

Chapter 44

SIDNEY ALAN BARKER

Birmingham

April 16th, 1997

VM = Vivian Moses; AB = Alan Barker; SM = Sheila Moses

VM: This is a conversation with Alan Barker in Birmingham on the 16th of April, 1997.

What sort of background did you have and how did it take you to Calvin?

AB: Well, I worked with at a time just prior to Calvin, I was working in the University of Birmingham in the Chemistry Department under Professor Morris Stacy. Previous to that I had worked under Dr. Peat, later Professor Peat, at Bangor University and the late...well, they've all now died...but also my team leader under Peat was Edward Bourne who was later Professor Bourne at the Royal Holloway College, London. Effectively, the scenario was that I had been a student at the University of Birmingham all the time since 1944, got my BSc in '47, PhD in '50. I never wanted to lecture. I only wanted ever to do research. So on getting my PhD I was offered a lectureship but I refused it. Has anybody ever done that before?

VM: It's rare!

AB: But in those days it was very difficult to stay on as a research fellow. I did get a research fellow with the Brewing Research Association under Sir Ian Harborough and later I became associated with The Royal Society. Harborough had entered me for a competition; I will look up the records and tell you what the name was. I went down to London and was interviewed by august people like Sir Lawrence Bragg and those sort of people; Sir Ian Harborough had deliberately absented himself. By the time I got back to Birmingham, I was told that I had got this fellowship. I got married in 1952 after getting my degree in '50 and suddenly, of course, I was aware that you should support your wife and have a more stable background. I then sought to get a lectureship; none was available until 1954. Then Professor Stacy invited me to dinner in the centre of Birmingham with a man called Dr. Gerald Pomerat of The Rockefeller Research Foundation. I was duly told that I could obtain a research fellowship at a destination then unknown. Eventually this turned out to be in September 1955; Dr. Calvin at the University of California at Berkeley for six months until March 1956 and then went east to work with Dr. Michael Heidelberger in New Jersey at the Waxman Institute, the microbiological institute that Waxman had built with the proceeds of the royalties of streptomycin. He was, at that time, in dispute

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with his (*indecipherable*) about the fact that they had not benefited from any royalties from streptomycin.

VM: What had been your particular area of chemistry, had you known anything about Calvin before then?

AB: Not really, not Calvin. I had from the very beginning, that means from 1947 to '50, worked on enzymes of the potato initially, under Dr. Bourne the team leader and Dr. Peat, later Professor Peat. I learned the lessons the hard way of proving what you found. In fact, within ten weeks I had achieved the first proved enzymic synthesis of an amylopectin from amylose. This was to be the last peg that Dr. Peat needed to get his FRS! Everybody was waiting for this achievement. The problem had been that the enzyme, which they called *Q-enzyme*, the branching enzyme had been unstable for several years. I had started in August, because you started then in those days, you worked through the holidays because it was still just after the war, and I'd started on isolating an enzyme called *amylosynthase* from yeast. I was disgusted because I had no success in following the Japanese directions. At which Dr. Peat suggested I use the same method of isolation on getting an enzyme or enzymes out of potato. He didn't brief me that the enzyme that he was working on was so unstable. I used the worst possible thing which was to precipitate the enzyme as a lead complex, no less, and then you removed all the lead as a carbonate and you isolated the protein which you then duly fractionated, and I fractionated into the phosphorylase enzyme, the one that synthesised the linear component of starch, and then the one that they called Q, or the branching enzyme, the one that converted amylose to amylopectin. They were remarkably stable having gone through this lead complex. After some preliminary work, I was told to duly synthesise amylopectin in large enough amounts to do an extensive examination and proof of the structure of amylopectin.

I therefore came in one day at 7:30 a.m. in the morning. I and a technician peeled one hundredweight of potatoes between us, remarkably by midday we had isolated a very large amount of Q enzyme; in the afternoon of that day we incubated it with amylose, monitored it and examined and isolated amylopectin by that night. Within a few days I had characterised amylopectin and my team leader, Dr. Bourne and Dr. Peat were overjoyed and rushed off a paper to *Nature*. I thought how wonderful it was to be a research scientist! Unfortunately, the next Easter following — that would be Easter 1948 — shortly after the paper had been published in *Nature*...

VM: With your name on it, of course.

AB: ...with my name on it...there was a complete rebuttal from Kurt Meyer and his main assistant. They said that the enzyme I had isolated really needed phosphate to do the conversion and I had said that it didn't need phosphate. Phosphate, of course, is required to break down amylose to glucose-1-phosphate. So everybody then was wondering whether the little boy wonder would come up and really prove it and grind the rebuttal into the ground. I had a very good teacher in Edward Bourne, and he and I ruthlessly, relentlessly, month after month did work that eventually absolutely proved that the enzyme did not require phosphate. We published papers to that effect in the *Journal of the Chemical Society* to that effect. Eventually, one by one, because

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there had been a follow-up by another person saying that...rebutting the work, eventually Kurt Meyer made a complete and utter rebuttal at the American Chemical Society. I think, when Stacy was there. We had presented all the evidence in 1949 at the International Biochemical Congress, I think, held at Cambridge at which Kurt Meyer was there when he listened to the evidence. He then went back, got somebody else to check out his assistant's work and found it was faulty. Therefore, the following year in 1950, when Stacy and he (Meyer) went to an American Chemical Society, Kurt Meyer said we were right and he was wrong.

