Chapter 13

ALEXANDER T. WILSON

Tucson, Arizona

June 5th, 1996

VM = Vivian Moses; AW = Alex Wilson; SM = Sheila Moses

VM: This is a conversation with Alex Wilson in Tucson on Wednesday, June 5th, 1996. Can we start, Alex, with your early history and how, in the first place, you ever got to be in Calvin's lab.?

AW: From when I was educated in New Zealand?

VM: That's right and what brought you to Berkeley eventually.

AW: When I was 16 I left school and got a job in the Chemistry Division of DSIR and I went part-time for the first year and then I went full-time for the next couple of years to get my bachelor's degree, and I worked in the analytical section and the sprays and insecticides section of the New Zealand DSIR. Then, I did my master's degree on the chemistry of tutin and picrotoxin (*spelling?*) which is a poison found in a New Zealand plant. (Tutin gets into honey sometimes and kills people.) I then graduated with a masters degree and first class honours and I was awarded a Fulbright and I went to Berkeley to work with Melvin Calvin.

VM: You knew about him at that time.

AW: Yes. I was very interested in the kind of chromatography and all that sort of stuff. In the last part of my time at the DSIR I have been trained, there were just two people — myself and the person running the section in radiochemistry that started a carbon-14 lab., which was one of the first carbon-14 dating labs. in the world — so I was quite interested in radiochemistry. So, I went to Berkeley. I was pretty young, I was only about 20 and I found Berkeley a very interesting place, coming from a small place like New Zealand. In fact, I could go up the hill behind the University and see more people in the Bay Area than there were in all of New Zealand at the time. Berkeley was the biggest university in the world at the time, 26,000 students, and I was living at International House which was half Americans and half foreign students. I found the whole system incredibly interesting.

VM: But you weren't a biologist at all, were you, before you went there?

AW: I had done my masters degree in organic chemistry but I was very interested in physical chemistry as well, and all branches of chemistry, really. I didn't know much about anything else than chemistry. In fact, my thesis with Calvin was basically a physical-chemical kind of thesis, although it was on a biological subject. In fact, in my prelims. they started asking me biochemical questions — they had a couple of there — and I didn't know much biochemistry. It developed into a big slanging match between the chemists on my committee and the biochemists on the committee as to how much biochemistry somebody doing what I was doing was doing. They finally compromised by saying that I had to do first year of biochemistry.

VM: This is when you first started, when they were setting up your courses?

AW: No, no. When I went for my prelims., which was a fair way along in my course. I had picked up a bit of biochemistry but I had no systematic knowledge of biochemistry. I was just looking at the system as a kind of dynamic circular chemical system.

VM: Can I backtrack just a bit, to before you went to Berkeley. Did you expect to work specifically with Calvin specifically on and specifically on photosynthesis? Was that your objective?

AW: Yes I did, That was my objective, although when I accepted and got there I was told that I didn't have to work with Calvin, I could work with anybody. But I still was very interested. Calvin was a very interesting person.

VM: Had your communication been with Calvin?

AW: Yes.

VM: Where did you meet him for the first time?

AW: I didn't meet him until I got to Berkeley. I had a scholarship to go to England but I decided that I would sooner go and work on (*photosynthesis*). I thought it was a fascinating subject because you are doing some pretty interesting things with such pretty simple systems, like co-chromatography and all that sort of stuff.

VM: Do you remember the first time you came face to face with him?

AW: Yes. We just sort of chatted. I was very interested in how interdisciplinary he was and, in fact, that's something I learned from him, to be interdisciplinary, basically to take knowledge) from one subject, techniques from one subject, and exploit it in another subject. I am probably now even more interdisciplinary work than he was because I have wandered all the way from chemistry through biochemistry and all the way to earth science now I am actually working on trying to recover information from ice cores.

VM: Which has a meteorological bent to it?

AW: Well, paleo...and the greenhouse problem.

VM: When you first got there and had your first conversations with Calvin about what you were going to do...do you remember?

AW: He said " we don't know what you're going to do. Why don't you just work in the lab. and help the other people and eventually we'll find something, we'll stumble across something which will make a good thesis" .

VM: What happened then?

AW: That's exactly what happened?

VM: Who did you work with?

AW: I worked with Andy (Benson) and some of the other people there, Al Bassham, and I found it very interesting. I learned a lot, about a lot of things.

VM: Were you working as it were as their assistants in what they were doing?

AW: I was helping them with experiments. The mistake I made at Berkeley was that I put too much time into my lab. work. You see, most graduate students who were there spent all their first couple of semesters doing course work whereas I was working in the lab. every day. I was good at working in the lab. and I had a lot of lab. experience, a lot more than most people of my age, because I had done so much work in working for the Chemistry Division (*of DSIR*) in New Zealand. I was probably a pretty competent experimentalist.

VM: Did they give you credit for the courses you had taken in New Zealand or did you still have to do what all the Americans (*had to do*)?

AW: No. I was obviously not going to get any credit, so I decided rather than do it in organic chemistry I would take the degree in physical chemistry. Besides, Berkeley was very, very good at physical chemistry. The average organic chemist at Berkeley knew as much about thermodynamics as most physical chemists anywhere else did. It was a very, very strong school in thermodynamics.

VM: And the Fulbright fellowship was going to support you for the whole of your period while you were there?

AW: Well no: it provided me transportation from New Zealand and I had some other kind of grant, which was related to the Fulbright, which gave me a certain amount of money every year. And then I got a teaching assistantship which actually turned out to be a very good thing because it gave me enough teaching experience to get a job at a university later in my career. Although, at the time I wasn't too interested in going into academic work because I'd worked in a government lab. all my life.

VM: How much time did the teaching occupy?

AW: The teaching was kind of interesting. At Berkeley (*for freshman chemistry*) they had 25 rooms (*sections?*) with about 25 students in each. They gave them an aptitude test and they put the best students in Room 1, the next best students in Room 2, the next best students in Room 3. They put all the odd people in Room 25 — and I got Room 25!

