so I'm in for another six months, but I really want to get out. That's what I'm doing at the present time.

VM: Thank you very much. That is a most illuminating discussion and it will go into the archives. Thank you.