Chapter 45

F. ROBERT (BOB) WHATLEY

Oxford

May 9th, 1997

VM = Vivian Moses; BW = Bob Whatley; JW = Jean Whatley; SM = Sheila Moses

VM: This is talking to Bob Whatley in Oxford on the 9th of May, 1997.

You, of course, were one of the people who was not actually in Calvin's lab. but who was looking at it from the outside: when did you first see it — or when did you first begin to be there and observe it?

BW: In 1948, the end of the year. I went from Cambridge to Arnon's lab. One immediately became aware that here was a going concern because, of course, they were starting to work on CO_2 fixation and it was beginning to go. I didn't have very many firm contacts with people in the lab. but it was quite clear that in Arnon's lab. we were very much interested in the CO_2 fixation work. The biggest problem, as far as I was concerned, was, of course, as a "new boy", learning new tricks, in Arnon's lab., we were simply doing continuation of work on the Hill reaction. That didn't actually bring us into any very firm contact with Calvin but, at the same time, one was, as I say, very much aware of interesting things going on there.

Subsequently, I went away from Berkeley in 1950 for a two year period, two and a half years, to Australia. There I, in a sense, lost contact with this immediate stuff but I was happy to get back to Berkeley again in '53 — you must correct me if I get my dates wrong. In '53 I came into a lab. where, in fact, three different people — Arnon, myself and Mary Belle Allen — had suddenly decided, from different points of view, that chloroplasts must be able to make ATP. And, of course, the connection with the Calvin lab., then, was, of course, that there had been suggestions of ATP's involvement in CO_2 fixation and, from sort of general biochemical principles that must be the case. In Australia I had had some work done by a graduate student, Bob Smiley, and he, in fact, had demonstrated to me quite clearly how difficult it was to make mitochondria from leaves. We know that respiration is small in leaves, we know that photosynthesis can be big in leaves and, therefore, one has a strong feeling that maybe its these rotten chloroplasts that do the trick. From other points of view, Arnon and Mary Belle Allen were already coming along to this view. So we actually set out to discover this, which was a rather crazy thing to do, and we tried to make whole chloroplasts. This was different from the Calvin point of view.

VM: This was already in ’54 when you came back?

BW: It would be the end of ’54 — end of ’53, beginning of ’54.

VM: Can I take you back for a minute to the earlier period, the ’48 to ’50 period? What was the climate for photosynthesis like in Berkeley at the time? You said there was “interest”. Were there seminars? Did people go to one another’s seminars? Was the place abuzz with photosynthesis?

BW: No, it wasn’t. It was more from my point of view an observation of Calvin doing things. We used to actually be amused to discover that reading Calvin papers, which were all group papers, of course, was interesting because a new compound would be popped in when they were trying to figure out how things cycled. As you are very well aware, this was a major problem and compound X would come in with a big question mark. The next paper there would be the same compound in without the question mark and in the third paper it would have disappeared. (*Laughter*)

VM: Without much evidence either way!

BW: That was amusement and that just represents that they were trying to find out how it worked.

VM: That was his style.

BW: That was the style. So we used to be amused by that and I may say it was something which in the Arnon group caused amusement and was completely alien to the way in which Arnon’s thoughts would go. He wouldn’t actually produce things like that. We are talking about Calvin here.

VM: Yes, of course.

BW: So in looking at this one, it was all a matter of it’s all flying with obviously doing important things here and the “lollipop era”, if I may call it that, was obviously a going concern. We were a little reserved, I think, because of the sort of biologists’ view that, in fact, you couldn’t actually have a standard *Scenedesmus*, or whatever it may be, because if you grew it under different conditions it might do different things. Biologists were very sensitive to this. I think we were always quite surprised, if you like and perhaps admired a little, Calvin’s view that, in fact, the organism was an organism and it had all the necessary things to make CO_2 fixation go. He was going to use it just as a catalyst.

VM: That’s very interesting you should say that. There weren’t any biologists in Calvin’s lab...

BW: No, not at all.