VM: You were the bottom of the heap, were you?

AW: Since I was a foreigner and actually was quite good at talking to people who couldn't speak English very well, because I was living in International House, they gave me all the people from Iraq and wherever. Most of them had already done some chemistry. When the exams came out, we came about fifth among all the things, so they gave one foreigner to look after all the other foreigners, which I thought was pretty astute. I would have spent, I don't know, I don't really remember, but it wasn't not all that much and I found it (*the teaching*) quite interesting.

VM: Aside from that, you worked essentially in ORL all the time.

AW: Yes, I would go to ORL and then go off to lectures. I had a chance to go to Alaska the first summer I was in Berkeley and I decided to work in the lab. and not go to Alaska; I regretted this later. But, I have since been to Greenland which is even more interesting.

VM: Did you have your own bench eventually in ORL?

AW: Yes, I had a bench all the time. In fact, I shared Andy's office with him.

VM: Did you; I see. In ORL?

AW: In ORL.

VM: At that time the big white table was there already?

AW: Yes.

VM: And you congregated around the table? Who were your contemporaries as you remember them at the time?

AW: There was Al Bassham and Andy, of course, and a little coloured girl whose name I don't remember who did the lab. looking after, there was a woman who grew algae, I have forgotten her name.

VM: It wasn't Althea Vann, was it? Was she a redheaded woman?

AW: No, she was a tall thin lady. I think maybe her husband was working there, too.

VM: In that case I don't know who it is (*Editor: perhaps Louisa Norris — Dr. Richard E. Norris was a postdoc. in the lab.*).

AW: They were working on trying to grow algae to get oxygen for submarines and things. Then there was — who is the lady who lives up (*in Northern California*)?

VM: Lorel (*Daus*) Kay?

AW: Lorel was sharing the other side of the bench with me. There was — who was the graduate student that died?

VM: Dan Bradley.

AW: Dan Bradley. Dan and I were about contemporaries on the PhD. And the guy you're staying with..?

VM: Murray Goodman.

AW: Murray Goodman; he had been doing a PhD for quite a while. When I was there, Professor Nordal, do you...

VM: I've never met him; I know his name though.

AW: He was a professor, a really old person, a professor of pharmacology from Norway, an expert in sedoheptulose and was extracting sedoheptulose out of avocados. He was quite a character. In fact, he used to give the girls a hard time. In fact, they wouldn't go up to the chromatography room unless I came with them! (*Laughter*) The first sexual harasser I'd met!

VM: He was there, presumably, for a fairly short period, was he?

AW: I suppose so. I was there for three years and people came and went.

VM: :As your period in the lab. developed and time passed, it became clear, presumably, what you were going to form your thesis on, or did it? How did your thesis work actually develop the way that it did?

AW: When I got there, I got there at a very good time, they were just doing the lollipop experiments where they were feeding carbon-14 bicarbonate to algae in the fume hood and then they were trying to do short experiments and controlled time experiments and chromatographing the result and counting the spots. As time went on, and then Peter Massini turned up and actually a Japanese person turned up who came from a big company in Japan; he was an expert in photometry and how to do absorption spectrum work through light-scattering materials. (*Editor: this was probably Kazuo Shibata*). It was kind of funny as he could understand me quite well I had a very strong New Zealand accent and everybody in the lab. would joke about my speech, you see, they couldn't understand me. In fact, I had a lot of trouble originally.

The first week I was there I went down to the store and asked for a cork to go in a bottle and they said "the machine's upstairs" . They thought I said a coke! I had terrible trouble with New Zealand slang. For example, the first Monday I was there we were doing this aptitude test for the first-year students and we were sitting in a big room full of other graduate students, and we were grading the results. I made an error: I said "does anyone have a rubber?" (*Laughter*). There was kind of a deathly silence.

SM: We have had similar linguistic experiences!

AW: At any rate, they had trouble understanding me initially. I got better at it as time went on and I spoke a lot more clearly and I didn't drop the ends of my words like New Zealanders tend to do. But the funny thing was that this Japanese guy goes over to Calvin's office, which was over in the Old Chemistry Building, and Calvin called me up and said "Alex, come over and translate for me" because Calvin couldn't understand this Japanese guy and the Japanese guy couldn't understand Calvin. I sat there and I just translated!

SM: Presumably the Japanese guy thought he was talking English?

AW: I could understand him because I was used to talking to foreigners at International House and I could talk slowly and he could understand me and I could understand him.

VM: As your work developed in the lab., when did your thesis direction become clear to you and to others, I guess?

AW: It slowly became clear to everybody that in a cycle like a photosynthetic cycle the reservoirs aren't constant sizes, in other words, they fluctuated wildly, depending on how much CO_2 or how much light or whatever; how much everything was. So, I had a bent toward instrumentation so I acquired the idea that maybe we could follow this by labelling. Instead of doing a tracer experiment we could use the carbon-14 to label everything and then change something and see how the reservoirs change. I had to quantify the whole thing. I had to make sure I could run good chromatograms and I worked out that if you humidify, raise the humidity, on the paper for the phenol you got a much better chromatogram. I had to dry the paper so that I got — I couldn't dry them from one side because I would get all the radioactivity on one side, I had to dry them evenly, and I developed a technique for spraying a phosphatase on the spots so that I would turn the phosphates into sugars and then I could elute them easily. I was running a tremendous number of chromatograms so I had to automate the whole thing so it went a lot quicker and I could actually do the work. I would set up the experiment and then everybody in the lab. would help me for 30 minutes while I took all these measurements on the samples. I had to develop the whole technique of building this fancy piece of apparatus which would control everything and doing all the chromatograms and then making the Scott counters work properly so I could get good data, and I had to have internal standards. Just doing one enormous experiment which took a long time and then doing another one.

