

Chapter 3

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Compton Dando, England

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VM = Vivian Moses; RQ = J. Rodney Quayle; SM = Sheila Moses

VM: This is a conversation between Rod Quayle and Vivian Moses on the 11th of May, 1996, at Rod's home in Compton Dando in Bristol.

I wonder whether we can start by how you ever came to be in Calvin's lab. in the first place?

RQ: I graduated in chemistry in Bangor and I did my Ph.D. in physical organic chemistry under E.D. Hughes. Although at the time my interest I felt, really, was more in biochemistry but at that time to get a biochemistry degree was really a rather difficult thing to do because there were only two or three universities that gave one. So I did my Ph.D. degree on physical organic chemistry with E.D. Hughes and then thought I should go to Cambridge to work in Todd's lab. because here was an organic chemist who was actually working on the chemistry of biological systems and I thought this would be a nice place to go to and I got a University of Wales Fellowship to go there. So I worked for four years in Cambridge under Alexander Todd and the problem there was the structure of some blood pigments in aphids. It was one of Todd's sidelines. His main line was nucleic acids but he did have several sidelines of which these insect blood pigments were one of them.

So I joined a research group of about three of us who then worked on the chemistry of these blood pigments: they were hydroxy quinoid compounds. It still was basically chemistry and I still felt that what I really wanted to do was to actually see or be able to work...how biological systems actually worked rather than determining structure or synthesising compounds. And I suppose that at that time one got to know, to hear about the work of Calvin and this interdisciplinary group that he had in Berkeley that was working on how photosynthesis worked. And although we knew about it in conversation, I suppose it was the fact that a close colleague of mine, Grant Buchanan who was working on the next bench to me, decided to go to Berkeley to work for a year with Calvin.

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VM: Had you read Calvin's papers at that point?

RQ: No.

VM: It was conversation?

RQ: It was conversation, the sort of scientific conversation you might have about what was happening where. Buchanan went to work on some sugar phosphate chemistry, mainly, and was very enthusiastic about the group that was working there and the fact that so many different kinds of scientists seemed to work together. So I thought, well, this might be the sort of place which would bring together what I felt I would like to do. I talked with Todd about it and he said well, if that is how you feel, I know Calvin quite well and I'll write to him, which he did. I had a letter back from Calvin saying he was interested in this; could I send him some details of me, which I did, and a little later Calvin wrote back saying he'd be very pleased for me to work there. He was particularly interested in the fact that I was a chemist who was an expert in natural pigments because at that time Calvin felt that the primary act of carbon fixation was somehow connected with the epoxide ring of carotenoids.

VM: This would have been in '53 or '54?

RQ: That was '53. And hence carotenoids were going to be the answer and, as I was a pigment man, this could be very appropriate. I got a Fulbright travel grant and Calvin fixed up funding through the Atomic Energy Commission and I arrived in Berkeley in September/October 1953.

VM: Were you married at the time?

RQ: I was married at the time. My wife was a teacher. She was a zoology graduate; she was teaching at the time at Ely and she came with me.

VM: How did you get there?

RQ: We sailed by the *Queen Mary*, as Fulbright scholars did at that time. There was a dock strike in New York which broke out when we were half way across and the *Queen Mary* was diverted to the only other deep-water harbour that would take the Queens, which was in Halifax in Nova Scotia. We travelled to the States by special train from Nova Scotia to New York. Then train across from New York via Chicago and the California Zephyr through to Berkeley.

VM: Did anybody meet you at the station?

RQ: No. No, we spent virtually our last money for a taxi up to the flat we were told would wait for us and we arrived there and I turned up at the lab. the next morning.

VM: Where was the flat?

RQ: Bonita Avenue.

RQ: Well now that I would have to give you but I couldn't give it to you offhand. Yes, Bonita Avenue, strangely enough. And we changed our flats three times during the two years that we were there.

