

Chapter 4

BRIAN ROBERT (BOB) RABIN **(with Sheila Rabin)**

Potters Bar (Hertfordshire)

April 8th, 1996

VM = Vivian Moses; SM = Sheila Moses; BR = Bob Rabin; SR = Sheila Rabin

VM: ...talking to Bob Rabin in Potters Bar of April the 8th, 1996.

Bob, can we start by my asking you, how did you come to go to Calvin's lab. in the first place?

BR: I obtained a Rockefeller Fellowship and Dr. (*Gerard*) Pomerat, who was really running the Rockefeller programme from New York at the time, was, I think, a very great friend of Melvin's and I think he, above everybody, persuaded me to go to Berkeley.

VM: Did you know anything about the place before you went?

BR: I think we knew a little bit about the path of carbon in photosynthesis and had read some of the reviews of some of the work that Melvin's group had done and it all seemed very exciting, although, of course, by the time I got there the path of carbon had been virtually traced, at least they thought it had been.

VM: So, before you went, had you had any correspondence with Calvin about what you were going to do when you got there?

BR: No. Calvin passed through the lab. in London and spoke to me very briefly. It was very interesting, the conversation, but he didn't say anything at all about what he would want anybody to do. He just said that I would be welcome there.

VM: So, you arrived with a clean sheet, as it were.

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BR: More or less, yes. In fact, I knew nothing at all about plants and rather little about photosynthesis.

VM: So, how did you travel?

BR: We travelled across the Atlantic by boat, on the SS United States. It was the most horrendous trip that anybody could have. We got into the end of a hurricane. The first we knew about this was when they came into the cabins and took everything that was movable out. My wife (*Sheila*) is a very poor traveller, and I really felt for her, but there was nothing I could do. When I went up to dinner, the dining room was almost empty. Sheila, of course, remained in the cabin, completely horizontal. The ship was tossing around like you had never seen. In fact, so much so that the screws would actually come out of the water and you could hear the engines speeding up. It was absolutely horrible. That's the worst experience I can remember.

VM: But eventually you got to the other side.

BR: We got to New York, and when we visited the Rockefeller to make the arrangements for onward travel, Dr. Pomerat took one look at Sheila and he said well you'd better go by air, which, of course, in those days was not very usual. So they booked us on the Red Carpet Service after spending a few days in New York at Rockefeller's expense.

VM: So you arrived where, in San Francisco?

BR: We arrived in San Francisco. We took a taxi as I remember, to a hotel in Berkeley, the Claremont, and we stayed in the Claremont one night and then we phoned the Calvins and they came and were horrified that we were in this very expensive hotel. That actually didn't worry us because Pomerat was going to pay from the Rockefeller Foundation.

VM: So, came the first morning when you went to the lab., presumably.

BR: No, it was not quite like that. Mrs. Calvin came along to the hotel and whipped us out and we stayed with the Calvins for a few days while we found a place to stay.

VM: So, you didn't go to the lab. for several days.

BR: I think I went to the lab., I don't think the first day we were there, more the second or perhaps the third day.

VM: When you finally got there, could you remember what it was like when you walked in? Who did you see? What did you talk about?

BR: One doesn't remember exactly what one talked about. I do remember it was fairly shambolic affair, a sort of wooden structure, very temporary looking. But everybody was very friendly and Calvin, I think, had to go off to Washington for a few days so it was not possible to talk to him. I think it was Dick Lemmon who was the first person

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I really talked to. I don't think Dick really wanted to talk about the scientific aspects or what Calvin might have wanted me to do. So, I never really had a programme. In fact, the program evolved, really, because at the time there was great excitement because it looked as if the Calvin cycle, as it was then called, might not be entirely correct. As you may remember, the methods that the early workers used for stopping the algal reactions was to drop it into hot alcohol. At the time, people had decided they would try other methods of killing the algae before they analysed the algal contents for radioactivity. They used cyanide and they got a very different pattern of carbon fixation and kinetically some of the compounds were very peculiar indeed. Everybody was very concerned about this because they wondered really whether the Calvin cycle was correct and if there weren't some artefacts that originated from the hot alcohol treatment.