SM: When you say that everybody would come and help you while you were counting?

AW: Doing the experiment: I would set it up and then everyone would come and would pass me the tubes and I'd take the sample and...

SM: So this was indicative of how people worked together in the lab.?

AW: Yes. Everyone got along very well together. We used to go on trips to Yosemite and we went on another trip to Pacific Grove to see the Monarch butterflies. It was a really nice group of people.

SM: That would have been in the autumn, wouldn't it?

AW: Yeah. Actually, there were only two graduate students in the whole lab.. It was really a much more industrial kind of situation.

VM: The way you describe it, it sounds as if people were not very proprietary of what they were doing individually but it was very much a team effort.

AW: That's right. We used to have a meeting every Friday morning and discuss what everybody was doing. And then Calvin would come to work on Monday morning with some kind of crazy idea he had thought about over the weekend and I came to the conclusion that people don't have better ideas than anybody else, they just have more ideas! (*Laughter*). Which is a pretty important point, I think. He was always coming up with ideas, many of which he filtered out himself, and the other ideas we would have to filter them out. One in every hundred ideas turned out to be a good one.

VM: Presumably the rest of you were thinking up ideas also. It becomes a habit, doesn't it?

AW: There was a big battle in the lab. as to whether there was a two carboxylation cycle or one carboxylation cycle. Calvin was the last one who believed the path of carbon in photosynthesis.

VM: What did he think were the two carboxylation events?

AW: He got influenced by the original kinetics that were done where we were doing the lollipop experiments. He got misled because the ribulose reservoir was building up as we let the algae just sit there. When you pushed in the bicarbonate, it immediately turned into CO_2 and the ribulose diphosphate dumped into PGA. After the cycle had settled down it would go on normally. It looked like there were two carboxylations. Of course, he was trying to analyse the kinetics. In order to analyse the kinetics he had to assume a steady-state which it wasn't. So it was kind of interesting.

VM: The rest of you argued against him, argued with him, that that was not the case.

AW: Right.

VM: So, the group of you foresaw the carboxylation of ribulose diphosphate at an early stage. Can you remember who first conceived of that carboxylation?

AW: It originated, the fact that the kinetics didn't work out very well meant that we didn't understand the whole system. Then Peter Massini did an experiment, turning the light off and on and noticed that when you turned the light on, within 30 seconds the ribulose diphosphate went down and the PGA went up by about the same amount. So it looked as though these two were related to one another but that wasn't good enough evidence by itself. The concept of fluctuating reservoirs and the concept of using, say, the cutting off of CO_2 or lowering of the CO_2...You see, I didn't cut it off; I took it from about 1% to .03%.

VM: What did you have, two big vessels?

AW: Two big vessels full of CO_2 which had the same specific activity, one was running at 1% and the other was running 0.003%.

VM: There must have been a fair amount of radioactivity in these vessels.

AW: I'm glad I wasn't doing carbon-14 dating at this time. They were pretty big vessels, sort of...

VM: Two or three feet across?

AW: Right. And you had to have bladders in the air. It was a pretty tricky experiment to get it organised so it worked and you had to monitor the CO_2 concentration, which I did on a recorder, and I had an infrared gas analyser in the stream. It was probably one of the first of the really controlled experiments. It worked very well. We got very good results.

VM: This was the sort of experiment that you were describing earlier, where everybody had to chip in.

AW: I needed them at the very beginning because I needed to slow the system down. I had the whole thing cooled to 6 degrees. I had a big thing of ice and I was taking water off the bottom of the ice. You know that if you put water in a Dewar with ice at the top, then the bottom of the Dewar is 4 degrees (that's the maximum temperature of water), I was taking that and putting it through my jacketed vessel, so I could slow it down to Peter Massini's experiment which was done at room temperature. I had to work really quickly, so I would take the samples as quickly as I could at the beginning and for this I needed some help, not for very long, just for the first 12 points or something; after that I could do it and it continued on for quite a while.

VM: Did you have to run this big experiment more than once?

AW: Yes, I ran it more than once and I ran it going both ways, going from 1% to low CO_2 and when that had equilibrated I took it back up to 1% again, and so the whole thing happened in reverse.

VM: That must have been a massive job with all the chromatograms you were running, spots you were counting.

AW: I had to develop evaporators for evaporating things on a massive scale, like this octopus deal where I had these little tubes, maybe 12 or something like that, all hung up with rubber tubing and being agitated so they wouldn't bump, to evaporate these things down. I had a system for transferring spots off a cut-off spot onto the origin of a new chromatogram.

VM: How did that work? I don't remember seeing one of those.

AW: Do you want me to show you a picture?

VM: Yes. Let me stop while you go and get it.

We are looking now at the paper*: The Photosynthesis Cycle: CO_2 Dependent Transients* and this is JACS, is it?

AW: It looks like it:.

VM: Volume 77. It shows the octopus. That's that; I can see that This was your elution apparatus? That's on page 5951 if we go and look at it again.

AW: I had several of those so I could elute several spots at once. The trick was to do the phosphatasing on the paper; so I would cut out the spots I wanted and then I would spray it with phosphatase, and then I would hang it in an atmosphere saturated with water and toluene, so the bugs wouldn't eat it, and that would turn all the phosphate esters into sugars, and then they were much easier to deal with. You can't elute the phosphate off quantitatively. I had to do everything quantitatively.

VM: It was actually quite a big gamble, wasn't it, that the experiments would really work, because there's a lot of elaboration in what you did and things might not have worked out. But, you know, I think it was not only well conceived but well executed and you were lucky. You might not have been as lucky.

AW: Mind you, I didn't have go get that far into it in order to find it wasn't...That's something else I learned: don't invest too much time until you know it's going to work.

SM: You designed this apparatus, did you have to construct it yourself, or did you have it built?

AW: I mostly constructed it myself but they had a workshop and they would make things that I wanted. It was a pretty impressive looking piece of apparatus.