RQ: Well, I walked up there, directed to the Old Radiation Lab., and I walked in through the door. I was worrying — I can remember this very well because we hadn't been married that long, my wife and I — and one of the few new clothes that had been bought for the wedding was a rather expensive sports coat in a particular kind of thorn-proof tweed that I'd had from Cambridge, from Buttress's in Cambridge. This was my best jacket. I walked in through the door and I was greeted by the sight of several people in the main lab. and Calvin, as I found out, was standing on the table in the middle with a camera on a tripod; Paul Hayes had got a molecular model on the table and Calvin, standing on the table, was levelling up a camera to take a photograph — it was thioctic acid he was photographing — and as I came in the door, I met Alice Holtham, the secretary, was near the door, and I introduced myself so she walked into the lab. to Calvin and said: "Here is Dr. Quayle from Cambridge." Calvin stopped photographing, looked at me, and shouted, "Hey, he's got a jacket just like mine!" (*Laughter*) My first impression, as you can imagine, coming from the austere hierarchy of Professor Todd's lab. into this atmosphere where the Professor was standing on a table photographing and shouted at a visitor about his jacket, was something I was quite unprepared for. In Cambridge there was a chain of command; you were under day-to-day supervision by one of the younger staff who would then report to Todd, who would then come and see you once a month, and there was no way he would be standing on a table shouting at newcomers.

RQ: No, I was just amazed and surprised as, of course, we had travelled to the new world for the first time, life was full of surprises and here was another one — a totally different kind of academic atmosphere. When he had finished his photographs he said, “Come along to my office and we’ll chat about what you are going to do.” I then followed him to his office in the Chemistry Department and we sat down and immediately addressed me by my first name: “What are you going to work on?” So I said, well, you did mention in your letter that I would be looking at the role of carotenoids in photosynthesis, so that’s the area I thought I would be working on. He looked slightly puzzled and after a few minutes’ reflection said, “Oh yes, I did mention that, yes, well you could work on that if you wanted, you know, we’re still quite interested, but I don’t think it is involved in the primary act; we think it’s something much closer to a sugar phosphate.” So he said, “If you really want to work on carotenoids you can but go and talk with Andy Benson and Al Bassham who are more concerned with the carboxylation reaction which I don’t think is going by the carotenoids.” And that was it. He was obviously busy.

RQ: No. And so back I went to the lab. and was surrounded by these friendly, eager people who wanted to talk and we started from there.

RQ: All on Day 1. I mean, there were some things to attend to. Andy Benson said, “Have you got your luggage?” I said “No, it’s down on the station somewhere.” So he said, “Well, we’ll get that up,” and we went in his car. It was Andy Benson and his family who were tremendously helpful. He was the sort of deputy head of the lab., and he just took us aboard, both of us; his wife had us in and they solved all kinds of practical problems of newcomers coming in from outside.

RQ: Well, there was no doubt that Calvin was the source, the bubbling source, of ideas. They came out of him — whenever he came into the lab. he'd got some new idea which was going to be revolutionary. He would come tearing into the lab. with this new idea which you'd have to stop and listen to and he'd pull those finger joints...

RQ: ...clickety click. If he felt you weren't quite, you know, keeping up with him he would sort of look at you and click, click, click. It was most off-putting. And then he'd bubble forth: it was this compound, that compound, and "You understand, Rod, do you, you're following me?" And then he would go away and, Andy, who would have listened to all this, said "Oh, that's his latest theory, is it? Well it's nonsense, it won't work because of this or that." So there was, between Andy and Calvin, there was a sort of tension, in a way. It probably was a creative tension, I think, but Andy could see reasons why something wouldn't work and he would know very well that in two days' time there would be another rush of ideas that would come in. Andy was a very good practical person, you know. He knew how the whole lab. worked, what you could do with radioactivity and how to cope with it. I think it was a pretty creative tension between the two.

RQ: As far as you could tell. It was something that I felt was the working arrangement between the brilliant chap at the top and somebody who had seen it all happen before, perhaps sometimes a bit frustrated that perhaps not enough credit was coming his way, I don't know. There was a tension there

VM: They settled you in the lab. and they gave you a bench, presumably?

RQ: They gave me a bench and I was really left to talk to Al Bassham, Andy Benson, Clint Fuller, who spent considerable time talking about it, and Calvin, who popped in and out, said, “Find out about thioctic acid, very interesting, this is where the energy is really coming — from the cleavage and shutting of this ring. While you’re sort of talking about this carboxylation reaction, have a word with Paul Hayes, who is doing some spectroscopic work.”

VM: He was doing experimental work at that time?