VM: ...as far as I can remember in the early days. The remarkable thing (*was*) that *he* felt that here was an organism that you could treat like a chemical and they set up, as you

remember, continuous culture devices. He felt that you could then use it any day of the week and it would be the same as any other, which was certainly closer than it was for leaves but not, perhaps, totally close. It was not until the late fifties. I think, that questions began to arise of were these things really behaving the same way every day. And then we did some work to show that perhaps they weren't.

BW: As I say, I think there was quite a lot of suspicion...well, if you like — but not suspicion in a derogatory way — whether there might be something wrong with this one. In fact, if I can turn this around in a slightly different way, à la Arnon: when we were, in fact, starting to grow spinach as our material, we tried to standardise this as much as possible. We have the myth that if you grow spinach in a container yea big by yea...

VM: You're describing something two or three feet by two or three feet...

BW: That's right...in a green house in water culture (hydroponics) — that's going back, if you like, to Soils and Plant Nutrition (*Editor: the department in which Arnon's lab. was located*) as it was then, that when it got to the stage after a certain number of weeks we could harvest it and we would actually say that the material was constant during the next two or three weeks. Now, I know that that's not true. But, in fact, it was a desirable and necessary myth to do anything. I think that's the same thing as Calvin did.

VM: Oh indeed. So, in 1948 Arnon's group was still in Nutrition?

BW: Yes, it was still very much in Nutrition. In fact, if I tell you that in 1954, when the first announcements were made of the CO_2 fixation and ATP formation by isolated chloroplasts, this actually was presented at an international congress in Paris and Arnon actually withdrew a paper on phosphorus, because he was still an expert in phosphorus from the plant nutrition point of view. In a sense there was very much "I'm still in plant nutrition". Arnon, in fact, got funding for work on manganese and molybdenum to continue in photosynthesis and CO_2 he regarded as an inorganic substrate, just the same as all the other things he had been interested in. That was the way he edged into this one.

VM: You were located in the building...what was it called; Plant Nutrition?

BW: No, it was called Soil and...the Life Sciences; it was in Life Sciences.

VM: Oh, you were actually...

BW: Actually in Life Sciences. The reason I'm forgetting this is it's now changed and they have actually moved into Hilgard Hall and then finally into a completely new building, the name of which I can't remember. Can you? Anyway, it's a new building.

VM: That's where Buchanan is?

BW: That's where Buchanan is.

VM: We saw him.

BW: If you can remember where he is.

VM: I can't. I know where it is but I can't remember what it's called.

BW: I'm not vaguely lost.

VM: So you were in Life Sciences? Fairly high up in the building?

BW: Floor 3. We had a little tiny lab. down on floor 1, the basement relatively speaking in that building, for a few of our things but mostly we were on floor 3.

VM: How big a group was it in the late forties?

BW: It was the three of us (*Whatley, Allen, Arnon*) with two or three other people coming and going.

VM: Students? Postdocs.?

BW: Postdocs. Students actually didn't flourish particularly well with Arnon because he had very positive ideas as to how they should operate. This, of course, tended to smother people.

VM: And he was the faculty member?

BW: Yes. I wasn't, no. I was actually in the College of Agriculture. It was a College of Agriculture group which, in fact, was nice because it was an 11 months appointment and we didn't have to look for funding for the remaining three (*months in the summer*).

JW: (*indecipherable*) during the build-up to Davis; this was part of the teaching that was left behind when starting up and half the people...

BW: That's a slightly later...Let me explain. It is simply that at the time, Soils and Plant Nutrition came together and were set up as a main group. Then, in fact, subsequently and in about 1953 or 4, maybe even just before that, the Biochemistry Department in Berkeley was founded. You may be surprised at how late that was.

VM: I am.

BW: Because, in fact, biochemistry used to be all over the place because it was being applied to particular projects. When that (*the Biochemistry Department*) was set up, most of the straight biochemists who were in Soils and Plant Nutrition migrated up to Biochemistry and subsequently they moved to Davis. The history of Davis is interesting because, of course, for the first two years students in Davis wanting to do

scientific subjects did them in Berkeley and then they specialised in the Davis specialisations.