VM: To move you on toward the time you went to Berkeley, you were, in effect, a carbohydrate-enzymological chemist-biochemist.

AB: That's right. After my PhD for the Brewing Research Association, I started working on bacteria, isolating enzymes, particularly from *Aspergillus niger*, doing various enzymic conversions, mainly breaking bonds and making a new one, conserving the energy — generally without any phosphate being present.

VM: Before you got to Calvin, had you had any prior communication with him about what you might do there?

AB: No. I arrived on the doorstep...

VM: Married?

AB: Married with my wife. We had landed at New York to be greeted by Professor Michael Heidelberger, we stayed the night at Central Park West there, we saw the Rockefeller Foundation man in New York who duly provided us with the necessary tickets to get to Chicago on the train. I remember we bought some provisions including some frozen orange juice which duly exploded in the toilet that was attached to the compartment. At Chicago we went, I think, to Buffalo to see Niagara Falls; I'm not quite sure but eventually we arrived in San Francisco, got to Berkeley.

VM: Anybody meet you?

AB: I don't recollect now. Calvin had a wonderful secretary who managed to do sort of everything for all these foreign students. At the time I joined Calvin, in September '55, he'd got about 30-32 students. There was still a semblance of wartime. Bassham came in his uniform, his reserve uniform occasionally. (Now I have to refer to photographs to remind me; just a minute.) Alice Holtham sort of literally mothered we foreign students, looked after wives, got me a flat, my wife and I a flat, just opposite in Hearst Avenue, I believe, overlooking...one could always smell the eucalyptus trees which I do now when I go over to America. And really took care of the group. Some of the people there at the time, and we were in the old hut, the Old Radiation Laboratory still, I remember I spent my 30th birthday there and suddenly realised how old I was. They had a party in the lab. for me, I remember, it was April 13th.

VM: Who were the people in the lab. with you, do you remember?

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AB: Yes. Right opposite me was Pekka Linko and on the other side was an American student whose name I forget at the moment. One of the things I learned from going to America was to look at men as well as their achievements. I always had this thing, which I always tried to practice, that I wasn't teaching people chemistry I was teaching people chemistry plus how to use it, how to live, how to treat people, how to train people. I therefore was very interested in this man who was destined to be a Nobel Prize winner. Without Calvin knowing it, I was at that time sizing him up for Professor Murray Stacy who was on the Council of The Royal Society. And he (*Calvin*) was at that time, very soon afterwards, to become a Foreign Member, I think, of The Royal Society and eventually to get his Nobel Prize.

One has to put in context that it is very difficult to control 30 bright people of all nationalities. One gets as much from working for the great man as from the people you are actually surrounded by, from all these different countries. Calvin did have a nasty habit in my estimation. He would come through the back door and he would go immediately to the person who was having success in his research. At that time it was Pekka Linko and he would say "good morning, Barker" and not talk to me. He would say good morning to his American student and off he would go. So it became my great and burning ambition that this man would come to my desk and he would come every day and I would lead him and show him how to apply what he was trying to achieve to the benefit of mankind! That was my burning ambition, although he didn't know that at the time.

VM: You haven't told us what you were working on when you went to the lab.

AB: Calvin knew what his photosynthetic cycle was except that he couldn't prove the last piece of the jigsaw. If one goes from A-H in compounds one knows there's a missing piece to complete the crossword or jigsaw. He knew what it was, it was erythrose-4-phosphate, but he couldn't prove this. One of the things that Calvin benefited from was that each foreign student coming from a different country brought a new technique. He might at that time have the latest NMR because he was just getting from Perkin-Elmer the NMR which wasn't going to help him very much at that time. It didn't help him, certainly on the photosynthetic cycle. But I brought paper electrophoresis. I had become quite clever at separating things and devising ways by paper electrophoresis and so on to separate them. This is just what Calvin needed; he needed the ability to separate a radioactive spot that he presumed would be the area where he would find the last missing piece but he wanted to prove it without isolating it. Therefore, you had to do several things to it to follow the radioactivity, such as make it migrate as a negatively-charged entity, find out the strength of the acid by re-running it in an acid solution rather than neutral, show that it still ran as a charged molecule so that it was not something like a carboxylic acid — it was a stronger acid — treat the unknown radioactive spot with a phosphatase of wide applicability, get a neutral entity which then you separated and, because that was unstable and that was what was beating Calvin. He didn't appreciate that some of the small carbohydrates were highly unstable and what you needed to do, as they were produced by the enzyme, you literally reduced them to stable erythritol. Very soon he had a bag of proof. But the key things that I learned was, of course, it was Bascham showing me

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the practical way to do things, Bassham taking me into the more highly radioactive laboratory where the phosphate work was done, watching me that I didn't contaminate myself and other things, because I was a complete novice. He was a very good and practical teacher even to somebody like myself who was supposed to be a postdoctoral fellow. Calvin was literally a human dynamo, I suppose. It was a little disappointing initially that he had given me this project. I know he needed to solve it. It was the thing that had to be done. But effectively, other than introducing him to the techniques of paper electrophoresis and teaching him the instability of small carbohydrates, he and Bassham were telling me what to do almost like a technician.