VM: That formed the main thrust of your thesis, did it?

AW: The thesis that I presented, yes.

VM: This proved definitely the cycle existence and the transients in the cycle.

AW: It showed two things. It showed that the thing upstream from CO_2 was ribulose diphosphate or something that's very closely related to it. It might have been a product from it that had a very short lifetime or something that was very small and you couldn't pick it up on a chromatogram. And it showed that probably in cyclical biological systems the reservoirs can fluctuate up and down very quickly and you could see the wave go back and you could see the wave go forward if you look at these pictures.

VM: Yes, that's right. I remember these and I remember how important they were at the time. This is the one on page 5952. These successive waves going around the cycle, in both directions, and then they conflict in the middle.

AW: I wondered at the time if you couldn't use it for other things, like determining what compound picked up nitrogen in nitrogen fixation. I considered doing that when I was back in New Zealand but I never really got to it.

VM: The difficulty, of course, is that there's no radioisotope...

AW: I could have labelled the carbon compounds in, say, *Azotobacter* or something.

VM: But by then you were on to other things.

AW: I was doing other things.

VM: I don't think anybody did that experiment, that I recall. What about life in the lab. as you knew it? You worked, as you said, in the main room of ORL; you worked in one of the....

AW: No, I didn't. Actually, if you leave the room that had the big white table and you walk through, on your left there was a place where they washed the dishes. My bench was right there. There was a double bench there, room enough for four people, and my actual bench was right there. Who was the girl who lives in California?

VM: Lorel Kay.

AW: Lorel Kay worked next to me. If I looked through to the other side, my apparatus was on that wall. If you turned to the left there was an office. Andy had the desk towards the bay and I had the other one.

VM: There was free movement of people, of course, in and out of the building and all around the building all the time, wasn't there?, and you were running up and down the stairs to deal with the chromatograms and into the cellar to deal with the counting. Earlier on, you were telling us some entertaining stories of your life in Berkeley —

what you did in International House with straws, for example. Well, why don't you...?

AW: There were a lot of interesting people at International House and International House was run rather autocratically. We used to think of things, while we were having our evening meal, we would think of all sorts of terrible things to do, one of which was to take the cover on the straw and dip it into some honey and shoot it up at the roof of the dining room which was a very, very high roof. Of course, it would go up and it would stick on the ceiling and just hang down. By the time I left there were hundreds and hundreds of straws up there. Not that I put them there; it was just that other people took my technology and...

VM: You did not experiments with straws did you?

AW: I thought it was going to work but I didn't want to be thrown out for doing it, you know. You can't get thrown out for thinking of something with intellectual input but you can get thrown out for physical input.

VM: You didn't have to persuade people very hard to try. What else did you do at the straw International House which was entertaining?

AW: We used to think of all sorts of things such as how to get from the boys side to the girls side using the freight elevator. We never actually did it, but we actually got accused of doing it. When e were called on the mat for doing it said "Did anyone ever do it?", but nobody ever did. We had a lot of theoretical pranks that we got up to.

VM: I don't know the structure of d International id House because I never lived there. There were two wings, were there, boys in one and girls in the other?

AW: There were different floors for boys and girls.

VM: How did they aim to keep you apart? Was it clever what with the staircases going to one set of floors....

AW: Yes it was, that's right.

VM: But the freight elevator notionally served all the floors?

AW: You could have done it with the freight elevator but I never actually did it with the freight elevator because I had no one to go to on the other side.

VM: That's hard luck, isn't it? Then you were telling us that you had interesting questions with your immigration papers.

AW: Miss Kittredge, who later became Mrs. Wilson, was a really interesting person.

VM: Not your Mrs. Wilson?

AW: No.

VM: She didn't marry you?

AW: No; the reason I remember her name is because it was the same as mine. But she used to run the Chemistry Department with an iron hand. She used to do the most incredible things. Once, actually every semester or every year or whatever, I had to get something from the University saying that I was a student in good standing. I would go to Miss Kittredge and I would say: "Miss Kittredge, I need something for the immigration department". So she would get out an official piece of paper, type a letter, sign "K. Pitzer" on the bottom and give it to me. Which I was pretty impressed; it didn't take very long, just took a couple of minutes.

VM: And the immigration department was quite happy with this!

SM: Except that Calvin had suggested that you go and consult her on something...

AW: There was something else. I don't remember what it was, but I asked him if I could do something and he said "Alex, go and see Miss Kittredge. She's the one who's going to have to make up her mind eventually, anyway!"

VM: You told us also that you had an interesting run-in with the question of signing some security document.

AW: I came to Berkeley as a young person of about 20 and coming from a small country I really had trouble treating America very seriously. I entered in the middle of the McCarthy era and two things happened to me. One was, after I had been there for a very short time, somebody called me up and said that I would have to go and do an English test to show that I was proficient in English. I said to them, "I speak the Queen's English; what do you speak here?" They finally let me off.

You see, not too many New Zealand students had been to Berkeley; now they are much more sophisticated about what there is in the world. The other thing I had to do was sign the Loyalty Oath which said that I was not a communist, a member of the Communist Party, and that I promised I wouldn't overthrow the government of the United States with force. I always thought this was kind of strange because I came from another country. I read on the bottom of it that I was subject of Her Majesty Queen Elizabeth II and I wasn't too sure what plans she had for the United States. I felt that I really couldn't sign this. They let me get away with it, which is pretty incredible, really. I might have been the only person in the whole McCarthy era that didn't sign the Loyalty Oath.

VM: I suppose, I don't know what the rules were. I think that was a splendid thing to have done. (*Laughter*).

AW: I had a humorous bent at the time.

VM: Was the lab. atmosphere generally jokey at that time?

AW: Yes, it was.

VM: Light-hearted?

AW: It was light-hearted.

VM: But people worked hard and long hours, did they?