VM: This was Paul Stumpf and people like that?

BW: That's right — and Hassid, Barker, all these people. They'd actually all been in Plant Nutrition originally and then they moved on. So there was a hydro(*carbon?*) view and a metabolism view. And Eric Conn. So they all were temporarily in Biochemistry in Berkeley, having been in Plant Nutrition, and then they moved off.

VM: I mentioned this before to you, but let's push it a bit. What was the communication like among the biological sciences, or the people working on biological problems, around the campus? Because you were some hundreds of yards away from Calvin's lab., which was Chemistry- and Radiation Lab.-based. What sort of communication existed at that time?

BW: Mostly casual, very informal. And I think that if one wanted to go and find out things, that was easy to do. There was very little, as it were, official flag waving being done.

VM: Were there seminars which people attended?

BW: There were seminars within particular groups. In the Arnon group, for instance, we had our own household seminar. We used to try to get people to come to that from time to time, particularly if there was some relevance, but it was mostly very much in-group. I think that would be fair. Roger Stanier, for instance, was a mover and he used to appear from time to time and would organise seminars. In fact, at one stage he actually did organise seminars formally in photosynthesis with a biochemical viewpoint and he used to bring along his interest in the microbes and Arnon used to bring his stuff, both in microbes and chloroplasts and Calvin used to come along with his group. And in fact these seminars very seldom lasted for more than maybe two or three simply because they could never get Arnon and Calvin to talk to each other.

VM: Which does, indeed, bring me to the Arnon-Calvin relationship. I know, of course, that there was antagonism between them of some sort but I don't have any very clear idea of what its origin was or what the nature of it was. What did it look like from you...? What do you think was the problem between them as far as you saw it?

BW: I think it was just the philosophy of the people. I'll comment, if you like, on Calvin but, in fact, the first problem, if you like, is that Arnon actually had to have an experiment in his hands to make any progress in thinking — even though it was blindingly obvious that this is what was going to happen, he could not accept that until he had done his own experiment. It's not that he disbelieved other people but he needed just to check out and he was only happy if there was an experiment. This sometimes led to silly difficulties, but never mind!

When he would talk to Calvin, Calvin used to have a much wider view and certain things to him would be obvious. He (*Calvin*) would say, in a particular situation, "this is the way it must be". Arnon would say "how do you know?", meaning "what's the

experiment?" And that would be the end of the conversation. Because to Calvin there was a lot of background information coming along and he could predict this one with real certainty whereas Arnon actually had to have this experiment in his hand. There was an impasse.

SM: Calvin could be wrong occasionally.

BW: Indeed, not infrequently. So occasionally could Arnon although, in fact, because he was so experiment-based, it was a matter of interpretation rather than the experiment being wrong. So he wasn't picking up information from all the way over. He actually would collect information from all over and then, as it were, in his mind design an experiment that would convince him that the step forward could be made. Then, subsequently, that was fine; he could accept it all.

VM: So they just simply couldn't live with one another's approaches.

BW: No; I think their approaches were completely different. I mean, Arnon used to say "he thinks like a chemist". It's true, I suppose.

VM: I never actually heard Calvin say anything about Arnon. Calvin never mentioned Arnon.

BW: That's not quite true. I will tell you. There's a thing called the *Calvin cycle*, as you well know, and this, in fact, in Arnon's laboratory was referred to as the *reductive pentose cycle*. This is just because there was an antagonism, not expressed for any particular reason apart from the general philosophy, but Arnon used to say, quite cheerfully, "well, the Calvin cycle is fine, but, in fact, except for the ribulose *bis*phosphate carboxylase it's the same as everybody else's reductive pentose system. Calvin doesn't seem to give any credit to that and we ought to give credit to it." It's fair enough, and it just depends how you are going to operate. When it came to talking about particularly...excuse me, I've lost my way here. The reductive pentose cycle was fine and you could just see the different philosophies.