VM: You published this work on electrophoresis?

AB: This work was published in *Biochim. Biophys. Acta* (21, 376, 1956); it carried the names J.A. Bassham, S.A. Barker, M. Calvin and U.C. Quarck. Calvin, 40 years later, was asked about this work because this was the last piece of the jigsaw he needed. He had the conception, the brilliant conception of how the sun's energy was trapped, but he needed just this last bit to prove his complete sequence. He was asked 40 years later, that's in 1996, I believe, what was the most difficult thing. The last piece to him was the difficult thing because he hadn't got the techniques and he hadn't got the carbohydrate knowledge. This man Gerald Pomerat was a very, very clever man for the Rockefeller Foundation and he literally knew who to put with what. He not only knew the great scientists in America but he was travelling constantly in Europe and he was a catalyst. It is significant to say that after that The Rockefeller Foundation never did much good for America before it was doing good in this way of fertilising European and US scientists. In 1956, following the Hungarian rising, all the Rockefeller Fellowships for Europeans were stopped and the money used for the people who had escaped from Hungary. I don't know what happened subsequently, whether it was resurrected. I felt it was a great loss to America, and a great loss to England, because it was cross fertilisation of the best and highest quality.

So he had taught me the technique of two-way paper chromatography which was the essence of...

VM: This was Bassham?

AB: Calvin and Bassham had taught me this technique of two-way chromatography which enabled them to separate all the radioactive compounds. I thought that was something. It was like a fingerprint of a cell at any time. I think that Calvin had got this brilliant conception of all he wanted in life was to show how the sun's energy was converted into chemical compounds. He couldn't see that technique that he had developed there, plus the techniques that the students had brought to him, would enable man to benefit in a practical way from this work.

Therefore, I set about secretly starting my own research. What I did was to send to Dr. Timpson at the Mellon Institute and ask for the latest anti-cancer drug. He sent me in 1956 azaserine and I couldn't have had a better compound to put into the photosynthetic network. Calvin had two strains of algae that the two coloured girls (*Alice Smith and Altha Vann*) cultured every day and gave us students. One was

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called *Scenedesmus* and the other was called *Chlorella*. Luckily the first one I used was *Scenedesmus* and I did attract the pathway of radioactive carbon in the presence and absence of azaserine. Then this brilliant method of separating in two directions gave me a map which showed that the azaserine was blocking at all points the transamination step. From the keto acids out you went to any other metabolic cycle, you found the same thing. That was the thing that brought Calvin to my desk. Then he would come every day and he was very, very excitable, he used to get very excited, there used to be long discussions. We published that in the *Journal of the American Chemical Society* in 1956 (78, 4632).

VM: And who were the authors on that?

AB: The authors on that were as before: S.A. Barker, J.A. Bassham, M. Calvin and U.C. Quarek. By the way, I forgot to mention in this article that I mentioned where Calvin is going back 40 years, there are pictures of myself and Carol and Bassham and the four of us, yes.

VM: You several times talked about Calvin and his brilliant concept: how did you find him when it came to nuts and bolts of doing scientific research? I appreciate you've been talking so far about the big picture. What about the little pictures? Was he good at that?

AB: That's a difficult one. Because, you see, by the time I knew him, my concept of the man was that he had worked under very difficult circumstances and achieved brilliant results. He had managed in an old dilapidated wooden hut to achieve a dream and he was rather like men I met afterwards (I'll tell you about that in a moment). So by the time I met him he wouldn't be a man who would be showing me or drawing me little pictures.

VM: When he sat down at your desk and looked at your own data, was he good at analysing it, was he good at suggesting with you what might come next, things of that sort?

AB: Not really because, you see, I wanted him to come to my desk; therefore I had done all the planning myself. I had sent away for the antibiotic, I had done all the experiments and told him about it afterwards. Of course, the idea that I had, and what made Calvin very excited, was that he'd got a technique that would enable drugs to be assessed in their metabolic action. I don't think he subsequently did anything with this golden opportunity. I myself couldn't do it because I would have needed three or four years to have set up all the techniques that Calvin had in his possession. At the time he was extremely excited about that possibility because it would save a lot of animal testing. And we are still talking about that in 1996. It's a thing that stuck with me all the years, that there seemed to be a complete inaction on the part of biochemists in trying to find practical ways to find out how drugs worked.

VM: Not entirely: when I was...