AW: I worked very long hours. They used to publish papers with lots of lots of peoples' names, everybody in the lab. on it, with Calvin at the end. Andy used to say this is what I call "isotopic dilution", which I thought was very funny.

VM: But Calvin contributed to the papers, didn't he; discussion and he used to look at the manuscripts?

AW: Yes. Actually, something very funny happened. Who was the English guy that studied...?

VM: Grant Buchanan.

AW: Grant Buchanan came and he lived at International House so I knew him quite well. He came, very English....

VM: Scottish, excuse me.

AW: OK, Scottish. I remember the first day he was there. Calvin has just written a paper. Calvin used to dictate these papers and Marilyn used to straighten out all the English. So, Calvin gave him the paper and asked him if he wouldn't mind reading it over the weekend and commenting on it. He went through and corrected all the grammar and Calvin didn't even know what a split infinitive was; and Calvin got pretty upset about this. I thought it was kind of interesting.

VM: Yes: kind of interesting. Generally speaking, I thought that Calvin was pretty good on manuscripts. I must say that I don't remember correcting his manuscripts, but he never used to play too much with my manuscripts.

AW: It's just that Calvin didn't worry about things like split infinitives!

VM: No, that's right. He was, of course, very keen to publish things and make sure they were published competitively early and that nobody else got in. When you were working on these kinetic problems was there any sense of other people in other labs. working on the same sort of thing?

AW: No, I don't think so. I think we were the only people in that business. I don't think anybody had even conceived of it. Maybe even today not too many people appreciate it.

VM: What did you think of the lab. as an institution, as a group of people, and the structure of it, led by Melvin and the way it was run? In the light of what you saw there and what you have seen later, how do you look back at that lab.?

AW: I think it was a very nice group of people to work with. Everyone was very friendly. As I say, there weren't that many graduate students. I think I was very fortunate to be in that particular environment. I kind of learned a lot of things, one of which I talked about before, which is good people don't have better ideas than other people, they just have more. The one thing was that people...I was quite impressed by how Calvin would, perhaps subconsciously, reorganise the order in which things were discovered to make it appear more logical. Because it was just too much trouble, probably, explaining to people what really did happen...

VM: ...in terms of the sequence of events.

AW: Yes, the sequence of events. The fact that a lot of things were discovered kind of accidentally.

VM: Such as? Got any examples?

AW: In a way the experiment that Peter Massini did. They weren't looking for that, they were looking for something else. Something which is very interesting, I found, later going into isotope geochemistry. We used to talk about what are now called the C4 plants and how they stored malic acid at night, and we all knew about that. Most isotopic geochemists, stable isotopic geochemists, knew that there were two kinds of plants, some which had δC^{13}s of about 12 and others that had δC^{13}s about 25. It was amazing that we didn't realise that there were different kinds of photosynthesis, or rather the C4 plants, I guess, just have a thing in front. It wasn't until the Australians...

VM: Hatch and Slack, wasn't it?

AW: He worked for a sugar company, therefore he was working on sugar cane, and he noticed that when he did a lollipop experiment, or the equivalent of it, he got a great mass of malic acid. That's kind of interesting because when I first came to Calvin there was somebody else in Chicago, who was a competitor of Calvin's

VM: Gaffron, I think.

AW: Gaffron; well, he was into the malic acid cycle. He was into the Krebs' type of acid cycle and not into the phosphate business. So in a way, it was kind of interesting how this thing should have been so obvious to everybody, and it took somebody else in Australia doing a completely similar experiment to discover C4 plants.

VM: I wonder whether that isn't a reflection of the very great dedication to solving a particular problem.

AW: Yeah, I think it might be.

VM: Maybe one gets a bit blinkered. Your eye is so fixed on the ball that you tend perhaps...

AW: In fact, we were really studying the path of carbon in photosynthesis in algae.

VM: Yes, and assuming that everything else was going to be the same.

AW: And yet when you live in the desert, like in this (*Editor: i.e. Arizona*), you can see that what the plants do, they take in CO_2 at night, when it's humid and they're not going to lose too much water, and they close up and make malic acid. In the day, they turn malic acid back into CO_2 and run up through the Calvin cycle.

VM: But still, I think you will agree that to tackle the problem as it was perceived in the mid-forties, of how do plants fix CO_2, to choose a system with which to do it, to choose a system that, in the end, the one that they chose you could work with, and to push the thing through was a very considerable...

AW: I am really more wondering why I, myself, didn't realise it. I knew there were two kinds of plants.

VM: It's difficult to think back, isn't it?

AW: Something can be staring you in the face and you don't see it...

VM: ...until suddenly it clicks and then you realise...You commented earlier on about people discovering things serendipitously, not necessarily by looking for them. Does that come directly out of your experience with this?

AW: I think that people discovered the...it should be pretty obvious to a physical chemist that if you have a cycle, the reservoirs should change up and down. But nobody ever thought about it. They thought that the reservoirs were fixed sizes.

VM: Even under the conditions in which you actually handled the material? Because you took the algae out of a growth culture of some sort and centrifuged it, and resuspended it in some other medium convenient for the purpose. You then put it in the vessel in which it hadn't been, shone lights on it, and it didn't dawn on all of you that things were happening inside the plant when you did that? And, in spite of all that, you felt, or some people felt that...

AW: You did want to make the plant happy but you didn't want to put too many nutrients into the suspended liquor otherwise it would ruin your chromatogram. You assumed that it was happy and then you squirted some carbon-14 bicarbonate and tried to find out what the first thing was and the second thing and the third thing. But it took a long time to figure out that if you did the kind of experiment that I did it gave you a chance of seeing what was upstream. That was going from a static system to a dynamic system.

VM: I think the idea that if you are looking for a simple linear sequence of events it really doesn't much matter what happens to the pool sizes. It's only when you get the complicated interactive events which, by the time you were working in it, had become all-important to the (*problem*), that the pool sizes can make or break your understanding of what goes on.