I remember talking through the data with some of the German workers who were in the lab. at the time who had done these experiments. What immediately occurred to me was that they had forgotten some of their elementary chemistry in their great hurry to produce something new. What they had forgotten was that a five-carbon sugar like ribulose diphosphate has a naked ketone group which would react very readily with cyanide and, in fact, it was not a cyclic sugar in the normal sense. I talked to one or two people, I think Ning Pon particularly, and we wondered whether half of this wasn't due to cyanide reacting with the ribulose diphosphate. So, I think it was actually before I talked to Calvin in detail we had already started off to do some experiments on this. Then Melvin Calvin organised some radioactive cyanide, and we presented this idea to him and said we thought this might be an artefact...

VM: We is you and Ning Pon?

BR: Yes, we had gone through it. In fact, Ning had gone through the literature, actually, and found some branched-chain materials like hamamelose, and so on, which other people were thinking were intermediates in the carbon fixation but could clearly originate from the attack by cyanide. What puzzled everybody at the time was that if this was so, you would expect to see an acid at some stage but prior to that you would see a cyanide derivative. Why an acid was never seen from the hydrolysis of the cyanide was very simple really. It was that it lactonised very readily.

VM: This was the famous hamamelonic acid.

BR: I don't think this was the correct description of it. Because hamamelonic acid has to be from hamamelose and this isn't hamamelose this is an adduct from cyanide which is hydrolysed. When we talked about this to, I think, Helmut Metzner and Helmut Simon they thought this couldn't be because you never saw the acid component. Ning and I argued very furiously that the acid wouldn't exist really because it would lactonise very rapidly. At any rate, it was then possible to test all this out because Melvin had organised some radioactive cyanide and we did the experiments with cyanide and got exactly the same pattern of fixation as when you used radioactive carbon dioxide. Well, it wasn't exactly the same, but it was close enough to indicate that there was a very distinct possibility that it was an artefact. So, that took really

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quite a lot of time, because, as you know, everything was done on chromatograms and everything took a long time to do.

VM: Can we go back a bit to the beginning. You say you really developed this project with Ning before you ever got to talk to Melvin about it in detail.

BR: Yes, I think that's true. I think Ning may recall this as well. But that's my recollection of it, that we chatted about this and it kind of emerged.

VM: So, by the time Melvin and you and he (Ning) actually got together, it was agreed that that was what you were going to do.

BR: Exactly; I think he (Calvin) was very enthusiastic about it. And, of course, he added an awful lot to it because he knew much more than we did about the chemistry of these sugars and the possibility of an attack of cyanide on the keto-sugar. Most sugars, if they are in a ring structure, are not that readily attacked by cyanide. It was just an oddity, really, and it was just unfortunate that cyanide was used.

VM: Where was your bench? You had your own bench, presumably?

BR: Yes. Ning and I were opposite each other but we used to share the facilities., We were at one end of the lab., as I remember. We used to use other people's space. When we used the radioactive cyanide we went into a different area completely because that was considered to be relatively dangerous although the amounts of cyanide used weren't that great.

VM: Had you worked in a lab. like that, that style of people interacting, before you went there?

BR: No, no.

VM: How did it strike you when you saw it, when you found yourself in it?

BR: I think it was so friendly that it would have appealed to anybody. Unless you wanted to do something totally of your own, which had nothing to do with the rest of the lab., then that might have been difficult, although I don't think actually Melvin would have objected because he got so enthusiastic about everything that was worthwhile. He was a great enthusiast and tremendous at encouraging people. I think it would have appealed to almost anybody. Of course, the lab. was full of such very good people. It was really the cream of some of the European laboratories who worked there and were working there at the time. There were always visitors coming through the lab. It was a very vibrant place. The people discussed things together and then there was the eight o'clock seminar, which didn't greatly appeal to the Englishmen, but most of them appeared, sometimes a little late, but we were always there. What impressed me was that Melvin always led the discussion and always had something to add to it. He was one of the very few scientists who could talk physical chemistry to physical chemists and biochemistry to biochemists, and I think those sorts of people are very rare indeed.

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VM: So, if you worked with Ning, as I remember Ning was not an early riser, but he didn't go to bed early, either. So, did your day actually overlap much with his?

