

Chapter 7

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VM = Vivian Moses; AB = Al Bassham; SM = Sheila Moses

VM: This is a discussion with Al Bassham in Berkeley on the 25th of May, 1996.

Al, you were saying just now that Sam Ruben was instrumental in your getting into this business (*i.e. photosynthesis*) in the first place.

AB: That's right; it's not of great import to the discovery, but it's important to me because it really influenced me to be Melvin's graduate student and work on the path of carbon in photosynthesis. When I was an undergraduate at Berkeley, a chemistry major in 1940, perhaps it was the spring of '41, the first year, as you may know, the chemistry professors, especially the younger members of the department, used to take laboratory sections and be the laboratory instructor. I had the good fortune to have Sam Ruben as one of mine. One day, instead of talking about the usual protocols for laboratory experiments, he said I would like to tell you a little bit about my research because it's kind of interesting. He said recently we've gotten access to carbon-14, a radioactive tracer, as a result of atomic experimentation going on here (*at Berkeley*), Oak Ridge and other places and we are now using it as a way of tracing what happens to carbon, whether it be in biological reactions or chemical reactions.

My particular project is to follow carbon dioxide during photosynthesis, where carbon dioxide is taken up to make sugar. He went on to talk a little bit about how they used this radioactive isotope and how they were analysing the products and so forth, and hoped to find out the pathway. That was all there was to that. Then the war intervened and I went into the Navy for three years. I came back as a graduate student at the University of California. By the way, they didn't want to accept me as a graduate student because they have a policy against taking people for graduate work who have done their undergraduate degree (*at Berkeley*). But they made an exception in my case, perhaps because I was a veteran and pleaded that it would be inconvenient for me to go elsewhere. So, I came back in for course work only and after one semester, why the dean called me in and asked me if I would like to work for a PhD, because he had heard something good (*about me*).

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VM: You already had your bachelor's degree?

AB: I got my bachelor's degree during the war. I didn't quite have enough units when I left Berkeley but I finished all my requirements. While in the course of my Navy duty I got a chance to go to radar school, pre-radar school was at Harvard, this was following my getting my commission at Columbia (*University*), and the radar school at MIT. I later learned after a bit that you could apply for credits for the work that you did at Harvard done by the graduate.. the electric engineering school. So I got these credits and the University of California notified me that I was awarded a degree while I was out at sea in the Pacific. So I came back and, as I just said, re-enrolled as a graduate student. So anyway, I was allowed to become a PhD candidate at the end of the first semester and I wanted to go into some phase of organic chemistry. I got a list of professors to go to and Calvin was on the list, the first one on the list. So, he was the first one I went to see and he provided me with a list of his research projects. The first one he mentioned was the work with carbon-14, both with photosynthesis and also with some organic reaction mechanisms. And, of course, being with the big professor I listened politely to all of his research proposals. But I had already made up my mind as soon as I heard about carbon-14 in photosynthesis, because of my experience with Ruben. And so that's the one I told him I wanted to work on and that's how I got started in that project.

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

AB: I met him in the old red brick building, the one that dates back to around 1900, when it was modelled after a German chemistry department.

VM: What was your impression? Had you heard of him?

AB: Frankly, I hadn't really heard of him. I had never had a course from him. He was a young professor and I had been gone in the war and I didn't know much about him. This was the first time I had actually encountered him, at this meeting.

VM: In the light of where you had been in the previous years you weren't very familiar with the university atmosphere and the way things were done.

AB: I wasn't. There were some other organic chemistry professors such as Jim Cason that I had taken courses from whom I liked very much. But I wasn't tempted to go to any of them after hearing about this possibility to work with carbon-14.

VM: What was the set-up when you made this arrangement with Calvin? He accepted you and you agreed to work on photosynthesis.

AB: You mean...

VM: Physically where were you, who was there, what did you do?

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AB: Where we worked, of course, as you perhaps know, was in the Old Radiation Laboratory...

VM: From the beginning?

AB: Yes, from the beginning...the old wooden building. This was a building which, I guess, had been originally built as a temporary building for some engineering studies many years ago, but had been given to E.O. Lawrence, the inventor of the cyclotron, when he began to make himself famous with his cyclotron inventions back in the early thirties. He had had a 37-inch cyclotron there as well as a shop for doing all kinds of machine work and a nice shop for doing glass work and a carpentry shop. These were all very important things as you will see to our group later on. They were originally put in there for E.O. Lawrence. Anyway, things were already starting to move to The Hill, to the 184-inch cyclotron and the other things that were there, but he still had something going on in our building. But space was being vacated and it was given to Professor Calvin to carry out the carbon-14 work which also, of course, was a part of the Radiation Laboratory — it was then called the University of California Radiation Laboratory, or UCRL. Initially, I think it was just a lab. on the (*west*) end (*of the building*) and an office or two. Shortly after I got there we got access to a central area where the cyclotron had been before it was moved away. We refurbished it a bit, but it was still a big open area with desks (*and benches*) around which led to a lot of the communication between the various people that worked there.

VM: Is that where the big white table was ultimately placed?

AB: And the big white table was ultimately put out in the middle of that.

VM: Who was there when you joined?