AW: We were really trying to see what was upstream and what was upstream of that, which was the triose phosphate.

VM: When you were doing this work, which would have been...you went there in 1951 and stayed until 1954?

AW: Yes.

VM: So you would have been doing this work...

AW: '52, '53.

VM: '52, '53. By that time did you have the breakdown of radioactive concentration in all the carbon atoms of all the sugars you were looking at? Was that part of it?

AW: People were doing it, though.

VM: But it wasn't all laid out yet; it wasn't all laid out ready?

AW: People knew that when you took the hexoses, the two middle atoms were hot, which meant that probably the two PGAs came together from a C3 to a C6. But no one had worried too much about what was upstream.

VM: The arguments that later developed about the labelling patterns in the pentoses and the heptoses, of how it couldn't be C1 it had to be C3, etc., was that after you did these kinetic experiments, do you remember?

AW: In parallel, probably.

VM: It was all part of the ongoing thinking of the lab. at that time?

AW: They were trying to figure out the path of carbon to see where the carbons were in the individual six- and seven-sugars and I was really on a different tack altogether. I was trying to do it some other way, by building a fancy piece of apparatus and stopping the CO_2 and seeing what built up.

VM: Was everybody talking all day, every day, about this? Were you aware of what everybody else was doing?

AW: Yes. We had regular seminars every Friday and people had to talk in sequence about what they were doing.

VM: Even that, any individual didn't talk except at fairly long intervals. If you waited to hear via the seminars what people were doing...

AW: People knew what everybody else was doing. Everybody talked about what they were doing all the time.

VM: Continuous topic of conversation?

AW: Yeah, right.

VM: To change the direction slightly, what's your memory of the social scene in the lab.? What did you do for relaxation? Did you tend to mix with other lab. people, did you have your own friends? What did you do?

AW: My social life was more related to International House. Besides, most of the people in the lab. were older than I was. I wasn't married and they were kind of married. I mostly associated with the other members of the group when we went on trips to Pacific Grove or Yosemite or somewhere like that.

VM: You were a mountaineer with them? Did you ski as well?

AW: Yes.

VM: Did you do that often? Did you take a lot of time out of the lab.?

AW: I used to work pretty hard. I used to work hard and play hard. I'd work most weekends and most evenings. International House wasn't that far from the lab.

VM: I think to have accomplished a PhD of the complexity that yours actually was within the period that you did, and to have done TA-ing and to have taken courses, you must have worked pretty concentratedly, over much of the time. What about social events inside the lab. — did you visit one another's houses, did Calvin invite people up to his home?

AW: Not very often. I don't remember socially interacting very much with the members in the lab.

VM: Did their wives and children come into the lab.? You knew who they were?

AW: Yes.

VM: How about the events inside the lab., the Christmas parties? If you were designing techniques for blowing straws up to the International House ceiling, you must presumably have contributed something of the sort to the ORL Christmas events and parties.

AW: I have a lot of recollections of International House, because it was pretty big and the people were more or less my own age. But most of the people in ORL were considerably older than I was and had growing children.

VM: You felt a social gap between you and them.

AW: I suppose so, although I did enjoy going to Pacific Grove and Yosemite and those places.

VM: (*This side of the tape is nearly finished so we'll run it out and open up on the other side.*)

About the ORL itself, people have very fond memories of that building. I think most people who worked in it talk about it with appreciation and they comment on the influence that the structure of the building itself had — no separate rooms, minimum of walls, minimum of dividers, people didn't work behind closed doors, and that sort of thing. What do you feel about that as a way of running a lab.? How do you think it affected the way the group ran?

AW: I think the big white table was a good thing because if anybody wanted to talk about their work they could bring out their chromatograms and spread them on the table and eventually everybody came around and talked about it. I remember, I had done some weird experiment and — who's the guy who stuttered?

VM: Grant Buchanan.

AW: Grant Buchanan; he saw something really interesting in my chromatograms and I gave him the spots. I had managed to break down something by accident which he had been trying to do for a long time. I just stumbled across a way of doing it. I had two extra spots and I didn't know what they were. So, everybody really did know what everybody else was doing. The big Round House was a much greater development than that. The complaints I heard about that was that noise travelled a lot and it was hard to get a nice quiet...see, I could go into the office which I shared with Andy and I would be quiet, although there was a big glass partition there.

VM: But other people didn't have offices, did they? Many people simply had desks at the end of their benches so they did have whatever room noise was going on in ORL.

AW: In the room with the big white table, that was arranged so there was a desk at the end of the bench. But in the lab. I was in, I just had a straight bench, maybe seven foot of bench or something. So I never had any place to sit around in other than the office.

VM: You could go and hide in the office whenever you wanted peace and quiet but the other people really couldn't. I don't remember that there was a great hubbub of noise; there was obviously conversation, particularly at coffee time, but presumably in your day, too, everybody took their coffee together, more or less, didn't they?, and they all congregated around the table.

AW: I was very impressed the way they made tea.

VM: How did they make tea?

AW: Coming from New Zealand...This was in the morning. They would put all the cups out, full of moderately hot water, and take a tea bag and dip it in and say "How many dips would you like?" I remember saying that the Queen would never have approved of this!

VM: Certainly not! For many years we used to import our tea — and even this time we brought tea with us, to be on the safe side. But you did see the round building?

AW: I never worked in it, but I saw it later.

VM: You know what happened. The old building got demolished and therefore they had to move into something new. This (*the Round House*) was an attempt to recreate the atmosphere (*of ORL*) in a modern sense. When you saw it, what did you think of this building?

AW: I thought it was a pretty interesting building. It means that you do interact. At least you know the people. If they are in separate labs...I never had too much to do with the group that was over in Donner. You know there was a group over there that synthesised chemicals and fed them into people and figured out how the hell they metabolised them. Interestingly enough, they are just doing that kind of work again using the IMS in the lab. that I'm in. I never had very much contact with them at all.