BR: In the middle of the day we worked together. The morning I set things up and it was very good because I could always leave early for Ning to complete the experiments. As you know, most experiments fail for reasons which are either obscure or due to the stupidity of the experimentalist. So, it was fine, really, and if he wanted a day off it was all right, and if I wanted a few days off, that was fine as well. And nobody interfered with us.

VM: So, essentially the whole of the experimental work that you did there you did in association with Ning.

BR: Yes.

VM: Tell me about the life in the lab. as you remember it.

BR: Well, we went up to the mountains skiing, as I remember, and it was very, very friendly. We'd never skied before but there were plenty of people to help you. I seem to remember also that we went camping for the first time that we had ever seriously gone camping, and, again, there were lots of people to lend you things and help you out, although really you didn't need any tent, you could sleep out in the open, up in Yosemite. We spent a lot of time travelling around America. Nobody ever suggested that we should spend more time in the lab. than we did. We were never interfered with. I think people made reports and they discussed it with other people in the lab. — Melvin was always walking around, talking to people, almost every day. He would do a tour of the lab, as you remember yourself. He never ever implied or suggested that one should spend more time working. If you were going off somewhere on a trip, he just said "Hope you have a good time".

VM: Did you actually work long days there?

BR: Sometimes and sometimes we had long weekends. It was an excellent atmosphere. People worked when it was required so to do. Sometimes the experiments that Ning and I were doing were extended and sometimes he stayed and sometimes I stayed.

VM: I guess you lived close enough to come back in the evening.

BR: Oh, indeed yes. We lived in a flat in Berkeley, I remember this very well, run by a very odd Hungarian called Voitov.

VM: I remember that name (*laughter!!*)

BR: It was fine, really. I think Melvin's greatest asset was Gen because she really looked after people and made them comfortable. I think we would have had great difficulty finding a suitable place if it hadn't been for her. She drove Sheila around until they found somewhere satisfactory and I think she showed her where to go shopping, and

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so on. Remember we had come from a Europe which had hardly emerged from rationing, hadn't emerged from rationing actually. We were not used to seeing food of this sort whatever.

VM: This was your first trip to America?

BR: This was our first trip to America. I think she (Gen) was a tremendous person, really. She helped everybody.

VM: Apart from the very unusual character of that particular lab., coming from the English atmosphere of 1956 into America of 1956, were you struck by different styles in the way the scientific establishment worked, the way the laboratories worked?

BR: It was completely different. All the work I had done (*in England*), I had done myself, really. I don't think I ever had anybody to supervise it. One could always go and talk to people and you were always welcome in London. But it was never on any sort of formal basis. I think what Melvin did was always made his presence felt; he was always there and he was always a source of information and was always very happy to discuss problems with people. I might say he was one of the few people who always had something constructive to offer. I think what was my great impression was the sheer extent of his scientific abilities. I have never met anybody who had the same knowledge as he had of science in general, whether it was from the physical aspect or the biological. I think he was really a kind of one-off, really.

VM: Were you used to a lab. as well supplied as that one?

BR: No — no, no. In London we made our own thermostatted water bath because we had run out of money. There was nothing like that in Melvin's lab. Everything was available. If you wanted radioactive cyanide, Melvin obtained it. Radioactive C¹⁴-cyanide was a very odd material to get hold of. You or I would find great difficulty in making it ourselves and that was true of everything. There was no shortage of resources.

VM: Did you find yourself talking about all of these problems with lots of other people besides Ning with whom you were working directly?

BR: I think mainly with Ning and Melvin. We used to chat to the other people, but I think not in the same sort of way. Of course, I think it was a little bit difficult with Helmut Metzner who had done the original work with cyanide. We tried to keep him informed of exactly we had been doing. I think that clearly they had made a blunder in interpretation. I think it was a very understandable blunder and I think it was a mistake that anybody could reasonably have made. It really wasn't that predictable that a five-carbon sugar diphosphate would be attacked by cyanide as it happened. Obviously, that makes it rather difficult. But I must say that Melvin was fine, once we had shown Helmut the information he accepted it because he is a very good scientist in his own right and he just needed to be convinced by the facts.

VM: That was the only experimental problem you worked on while you were there?