AB: Let's see if I can remember them all. Andy Benson, of course, as you know had done most of the initial experimental work and there was Sam Aronoff, a postdoc. who came from the plant physiology department, I believe, and later moved to other universities, I think, and ended up at Simon Fraser (*University*) in Canada. His role was to tell us about plants: we were all chemists, including Calvin, and most of us didn't know anything about plants at the start. There was another graduate student, named (John) Weigl who worked for a while and eventually went to Xerox. I never heard from him after that.

VM: He died about '82.

AB: Yes, I guess I knew that. Then, there was a technician — a fellow, a red-haired guy. What was his name? It may come back to me later — I can't remember right now.

VM: It wasn't Goodale was it?

AB: Oh yes, it was Tom Goodale; actually, the one I just referred to replaced Goodale. Goodale was the first technician we had but he didn't last long for one reason or

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another. I mentioned all the others, I think, except perhaps we had a dishwasher at the time, someone who took care of the lab. That was about it, when I first started there.

VM: Very small.

AB: Very small at the beginning. It grew after a bit. Later on other students came, that we'll be talking about later, like Wilson and Goodman and so forth.

VM: When did you actually join the group?

AB: I started as a graduate student in the fall of '46 so this would have been about January '47 I believe.

VM: Was the group in ORL already set up or did you get the sense that they had just moved in, or were they well-established in there?

AB: As I have described it, they had only a small part of the building. Even as I went in the space was getting larger because initially they just had the lab. on the (*west*) end and Andy Benson's office which was on one end of the building. I don't remember when I first moved in, I think we already were expanding into the other room and after a short time later we got space upstairs for the chromatography work and so forth. It's a little hard for me to remember precisely what we had when I moved in.

VM: What did you start working on when you got there?

AB: The first job that I was given was to chemically degrade very small amounts, and lightly labelled amounts, of succinic acid and malic acid. These were four-carbon carboxylic acids which Andy had identified as being minor products of photosynthesis with carbon-14. It's kind of an interesting story, too, in that although these turned out not to be involved in the carbon reduction cycle, later on people did discover a shuttle mechanism, called the C4 cycle, involving malic acid, not succinic acid, involving malic acid and it has some importance in certain plants. At the time, we thought it would be involved in the basic incorporation of CO₂ in all plants. So in order to find out whether the carbon label was migrating from the carboxyl group, where you would expect to find it first, in a carboxylation reaction, to the central carbon atoms, where it should be found if there's a regenerative cycle, it was necessary to degrade malic and succinic acids and determine the distribution of carbon-14. That was my first job.

VM: Did you work out the methods for that or were they already established?

AB: They had to be worked out with respect to the radioactivity in small amounts. People had chemically degraded (taken apart) acids before and there were classical methods in the German literature that I had to look up for doing degradations. But all this had to be done on a micro scale because there wasn't much radioactivity in these compounds and so you had to work with as little carrier, that is unlabeled compound, as you could get by with.

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VM: This was pre-paper chromatography?

AB: This was pre-paper chromatography .

VM: How were these materials isolated?

AB: I believe they were isolated by ion exchange columns by Andy Benson.

VM: One you point you made, which I would like to ask you: you said it was the first job you were given. Now as an incoming graduate student, did you have a topic which was going to be your thesis topic which was your own and you could develop the way you liked or did you immediately join in with a group? I'm not quite clear about the...

AB: In this case, I really joined in with the group. As a graduate student I was involved in a number of publications but they were publications with a lot of other people working in the team. It wasn't, as more commonly the case, that I was given a specific topic of my own to work on. I guess you could say that my specific topic initially was the degradation of the carboxylic acid but that evolved as time went by into a broader scope of things. The title of my thesis still had...was something like "The Path of Carbon in Photosynthesis: The Carboxylic Acids".

VM: So, you had a sense of theme that went through your thesis research.

AB: Yes, it was a subdivision of the whole thing but it was a kind of a theme that I was involved in.

VM: You had your own bench...

AB: Yes, I did.

VM: ...presumably, in that room with the...?

AB: By the time I got there and got established, I had a bench in the big room. Initially, I guess, it was some space maybe in the little end lab. I don't remember exactly.

VM: The big white table, which appears so prominently in everybody's memories of the whole group, has always been referred to as the place where you look at the chromatograms. In the beginning, there weren't any chromatograms. When did the big white table come? Was it there when you got there?

AB: I don't think so. I think it did come after the chromatograms because the reason it was white...Our lab. benches were, at the time, generally lined with black Formica (and black was the colour we used) and we did develop after a year or two the need to look at the radioautographs of the paper chromatograms. At that point we needed a big white surface to set them on. That's when I think we put together a bunch of cabinets in which we had chemicals stored and then had Ralph Norman, the carpenter at the time, build us a top for it that would just fit over those cabinets and make a big white

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space made of Formica. (*Editor: The big white table was in existence by 1948 as it shows in photos of that year's Christmas Party.*)

VM: It never really was a table, it was always a...

AB: It was never a table at that time.

VM: The top surface, as I remember, something like: was either very hard plastic...

AB: It was white Formica, and we had experience with that, a good material unless you put alkali on it, in which case it blisters, and we used that for the table.