VM: Did you know them? Do you know who they were? Do you know their names?

AW: Well, yes, because I met them at seminars. Who was the guy who ran that?

VM: Bert Tolbert.

AW: What happened to him?

VM: He's in the University of (*Colorado at*) Boulder. Well, he's retired from the Chemistry Department.

AW: I knew Bert Tolbert, but I never really got intimately related with what they were doing because they were in a different location on the university.

VM: That's interesting because other people were not in your position. I think that's because you were a graduate student and the people who were older and had grown up before they divided so obviously knew them much better.

What happened to you as a result of your photosynthesis experience in Berkeley? Without spending hours — potted history.,

AW: After I left Berkeley...I really was there for three years, less a summer, and that summer I got into the course that van Niel was running. It was a really interesting course because he would only take people, like me, who didn't know any microbiology but were working in a microbiological field. He was a really interesting person. We went down to Pacific Grove and we rented a house. There were seven of us living in it and we kind of lived microbiology. He would run it three days a week — Monday, Wednesday, Friday — and ran it about 14 hours a day; it was really, really interesting.

VM: How long did it go on for? All summer?

AW: I think all summer, yea. Pretty interesting: I learned a lot of microbiology. He taught it from a historical perspective. We even did things like...I remember we did an experiment trying to see why no one had ever managed to ferment orange juice. You can ferment just about everything, but not orange juice. So we tried to ferment some orange juice. We went looking for crab, for those little crawlies...freshwater crayfish. We had a wonderful time.

After that I drove across the country and had a job at Standard (*Oil of*) Indiana as their radiotracer person. That was very interesting. I worked there for a couple of years and made a lot of money and organised a trip, four people, a Scot person who worked for Standard of Indiana and a Scot person who lived in Canada and a Dutch person who lived in Canada, and we bought a car and drove it down through South America and up through Africa. We sold the car and I went across country to New Zealand. I went back to the job I had originally except that it had changed meanwhile in the New Zealand Institute of Nuclear Science. I did the same kind of work.

VM: Same kind of biological work?

AW: Well — sort of. I was kind of interested in instrumentation and the relationship of one thing to another. I invented the paper chromatographic version of scintillation counting.

VM: I remember: in a dish of scintillation fluid?

AW: I was trying to study germination using tritiated water and I wanted to find out what compounds the water got into. So, I would run an ordinary chromatogram, just using the kind of technique that was in ORL, and then I got a very, very fast film, which actually wasn't too different from the film we were using, and I would put the chromatogram in the dish of terphenyl and toluene, which emitted the right kind of radiation, and put the film on top. I could pick up the tritium very well, and I did a whole lot of work and identified the compounds you got from germination. I kind of went into the tritium version of the path of carbon in photosynthesis.

VM: When was that?

AW: That was in Wellington.

VM: Which year was that, do you remember? It must have been not later than about '57 that you did that.

AW: Yeah, probably.

VM: And the reason was that in the early part of '58, I think, I did some tritium work in Berkeley using vast amounts of tritium to follow what we hoped would be the path of hydrogen in photosynthesis. I was aware, now that you reminded me, aware that you had done that. Somebody must have told me that you'd done that. But we didn't use your technique. We used the same technique as before, but we were using so much tritium that it came through anyway. I guess you were probably not using curie quantities of tritium. I used 5 curies of tritiated water in a glove box with Radiation Lab. people protecting me, and stuff of that sort.

AW: We could use similar quantities of radiation to carbon-14 except, of course, tritium has only got about a tenth of the energy of carbon-14 so we would probably have to use about ten times as much in terms of curies.

VM: So, that's back in New Zealand with germination problems. How long were you there?

AW: I left the Institute of Nuclear Science after being there for five or six years, in Lower Hutt, and went to the University of Wellington. I was there for about ten years in the Chemistry Department and then I was offered a position as Dean of Science and Professor of Chemistry at a new university they were setting up in Hamilton. It was quite a tricky job, setting up a university: I got interested in the economics of universities. The thing was, we got it off the ground with pioneering kind of people, but then the university got more and more complicated, as universities do, so I'm afraid I'm a pioneer rather than a runner. My marriage broke up, partly because of all the work I had to put in. And, I got involved in the Antarctic. We used to go down there in the summers which was kind of a fun thing to do. There were so many easy things for a chemist to do down there that I published a lot of stuff on the Antarctic.

VM: This was, by now, in the sixties, was it, or the seventies?

AW: The sixties.

VM: Did you ever meet Ozzie Holm-Hansen down there? He's an ex-Calvinist who's at Scripps and an Antarctic marine biologist.

AW: No, I was a land person. The people I ran around in Antarctica I learned a lot of geology from. I'm probably the last geologist to learn something completely in the field and not in the classroom. And my marriage broke up, I was offered a job, here in Tucson, as Director of Research for a big mining company which had decided that they weren't going to be able to smelt copper in the normal way. They had big plant; well, it was supposed to be a pilot plant but it ran 100 tons of copper per day using hydrometallurgy. In other words, basically you dissolve up the mineral chalcopyrite in a very acid, very salty (?) solution and then you electrodeposit it.

This problem was completely out of their knowledge. The metallurgists that they hired don't know anything about that sort of thing and they were running into all sorts of problems. So, on the advice of an Israeli consultant they had that I had negotiated with solar heated lakes (?) in Antarctica, I was offered the job, to try to straighten out some of their problems. They needed a general analytical, physical-chemist . We got the plant running. It was an interesting process but like the rest of my life it was 30 years ahead of its time! I worked for them for about eight years and then Pennzoil, which we were the mining subsidiary of, shut the whole thing down and I was given early retirement. So I went to the University of Arizona.

VM: As a faculty member?