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BR: Yes. This took quite a long time, really. Remember that there was no experience with handling radioactive cyanide in any event, so that all had to be learned. Everything else had been done by dropping the algae into hot alcohol. I don't think anybody had used any other killing techniques prior to Helmut Metzner. I think Metzner was absolutely right that you needed to apply new killing techniques to the problem in any event. I don't think anybody would say that what was then called the "Calvin cycle" had ever really been proved in reality. I think people knew PGA (*3-phosphoglyceric acid*) was formed fairly rapidly, but nobody had any kinetic information, real kinetic information.

VM: There was a paper by, Wilson was one of the authors, in about '54 or '55 in which the cycle in the form that it was finally adopted was first put forward and there were concentration kinetics of one sort or another, turning the light on and turning the CO₂ on and off, things of that sort.

BR: I don't think anybody had ever shown that any of the intermediates were kinetically competent for the whole cyclic process. That's an enormous task and you don't wait to do that before you publish the system. I think what emerged stood up very well in real senses. The first reaction, of course, of ribulose diphosphate with CO₂ is still a very enigmatic process. You can get oxygen opposed to carbon dioxide, and so on, and there are an awful lot of complexities in the whole thing. I think that what was the great triumph that it was more or less the outline of it has survived. It's proven, it's in all the textbooks and it's generally accepted as one of the prime mechanisms, not the only one but certainly one of them.

VM: I guess your main contribution for that visit, as distinct from the later one, was to resolve the cyanide.

BR: The work I did was almost entirely that. The other thing that one learned a little bit about electron spin resonance and NMR because there were physical chemists in the lab. who you chatted to.

VM: Like who?

BR: I'm just trying to remember.

VM: There was a fellow called Power Sogo.

BR: Yes, he was there and...there was one other person. You get confused with the two visits. Remember that I was there in '63-'64 as well. Then it's hard to remember who exactly was associated with what.

VM: On your first visit you arrived in September '56 and how long did you stay?

BR: We left in July (*of 1957*).

VM: Did you motor back across America?

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BR: We did indeed. We did a very extended tour, eight weeks.

VM: Then some years later you decided to come back.

BR: Yes...

VM: Tell me about that. Why did you decide, how different did you find it, what did you come back to do — things like that.

BR: Melvin invited me back because he wanted an enzymologist, although Ning was still there. Ning was a very capable enzymologist and doing all sorts of very interesting things. At any rate, Melvin invited me back and so I accepted and University College agreed that I could go for a year's leave of absence.

VM: What were you? A Lecturer at UC (*University College London*) at that point?

BR: Yes. That was a great convenience for me.

VM: Can I backtrack just a minute? Before you went (*to Berkeley*) the first time, what was your position at UC?

BR: Assistant Lecturer. I had just been appointed actually.

VM: So they gave you a leave of absence then?

BR: Yes.

VM: Back to '63. When you came back the second time, then, obviously you knew something of the set-up even though it would have changed somewhat. But did you have then a clear idea of what you were going to do?

BR: Yes. I wanted to do some enzymology and Melvin told me to go around and talk to various people in the lab. to see where was a sensible place to fit in. I mean I got on very well with Pat Trown who was very interested in how the carboxylating enzyme worked and so that was essentially what I worked on almost entirely with Pat Trown.

VM: Where were you when you arrived whenever it was in 1963. Which building was then in use, where did you go?

BR: The Round House.

VM: You went into the Round House.

BR: I thought this was the most bizarre structure that you could think of. There were all sorts of jokes around about Melvin going up and down the middle, you know, and when he was away in Washington his ghost was scurrying up and down (*the staircase*), things of that sort. There were all sorts of witty people. It actually worked

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extremely well because people met for coffee in the middle, you may remember, and they talked to each other. It was a strange atmosphere. What I thought, it was really a bit too open for my taste. I don't like that open sort of laboratory although many people have followed that pattern.

VM: Had you known anything about the new building before you saw it?

BR: No, no.

VM: But you knew it was there.