RQ: He was doing directed spectroscopic work. Calvin said, “We want some more model compounds to study the spectra of. Maybe you could make some thiozolidenes, that’s what we need. Have a word with Paul; he knows what structures we want.” So for the first month, I suppose, I just did some straightforward synthetic organic chemistry making some model compounds for spectral work.

VM: This was in ORL?

RQ: This was in ORL.

VM: Can you remember, you were in the big room?

RQ: I was in the room with the big table with the white top upon which chromatograms were spread.

VM: Who were your neighbours?

RQ: On one side of the bench was Dan Bradley, with a noisy calculating machine on the bench, and on the other side was Al Bassham, through the grid on the other side was Al Bassham, so I was sandwiched between the two.

VM: Did Al at that point have his glassed-in office in the corner of one of those rooms? That must have come later.

RQ: No. Andy Benson and Clint Fuller had an office — they shared an office which was glass partitioned off from the rest.

VM: It would have been that office that Al moved into when Andy had left?

RQ: Presumably.

VM: And people congregated around this big white table, did they?

RQ: Yes, yes, it was the focal point. Everything was laid there; chromatograms were laid out there, Calvin was in and out and would pass judgement on these as he dashed in and out. Coffee was taken in the lab. The two Negro ladies, the glass washers, Altha (*Van*) and Alice (*name?*), used to make the coffee which we drank in the lab. I don't know how much radioactivity was taken in at the same time. The precautions were, by today's standards, minimal.

VM: So what specific project did you actually start working on when you got organised?

RQ: Well when I finally got organised it was obviously clear from the *Path of Carbon XXI*, it was quite clear from those beautiful, bouncing pools in the experiments that were done, which I think one of the great classics in dynamic biochemistry was the way they studied the pool sizes on changing the conditions with switching the light off or replacing CO₂ with nitrogen. I thought that was just an amazing piece of work. And it was quite clear from that that there was some compound in the diphosphate area that was probably the acceptor. I mean everything pointed to it. When you suddenly cut the CO₂ off, a compound suddenly increased in size judging by the radioactive area and it was in the diphosphate area and logic told you that that must be the acceptor or close to it. And so talking with the others it seemed that what we ought to do was to repeat the experiment in a lollipop-type apparatus but with no tracer, flush the system at steady state with nitrogen so that you bumped up the size of the pool, kill it with alcohol, run it as a stripe on chromatographs with markers of a radioactive diphosphate area at either side, cut a strip across and elute it and whatever was in there should be the acceptor which, if you then did an incubation with the eluate and ¹⁴CO₂, you ought to get fixation. And that's what we did and it worked. Then the next part was to confirm, well, what is it in the diphosphate area that is doing the fixation? If it's a diphosphate we ought to be able to separate it and at that time you separated it by ion exchange chromatography. So the next thing was to set up a separation procedure on ion exchange columns and then make an enzyme extract to do a long-term incubation and then separate out the compounds on the ion exchange resin.

At that time in order to make a diphosphate from a monophosphate, if you wanted to use a biological system for doing it, then you would purify or semi-purify a kinase system. And I remember we thought we would use a crudish extract, get a kinase activity there and the kinase extract will probably give us a mixture of, you know, hexose diphosphates, there will be di- and tri-pyridine nucleotide phosphate, and hopefully the acceptor, and we know how to separate those but we need to find out how to follow the kinase activity. And the way that we biochemists did it at that time was to work in a Warburg apparatus with bicarbonate buffer and as the kinase activity proceeded you got mole for mole acid equivalent in a bicarbonate buffer and you measured the CO_2 . Well I had never seen a Warburg apparatus. And when I reported to Calvin that the biochemists seem to run these kinase experiments manometrically with a Warburg he said, "OK, that's what we'll do then." I said, "Well, I have never handled a Warburg, I don't what they're..." He said, "Well learn! We're here to learn!" And so I did.

VM: Was there one in the lab.?

RQ: No, there wasn't one in the lab. — I had to talk to the biochemists, and I knew some of them anyway, and we got together a manometric apparatus and with trial and error we learned how to do it. We set it up in a little dark room on the ground floor as the only place there was room to put another shaking incubator in and it was a room in

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which Dan Bradley had got a stroboscope attached underneath a water bath and he was doing stroboscopic work on some part of the physical chemistry. It was the weirdest place to work because every now and again he would turn the lights off and switch his stroboscope on and jump up and down to coincide with the frequency of his flashes.