AW: No, as an adjunct professor (well I suppose that's faculty), which meant I didn't have to do that much teaching; I can just do research. I decided that people like me ought to try pick some difficult problem and try to solve it, so I tried to develop, and in fact developed, sublimation technique which is pretty tricky experimentally, way beyond the expertise of glaciologists, but they needed something like this in order to recover the gases quantitatively from ice cores and measure the various isotopes and their quantities. That's worked out very well.

VM: And you're still there doing it?

AW: I'm still doing that.

VM: While you were talking, I was thinking of two last questions, I'd like to ask you. One of them going back, of course, to Calvin's group, one of them is: what your view is of the group as you knew it as a research organisation? Do you think it was an effective way of doing it?

AW: I think it was a very effective way of doing it. They had people of great diversity of expertise. Calvin himself was very broad based. He was originally an inorganic chemist (*Editor: incorrect; he was a physical-organic chemist*) and it's just exactly the kind of group to do it, although it actually required — I think was it Bill Stepka who brought in the chromatography?

VM: Yes, it was.

AW: OK — if chromatography hadn't have come in, they would have never gotten anywhere. So, they tried doing it with phosphate chromatography, ion exchange chromatography, but Bill Stepka came in — actually, this was just before my time — and brought paper chromatography and radioautography and that's really what they needed; that really broke it through.

VM: Would I be correct in concluding that it would be very difficult to design a group *de novo* to do what they did? There was so much happenstance that developed in the course of their work that you couldn't have sat down in 1945 and said "this is what we are going to do for the next ten years" .

AW: If they had tried to do it in 1940, before the invention of paper chromatography, they wouldn't have done it.

VM: Also, there wasn't any C^{14} in 1940; it was only just discovered.

AW: Right. You needed C^{14} and you needed paper chromatography.

VM: Well, the C^{14}, of course, Calvin had. He knew about C^{14}, and was offered it by Lawrence so that part was OK.

AW: Ruben had already started to work on carbon-14. He had mainly been working on carbon-11, 28 minutes half-life.

VM: And they had tiny quantities of C^{14}. The chromatography, as I'm sure you're right, was the big breakthrough which actually enabled them to do what they did.

AW: They came up with innovative things like co-chromatography and then I quantified it. It was really cheap stuff to do.

VM: But there is another point and that is: you say that had Stepka not brought it in. Stepka must have got there in something like '52, or '51, something like that.

AW: Probably before that because I was there in '52 and I don't think he was still there. He must have come in about '50.

VM: The invention of chromatography actually took place in '44...

AW: '46.

VM: '44; '44, I think, and that means it was known. Even if Stepka hadn't brought it in, one can argue that they would have realised.

AW: Oh, I think it would have come in eventually, but you'd had to know how to do it.

VM: Well, you send someone to somewhere else where somebody is doing it.

AW: When I was there, just as I came, they learned how to chromatograph organic phosphate because when I arrived they couldn't do that. All the phosphate esters stuck on the origin. You have to wash the paper with oxalic acid to get rid of all the heavy metals, and also there was (*indecipherable*).

VM: I remember them doing that.

AW: That was a big trick. I think Stepka just could chromatograph things like malic acid, but not the phosphates. Working with phosphates was a tricky thing. Acid-washing the paper and I was involved with spraying phosphatase on it once you got the

phosphate ester separated. You also had to humidify the paper if you wanted to get them (*the compounds*) apart from one another.

VM: In your day of working with chromatography of phosphates, did you over-run the chromatograms so the solvent dripped off the bottom for a long time in order to spread the phosphates out?

AW: No.

VM: That must have come later. I vaguely thought that I must have invented that myself, but I can't remember whether or not I did, or whether somebody else did. Of course, it spreads things out and you get rid of a lot of the junk off the front end, stuff you don't want, and then you get a lot more further back.

AW: If I did the chromatography right, if I had well-washed paper and humidified it in the phenol, I could get nice, tight spots. I had to get the spots hopefully under the Scott counter, so you really had to know how to run good chromatograms.

VM: The over-running chromatography arose when we wanted to look for very minor components, which people couldn't find, like erythrose-4-phosphate and carboxylation products.

AW: You mean you ran it on a line, one-dimensional.

VM: No, no. We ran it two dimensionally, but we ran it about twice as long or three times as long so that the phosphate area was spread out over the whole paper.

AW: Were the spots really big?

VM: The spots were bigger but, if you kept your fingers crossed, there was enough discrimination; they didn't merge.

The last thing I wanted to ask you was about actually producing your thesis because you are the first former graduate student in that lab. I have talked to. As you say, there weren't many.

AW: I had a really good deal. They produced my thesis as a ORL report.

VM: You had to write it...

AW: Yes.

VM: ...the secretaries typed it for you...

AW: As an ORL report.

VM: ...and produced the figures...

AW: Yeah.

VM: ...and that was it.

AW: Then I could rebind a copy of it as a thesis. So I got my thesis typed for nothing!

VM: Very well placed compared with many graduate students.

Well, that's as far as I can think unless you can have any more stories...

AW: The only story I have, which to me is hearsay, I wasn't there...I thought Calvin was a very good lecturer. He went to this lecture, I believe in England, and he gave this lecture when he was very good at giving lectures, and he wrote on the board this is one possibility and this is another possibility, and, in fact, the people back in Berkeley are working on it at this very moment. And somebody walks in with a telegram and he opens the telegram and crosses one of the possibilities off! I thought that was very clever. I always wanted to do that myself. (*Laughter*)

SM: He'd arranged that?

AW: Oh yeah. It was obvious that he had arranged that, it was obvious to the audience. The audience were just taken in, you know.

VM: But they liked it.

AW: But they liked it, even the English. (*Laughter*)

VM: I think that's splendid. Well, OK; on that note, let me just thank you very much for your time. It has been very well worth while our coming to see you and I was very pleased to meet you after all these years of simply knowing your name on a paper and as an inventor of scintillation for tritium chromatograms.

AW: Thank you.