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AB: Not entirely. As an example when I later was working on the biological activity of compounds and I was working on (*indecipherable*) acid and went to the virology unit at the East Birmingham Hospital, then I was told of things like eggs that you injected an unknown compound to find out about its toxicity, one of the simplest ways of finding out whether something is toxic or not is actually to put it in an egg, not to use on an animal. I did over the years see several promising techniques along those lines. But nobody seems to put it together as an act, a true act — what I am going to do is to develop and test drugs and I'm not going to use animals, that logic...until the last extremity. It's always animals first. People seems to be trained to test the things on animals and they don't seem to take advantage of these techniques that are lost in the literature.

VM: Were you aware of any other group, outside the Calvin group, developing or using that sort of technology that you used and that many others used so successfully?

AB: No, no, not at that time.

VM: Later?

AB No, no. What happened was, you see, I didn't have the normal year with one person. I had been told, almost, I should go and work with Professor Michael Heidelberger (*Editor: Michael was Charles Heidelberger's father — Heidelberger was one of the four original PhD chemists who started the Bio-Organic Chemistry Group in 1946-1947*) because that was the next six months. Stacy had been taught by Heidelberger so, as it were, his "son" in certain terms was also to go. And, of course, Heidelberger died recently aged 100. When I went to the Microbiological Research Institute (*at Rutgers in New Jersey*), it had been newly built, I saw another Nobel Prize winner, Waxman, and, again, one saw a completely different man, a man again with a dream, nothing else but the dream mattered. So people didn't matter. But until you had seen his autobiography, you didn't understand this man. He was like an insect...

VM: This is Waxman?

AB: Waxman...he was a Jew, he had learned everything; he had been prevented from going to school as a Jew in Russia so he'd learned everything. You saw him being what *you* thought was rude, leaving a lecture because he couldn't stand receiving knowledge through his ears, he was used to receiving knowledge through his eyes. I saw in California the man *I* think was the top man that I ever met that was worthy of being a Nobel Prize winner, and that is Pauling. Pauling came to Berkeley. It was wonderful. He gave twelve lectures on how he solved problems. It was enthralling because here was a man revealing how his mind actually worked. Calvin never did that. Calvin had goals and concepts and how to achieve them but Pauling was revealing to ordinary scientists his innermost secret.

VM: How was Calvin with people in your experience? You said that Waxman was...

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- AB:** Calvin was very caring and very kind but he was like a modern business man, no time if there was no results. He'd got to the stage where, as I said, he would spend as much time as possible with the people who were getting, as it were, the success for him and not...he was not a teacher — at that time. That doesn't mean to say that early on he wasn't a teacher but he wasn't a teacher in that respect.
- VM:** You must have presumed that there were things, nevertheless, that you learned from being in the place.
- AB:** Oh yes. The other problem, I think, was that America was about to break out into a turmoil and the two girls, coloured girls, were in the same laboratory room as myself and Pekka Linko and the American student. Every day they used to grow the photosynthetic algae and it was a sacred trust. One, as I said, was *Scenedesmus*, on which I had done the work on azaserine. The work that wasn't published was on *Chlorella*. You see, the azaserine, to do its action on *Scenedesmus*, had to get through the cell wall. When we used *Chlorella*, it didn't do a darn thing. The controls and the results were exactly the same. And again, it was as though I was trained to teach Calvin a lesson. I know that's pompous but it is a lesson. There's a lot of lessons in nature that I almost store up for myself from other people. They become almost, as it were, generalities. It's a thing that I learned more from Heidelberger. I mean, Heidelberger at the same age or an older age and Calvin but, with only two or three students, was a great teacher. He taught you with his own fair hands, very much like Bassham did, and he was teaching you immunology. But he was teaching you in great detail in explaining the immunology and the immunochemistry and I was explaining to him the structure of the *Pneumococcus* polysaccharides which we were using.
- VM:** In the case of Calvin, as you say, there was a much bigger operation and Calvin was like some sort of...
- AB:** That's right. It was like a business manager, I'm afraid.
- VM:** You've told us something of what you learned directly from Al Bassham. What did you learn, do you think, from the experience, the experience of being in that lab. at that time?
- AB:** It was wonderful. Within that area, round Berkeley, there were seven Nobel Prize winners at that time, most of them were up on The Hill in the proper Radiation Laboratory where money was no object. There were great men on the campus and we used to, my wife and I, although she wasn't a chemist, we used to go and listen to these people. People like Pauling telling you how they solved problems; I mean he would take the example, which I could clearly understand, that you took an X-ray photograph of an unknown compound and ordinary people, like the people I had been associated with the X-ray department at the University of Birmingham, took three years to do the structure of glucose-1-phosphate in those days; you know, we are talking about Pauling had this concept that he would be like God, he would arrange the atoms in the molecules in the lowest state of energy or in the way that he thought they should be arranged. He would then predict what the X-ray picture should be and he would give himself about ten tries. Now, if he failed, he failed. But it didn't take

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him long to do that operation and, if he found one that fitted, he would have done it in a month whereas somebody would have taken three months. Not only that, he was doing much more than a man who was sort of pedestrianally solving a X-ray picture. He was pushing science forward at a much more rapid rate. He was showing he understood why the atoms were arranged in this way, not just finding out that the atoms happened to be arranged in this particular way.