VM: He jumped up and down?

RQ: Yes, he would jump up and down so that, to your eye when he'd got it right, he appeared to float in the air. He synchronised his jumping to the flashing and you have this weird figure of Bradley 3 ft. off the floor.

VM: You only saw him, of course, when the light was on and when it was off...

RQ: ...he bumped on the floor. It was pitch dark and he had a wonderful illusion going which provided a lot of fun in the lab.

VM: Yes, people used to watch this, did they?

RQ: Fortunately, the approach for this carboxylation reaction was a good one and I think we had Andy Benson, Clint Fuller, myself and Melvin on the publication. It was really one of those discoveries that came simultaneously with virtually the same thing from another lab. and if you look in the publications in 1954 in the *Communications to the Journal of the American Chemical Society* you will find that the note we published was followed by one from Horecker's lab. with Weisbach, Horecker and Smyrniotis, in which they used a spinach preparation but they worked from ribose-5-phosphate and ATP. We were just that bit further along with having got hold of ribulose diphosphate, whereas Horecker was presuming that was what he was making.

VM: He hadn't laid his hands on that yet?

RQ: He hadn't laid his hands on the sort of Holy Grail compound which was RuDP (= *ribulose diphosphate*), so we were just that little bit ahead but in all fairness the two labs. were side by side on that.

VM: Did you know about Horecker's work and how close they were?

RQ: Strange to say, Horecker's note was sent to Calvin for refereeing and it came into the lab. at the time that we were publishing and Calvin came into the lab. with this manuscript, looking concerned. But yes, we knew about it in that it did come in for refereeing and the two papers were published together.

VM: But you'd essentially done your work by that time.

RQ: We had done it and we were home and dry on that. Then after that came the whole business of isolating enough RuDP chemically to actually do some proper chemistry and biochemistry. It is one thing just eluting something off a paper chromatogram in

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microgram amounts and another thing to get it in substrate chemical quantities. We then moved on to large-scale incubations with partially purified extracts and kinase to make RuDP enzymatically, purify on ion exchange, precipitate as a barium salt and then do some chemistry and biochemistry.

VM: And you got sizeable amounts did you, workable amounts?

RQ: Oh yes, we were working with something like 150 mg. of barium salt so we were working on enough to determine the liability of the phosphate groups; we could identify unequivocally what sugar it was, we could identify unequivocally where the phosphates were and towards the end of my 2 years we were trying to find any spectroscopic evidence for the form in which the sugar phosphate worked — the most likely form seemed to be ene-diol, and we were trying very hard to see if we could get any spectroscopic evidence for a proportion of ene-diol in solution. But I left at the time where we really hadn't got any strong evidence.

VM: I remember two or three years later people were going great guns — Bob Rabin, in particular, was looking at possible enzymatic reactions for carboxylation. Were you already thinking of these at the time, when you were working on the ene-diol possibility?

RQ: Oh yes, certainly when we looked at ribulose diphosphate as a substrate we drew out on the blackboard the different chemical forms that it might take in solution and wondered whether a cyclic phosphate would be there. We wondered whether the carbonyl group would migrate to another part. We wondered about ene-diol. We wondered about acetal configurations. But the one we kept coming back to was the carboxylation of an ene-diol form a bit like PEP (= *phosphoenolpyruvate*) carboxylate as the model. That was the one we felt... The weakness of ribulose diphosphate was, to us, that due to the fact that it was a pentose-1,5-diphosphate it couldn't have a ring. Hence, it was a sugar phosphate with a free carbonyl, no possibility of furanose or pyranose rings, and hence you could do things with it that you couldn't do with a hexose.

VM: As it turned out, presumably what you learned there about the carbon cycle and radio tracer work was the springboard from which you developed all your C₁ stuff later on.