VM: The lab. benches, as I remember in ORL, were the ones with chemical racks, somehow closely associated.

AB: They did have, yes.

VM: And so presumably they were designed by chemists. The whole atmosphere was a chemical lab.

AB: Very much chemical, yes.

VM: So the biology...

AB: We were novices with plants. We had to learn everything about plants. People like Sam Aronoff helped us in getting algae cultures going, and so forth.

VM: It turned out, of course, very much a biochemical activity as it developed. Who was the biochemist? I guess you all became biochemists in the end.

AB: By and large the biochemical work was done by chemists who learned biochemistry, people like Rod Quayle who came and tried to isolate enzymes — did isolate enzymes, and do forth, who had never done that kind of stuff before. I am trying to remember who was more of a biochemical nature. Clint Fuller, perhaps, had a bit more biochemical background than some of the others. You'll have to check that with him; I think he did. Of course, Sam Aronoff was a biochemical plant physiologist. People like Calvin, Benson and myself and other graduate students were by and large untrained in biochemistry. Some years later, I taught, as an adjunct professor, biochemistry about five years and I always thought it was a little bit ironic that I was teaching biochemistry when I had never had a course in biochemistry (*laughter*).

VM: That's the nature of a pioneer, isn't it? You start something which other people haven't done.

When you first started, what sort of plant material was being used?

AB: I was going to say that we started with algae but that's not true. We did do experiments with leaves. What leaves did we use? — soybean we used, I think.

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VM: Barley, perhaps?

AB: Barley was used? Yes, that's correct. What else? Well, eventually, after a time, a lady scientist named Vicki Lynch came in, who was also very much trained in plant work. She and others did a study of a number of different phyla of organisms, different kinds of plants and so forth, and tried to see if there was a universal pattern of carbon fixation in all of them, which there was.

VM: I was wondering how the use of the *Chlorella* and *Scenedesmus* started, since you were all chemists. It's true I can see that Sam Aronoff could have introduced you to the idea, but somebody must have made a decision: we are going to get a way from barley leaves, or whatever.

AB: I think that the algae, of course, were very appealing, for a chemist, because they could be treated almost like chemicals. They are not, of course, they are cells but you can pour suspensions of them, you can bubble gas through them, and you can do all kinds of thing with them. And, very important, you can kill them — you can stop the biochemical activity quickly. That's why they (*algae*) were used.

VM: Were you there at the beginning of the algal use?

AB: I may have been. I can't swear one way or another the actual moment they were introduced.

VM: I remember them, but this was ten years after that, as being, these large flat-bottom flasks in the shaker thing over lights. Was that the earliest form of continuous culture?

AB: That was, yes. The shakers going back and forth in the water bath. I think I may have been there from the beginning but I didn't have anything to do with setting it up. That would have been done by the people who knew more about plant physiology.

VM: Vicky Lynch, perhaps.

AB: This may even have gone back to Sam Aronoff's time. I'm not sure.

VM: Was there someone even at that time whose job it was to look after these cultures?

AB: There always was Vicky Lynch or somebody like that.

VM: The first one I remember, I think, was Pat Smith.

AB: That was much later.

VM: OK. So these algae were growing and at some stage, presumably, you began to concentrate (you collectively)...concentrate mostly on the algal tissue as a...

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AB: I used them a lot in the early years of my work because they lent themselves to kinetic experiments, taking samples where you vary the length of time of photosynthesis from a second or two on up to several minutes.

VM: The “lollipop”. Can you remember the beginning of the lollipop?

AB: Again, I’m not sure. I didn’t invent it. My guess would be that it was someone like Andy Benson who would have invented that because he is very good at all experimental set-ups and he probably foresaw the need to make a thin vessel with a stopcock at the bottom and the top, and ways of bubbling gas through it. I probably can remember about the time it (*the first one*) was built but I couldn’t put my finger on the precise date.

VM: Presumably that was a facet of using algae because it wouldn’t have been much good...

AB: Oh absolutely. It (*the lollipop*) was used just for algae. In later years when we wanted to do leaf experiments we went to a lot of pains trying to make something similar to a lollipop with faces on it that you could detach and pull the leaf out quickly. It was never quite as easy as it was working with algae. With algae, as you know, you just put on a little pressure and you turn the stopcock and squirt out a sample.

VM: Who actually built the stuff? Was there a glassblower?

AB: Yes. As I said earlier on, there was a glass shop (*in ORL*); we inherited, as it were, the use of the glass shop. The shops were still used by people on The Hill. We inherited the use of the glass shop, the carpenter and his tools and the machine shop. We could always just go in and talk to the people directly. We were in better shape than the people up on The Hill because they were right next door to us.

VM: I see; these were actually The Hill...

AB: For a while they were also used by people on The Hill but I’m sure that very soon they got their own up there. But for a time, why, they were very handy for us and that went on for several years before they eventually...

VM: So that was a major factor, perhaps, in the way that you...

AB: They were marvellous. I think that any scientist in a department would be absolutely thrilled at having the kind of access that we had to glassblowers, to machinists and to carpenters to do their work. We tried to maintain that as best we could in our budget over the years as we moved around, although, of course, we had to give up some of it; in particular, with respect to the glassblower and the carpenter, we always have had them for a number of years afterwards in the lab.

