

Chapter 12

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La Jolla, California

June 4, 1996

VM = Vivian Moses; AB = Andy Benson; SM = Sheila Moses

VM: This is a conversation with Andy Benson in La Jolla on Tuesday, the 4th of June, 1996.

Andy, can we start from the very beginning, before you ever got to Berkeley because when we were talking to Martin Kamen last week he said that you worked with him on photosynthesis long before the Calvin group.

AB: What you are saying is pure typical of you and Melvin and most people who are interested in photosynthesis. None of them realised what went on before 1946 and it's a darn shame. Most of it's my fault and also bad luck...Here's a (*newspaper copy*) of the story of the disaster that happened just after I left Berkeley; that was in the summer of 1943.

VM: And this is Sam Ruben's accident?

AB: Yeah. You will understand it because you know the chemistry.

VM: What happened with you personally? How were you involved with Sam and Martin? What did you do?

AB: That's an interesting story...

VM: Please tell us.

AB: ...and you'd better know it. I came to Berkeley — my father took me over there to see what was with chemistry — and so we were ushered into the office of Wendell Latimer. You remember that name?

VM: I remember the name and he has a Hall, of course.

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AB: He died so they could make a Hall out of the space. He was a brilliant man and extremely influential in American chemistry. (*Telephone bell...*) I was interested in chemistry; that was 1934.

VM: What stage in your life was that?

AB: I had just graduated from high school. So, I arrived in Berkeley in September of '35 and enrolled in chemistry. As I was explaining here earlier, the first step in chemistry was to give an examination for all the six or seven hundred people who signed up for Chem. 1A. I think that was a good system. They would grade all these people and put the top 20 in Hildebrand's class and the next 20 in, maybe, Seaborg's class or Libby's or somebody else and they went through the whole faculty of chemistry in the lab. sections of Chem. 1A. The first semester I had Hildebrand once or twice a week for half an hour or so. But it was a great group of people and they were the top guys anyplace. The second semester I had Latimer and Latimer's son was also in one of these (*sections*).

VM: These were lab. classes?

AB: Yes. There would be 20 students and a professor on an informal basis. It was fantastic. My advisor, as a freshman, was Ronald Olson who was a professor of anthropology. He didn't know chemistry from nothing but that didn't make any difference. I became interested in anthropology and I took a few courses. The Anthro. Building, as you may remember, was right next to ORL. For some reason or other Olson was the expert in the department on the Indians of the Pacific Northwest. Since 1968 we (*Gerard Milhaud, myself and other collaborators*) have been working with spawning salmon in British Columbia and our headquarters is in a little Indian village of Alert Bay (*on Vancouver Island*) with just the Indians who made Frans Boas famous as the founder of modern anthropology. So it was just fortuitous and a typical example of the advantages of Berkeley for a simple student at that time. There was just no problem of communication between students and the top guys in any field. So I appreciated that immensely.

Just this last year, in 1995, there were three students from La Jolla High School who were named the top three in California in the Westinghouse science talent search and one of them was named Frans Boas.

VM: Was he related?

AB: Almost. He must have been a great, great grandson, or something like that and he didn't know much about these Indians and he had never been up there. Last year I took him up there and he got to meet the people who knew all about Frans Boas. Then he toddled off to Harvard to start his college work. He was quite thrilled with the experience, too. This was typical of Berkeley; it stimulated a lot of interest.

VM: How big was the freshman chemistry class when you were in it?

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AB: Six hundred and fifty or something. It was not small. When I graduated, I applied to different schools for teaching assistants and I was accepted in about three places: Johns Hopkins, Princeton and Cal Tech. Naturally, I went to Cal Tech. I didn't know why I was accepted at Cal Tech but immediately you fell in with the top guys in science, today actually. One of them was one of the top mountain climbers of that period, climbing K2 in the Himalayas and things like that, and the other was a top ski guy on Mt. Rainier. Gee whiz, you really have exposure to that kind of stuff that later was important with Bert (*Tolbert*) and Dick (*Lemmon*) and Hans Ostwald in ORL. I did a lot of climbing in Yosemite and the Sierras, everywhere, rock climbing and ski camping.

VM: You went to Cal Tech with a bachelor's degree, not with a Ph.D.?

AB: The real smart people at Cal Tech are the undergraduates; the graduate students are second class. The undergraduates are all hand-picked by Cal Tech faculty. They cruise around the country and interview every applicant personally and that determines who is selected as undergraduates. It gives a pretty high class, energetic group of students. The exposure to these students as a TA (*teaching assistant*) at Cal Tech was a good experience, to realise how good people are naturally.

VM: Which year did you go? You said you went to Berkeley in '34.

AB: I graduated in '39. I went to Cal Tech in '39 on a hot as hell day, it was 106 or something like that, and we had to take the prelim. exam in chemistry which was, as you can imagine, pretty brutal. I didn't study physical chem. because I figured I knew more physical chem. than they did and I passed it but I think Pauling came over to me and said "Yes, you did all right but not good enough for somebody from Berkeley". They made me take it a second time. Next time I cracked the books so it was all right.

I did my thesis in synthetic organic, the study of the structure of sphingosine which was not established at that time. My professor, Carl Nieman, was interested in that and he had four people in our laboratory working on sphingosine structure. I went to work on the relationship of the position of the two hydroxyls in the amino group with periodate. By the time my thesis was written I knew a lot about periodate oxidation. , And as you are aware, that was instrumental in the way Al and I succeeded in degrading compounds. (You can close that door, if you want.) That paid off. The rest of it (*the research work for my PhD*) was synthesis of thyroxine analogues. I knew a lot about fluorine chemistry, through the fluorinated thyroxines. When I later came to Berkeley, Melvin was concerned with fluorinating TTA (trithenon...ketone (?)). I had something to do with that; it nothing to do with any photosynthesis.