There was one very unfortunate occurrence and that was, I think, it was in *Physics Review* or something, one of these journals. After several years of Arnon doing his work on the actual isolated chloroplast CO_2 fixation of the ATP, there came a time when Calvin and a group actually published a review paper and in this review paper they state that the chloroplast can do the following things, so and son: they can make ATP and they can fix CO_2 and whatnot, and there is no quotation of this work from the Arnon group at all except that Arnon actually had to change his mind! So any time Arnon was wrong and changed his mind, he was quoted, whereas the implication is quite clearly that all this work is done in Calvin's lab. Now, I find this rather sad that it should be that way. Because, of course, from the scientific point of view it doesn't matter. But from the personality point of view it's really discouraging. I don't know whether you have come across that one before.

VM: Echoes of various sorts of relationships.

BW: That was a strange one.

VM: When you came back then in the early fifties, ’53/’54, had things changed, had they got more entrenched?

BW: I didn’t have enough information to allow me to make any serious comment on that one. I don’t think that there could have been any very obvious difference in the relationships; I mean, I think they were all very curiously opposed, as it were. I do know that Roger Stanier tried quite hard to get them to get together because he, as a partial outsider, could say “well, look, we in microbiology are doing work and Arnon is doing work on microbiology and he is doing it on chloroplasts and Calvin is doing on algae, and we are all interested in photosynthesis. We have a powerhouse here, let’s get together.” Well, it never worked.

VM: Arnon’s group, and indeed Stanier himself, were not actually working directly in the same area as Calvin were they?

BW: No.

VM: There was no obvious competition or treading on toes there.

BW: The only potential treading on toes was, in fact, from the point of view of chloroplasts will do the whole of photosynthesis. That is, in fact, a leap forward, as it were, a conceptual leap forward, which was in fact a significant jump simply because right up to ’54 or so, ’55, one of the problems was that many people believed that chloroplasts made something which was given to the mitochondria — and there was very good evidence for this one — and, in fact, Arnon is one of the people you can quote in favour of this, I mean in the early days. That conceptually was a difference.

Then you actually had the business of the chloroplast doing the whole thing. Of course, you can discount that because the rates of CO_2 fixation were so puny that they are not worth considering. Well, in fact, we had this interesting business of being able to get the rates up on a chlorophyll basis by the very justifiable cheating trick of taking the chloroplast preparation, which was as whole as we could make it but we knew it was leaky. Then we said, “well, we’ll throw away most of the green, which is doing the photochemistry, and put in the juice that is coming out of the chloroplast extract.” Then, on a chlorophyll basis it was actually doing quite well. Philosophically speaking that’s quite amusing because if, in fact, we had made chloroplasts, which people did later on, which would do CO_2 fixation at rates quite comparable — and that’s to the Calvin lab. and Walker and people like that...

VM: ...that was already into the sixties.

BW: That was in the sixties. When that came along, that was fine. If we had made chloroplasts like that in the beginning we would not have been able to demonstrate ATP formation because intact chloroplasts won’t do it. We, of course, went under the

cheerful misapprehension that we had intact chloroplasts whereas we had in fact what are officially classified as *naked lamellar systems* (*indecipherable*). Hence, they could actually see AMP or ADP if it was offered to them; they ate ADP like nobody's business. In a sense, we were trying to do something and we failed and, because we failed, we were able to demonstrate ATP. It's quite interesting and from a biochemical point of view, of course, there was a lot of stuff that was going at the time on CO_2 fixation in the CE system.

In fact, Arnon had a year's sabbatical at this time and I very foolishly thought "well, wouldn't it be nice if you put in different potential donors and acceptors into the system which were carbon intermediates to see what the products were and how quickly they came?" Because if you put them on the side leading up to CO_2 fixation, and not on the reductive side, you would actually have a different pattern: depending where the bottleneck was, you get different accumulations. I did a lot of these things which, in fact, were much better demonstrations in some ways of the Calvin cycle than anything you could do with the lollipop method.