RQ: Yes. I arrived at Berkeley really as an organic chemist. I could not have written down the TCA cycle; I couldn't even have written down the glycolytic pathway. My ignorance was as profound as that. All I knew was that I really was fascinated by how a cell works its chemistry — that was what really interested me. So when I arrived I started to read the biochemistry textbooks and I worked my way through Needham's *Dynamic Biochemistry*, Baldwin's *Dynamic Biochemistry*, I worked my way round the TCA cycle, up and down the glycolytic cycle, pentose phosphate cycle — they were all absolutely new to me. And, of course, they were the *lingua franca* of the lab. Everybody spoke in terms of these cycles and sequences; so with a few months of that I felt I'd got hold of the main parts of central intermediate metabolism and certainly learned what makes a cycle and what you have to do to establish it. And so

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by the time I finished there in two years I considered myself almost a biochemist then.

VM: When you went back to Oxford, just to look forward because it's a suitable time to do so, I remember, but I may not remember this correctly, but did you and Hans (*Kornberg*) agree that you would do the C_1 and he would do the C_2 ? (*This is an error: Kornberg did the C_2 , not the C_3 .*)

RQ: At Oxford, yes. When Hans and I — he came to the lab. as a Commonwealth Fund Fellow in the summer of 1954.

VM: Is that when you first met him?

RQ: That is when I first met him, and that, in itself, was quite interesting because I was working in the lab. one summer afternoon. Calvin rang up from his office and said, "Rod, will you come down to my office; I've got Kornberg here. Kornberg is coming to work in the lab. for two or three months and I'd like him to work with you. So can you come down?" Now, to me, Kornberg meant Arthur Kornberg. I was amazed and horrified at the thought that Arthur Kornberg...; well I thought it was weird that we hadn't heard that Arthur Kornberg was coming here for a visit. And what was even more weird is that Calvin should think he should work with me when I was only just learning some of this intermediate metabolism. Anyway, with some trepidation I went down to him and there was the young Hans Kornberg, whom I hadn't heard of and whom I had never met because he'd come from Sheffield working on urease in cats and had been for a year as a Commonwealth Fund Fellow with Racker and hence was working in pentose phosphates, although I didn't know that. And so Hans and I collaborated in which we did a lot of inhibitor work on the photosynthesis enzymes, you know, what inhibitors worked and what were the consequences in cell-free systems, and so on.

So that was the start of an association with Hans and it was renewed when we met again in England. I was working in the Colonial Products Laboratory, which is the job I went back to from Berkeley. He was in the MRC unit with Krebs. And as a result of that meeting I was brought into contact with Krebs who ultimately offered a post in his MRC unit, which I joined. At that time, of course, Hans was working on the biosynthesis of cell constituents from C_2 compounds and I helped him for about the first year doing some isotopic degradations that needed doing. In the meantime, between us, we sort of carved up — he would concentrate on biosynthesis from C_2 s and I would go down to reduced C_1 s, really.

VM: And that actually carried you through much of your experimental work?

RQ: And that actually opened up into something that kept me busy for a very long time.

VM: But to come back to Berkeley and the early '50s when you were there — the seminars, the Friday seminars?

RQ: Yes, they were, to an English person coming in from the formal atmosphere of seminars in Cambridge where people did have to give seminars but they were well prepared, delivered at tea time — the Calvin seminars were terrifying. Eight o'clock in the morning with, at that time, no warning. It was Calvin's thesis that anybody should be able to tell you what they're doing. You know, if you are working at the bench and if somebody asks you what you are doing, you should be able to tell them! So it was nothing more or less than Calvin coming in at eight in the morning, and he'd look round and he would say, "(so and so) I haven't heard from you recently, tell us what you're up to". And it was as unprepared as that. Nobody knew when they were going to be dropped on. Once you started with a seminar, Calvin could tear you to pieces. He got lost in the science, totally divorced from any personal feelings, and he would shred you. If the science was bad he would be so carried up with it that he would shred you to bits if he thought your science was sloppy. And I have seen someone — I certainly saw one (a female research student) actually reduced to tears because Calvin **would not let go** about some sloppy work until in the end she just burst out crying. And then Calvin would look very upset — what has he done...

VM: Did I do that?

RQ: ...you know, what happened? So they were terrifying and they were unpopular, not just because of the fact you had no warning but because you were continually needing to present some figures. It's one thing talking in general terms about what you are doing but in a seminar audience people want to know the evidence and you can't carry all your manometric data or whatever in your head, you need to have it with you.

VM: So did you come in with sheaves of paper?