VM: When you first joined, and there was this small group of people in ORL, presumably you had a very easy and close social relationship in the lab., you were always talking to one another.

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AB: That's very true. As you know, when you there in he group later — it was even more true when we were smaller, of course, because there were fewer people to be involved, and everybody knew everyone else well — we did, I might add, to have more people, we combined our social events lots of times (birthdays and special things) with the group that was in Donner (Bert Tolbert, Dick Lemmon and those folks who were working on mechanisms of organic reactions using carbon-14 or doing animal studies as the case may be). When we needed to have enough people for a decent small party, we all got together.

VM: Did you feel very close, the two locations?

AB: Yes, we felt very close and Melvin Calvin, of course, did everything he could to maintain that cohesion. One of the other things that we might want to talk about a little bit more was the group seminars that we had regularly every week at eight o'clock in the morning, to the horror of the British delegates to our lab. (*laughter*) but it got everybody going. At these meetings, they weren't formal in the sense that nowadays you have a seminar and you have a schedule of speakers and they know months ahead that they are going to talk. The way they worked was that we all got together without any knowledge of who would speak and Melvin would point to somebody and would say "Al, you tell us about what you are doing this morning". Of course, Melvin was pretty much on top of what everybody was doing so he had an idea who might have something interesting to say. But, when he pointed to you and said your name, you had to present your talk whether you had anything to say or not.

VM: And you had to do it without props, presumably.

AB: No slides, no props of any kind. If you thought you might be called on you could bring something along, but usually you didn't, because it was in another place, another building. They were held over in the Donner Lab., for instance, and we were in another building about a block away.

VM: When you first joined the group, the seminars were already going?

AB: Yes, I think he must have initiated those meetings from the very start. Marilyn, or someone like that, could tell you more about that.

VM: In the early days, you all sat around a table did you?

AB: I'm trying to remember the configuration of the first room we were in. I don't think so. I think it was more or less of a conventional small room with seats and the speaker up at the head. I don't think we had a table. That was something we introduced later on when we had the opportunity to configure the room.

VM: Melvin, of course, sat up front!

AB: Yes, oh yes.

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VM: On the right-hand side.

AB: Sort of in your face, as it were. As soon as you said something he didn't agree with, he was on top of them immediately.

VM: He has always been like that?

AB: He has always been like that. But, people who worked there didn't take that too personally because they knew he was like that. But, I must say, in later years some of our visitors were very much upset, people were used to a more formal procedure. Being jumped on as soon as they said something that he thought was foolish, was a little hard for them.

VM: Presumably, soon after you joined, the group was in an expansionist phase?

AB: I think it was. I think we were expanding more or less continuously for almost the whole time, especially the first 10 or 20 years, and that's not too surprising. Because there were very few places in the world where one could go and learn about radioactive techniques. Of course, radioactive isotopes did diffuse to various parts of the world, and some of the early work was done in other countries, but we had a very good access to both the radioactivity and to substantial financial support to carry out experiments. Those weren't the days of writing grants to NSF or NIH or anything. We made a kind of a quarterly report to the AEC, which was the controlling body, in which we outlined what we were doing, what we had found, and so forth and they more or less took the reports and sent us money — not unlimited, but generally enough to carry on the work and maybe expand a little bit.

VM: I think those were the days when the AEC was very keen to have non-military activities.

AB: That's probably one of the reasons.

VM: So, in the course of your PhD work which took you five years, or something of that sort?

AB: I didn't take that long, actually. I started in the fall of '46 and I got my degree in '49. It took me three years, basically.

VM: Did you not have to do courses?

AB: The first semester, that I talked about, I did courses. After that, I took a few courses but not many and mostly I just devoted myself to (*my experiments*). We all had to do teaching from time to time in the sections. I was supported initially by the veterans' programme, as a W.W.II veteran. That didn't last very long before I was offered a graduate student research stipend. That replaced the veterans.

VM: By the time you had finished your graduate work in '49, how far had things gone in the path of carbon?

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AB: They had gone quite a ways. I don't know when the most definitive publications came out. I always think of *Path XXI* as being a definitive publication but it wasn't the first announcement of the carbon cycle by any means. Melvin, of course, was invited to speak and gave lots of talks here and there and some of them were later transcribed in the proceedings. I think by 1949 I think we had a pretty good handle on the cycle, even though the most definitive article didn't appear until about 1953.

VM: That meant that since the whole activity became highly dependent on paper chromatography, that must have been introduced well before '49.

AB: Oh yes, absolutely.

VM: Before you arrived, or did you witness it?

AB: No, no. As I said earlier. when I arrived, Andy Benson, aided. I guess, by Tom Goodale and other people, was isolating things with ion exchange chromatography. Bill Stepka came up to our lab. shortly thereafter (I don't know the precise date — he'll be able to tell you that) and he had been working with amino acid separation by paper chromatography, following the methods invented by Martin and Synge in England. He already knew how to separate out alanine, aspartic acid, things like that. Very soon we discovered they were indeed labelled, at least after a minute or so of photosynthesis, or even maybe 30 seconds they would begin to get labelled. Things like alanine and aspartic acid.