VM: You were not using, were you, chromatography and radioactivity?

BW: Mm.

VM: The same sort of thing as...?

BW: Yes, exactly the same thing, sure; it there: why not use it?

VM: Yes, indeed.

BW: Mary Belle Allen and I and a gentleman called Rosenberg, Lawson (*spelling?*) Rosenberg, were all playing with this one. In fact, they concentrated more on the carbon side and I concentrated more on the phosphorus side. It was all done together. We end up with this work being done during Arnon's leave. The fact that he could confirm what Calvin was saying in a rather direct way, I would have thought would have amused him very much. But he actually didn't want it published.

VM: What happened? What was Arnon's reaction when Calvin won his Prize? Was he very upset about it?

BW: I think he was very upset and he called me in to tell me the news. He was at great pains to say that it was nice but not all that important. I'm sure he was very much cut up about this one. I think particularly because he had done his best to cultivate Europeans, particularly Warburg, to try to get himself known in this sort of position. No, I think he was very much depressed by this because, of course, it meant that his chances of doing anything were out of the window. Because you're not going to get two Nobel Prizes for different aspects of the same problem.

VM: Even though Calvin and Arnon themselves may not have any close personal relationships, did people in his lab. know people in Calvin's lab. on a personal level?

BW: I think we used to interchange. Benson was fine; he was good. Of course, he left at a time when he was beginning to get on Calvin's nerves. In fact, I think that there was some sort of difficulty going on here that would be a problem. Of course, the difficulty was that Benson was very much the experimentalist there, getting in and doing things. When he went away he just left all that stuff behind him, which was OK.

JW: Rod Park?

BW: Yes, Rod Park.

VM: That was later.

BW: Yes.

VM: Park was '59-ish by the time he got there.

BW: I'll tell you a silly story about that, which is not actually apocryphal because I heard it from Rod Park. In fact, he brought down some ribulose *bis*phosphate and we gave him some CE and we actually did the experiment to show what the products were in a test tube, being that part of the cycle. Which was fine. When he got back to Calvin's lab., Calvin said "I've drafted out a paper, just fill in the numbers", as it were. Rod Park's reaction to that was, as you might expect it to be. He said "no, I'm not going to give you that sort of information. I can just tell you that CO_2 fixation on ribulose *bis*phosphate gives PGA labelled in the right place". And left it at that. That very interesting piece of work only got published as a footnote in an Arnon review because of the difficulties of not being able to communicate.

VM: You said just now...you used the initials *CEN*?

BW: *CE*, chloroplast extract.

VM: Yes, I'm sorry...

BW: No, no, that's fine. I have "CE" built into my brain!

VM: Whoever reads this in the future will not understand CE unless it is explained.

JW: Another thing that's very important and that is that Arnon didn't know how to talk to people. He talked to the children...he sort of petted them and talked to the children and he found it very difficult to get on with people generally. I think that was also part of the problem between him and other groups like Calvin's group; it was not just the Calvin one, it was generally.

VM: I think that it was an impossible relationship, personality-wise. Arnon was a very reserved formalistic sort of person.

BW: Yes, very formalistic.

VM: I didn't know him well enough so I can't actually...

JW: He was one of the kindest people imaginable, he really was...

VM: But reserved.

JW: ...but very reserved.

VM: Calvin was very ebullient on the surface but had no patience at all for anybody with whom he disagreed. It was fine so long as things were going his way but terrible if they weren't or if there was any conflict. I can imagine that these two simply had totally the wrong sorts of personalities.

SM: He also was not at ease socially...

VM: Calvin?

SM: That's right...not on a personal level.

BW: Arnon wasn't either.

SM: As a scientist, yes...

VM: Can I remind you of something you've said before we actually started talking, about the way Calvin or Mrs. Calvin used to address you ?

BW: No, no. Calvin himself obviously had the view that my name was "Arnon and Whatley"...

VM: "Arnon *and* Whatley"?