VM: It sounds as if the lectures you went to by Pauling had as much, perhaps even more, influence on you than the time you spent in Calvin's lab.

AB: I never thought that anybody could manage 32 people and I don't think you can. You cannot. You have to be selective. You've got an objective. It's like somebody running a company, you're very efficient, you've only got a certain of time. I don't know whether he had had his first heart attack at that time...

VM: I think he had, yes.

AB: ...and therefore, he would be harbouring his strength. One has to take all these things into account when you are assessing the man. He gets his Nobel Prize, as far as I'm concerned, with a brilliant concept and getting the means to achieve it relentlessly. That's what he gets it for, not as a great teacher.

VM: As a great thinker?

AB: I couldn't assess that process because I am coming in at the last bit so I'm not allowed to do that. You don't know who contributed what. Certainly, there was lots of talk about the man that left (*Benson*) that he had a great deal to do with the grand work. He'd left by the time I arrived.

VM: This was Andy Benson?

AB: That's it. Again, I can't say that. Bassham wasn't a thinker. He was a teacher and I believed that Benson and Calvin were largely responsible for the result. The thing was carried on by its own momentum with this influx of foreign students.

I spoke before about Calvin's secretary. With a man like that, running 32 people, you do need to have a very efficient and caring person, somebody who is not only just a secretary to type endless papers you are going to produce with all these number of people. If I produce two in six months...I don't know what the output was in a year from Calvin's lab. with 32 people. You needed somebody extraordinary and she was extraordinary, Alice Holtham. I recollect on Thanksgiving Day on the November we students were taken to her mum and dad's house, Mr. and Mrs. Holtham. Our great friends there were Carol Quarck, who is on two of the papers, Nel, I think it's van der Meulen of Holland.

VM: Yes: we're going to see her in a couple of weeks.

AB: Remarried? Remember me to her. Her name is...

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VM: Prins-van der Meulen.

AB: I used to be a consultant at Gist Brocades and I used to travel every six weeks to Delft in Holland but never deviated, I'm afraid. This (*photo*) is Thanksgiving Day with Mr. and Mrs. Holtham — you can see Carole there; now you can see also the two coloured girls, Altha (*Vann*) and Alice (*Smith*), is it? I'll get their names correctly in a moment. But typical of the Holthams is that colour and creed didn't matter.

VM: Do you remember this character here?

AB: Yes; I think he's coming up in a moment.

VM: Do you remember his name? I'll tell you; it's Ning Pon.

AB: That's right, it's all written down. This is the apartment in Berkeley, one-bedroomed apartment. We overlooked the campus and therefore we didn't have a car which was again different from all the other students. That benefited us a great deal because, among the things I learned, I learned about American people and I wouldn't have learned about American people if I had a car; I went past them. By not having a car we had to do something different from all the other students. There was a wonderful lady at International House in Berkeley for the foreign students. Every other weekend we went in a troop of 30 or 40 in a bus to places like Paradise in Northern California. The Round Table there would then meet us. This is the really extraordinary thing because we were black, yellow, from Afghan; we were all colours and creeds. These ordinary citizens of Paradise or the other towns we went to would on no account have had coloured people — some of them wouldn't have had coloured people as guests. But, as we got off the bus, it was number order and you were numbered with the host and that was it.

Tape turned over

Typical of the things I learned was how you controlled peoples' minds. I had come to do chemistry but during the last world war, as a boy of thirteen or so, I was very interested in the Philips short-wave radio we had. I remember my father and I used to listen to America, Australia (the cricket there), to Haw-Haw, we listened to Moscow; there was no barriers there. When I got to America, of course, the era had come and gone. There were no short-wave radios in sight; everybody's radio was now medium or long-wave. Here, you have a great country and somebody in Wisconsin, at the time of the election, somebody in Wisconsin cannot find out what somebody in California is thinking because you've either got a national network or you've got a local network of California news. What you *haven't* got is an ability for news to transmit between California and Wisconsin. Perhaps the Internet will do that for us.

VM: Incidentally, before you leave it, one of the things that many people have talked about is what you termed the "old hut", the Old Radiation Laboratory. People have felt, some people — I wonder how you feel — that it was a factor in determining the quality and the character of the group of people who worked in it. Do you think that?