BR: I knew there was a new building, because we knew that Melvin had raised the money to put this together, but there were no reports of it. Nobody had come back and told us what it was like. Again, you see, it was so superbly well equipped. It was extremely well managed, actually, in other ways, in terms of the availability of materials and there were always people to do the ordering; you really didn't have to do very much yourself except say what you wanted. So, Melvin was very lucky: he had some excellent back-up. I think (*Dick*) Lemmon, particularly, was mainly responsible for the organisation and he seemed to be the man who sorted out all the problems. I guess if they became very difficult they got referred to Melvin. But, again, Melvin spent a lot of time talking to people but nothing like as much as he had previously. But, then everything had gotten bigger and I think Melvin spent much more time in Washington, advising the President.

VM: By then he was on the President's Science Advisory Committee and he was going to Washington quite often. Do you think that there was a successful carrying forward of the style of working in the old building (*the Old Radiation Laboratory*) into the new one? Do you think they did a good job of designing a building, since they had the opportunity of a new one, to incorporate what everybody felt had been learned from the style of working in the old one?

BR: I don't think anybody could ever recapture the way the old building worked. There was something completely different about it.

VM: The spontaneity of having developed that way.

BR: I think it (*the new building*) was too artificial, in a way. Here was a building which was deliberately round in order to force people together. It doesn't ever work out like that.

VM: You first went into it when the building was very new: I can't remember exactly when it opened, but something around the time when you arrived, so you must have been in on the first days. Then you came back, at least for some sort of a visit, several years later, I can't remember for how long you were there, a month or two. How do you think the building had settled down in that time?

BR: I think it was a very efficient mechanism for producing scientific information. I think the old building was a source of originality. I think the new building was a source of

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exploitation of what was known. There's a very big difference. Why they were so different I honestly can't tell you. But I think that the first time I was there (*in 1956-7*) it was about ideas, even if the idea was to show that the cycle was really not entirely correct. The second time I was there it was much more modern science. People were thinking about electron spin resonance and computers and how to put data together and how to analyse data better. I don't think there was the same level of originality.

VM: But that would presumably have happened anyway.

BR: Probably.

VM: Not just a building-related phenomenon.

BR: I think, you see, there was not much left to do in photosynthesis in the way that it had been done previously. Melvin was much more interested at the time really in the light reaction itself and how that functioned and the physics of it. But I don't think the technology was available really to tackle that at that stage (*in 1963-4*). It has come along since, but that, I think, was the main thrust of his interest. The other people who were doing the more classical biochemistry were left to their own devices, was my impression, which was very different from the first time I was in Berkeley.

VM: There were a number of significant differences. For one thing, the group was united. Remember when you were first there, there was a group in Donner which was rather separate. Then it had become much bigger and it had branched out into a number of areas simultaneously. There was not the cohesion, I think, of a unitary group that we experienced in the '56-'57 period, in the new building.

BR: No. I think that what was missing, really, was the presence on a day-to-day basis of Melvin, in all truth. When I was first there (*in Berkeley*), the overwhelming impression I had was the dynamism of the whole outfit was Melvin Calvin. People had enormous respect for his abilities and his talents. The second time I was there (*in 1963-4, in the new building*), Melvin just wasn't there sufficiently. He had so many other things to do and, of course, it was much more diverse. There was much more physical chemistry which was a little bit different from what the reputation of the lab. really rested on.

VM: Those eight o'clock seminars, do you think they served a useful function for everybody or just for Melvin?

BR: I think they made people get up early, which I suppose, is valuable. I don't think...I don't know, really. I think you can have seminars at any time of the day, truthfully. Eight o'clock happened to suit the style of Melvin Calvin and fine, and people accepted that. But I think the most important thing was when Melvin walked around the lab. and talked to people individually because I think you learned much more — I mean, I learned an enormous amount just talking to Melvin because he had this inane (?; *innate?*) depth knowledge of physical chemistry and organic chemistry which very few biologists had. You couldn't do that in a seminar.

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VM: At this point Sheila, Bob's wife, is joining in because she has some very vivid memories as well of what it was like, especially the contrast between the first and the second visits to Berkeley.