VM: This was known from the (*ion exchange*) column work?

AB: The column work may have revealed the presence of labelled amino acids. Andy Benson would be a better authority for that than me. In any event, we use it to separate amino acids by paper chromatography which was much quicker and easier and we discovered others which I am sure he didn't find with the column work, things like serine and so forth. Andy, I think especially, and, I guess, Melvin Calvin and others recognised the power of this two-dimensional method but it wasn't very good for separating the phosphates. When they used the solvents that one uses with amino acids, you get just a big glob of stuff down in the corner; they didn't move far enough. So Andy Benson, really, I think, was the one who experimented with — tried different solvents until he got one that would separate the sugar phosphates. He can probably tell you the date of that better than I can or we could look at some of the publications. By the way, I brought most of the *Paths (of Carbon papers)*, I have them bound up out in the car; I imagine you have them all.

VM: I would very much welcome borrowing them while I am here (*i.e. in Berkeley*).

AB: I would be happy to loan them to you.

VM: Thank you very much; that would be very helpful. When paper chromatography was first introduced, you didn't have any dedicated facilities for it?

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AB: That's right.

VM: As it's a smelly activity, where did you do it?

AB: Initially, in the lab., but, as you said, it was very unpleasant. It was particularly unpleasant because some of the early solvents we tried were even worse than the ones we used later and the ones we used later were pretty bad. I remember some of the early ones involved imines like lutidene; it really was awful. Somebody (Calvin, Benson, somebody) knew that there was space in the second story of the ORL which had been used for offices for, I guess, E.O. Lawrence and those people, but they had vacated them or were about to. They made arrangements to get hold of that space. It was renovated and changed a little bit and we put the chromatography tanks up there. It was still smelly but you didn't spend too much time up there. In retrospect, I think we should have gotten money and installed a good vapour escape system but we didn't do it.

VM: There must have been a rapid investment, then, in chromatography equipment. There were the big boxes and there were also the stainless steel trays which were designed by you — not you personally; by the group?

AB: Yes, they were; I don't know now who designed them. They contracted out with some kitchen supply people or somebody like that. The first chromatography boxes were wood, I think they even had just paraffin linings, which wasn't very satisfactory with organic solvents. Then we got the idea of putting Formica linings (*in the boxes*). We had Ralph Norman (*the carpenter*) build some of those. They gradually evolved into the kind of boxes that we used later. Over the years, I guess, we went more and more to stainless steel and things like that.

VM: You remember the early ones which had sleeves that you fitted over the rails in the tank and the papers went over those? When you wanted to take the papers out you put stainless steel clips over them and you took them (*the papers*) out and hoped they didn't tear, while you were suffocating.

AB: Yes, that's true.

VM: You remember there was a Martian helmet that someone wore?

AB: Yes, that was me. That's one of my few contributions. it wasn't a very good one. I was horrified at all these organic vapours we were breathing so I tried to get some kind of helmet with an air supply to it that people could put over their head. It was so hot and uncomfortable that unfortunately most people didn't use it. But I thought people should at least have the option of being able to work with this stuff and not breathe the fumes.

VM: At the time when chromatography came in, you must also (I am using "you" again in the collective sense here), you must have decided to go to radioautography. All of these things were new, not just for you but just generally because they were new inventions. Do you remember the introduction of radioautography?

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AB: I more or less remember it but I don't know the precise time or anything like that. I guess, I'm not sure, trying to remember: I just don't know how it came about. It was introduced very shortly after the use of paper chromatography. Obviously, we had the radioisotopes to detect and we wanted to find out where they were and this was a way to do it.

VM: Then there was that underground room in the basement, the dark room where you loaded the stuff up.

AB: That was my doing. The reason it was my doing was, as I told you earlier, that I was charged with degrading these chemical compounds and finding the extremely small amounts of radioactivity that were in the middle positions of the carboxylic acids. I soon discovered that I couldn't do this by regular counting with the old-fashioned lead-shielded counters because we had a cyclotron in Crocker Laboratory, right next door, which they used to turn on at night and other times, and gave a variable background which was higher than the amount of radioactivity than I was trying to detect. So, I searched around for a place that would be more shielded. I don't know who mentioned to me that there was an underground area. At the time, when I first went down there, it was just dirt but there was some sort of foundation or other types of cement walls which, perhaps, had supported the cyclotron or some heavy equipment, I'm not quite sure what. Anyway, we put our counter devices down there. You know what these devices were. They had these lead shields and then they had a Geiger tube inside and there was a slide that went in that you could insert your little planchette on which you had mounted your chemical with radioactivity and slide it into there and turn it on. You could set it to count for long periods of time, or long periods of background, whichever you needed. That helped some but it still was pretty hard to work down there. So eventually we got money from somewhere (I didn't get it but the people —Melvin, I suppose, did) to have some work done. They excavated a bit, put a cement room in and a stairway leading down there which made it easier to get up and down.

VM: Before too long you were using those end-window Geiger tubes with mylar windows and gas flow. Where did they come from? Who invented them?