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AB: I think: yes. One of the things I think you learn is that machines rule man and the other thing you learn is to keep men well away from machines! If you automate a process, the one thing you shouldn't do is to put a man near the machine because he still believes he's working a full day's work. I practice that. When the machines came to automate analysis, I found, if I put them in the lab., I got no work done. By putting them into a room somebody had to find something to do. I know that it sounds again horrible but I found this and if I answer that question what I'm indicating is that we were still in an era where we *made* the machines, where we *made* the electrophoresis machines. We didn't buy them; we made them. When I got over there, all I did...there was no company to order them from, Calvin had got a workshop, I went to the workshop people, told them what I wanted, set up the apparatus — and that is completely different. You're in an era where you've not reached the black box, where you've got an automated system, where the people who do analysis don't know what's in the box and their mind suddenly stops working because it's like the software today — it is an impediment to original thinking. If I had my way I wouldn't teach students with machines; I would teach them in another way, the way, in effect, in the era we were taught. We did things ourselves, everything from paper chromatography: my father had to make the tank because it was a bigger tank, I wanted a different thing, I was doing two-way chromatography just like Calvin but not for that purpose. But it was in an era if you walked into the laboratory, you had the flask but you didn't have the machines. You had spectrophotometers, very little else.

VM: So you had to be much more hands-on and much more creative, you think, than people are now.

AB: Yes. But Calvin did have the workshop facilities to do that...

VM: ...and without that it would really not have been possible.

AB: ... it wouldn't have been possible. I would have been lost; I wouldn't have carted my paper electrophoresis machine over and by the time it had come, as it were, it would have been too late.

The flat we had was very interesting. I remember the bed came down off the door; as we moved in the bed, it creaked. Eventually we got into the habit of taking the mattress and putting it on the floor and, thereafter, for six months my wife and I slept on the floor. We were very happy. On the Rockefeller Fellowship, which was non-taxable at that time, we tried to save for our first car. Because we hadn't bought a car in America, we had saved enough money between September and, I think, February of the following to buy a new Austin A40. I duly deposited all these dollars down in San Francisco for delivery of a car in Birmingham, England when we came back in June. Then disaster struck because it was the year of Suez. There was an emergency budget in February or March of 1956 and then I really learned how to live on no money. Because we had to...and VAT went on — or the equivalent of...purchase tax in those days and my wife and I really learned how to live on practically nothing. We really studied the Americans, worked out what they didn't like; it happened out to be

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what we liked. They don't like offal, they don't like lamb, they don't like...that's a generalisation but we're talking about kidneys, liver and that sort of thing. The Americans don't really go for them or they didn't go for it in that time. That was the sort of thing we enjoyed. Instead of California wine, I think we bought a gallon of cheap sherry or something. We certainly enjoyed ourselves and we just achieved, before I went east, the remaining money.

On the social scene, we all held parties at each other's house. There was never any ill feeling among the students at any time, the research students, research fellows. To me that's part of the success. Everybody was willing to help everybody else. It was, you know...sparked off great camaraderie. The people — I'm looking at the picture outside the Radiation Hut, ORL, in 1956 — there was Altha (she was one of the coloured girls who grew the algae), Al Bassham, Ning (*Pon*), Pekka Linko, Carol Quarck, Hans Grisebach (who she married), Alan (that's me), Jean (*Bourdon*) (the Frenchman), Ozzie (*Holm-Hansen*), Alice (*Smith*) (the other coloured girl) and Paul (*Hayes*) and Kazuo (*Shibata*). That was the group at that time in early 1956...

VM: In that lab.

AB: ...in that particular lab.; there was another lab. elsewhere. There was another man, I'm looking now at another picture outside the hut. there's Ning and Ernst in addition to the names...yes, that's right: Ernst and Ning were there. And I've got pictures of all of these people; I think this was the greenhouse where they were growing the leaves. One of the things about these two coloured girls is at lunchtime I noticed a strange thing. They almost pleaded with Pekka and myself and Carol and so on to use this algae. If at lunch time we lads and girls hadn't used it, I noticed a strange ceremony. And it was that they made a soup of the *Scenedesmus* but they never made a soup of *Chlorella*. If you recollect, it was the *Scenedesmus* that responded to the azaserine, not the *Chlorella*. Much later, when the space ship went up, so the space men went up, I always thought that if I had to complete the photosynthetic cycle inside a space ship that the one I would first go for was *Scenedesmus*.

VM: When you say they made a soup, you mean to eat?

AB: Yes, they drank it, they drank it. They cooked it and ate *Scenedesmus*.

VM: Did you taste it, ever?

AB: No, I don't think I did. But when I asked them, it was always the *Scenedesmus*, never the *Chlorella*. I remember talking with the Russians in 1960 about this when I went over to Moscow and Kiev. It's quite fascinating.

Alice Holtham, the secretary of Calvin, was a real driving force and she saw that, for example, my wife was introduced to one of her friends who just had a baby — you know, the usual showers and all this sort of thing. We made friends outside the circle of people. I'm looking at another picture with another name that I haven't mentioned

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yet, Masao. But you can see here that we are visiting around — Masao, myself, my wife, Carol and Nel going to the Carmel mission. We went to...but we met some...I learned a great deal, as I said, about Americans by going on these trips with International House. One of the first ones we went was man who had made a fortune out of insurance, had retired and then, like so many Californians, had started to think why we were here, the origin of life. He'd started to study all the religions in the world. In fact, he'd got a wonderful library and at that time he was into spiritualism and, typical American, he had gone down to San Francisco for a seance and, of course, nobody ever does this but Americans do, he had taken his tape recorder. And I remember he played the tape recorder on how Atlantis came into being and the medium told a wonderful story — didn't matter whether it was true because it's just what...again full about things that happened in this world. People feed on other people, in a sense — whether it's harvesting people's minds. One sees how power is manipulated, how people are controlled.