SR: For me this was quite a trip and my first experiences in Berkeley were to be taken out with Mrs. Calvin — which I called her, "Mrs. Calvin", although everybody said "Gen". I felt that this was not proper or right, being English — and going to the University CO-OP supermarket and going in to buy things and coming out with nothing. Because I had gone with a bag to pack my shopping only to find that everything was done for you (*and groceries were placed*) into paper sacks. I noticed that Mrs. Calvin was buying things like eight tins of, maybe, fruit for a dollar and I just could not come to terms with this because up until I got married meat was still on rationing in England and so the opulence of everything was just overwhelming for me.

As far as the lab. was concerned, I found this to be so friendly and everybody was on first-name terms which is so foreign at that time in England. Everything (*in England*) was formal and starchy: Professor this and Doctor that, in Bob's lab. in University College, although everybody was very friendly, you still approached them on a formal basis. Professor Baldwin and Mrs. Baldwin, they were like the king and queen of the Biochemistry Department at that time, and one was in fear of them. Going to see meet Professor Calvin and his wife, and staying with them for some time before we got our flat, it was so different. It was like being with friends and they encouraged you to use their first names but I still found this difficult.

Going into the lab., everybody was so friendly. They were so nice, they were so helpful and you just got into the swing of things. Taking us away on trips, camping, skiing, introducing us to people to buy cars, service our cars. At one point our car broke down for a major trip and the guy said "borrow my car and when you come back yours will be ready". This, to us, was so different. First of all, Bob and I didn't even have a car before we left to go to California — our real first car was the one we had in California.

VM: How did you see the social life in the lab.?

SR: It was very good. Everybody wanted to mix and we all did. It was just so nice. I shall never forget it. It was one of my very first impressions was how friendly everybody was, especially Calvin, who was very, very nice to people who had no knowledge of science at all. He would spend hours with you telling you how things worked but, he never tolerated a scientific person who didn't know. He just looked at them with disdain and scorn and would walk away; he just had no patience at all with them.

VM: What sort of difference did you spot from the time you first went in '56 and came back again in '63? Was it a different group? Clearly you must have known some of the people.

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SR: Yes, it was a different group. There were not the foreign people, the Europeans there. There was definitely nowhere near that sort of type of lab. It was nice, still. Of course we had children, all of us had children at that point. We were all more constrained, although we did go away quite a lot camping with the kids and enjoyed that very much. I can remember going down to San Diego and seeing Ozzie (*Holm-Hansen*), who was at the Scripps Institution of Oceanography.

VM: “Ozzie”, I should say, is Ozzie Holm-Hansen.

SM: And he is still at Scripps.

SR: There was not the sort of camaraderie we had on the first visit, of meeting all these new people. Maybe because it was so different for us then but I can’t remember meeting anybody so different as we did then; we made so many new friends then.

VM: I suppose there were lots of us who were coming out of Europe at that time (1956-1957) in our various ways, being mind-blown by the first experience in America. We were all that much younger. We were all unencumbered and perhaps we mucked in and socialised more freely and more completely than we did later on.

SR: Maybe we did. But even going back in ’63-’64 it was still quite different to England. They still had much more than we had and they could still afford much more than we had. But when you look now at the differences, it’s not so great. In fact, shopping here is probably, in my opinion, is much better here (*in England*).

VM: You also have some memories of what you did at Christmas time out in the relatively warm climate of California?

SR: Yes, I do. I distinctly remember going to Stinson Beach, the whole lab. I think went, and we had this barbecue there. That was certainly something new for us. I don’t think we had ever been on a beach barbecue in England at Christmas (at any other time). Also, remember that you and Sheila and Bob and I went to the vineyards in the Napa Valley and this was also sometime in December, and I have pictures of us sitting amongst the vines, having our picnic, which was quite different. Also, when we went to San Diego, that was around January time, and I can remember Bob going to visit some laboratory near Hollywood, or somewhere you went, and I sat on the beach with the children (*this was in 1963*), it was about 77 degrees Centigrade (*this must surely be Fahrenheit!*), there wasn’t an American in sight. There was just me and the two kids on the beach, and this other woman. She came across to me and said “Hello” and I said “hello”. She said “you’re from England” and I said “yes”. As it turned out, she had come from Muswell Hill (*a London suburb*), which I thought was quite something. There were no Americans — -it was too cold for them.

VM: I remember the first Christmas we were there in ’56. Sheila and I drove to Los Angeles to see some people and came back to Berkeley on Christmas day in our convertible, our bright red convertible, with the roof down. That was an unbelievable concept to be doing that on Christmas day.