BW: ...and he had to be introduced to me each time, or I had to be introduced to him, rather, each time. "This is Whatley": that's fine. We would carry on whatever we were supposed to be talking about; it was usually only when I was visiting up there with other friends. This happened every time I went up there. He had to be introduced to me, he would never carry this on. Later on, when I was on sabbatical leave here and I was at Cambridge with Robin Hill, we went to a garden party, which The Royal Society was running, and I told Robin Hill that Calvin always requires me to be introduced to him. Robin Hill started to introduce me and Calvin almost fell upon me, "I know Whatley". That was fine and we got on perfectly well. When we got back to Berkeley about three months later, I had to be introduced to him again. This was part...I was contaminated, I'm afraid. (*Laughter*)

VM: One point which has been made by lots of people, and I wonder what you think of it: people are very enamoured by the memory of the building, the old wooden shack in

which the group was housed. You remember, it was a building with few partitions, it was an open lab. Many people feel that contributed a lot to the way the group ran. Did you get that impression, coming into it occasionally?

BW: I always thought that, in fact, the elegance of your surroundings are not particularly important as long as the tools are available. And they don't even have to be particularly complicated tools as long as they work every time you want them to work. Perhaps I didn't notice; perhaps I thought it was the way it should be. But I do know that when they did move into the Round House the suggestion was that the office in the middle occupied by Calvin would remain stationary and the rest would revolve around it! (*Laughter*)

VM: Your group in Arnon's outfit was presumably more conventional in structural design, was it?

BW: Just the long corridors...

VM: With rooms off it?

BW: ...with rooms off, that's right. We used to have one room in which we put (*momentos?*) and rubbish, and another room where we did the radioactivity, and another room for cold room and things like that, and another room where we put centrifuges.

VM: We had that when the old building was demolished and we all moved down to the Life Sciences Building basement for several years. But then, of course, it was exactly the same structure. People hated it; they'd been used to this open plan.

BW: Well, I can appreciate that. When the laboratory in Davis was designed to accommodate people who had started off in Plant Nutrition and then gone to Biochemistry, they actually got rid of all the corridors essentially, they had very wide corridors which in fact were the instrument rooms; then there were a few offices and labs. put on the sides.

VM: What was the funding like in Arnon's lab. in the early days, where did it come from?

BW: It came from the Navy, it came from molybdenum mining companies (manganese — same family), the Navy and then the National Science Foundation. Virtually all the money came at that particular time from the Navy to begin with because they were interested hypothetically in changing CO_2 to oxygen and this was all...

VM: This was for submarines, presumably?

BW: The cost of the light was (*indecipherable*).

VM: Yes.

BW: But, you know, this was the sort of thing...And the interest in manganese, of course, went on for quite a little time because of manganese and CO_2 fixation. Later on, rather more importantly although we didn't know very much about it at the time, that is that the four manganeses were required to give one O_2 although we did actually have some clues on this one because we did good old plant nutrition analyses of isolated chloroplasts which would do this, that and the other and found, in fact, a lot of manganese there and very little in the rest of the plant.

SM: May I ask you a question going back a bit? You mentioned that Benson was getting on Calvin's nerves, or something.

BW: According to Benson, he left more or less following Mrs. Calvin saying that "if you go on like this, Calvin will have a heart attack". That's my view. I don't know quite what the...I'm not privy, but I do know that statement was made and fairly soon after that Benson picked up a job.

VM: So there was some friction...

BW: ... there was some but I don't know what it was, definitely. Which was perhaps surprising because Benson is such a happy-go-lucky gentleman. This is the problem between your scientific personality and your everyday.

VM: There are people who are very divided.

BW: Scientists have to be divided because they get so depressed.

VM: Well, you don't sound *very* depressed!

BW: No, but I mean scientists assume that whatever they do is wrong as a working hypothesis but they also expect to be paid at the end of the month. You have two completely different scenarios!

VM: Just to go back to Arnon's funding for a second: did you have to work hard for the funding?

BW: No. He did it...

VM: And it was adequate?