And going back to this, in one of my visits to the north we were taken to the local newspaper office, say Paradise as an example. In those days you would see the news coming in on the teletype. When you asked what the news is, the news had been sold to them, as news is, and I tracked it back because I then went to San Francisco and saw the big newspaper. You see that news came into America in those days at two points only: San Francisco and New York. It was then sold and so the news for Paradise was somebody who owned the news in San Francisco selling it to somebody up in Paradise. And, you know, it's the same thing today. That gentleman in Australia who buys up all the news media is, unfortunately, the Internet has sort of overtaken it but I think it's the thing that will break down all barriers.

VM: We're getting a long way from...

AB: I know it is but you asked me what I learned and I learned a lot more than chemistry and that is what I always transfer to my students. At this time, for example, the Russian Ambassador for Saudi Arabia is a student of mine; I taught him more than chemistry. I think people of the calibre of Pauling and people of the calibre of Heidelberger (who, I thought, should have been a Nobel Prize winner) teach more the whole man. It's very difficult to find people. I learned a great deal from these people.

I also learned: if we take Pekka Linko, he was a wonderful fellow; somehow, you seem to be able to remain friends without seeing people for years and years and years. Yet, like I teach my students if they are ever called on to help one another, whatever religion, creed, Communist, Jap, Iraqi, they help one another. Because woe betide them if they refused to do this. Now, Pekka Linko was this sort of type. When we were on the east coast and we lived on a veterans estate, the veterans had come from Korea, and one day Pekka Linko drove up in a wonderfully new car.

VM: One second: was he living on the same estate as you?

AB: No. He had finished with Calvin...

VM: He was visiting?

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AB: ...and was on his way to New York. This is typical of people who worked for Calvin. They were all different but there was something magic about Cal...or Hans...the other people; there was something — it wasn't chemistry it was the things that motivated, the way their minds worked. If you took Pekka: Pekka was always low on funds and that's why at parties nobody bothered about who paid for what; if you'd got money you put it there — if you didn't, then it didn't matter. Pekka was always low on funds. So when he got to Chicago on his way back he had no money, no money literally no money, all he'd got was a ticket going from New York. And so, a man like that has to use his mind. So he bought, with his last few cents, the *Chicago Tribune* and there he found that people were advertising you...wanting you to drive new cars to deliver in New York. So, he flashes his international driving license and they gave him a beautiful new car and so, on his way...That strikes me: you know, that sort of man is different; they are different.

My students were the same, you know. I had a Lebanese from the American University and he was a Christian. I had a Syrian who's now a professor in the United States: his name was Atassi and he was of the ruling clan of Syria before Egyptians joined up with the Syrians. Now he wanted nothing to do with politics and he wasn't army. And when he got his PhD and went back, they wouldn't let him go to the pharmaceutical company that had engaged him, demeaned him and made...So he had to escape. Now, the man who helped him escape was my Christian and he sent his clothes out with an old lady; he escaped out of the mountains, he got to Beirut, it was Christmas. The man in Beirut sent me a letter...sorry, Atassi, this man, the Syrian, sent me a letter Christmas time, "Thank you, Dr. Barker (in those days) for the offer of a position in your laboratory." I hadn't given him one but, you know; and I then showed it to the university authorities and I used it to get him through immigration and got him here eventually.

The two technicians, coloured technicians, who grew the algae, there was some problem with them and Calvin. That was the time I crossed swords with Calvin; I defended them. One of them (*Altha Vann*) was, I think, about to be dismissed and I and the other students rather ganged up against Calvin. I had gone to a coloured church, my wife and I were the only white people there, because I just wanted to see what America was like. And I was very fortunate because after I left Berkeley all hell broke loose in Berkeley and it wasn't the Berkeley that I knew when I was there.

VM: When you left Berkeley, you worked first of all...you finished your Rockefeller period in Rutgers, did you?

AB: Yes.

VM: ...and then, presumably, you came back to England

AB: That's right. I was extremely fortunate. Within a few months Dr. Edward Bourne became professor at the Royal Holloway Colleges in '56 or early '57 and I took over his research team. Wonderful, made research team of 12-13 people and I kept that

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sort of numbers all through my working career. Eventually I had one, two or three, even three team leaders working with me.

VM: In Birmingham?

AB: In Birmingham.

VM: All your working life was in Birmingham was it?