AB: I'm not sure. You see, initially we used split mica (*for the windows*). It was quite an art, I guess. I didn't know the art but there were those who could split mica and get very thin sheets of mica to put over the Geiger tubes. As you can imagine, these were not very uniform and were easily broken and didn't work very well. I don't know who found the mylar (I would always guess that it would be Andy Benson since he was very clever at such things; but it might have been somebody else) to put over the tubes and made it possible to count with that. I don't know when that happened. I think we were stuck with the mica for quite a long period of time but eventually we went to mylar

VM: You remember that agonising business of shielding the bit you wanted to count from all the neighbouring pieces of paper with the bits of card, putting these mylar end-window tubes on that. That was an excruciating activity.

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AB: Of course, that led eventually, as you know, to the invention of the “monster” by you and, I guess, (*Karl*) Lonberg-Holm so we could do it automatically.

VM: That’s right and I think I mentioned last year that the relic of that (*machine*) still exists down there in the warehouse in Emeryville; I went to see that.

AB: We eventually elaborated on that. Let’s see: what did we do? We eventually rigged it so we could count both C^{14} and P^{32} . I don’t remember how we did that.

VM: You had two counters in sequence...

AB: Oh yes, that’s it: two in sequence.

VM: ...one had a thick window.

VM: It’s your version of it, your rework of it, that’s down in the warehouse. By ’49ish, when you’d finished your graduate work, what happened to you then? How did you make the transition from being a graduate student to being a regular staff member?

AB: A lot of the work (*on the path of carbon in photosynthesis*) was still going on and it was extremely interesting. Not surprisingly, I wasn’t anxious to leave and go somewhere else. It wasn’t so much that I didn’t think I could find a job somewhere else but it was such an exciting business and it wasn’t all completely wrapped up yet, so Melvin Calvin offered me a postdoctoral support at the lab. we had lots of those available from the AEC. I went to work as a postdoctoral fellow. Time went by and I stayed on. I got a National Science Foundation senior postdoctoral fellowship to go to Oxford for a year in ’56; that’s some years afterwards. When I came back from that, I was a sort of staff scientist rather than a postdoctoral.

VM: You had been a postdoc. up to that time?

AB: I’m not sure, you know, just when the change... It might have changed even before then because that would have been a period of quite a few years. Probably I was already a staff scientist before I went to Oxford. It gradually evolved.

VM: By the early to midfifties, this was going to be your permanent career. You could see it?

AB: That’s what it turned out to be; I didn’t know that at the time.

VM: You were in a stable position with research (*and support*) at that time. There were lots and lots of people, of course, who came through the lab. at that time and some of the papers have a very long lists of authors.

AB: Indeed.

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VM: What was the practice in those days? You and Andy and Melvin, presumably, were the main co-ordinators of the direction in which things flowed.

AB: I think, generally speaking, Melvin determined the list of authors and who should appear. I don't know what rules he went by, exactly. Sometimes he went alphabetically, which is good for me! Other times, though, he went more by who he felt had made the biggest contribution. I want to say that I thought Melvin was always very generous in attributing credit to those who had done the work but I couldn't tell you precisely how he decided. I remember on some occasions making suggestions that others ought to be included on the list — I don't want to be specific now because I don't remember specific... Sometimes we would have graduate students who would do key bits of work or technicians who would do key bits of work. On degradations, for instance, like Lorel (*Daus*) Kay did some splendid work on the sugar degradations and they obviously ought to be, I thought (and, I think, Melvin though too) included on the list of authors. I don't know whether that answers; that's about the best I can do.

VM: In the early fifties, before Andy left (Andy left in about '55, I think)...

AB: Is that right? I don't know the precise date.

VM: ...you and Andy and Melvin were really the people who were the ongoing core of the photosynthesis activity. So, presumably, the three of you, however you arranged it, were the main co-ordinators and directors of the way people were going. Each new batch of graduate students coming in, or postdocs., presumably talked to the three of you in order to work out what to do.

AB: I think Melvin mostly decided who was going to do what and then they would talk to us and we would get more specific. Sometimes his ideas would be more general. People came and talked, at that time, to Melvin and he put them in some direction or sent them to one of us to work on some general area. Maybe we would be more specific in what they would actually do. With the graduate students, he pretty much determined more precisely what they would do. I am sure he's the one who got Alex Wilson going on doing the transient studies that he did with carbon dioxide high and low and so forth.

VM: He was always the one formally responsible for the graduate students, as a faculty member?

AB: Yes, in those days. Later on some of us had graduate students who were nominally with Melvin but he would simply assign them to us and we would give them a problem. We are talking about the early days now.

VM: In the early days, he was very closely associated with all the graduate students, much more so than later on.

AB: Yes.

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VM: I also remember, and I am sure this must have been the case from the very beginning, that he was absolutely concerned on a day to day basis with every piece of information in the lab.

AB: Absolutely. The thing we always used to say was that Melvin would talk to a student or somebody and outline six months worth of work and came in the next morning and ask them what results they had gotten!! That's a bit of an exaggeration but it was kind of like that.

VM: He wanted to see every smudge on every chromatogram, every kink on every curve.

AB: Absolutely. He could make interpretations that nobody else would even think of, some of them right and some of them wrong, but he had a fertile imaginative mind for working out what something might mean.