RQ: Well, not really, because you wouldn't know what the hell to bring. And so after about a year and, I think, prompted too by the extreme distress of some of the more sensitive people, like this girl, suffered, I think people like Andy Benson managed to persuade Calvin that it would be better, and more profitable for us all, if we had a day's notice so that people could bring sense, as it were. So you would get a day's notice; the day before he would come along and say, "You, tomorrow."

VM: That second phase lasted a long time. What happened then was that people would go into total purdah for 24 hours preparing themselves for this, because it was decided at the Thursday lunches (and when I became part of the management team I was there) and we got fed up with that because it went on, because it took people out of circulation and we began to make programmes for months on end. I suppose some of the excitement then got lost as people really had prepared and prepared and prepared, but we got a more organised set of events.

RQ: Well, I was fully behind Calvin when he said “Anybody should be able to tell what you are trying to do and how far you’ve got.” You don’t need a week or two to prepare yourself to say it but you need some figures and a day is long enough. I was quite happy once the day rule had come. Eight o’clock in the morning to an English scientist was a shock because Calvin, of course, started work at about six and by eight

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o'clock all cylinders were firing and tuned and he'd got all his ideas, and it came as a surprise. But you got used to it and, having worked for a year later in my career in Germany, the German labs. started at eight as well and perhaps it was the English labs. which were, perhaps, too laid back in that regard.

VM: Were you there when Andy left?

RQ: No.

VM: By the time you left, which was in the summer of '55?

RQ: It was in the early summer of '55.

VM: Was there any sign of Andy leaving at that point?

RQ: We weren't surprised — no, as a matter of fact I'm wrong. I think he had gone to Penn State, now you mention it, because we did call in on him on our way home. Yes, I think he'd left, if I'm not mixing up calling in on him on another of our visits to the States. No, I think he must have left.

VM: So that you're not conscious now, at any rate, of any great upheaval at the time when he left?

RQ: Yes, as you mention it, I think I can remember a sort of tension between him and Calvin probably had surfaced to the point where...I'm sure, now I think about it.

VM: What about parties in the lab., the social side of things, what comes to mind?

RQ: Well, it was a very sociable organisation, it really was. We were a very mixed crowd; we were from all different countries; we were truly a multidisciplinary team with botanists, physicists, chemists, mathematicians — we were all there. Several of us were quite young postdocs and it was very, very sociable. People invited you to their houses for evenings, for dinners. We used to go up to places like Yosemite or down to Death Valley as a group in the lab. These would be organised at very short notice and Friday night would see two or three cars going up to Yosemite and back on Sunday night.

VM: Had you a car at the time?

RQ: We never had a car. So we were always carried about in other cars.

SM: Did you find that the social life in that group, the way that things worked, coloured the way in which you worked with your own groups later?

RQ: Yes, I think so. We certainly, I think as a result of that, those of us who had been in that group, I think we did get used to the fact that research groups needed a kind of cohesion in the form of, certainly, seminars of an informal kind.

(Tape turned over)

RQ: I think — it brought home to us, in way we'd never been exposed to in England — was the fact that a really dynamic kind of research environment was engendered so much by the people at the top being actually part of it on a day-to-day basis. The idea of a senior scientist doing his research through some deputies who were doing the supervision just seemed to us, after that experience, quite a strange way of doing things. Of course, organic chemists tend to have huge research groups compared to biologists (you see some research groups of ten or twelve) and biologists, by and large worked with much smaller groups because the senior people are part of the activity and I think you learned that in Berkeley very, very quickly, to one's advantage.

VM: Have you worked in any other American labs. apart from Calvin's?

RQ: Yes, I spent a summer in Seattle as a Walker Ames Professor and the host lab. belonged to an ex-postdoc. of mine, Mary Lindstrom, and she, at that time, was an Assistant Professor. She is now a full Professor at Cal Tech. And there it was the same sort of thing. She ran a very tight research group which knew each other very well, they socialised with each other and she, as the research head of it, was there with them all the time. And I spent a summer there.

VM: I wonder whether in Britain nowadays, at least biochemistry labs. are now more like the American level you first met in Calvin's lab. than they were once upon a time.