AB: It was indeed except that, in '66-'67, I went to Canada for six months. Prior to that six months, I spent three months as a visiting professor at the University of Chicago, giving lectures, living in Oak Park. Then I moved up to Kingston, at Queens University, Ontario with Dr. Jones, Professor Jones in those days, and I went right across Canada, lecturing at Vancouver and...It was the time of Lemieux so I went to his place, Saskatoon. I went to Quebec. I gave a lecture at all these universities on behalf of a sort of Commonwealth Research Fellow; there's one chosen each year.

VM: Did you ever become a lecturer in England?

AB: Yes. I became a lecturer in the year before I went (*to the United States*) so in '54 I became a lecturer, I had a year's leave immediately for '55-'56. I became professor in '69. Prior to that, up to '60, I had been a man in an ivory tower. But I bought this house in Sellyoak in that year and I had intense kidney stones wondering how I could pay for it! So for the first time I became interested in making money. I therefore decided I would become a consultant. You don't become one! You are invited! While I was being "invited" I wrote two books, *Polysaccharides and Microorganisms* and *The Carbohydrates of Human Tissues*. I didn't make any money but it taught me a great deal; I became an "expert" in those areas.

Then, very soon, I did become a consultant. I did this by an underhand way. I was asked to write an article in *Endeavour* in those days — I didn't write one, I wrote several — and I put there theories that I wouldn't have been allowed to put in the *Journal of the Chemical Society*. This attracted the attention of Abbott Laboratories in the States, Hercules and so I eventually became a consultant for ICI, Wiggins Teap. In the year I retired, I was consultant for about fourteen companies. I stopped going to America in 1974 and concentrated on Europe and England.

I received a further impetus at the age of 62 when ten people had to be, as it were, prematurely retired from the department. Since I was the oldest, I volunteered first and I spent the next three years, from age 62 to 65, working one day a week and then I used the university facilities on my own to really become a consultant.

The other thing I was interested in was that, when I went to Canada for the purpose of lecturing on behalf of the Commonwealth research place, what I was after is teaching myself how to get information. So I spent six months, part of it, in the library following up stories like a newspaperman does, in effect, almost spying but learning the technology of information gathering. For example, you would see the results of twenty stories in that six months where I tested myself: I actually followed

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information up, I got stories and I sent them to England and they were all published in that period. They weren't published as research papers; they were published in newspapers. But I tracked the story of, for example, du Pont's hollow fibre. There were patents saying that du Pont has a fibre to spin hollow fibres but nobody said what they were for. I divulged that they were for reverse osmosis. I followed the story up. There was an advertisement in the bee research journal over there at that time that somebody wanted tenders for a kilo of bee venom and I cracked that story and located where and what for. So on and so forth: I did twenty stories. I enjoyed searching and looking at...I remember another story, was an Australian clinical trial going on in Sydney amongst school children. It didn't tell you what was being tried and what it was made from. I studied the patent literature and found that it was sucrose-6-phosphate that was being tried. It would stop about a third of human caries and I found also that it's in natural sugar in the sugar cane but that it's refined out.

VM: Last couple of minutes. Are you still doing chemistry?

AB: Yes. I still...my wife died of cancer and I nursed her here for fourteen months. During that time I went a step further. I started, before the Internet started, I actually do searches of world patent literature now and disseminate them to various companies. But during that I wrote a newspaper each week; I wrote a science newsletter for CPC Press, which was owned by the *Los Angeles Times*. I used to transmit every Sunday a twelve-page summary for executives of patent literature in biotechnology and used to transmit it from upstairs in 1994. I never used a computer before, had to teach myself how to use one and then went from FAX to modem and 12 pages used to disappear in seconds.

VM: And you still do that sort of thing?

AB: No. I had to stop it when my wife was in the bed and she was suffering intense pain from cancer of all sorts. That, I suppose, is where my training as a scientist makes me abhor my fellow men who go for money. Because the one story that I would like to break is which drugs are made and sold to the public and which are not. It is always, always for money. So I, who worked and gave my life for ten years working with doctors after I came back from working with Calvin, all on the human being and set up a network, gave it freely, when I came to an anti-inflammatory drug and I'd worked out with doctors exactly how it worked, how to assess it, everybody was pleased. The minute I said I wanted to cure rheumatoid arthritis, and I thought I knew how to approach it, no money was available, neither from charities or from companies. The tragedy is, you have an anti-inflammatory drug every day. If you cure somebody, your business is gone for ever. It is the same with cancer, it is the same with pain. My wife died of cancer in terrible agony and I was assured that if she went into hospice they would cure her. They have no idea or concept of how to cure pain. All they do is to make morphine, which is the cheapest painkilling drug. I was sitting up there looking at patients which show there are ten different ways of causing pain in the human body and there are antidotes to these. There are drugs there that man will never receive because then they will stop somebody making money. This is the tragedy of a life like mine. You learn all these facts that are not privy to other

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people. I think, you know, it's terrible that I haven't had the courage to throw my life on the line. You know, because you work for these companies.

VM: Anyway, I think we're going to have to...Thank you very much indeed.

AB: ...I don't reveal to other people.

VM: It has been very entertaining and gratifying to meet you after such a long time. Thanks a lot.