VM: But he was always amenable to being contradicted, wasn't he? If you had a better argument, he didn't pull rank.

AB: Yes. Some of us had to eventually, sometimes we had eventually to set the record straight, as it were, because he, like all geniuses, he had lots of ideas and some of them were brilliantly correct but there were a lot of others that weren't.

VM: He was a very, very stimulating man and I think all of us recognised that. Later on, you became interested in expanding out from the original carbon cycle work in order to the implications of it. When did that begin to take shape?

AB: It kind of evolved over time but I would say after about '55 or so, '56. I began to do very elaborate kinetic experiments with the help of Martha Kirk, who was a very talented scientist herself although a technician, and we would do these careful kinetic studies from the very shortest times and we got interested in the flow of carbon out from the cycle into secondary products such as sugars, amino acids, carboxylic acids and things like that. That led inevitably into consideration of secondary pathways. Also, of course, there came along after a time the C4 pathway which we kind of missed because we didn't work that much with C4 plants. We had to investigate this and look into what was happening there. Actually, Andy and Melvin published a C4 pathway as the path of carbon in photosynthesis, as a speculation. Some of the work that I had done with degradation showed that that wasn't right because the carbon wasn't spreading quickly enough to the centre carbons of the malic and succinic acids. Having tried it once and rejected it, we were perhaps slower than some were to embrace a new C4 pathway. It became clear, eventually, from the work of Kortchak in Hawaii and Hatch and Slack in Australia, and so forth, that there was indeed a C4 pathway. We had to take that into consideration.

VM: What influence did your year at Oxford have on what you did later? You spent the year with Krebs in '56-'57.

AB: That's true. Probably: somewhere on what we were just talking about — the other dark metabolic reactions in plant cells that come after photosynthesis. Most of my

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time in Oxford was really spent trying to develop a method for dealing with very small amounts of things again, based on my work done earlier. It didn't really come to anything too great. It was a nice experience and I enjoyed it very much and my association with Professor Krebs but I don't know that it had any profound influence on what I did, really.

(Brief gap in the recording)

VM: It adds life to the picture of the place: do you remember when, etc. When somebody dropped something, when somebody did something bizarre, whatever it was. We had lots of parties, I remember. There were always excuses for parties

AB: Oh yes.

VM: Birthdays and Christmas.

AB: We always had a nice Christmas party.

VM: These trips up to the mountains and to the deserts and so on.

AB: That goes way back to my earliest days (*in the lab.*). Soon after I got there, I learned that people like Andy Benson and Bert Tolbert were going off to the mountains and they, of course, invited me to come along. I had lived in a mountainous area myself in the past and enjoyed the outdoors although I had never really done backpacking before. So, they introduced me to backpacking and mountain climbing. We had quite a few people in the lab. who liked to do that same sort of thing. We used to sometimes initiate people, visitors, foreigners on that, and most of them enjoyed it — some of them found it a bit strenuous. One of the social events was to go off to the mountains and climb a peak somewhere. We did sort of rock climbing, although not fanatical rock climbing. By that I mean sixth-class climbing up cliffs. We used ropes and we climbed slightly difficult peaks. Dick Lemmon participated in that and a number of other people. I think this was a social occasion., I think Melvin Calvin always looked a little askance at this; he was afraid that we would get hurt and he would lose somebody that he needed in the lab. He was always relieved when we came back and dragged in on Monday morning, all still intact.

VM: Presumably you were talking path of carbon all the way up and all the way back.

AB: No. (*Great laughter*)

VM: Did people talk about this sort of stuff when they left the lab.? I seem to remember that we talked about almost nothing else.

AB: I don't know. Perhaps you talked about it more than I did. We were talking about it much outside the lab.

VM: Maybe you had more of it than I did. What about working all hours of the day and night: were you one of those who did that?

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AB: When the occasion required it. One of the things about Andy is that he always seemed to get going on the big experiments, in those early days, about four o'clock in the afternoon. This was somewhat to the consternation of some of the rest of us. That meant if we were doing a study with algae and we killed them about four in the afternoon we had to get them worked up and concentrated and extracted and reduced to something that we could put on a paper chromatogram and the chromatogram started. Sometimes it would be pretty late at night before we got out of the laboratory. We could only leave when we had the chromatographic box closed up, the solvent in and things running.

VM: Were there lots of experiments where the concept required many people to collaborate very closely?

AB: Yes, well, I don't know what you mean by many. It required teams. If you were going to do the kind of thing I just mentioned, where you harvest algae, centrifuge them down, resuspend them and do all these things, you really need several people to do it — draw the samples, control the stopcocks, do the timing, killing, and so forth. There was a stage where we usually had a small team of people working together, not a great many people, but three or four maybe.

VM: Was the pattern generally of the scientific work in the lab. one in which many people worked on more than one major project, some of them they would do by themselves and some they would do in association with other people?

AB: Yes, I think that's a fair statement.

VM: You found yourself doing that sort of thing as well?

AB: Yes.

VM: About the events which led up to the new building eventually. I guess the reason why ORL had to go was because the Chemistry Department wanted to build a new building.