RQ: Well, I came back to this country, and when I joined Krebs' MRC unit, as distinct from my previous time in England as an organic chemist, I was now in a biochemical atmosphere and the people that I knew and worked with and was associated with worked in small, highly active groups. I mean, Krebs was in the lab. every day. He had his own technicians. Krebs designed the experiments for them; he'd come down during the day to see how the results were coming out. It was the same sort of system. I think that there is a cultural difference between, perhaps, the English and the Americans, but more importantly, I think there was a discipline difference between the chemists and the biologists — a big cultural difference between the two whether they be Americans or English.

VM: You may not have felt this so much, coming from Todd's lab., but when I went to Calvin a year or two later, after you, one of the things which struck me, apart from all these things which you've mentioned, was the lavishness of the support. They had equipment which I had never dreamed of. Did you find this same sort of thing when you got there?

RQ: Yes. The equipment was not the limiting factor on what you were trying to do because they had it. If there was a good reason why such and such an instrument should be tried, they'd have it.

VM: And they would make it, if necessary.

RQ: If necessary they'd make it. Or they would buy something that was near to it and alter it. And I was very impressed at the way new pieces of equipment, like gas analysers, would come in in their boxes and within minutes the people like Al Bassham would have the cases off while they modified them inside to do the job they wanted. And that, to me, was amazing because to a chemist these instruments were boxes of tricks which you called skilled technicians in to modify and these people could do it themselves and expected to. No, there was never any lack of equipment at all.

I suppose I've been fortunate in that respect because when I came back to England we were part of the MRC and Krebs' unit was well supported. When there was a good case for something we got it, so I think I was lucky in that regard.

VM: So you left that group in the early summer of '55. Did you make a long trip back to England or a quick one?

RQ: We took a long trip back. We went back by train up the Oregon coast. The lab. saw us off on the station — the whole lab. turned out. They presented me with a huge Stetson hat, which I still possess, and they also made it, when the train turned in, they made it as if we were a newly-married couple. They showered us with confetti and we got aboard the train and sat up coach class up to Portland, Oregon, with some matrons looking at us, looking at me, anyway, in a very poor light as this is not a good way to be treating your young wife, sitting up on a coach class train. And we stayed with Howard Mason, the oxygenase man whom I know. We went on up to Vancouver, stayed with Khorana, whom we knew from Cambridge days, and then across Canada, ultimately to New York where we stayed with Paul Srere who was working in Racker's lab.

VM: Yes, I didn't realise that Paul had been in Racker's lab. I've known him in recent years.

RQ: So, we got to know Paul quite well.

VM: There is, I remember, a picture of five of you in deerstalker hats. Where did they come from? How did that happen?

RQ: They came from Lilywhite's in London and it was part of the fascination, I think, that some of the American's had for the English way of life. One of the peculiarities to the Americans of one part of the English way of life was the fact that some gentry wore deerstalker hats and I offered to send to Lilywhite's for a supply for those who would like to wear them. I've forgotten now how many people said they would like a deerstalker but Calvin certainly wanted to be in on it so an order was sent off for deerstalkers which arrived in Berkeley.

VM: You are on the steps: there's Calvin, you, Malcolm Thain, Clint (*Fuller*) and there was a fifth, was there? I can't remember now. (*It was Rich Norris.*)

RQ: I'd have to look the photo up; I've still got it.

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VM: Where's the hat?

RQ: Oh, the hat. Well, my hat did quite good service but it did shrink in the rain, as those things tended to. And mine shrunk to a point where I couldn't wear it anymore.

SM: And you don't have it?

RQ: And I don't have it.

VM: I was hoping to take a photograph of you in that hat.

RQ: I have the Stetson that was given to me and that hasn't shrunk.

VM: I'll take a picture of that if I may.

SM: Was there any connection with Conan Doyle as far as deerstalkers?

RQ: Sherlock Holmes. Yes, they knew all about Sherlock Holmes. There were all kinds of things like English tea and marmalade and stuff.

SM: They did have a very warm feeling towards us, I remember. They really did. And I remember once being in the Co-op, which I am sure you were familiar with in Berkeley, and some ladies were looking hopefully at the fish paste in order to make sandwiches for tea for the cricket club in Berkeley. Did you have anything to do with anything like that while you were there?

RQ: No, I don't think so, no. But English people, you are quite right, I thought, were regarded with a lot of affection there and they certainly seemed to be welcome in places.