BW: ...it was adequate. He pointed out to us that the University was beneficent and gave funds for research which counted for approximately two weeks in the year. So, the other fifty weeks had to be funded from outside. So that's what he did.

The science of the group, you asked me about earlier. We started off with a very small group and that lasted for a few years and, in fact, it was basically the three of us (*Arnon, Whatley and Allen*) with two more coming in and out again. Which surprised quite a lot of people when they discovered that a little group of three can actually

accomplish quite a lot but, of course, a group of three which is going can often accomplish quite a lot.

VM: Were you teaching?

BW: No.

VM: Arnon was teaching, presumably.

BW: He had a little bit of teaching, not very much.

JW: (*Indecipherable*) had moved to Davis, actually.

BW: When he went on sabbatical leaves, I used to do his main course for him and he also used to do a graduate course which was actually on consulting the literature...used to be on plant nutrition but it turned into photosynthesis, with time, just to represent current interests!

Anyway, we started off with this group of three and then, later on, as interest moved to photosynthesis by bacteria and things of this nature, the group actually had to increase and grow to take that and so we had another group of three or four with Arnon sitting on the top. I suppose at the end there would be about eight or ten as a going concern.

VM: It was always very small, then.

BW: Always very small, yes. We usually tended to work experimentally in our own little bits and this is part of the trick, if you like, of using bits of equipment to work and this includes people. We used to compare notes and so our weekly seminar would be quite helpful to keep us informed. Somebody kept stock and said "how about so-and-so?" It would work very well from this point of view. The kitchen seminar, if you like, used to include such discussions as to what new bits of equipment are we going to need, or can we move this way or shall we go this way — the answer might be "no" or it might be "yes". At one particularly fractious occasion, I was sufficiently annoyed to say to Arnon "what we need is a good decimal point placing machine". He was actually off-balance and it took me about twenty minutes to convince him that this was a joke!

VM: He was that sombre, was he?

BW: Yes, he could be. When we had people from Europe, we would have this very interesting situation where they would get quite intense in discussing. They would say and would call each other "Mr." Arnon would feel that you actually had to do something drastic, like parting the fighters. They weren't fighting; they were just expressing themselves fine. In 1960, it's getting on a bit now, we actually had a very good group there; in fact, six or seven were to leave within two years and they all became professors. It was obviously a going concern although they did their own thing.

VM: When did you leave?

JW: ’64.

BW: Yes, ’64, that’s right; I went to King’s College in London.

VM: What persuaded you to go back across the Atlantic?

BW: Basically an insecurity with the education of my children who were beginning to show some small signs of not being alienated but actually having to defend their parents for being different from the rest of the world.

JW: We didn’t like (*indecipherable*) schools for secondary education.

BW: Secondly, I had a phone call from King’s College saying “would you like to become”, that is an offer. Actually I had already had negotiations going on with Bennett-Clark, who had just moved to Norwich, and he actually was trying to get me a Royal Society professorship. This he was also coming along quite well; then it all fell by the wayside. I discovered afterwards that it fell by the wayside because King’s College had offered me a professorship — damn it!. I would have been very happy to have stayed in a non-teaching, non-administrative job.

VM: You stayed there for several years before coming to Oxford?

BW: I stayed there until ’71. My sabbatical was to come to Oxford!

VM: You’ve never been back to Berkeley to work, have you?

BW: No.

VM: And you, presumably, have visited?

BW: I went one summer to Davis which was also almost the same thing in terms of meeting people that we had known before. That was actually, however, only a summer visit, not a long term.

VM: You’ve held the headship at Oxford for twenty years?

BW: Yes.

VM: Until ’91?

BW: I thought I was going to escape that but my alter ego went to become Chancellor of Edinburgh University.

VM: Who was that?

BW: Smith, David Smith. He was going to take over for the last five years but he'd gone by then and he wasn't replaced for a while.

VM: Luck of the draw! Well, thank you very much. It has been very illuminating and you have told us all sorts of things that we haven't exactly heard from other people and we look forward to listening to it all and getting then typescript — and using it when we're writing up.