AB: That's right. The Chemistry Department was housed in a very old building, as I mentioned earlier on, a brick building, and it was really quite inadequate for modern chemistry courses. It needed to be replaced. Even before that they needed to find some more space. This could be done right next to the building in the place where we were in which was itself a "temporary" building, about 50 years old, was located. It came down to the point where we simply had to get out of that building (*i.e.*, ORL) and with much consternation, because we had had a very productive time there and we hated to leave it, we hated to give up the shops that I mentioned, all those things were terrible to lose. The best that the University could do for us at the time was to find us temporary quarters in the Life Science Building, in the bottom floor of the Life Science Building. They were quite nice laboratories but they were in no way comparable in terms of what we were doing to where we had been. But, we made the

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best of it and we went down there. I guess in the meantime Melvin...I don't know when he got his Nobel Prize relative to...

VM: '61.

AB: ...relative to the move that we made; you'll have to look that up somewhere. His doing that (*i.e., getting the Prize*) made it more amenable for us to get funds from various sources to build a the new building. We almost right away started to develop plans, I guess, for the new building.

VM: When the first move to LSB took place, what were you expecting would ultimately happen? Did you expect to get space in the new chemistry building that was being built?

AB: No, I don't remember that being the case. Maybe it was. If so, it would have been probably insufficient space, in our view. Again, Marilyn or someone may be able to tell you better about this. I don't remember specifically.

VM: It must have been a time, then, of considerable uncertainty...

AB: It was.

VM: ...partly because you didn't know how long you would have to stay in this space and what the outcome would be. And, of course, there was the separation from the people still in Donner.

AB: That's right. That was very sad, too. They were much further away. I am sure that almost immediately Melvin was hoping to somehow find a way to get us all back together in adequate space somewhere. I'm sure he was beginning to talk to all kinds of people, potential donors and agencies, that might be able to help us find a building. I'm afraid I'm not going to be able to help you much on the details of that.

VM: During that four or five years in LSB, how did you find the atmosphere of the group, particularly the photosynthesis group, because that was all that was down there? How did it change? How did it compare with what it had been in ORL?

AB: There are two things to keep in mind here. One is the transfer into less desirable quarters and the other thing is, of course, that the excitement was mostly over by then in the photosynthesis field and, in fact, Melvin was thinking of other things by and large because he saw more opportunities in other types of research. So, I was kind of a hold-over who continued to work on plant physiology. It wasn't ever as exciting a time as was during the mapping of the path of carbon in photosynthesis. That kind of excitement left. That's not to say, though, that some of Melvin's ideas didn't stimulate other excitements in the laboratory but it wasn't the same thing as it had been in ORL. But he, I think, always hoped somehow to regenerate that kind of an atmosphere by having a big laboratory that would serve some of the same functions as the original space had done. He could see that it wasn't happening so much down in the Life Science Building.

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VM: I suppose one of the things was that there wasn't so much of a single focus.

AB: That's right. As I said — we no longer focused specifically on the (*path of carbon*).

VM: When it came to the design of the new building, I remember, I'm not sure whether I am correct, that it was your idea to have a semi-circle on a block.

AB: I don't think that was my idea. It might have been Melvin's. I'm not sure whose idea it really was. What I do know is that when we kind of proposed that, the architects were sort of aghast and then they came up with the idea of making something architecturally more appealing by making the building a complete circle.

VM: I think they thought a semi-circle/block combination was too fussy for a building of that size.

AB: I think that's right.

VM: So, this building took several years before it was authorised and then before it was built. Do you think that the way it worked out was a good realisation of the sort of philosophy that everybody had felt underlay the ORL situation?

AB: I think it was. But we were faced with the fact that the group was growing all the time and with the fact that there were more (*scientific*) interests coming in and that, ultimately, the University would be introducing different team leaders in the form of more junior professors with their own ideas. To my mind, it was going to inevitably become impossible to maintain the kind of spirit that we had originally with a small group. You just can't take a small group and really expand it to a large laboratory and quite maintain (*that type of spirit*). Although I think we went fair a fair distance in that direction in the early years in the new lab. It gradually kind of dissipated as time went by.

VM: I guess that one of the things that happened was that the senior staff people were more advanced, more developed in their own careers, and each one of them had collected a group of some size.

AB: They had to be. If you were going to continue to command support and respect in the scientific community, you couldn't go on just being an assistant to Melvin Calvin your whole life. All of us, at one point or another in our careers, had to make a decision as to whether we even wanted to stay in Berkeley or go somewhere else and teach or do some other job. I am sure everybody who worked there had offers of other things. People stayed for various reasons. Of course, they love the area, but one important reason was that the new lab. would offer them opportunities to have groups of their own and students of their own and carry on something scientifically meaningful under their direction. That was necessary, I think, if people were going to stay.

VM: And, in fact, you were able to do that right the way through until you retired.

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AB: Pretty much so, although after Melvin retired it was quite a changed scene. But I still had good graduate students and things to interest me right up until the time I retired.

VM: Al, I think that's a marvellous collection of reminiscences so far. As you know, we are going down to Southern California and to other places and see some of our former colleagues next week and when we come back perhaps we'll have the opportunity of bringing things up for discussion again. Thanks a lot.