One of the overwhelming impressions on me was the fact, in the States, you came into a lab. and anything was possible. Coming from the division of academic departments in England, Departments of Organic Chemistry, Departments of Physical Chemistry, Departments of Chemistry, all very separate, and you were labelled as being one in that discipline. It was very rigid. And suddenly to come into a lab. where you were a scientist: you happened to be a chemist, but the chap next to you was a botanist and the other chap next to you was a physicist, and if you hadn't ever seen a Warburg manometer in your life before, if it was necessary to use it, you learned. Calvin, himself, would learn from the man who cleaned the lavatories. If the man who cleaned the lavatories had by any chance a good idea, Calvin would have it out from him without a trace of embarrassment. You wouldn't get a top English professor learning anything, well, from a junior research assistant for that matter, never mind the chap who cleaned the lavatories. It was: "We are all scientists! We are here to learn! If you have never heard about how to do that, learn it! That's what we're here for!" It was that, I think, great freedom on how you would tackle a problem, which in this case did demand different sciences coming together. And the fact was, of course, he could cope with it. He was such a polymath, himself, he thought everyone else

would be naturally a scientist rather than one kind of scientist. And that, I think, came as a tremendous eye-opener.

VM: There is something that struck me very much when I first went there and I don't know whether it is specifically Calvin or whether this is more generally an American phenomenon, but compared with the English lab. which I left in 1956 there was much, much more to-ing and fro-ing between the individual researchers. Everybody seemed interested in what everybody else was doing and they formed collaborations all the time. Whereas in England beforehand people kept to themselves more and didn't, as it were, muscle in. Did you find that sort of thing?

RQ: Yes. That was certainly true. There was a much more dynamic kind of atmosphere.

VM: In fact, 10 years after I got there, and I think it was literally so in '66, a chap called Ian Morris, who was an algologist from University College, one of Phil Syrett's students originally, who came there as a postdoc. and he came from the same place as I had and I observed him reacting in the way I had reacted 10 years earlier. This business of sitting and talking and somebody pops in and says, "Did you know so and so and have you thought of this?" It just hadn't happened to him in England just as it hadn't happened to me. That was part of it. I don't know whether this was specifically the result of Calvin or whether Americans just tend to do that and always have more than the British have.

RQ: Well, I think they are more, you see it now (*with*) Americans: anything is possible. If you really want to do something, do it. It's all possible.

VM: Don't look for reasons why not.

RQ: Don't look for reasons why not. I remember when I turned up in Oxford and when I was working out what I might do research in, I'd might go to talk to D.D. Woods, who was the Professor of Microbiology. And DD would give you 20 reasons why you shouldn't try. He was a very good scientist and his prognostications of difficulty were well founded but he would sit there and: this mightn't work and that mightn't work and have you thought why you shouldn't do this because that will happen. You go into somebody, say, in the Berkeley set-up there and he would listen to you and say, "OK, we'll try it tomorrow at nine o'clock."

SM: It is a very different sort of ethos. It's somehow a very English thing, this derivation of strength through adversity. It is good for you to be having a hard time, in a sense, and if you come out of this with something then it is really remarkable.

RQ: Yes, I suppose. But, well, it's optimistic. It may work, so let's do it.

SM: Do you think that any of this was coloured by, the difference, was coloured by the immediate post-war feeling in England? Because it still felt very much like that in the early '50s. Whereas, coming to Berkeley, there had clearly never been one (*i.e. a war*); it was very different.

RQ: It was very different. Yes, there was a big difference in the whole atmosphere. We came from post-war England, where we were still on rationing, into this great paradise out there where food was flowing out of the shops, equipment out of the labs. and money and so on. The whole thing was, from drab post-war England with your one and sixpenny worth of meat a week, quite something.

VM: You got used to it, didn't you. It didn't take long.

RQ: Well, you had to. I mean, for all Calvin's (I think he is a wonderful character) — he did not tolerate fools gladly. He'd tear you to bits if you came up with some sloppy science; well, I don't think he would want you there for very long. He'd have you out as soon as he could. He just could not bear somebody (*not following him*)... he got very bothered if he felt people didn't know what he was talking about, you know: knuckle pulling: "Are you following me?" Click, click, click. I don't know if he carried on doing it.

VM: Yes, he did and you are right. I'd forgotten but you are right. Now that you mention it, I certainly remember. I think he's stopped now, but still...