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There's a story I vaguely remember about gin.

BR: Yes. That's correct. Pat Trown was a great man for fermentation and he had a whole lot of juniper berries and he had these in a great pot. I remember, actually, helping him to distil this one time and produce some white-coloured liquid which was extremely potent. We poured this into the eggnog which you people thought was completely alcohol-free. I guess that was all terribly illegal and bad.

VM: You actually distilled your own gin?

BR: We did, indeed. I guess that's a violation of all sorts of federal laws.

VM: I'm sure of it. Perhaps we'll have to put this bit in anonymously.

BR: I think you'd better be very careful what you do with this! It was really very interesting material. First, it tasted exactly like gin, it was made properly. Pat had looked it all up. He had gotten literature on how gin was made, he had the juniper berries. I think he had actually used them in a fermentation with something else. I never, ever got to know what the brew was. I think it was, in some ways, more like our first visit. It was a ridiculous thing, really, to make gin in the lab. and pour it into the eggnog but it was done. It was the sort of crazy thing that would have happened the first time (*in ORL*).

VM: You haven't finished the gin story yet.

BR: Well, the gin story was very interesting because I think it's not proper for anybody to put a still up in the lab. in order to make alcoholic beverages.

VM: Melvin knew about this, did he?

BR: No, Melvin didn't know a thing about this actually. He must have been a little bit suspicious because he came in one day sniffing and said he smelled something very peculiar. I have forgotten what Pat Trown said but I remember Pat had a whole fermentation going and a proper still. The stuff that came out was really pretty good. Pat was a very good experimentalist and was good at making gin.

VM: Was there a sizeable quantity of this stuff?

BR: There was enough to lace egg nog and produce the desired qualities in the people who drank it. They didn't know they were drinking Pat Trown's gin!

VM: If I may bring you back from the gin to the science that you did when you were there on the second visit: you mentioned earlier that you had come back as an enzymologist to work on enzymology. What did you do then and how did that work out? You said that your agreement with Melvin was that you would work on the carboxydismutase.

BR: We did, I think, some interesting experiments. I think at this time it was very limited what you could do, actually, in the way of understanding enzyme action. We used

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some inhibitors and we produced a mechanism for the reaction which I doubt, really, would be acceptable today. We thought that one of the thiol groups in the enzyme was involved in the reaction and reacted with the ribulose diphosphate. And, indeed, that may well still be the mechanism. I think probably the mechanism that we produced, which, I think, Melvin thought was really quite good, and he had a considerable hand in formulating the ideas, explained it, whether it was the actual truth I really don't know. I think it is such an extremely complex enzyme which, of course, can accept oxygen in place of carbon dioxide, that it was really a bit arrogant to think that with the technology that was available then you could produce a chemical mechanism. But we produced one that you could present and people could argue about. And they used to get very hot under the collar because nobody ever accepted anybody's formulation of any enzyme reaction at the time.

VM: It was published?

BR: Oh, yes. We published some papers on it.

VM: Was there a strong reaction?

BR: You know, it was the usual thing. Some people said that this is a lot of horse manure and other people said this was a piece of great brilliance. It was actually neither one nor the other. It was not complete horse manure nor was it great brilliance. I think it was a carefully planned set of experiments and they could be used to demonstrate the possible mechanism. There were an awful number of imponderables and all sorts of things we didn't understand. We had no idea of what the structure was, we had no idea of what groups there were at the catalytic site. It was interesting, we did a lot of kinetic experiments. I think we found out a few interesting facts.

VM: This must have been the very tail end of the path of carbon. I can't offhand think of anything which could have come later, or much later, than that.

BR: Well, there was nothing left to do, really, in the path of carbon at the time. The lab. was, in any case, going much more physical. But there were always very good biologists in the lab. This was the other strength. There was always an expert in the areas that you might need to know something about but you didn't have any detailed knowledge yourself. You could always go and talk to somebody about things. That was the great strength (*of this lab., even in 1963*).

VM: Thank you, Bob, for that illuminating set of reminiscences which somehow, or other, I can't tell you how, will be incorporated into whatever Sheila and I write up.