VM: So all this time you really had no contact with biological concepts, it was all chemistry?

AB: My minor was in animal physiology and my first paper was in neurosciences.

VM: Where was that?

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AB: Cal Tech.

VM: When did you finish at...?

AB: I finished in '42. I did my work at the Cal Tech marine station for some ungodly reason on peripheral inhibition in the scallop muscle. My teacher, who was C.H.E. Wiersma, who was one of the founders of that kind of neurosciences, this was a great experience for me. It makes a nice calling card around here (*Scripps*) where there's a lot of neuroscience types because Wiersma's a sort of god in that business. Anyway, I came up with my thesis exam, PhD, and made a little story about the synthesis and all that and the faculty is free to ask idiot questions. They started asking me about the equation for radioactive decay — had nothing to do with my thesis.

VM: You hadn't used any radioactive materials in it?

AB: No, not in the least, and I didn't know why. I suspect Pauling asked the question. Then, years later, it dawned on me what was going on. Latimer had sent me to Cal Tech and I did all right, and then Pauling, who I was on good terms with; I took his courses. One evening I was up in the lab. with either a five-litre flask full of boiling methanol sitting on the edge of a crockery sink, which was pretty hazardous, and in walks Pauling to tell me that I didn't too well in the quantum mechanics exam and I'd better stick with organic. That didn't bother Pauling at all. Then I realised that these questions about radioactive decay had to do with Latimer's request to Pauling for somebody to be on the Berkeley faculty who knew some organic who would collaborate with Sam Ruben. I didn't realise this at the time or until a few years ago. This was a planned operation between these two guys who had very high respect for each other. Don Yost at Cal Tech was more of Latimer's type of chemist but Pauling had been in Berkeley as a student or a postdoc. with Lewis and so it was understandable.

That's how I showed up (*in Berkeley again*). And it was sort of presumed that...they gave me a lab./office about the size of this room in the Rat House.

VM: In the Rat House! Which was the Rat House?

AB: You don't know the Rat House? Where have you been?

VM: I'm not sure I remember which the Rat House was. Which was the Rat House?

AB: The Rat House was probably erased...was Lewis Hall built when you were there?

VM: Lewis is still there.

AB: I know. Was it there when you were there?

VM: As far as I remember, but I'm not sure.

AB: Before Lewis Hall was the Rat House.

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VM: It was an old building, I take it?

AB: it was done in 1915 and was originally a shingle-covered building. It was very well constructed by modern standards, all wood and totally unadorned, and it had, I think, two classrooms. Sam Ruben had a lab. downstairs and an office upstairs; I don't think he was ever in his office. I had this little lab./office where (*indecipherable*) and all of the carbon-11 experiments were downstairs where the counters were.

VM: Had Latimer offered you a job at that point?

AB: Hildebrand did. He was the Dean at the time I was offered the job, at \$2200 a year.

VM: That was '42, '43, something like that?

AB: '42.

VM: Was it explicit that you were going to work with Sam?

AB: No

VM: They left it up to you to find each other?

AB: Yes. I don't know who assigned me the space. But that was fine because Sam was the most wonderful guy in the world.

VM: What sort of job did they say they offered you? A job to do what?

AB: I was in instructor; I was the low man on the totem pole in the department. The term "instructor" doesn't have any connotation; nowadays it's very impressive. But Bill Gwinn and other people that (I don't know any that you know); Dauben came later and most of the guys came. I'm trying to think of any that came with me as instructors; there were about three.

VM: And you had to teach the students, of course?

AB: Yeah; I taught organic. Do you remember Randall, of Lewis and Randall thermodynamics? Randall had distilled too much mercury and it affected his brain and he was a little bit strange, but he had been a good collaborator with Lewis. He was teaching Chem. 105, (*indecipherable*) inorganic. Latimer called me in one day and he said that the students were rebelling because Randall insisted that they buy his book on thermodynamics which had nothing to do with (*indecipherable*) inorganic. Would I please take over Randall's lab. section and try to teach the poor kids something.

VM: You hadn't written a book at that stage.

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AB: I got along fine with the students. Then I taught Chem. 101, advanced organic synthesis and the freshman lab. Of course, being just an instructor, I had a lab. section of students from the lower categories of 20 students. Most of them were not too great students but two of them became very impressive professors.

VM: Who were they?

AB: One was (*indecipherable*) Moss who worked with Martin Kamen at Washington University and she and Martin were the first ones to discover nitrogen fixation in photosynthetic bacteria. The other one was — his name doesn't come to me — but he was a very well-liked and distinguished professor of plant biochemistry at Davis and he just retired.

VM: Plant biochemistry? Wasn't Paul Stumpf?

AB: No, no. (My computer will take a little while to line up and I'll give you his name.) He worked mostly on anthocyanins and that kind of chemistry but he was very good as a student, he got an A in my section. I've still got my class book so I know who was there.

VM: Did you know about Sam before you went to Berkeley?

AB: I knew about the discovery of C^{14} .

VM: Did you know about the C^{11} and the photosynthesis work?

AB: Not much, I was an organiker but it didn't take me long to learn.

VM: When C^{14} was discovered, was that of great interest at the time? It was before my time.

AB: Of course.

VM: Was it well publicised at the time?

AB: Yes. There was a big article in *Life Magazine* about what they were doing. The real original publication was certainly a breakthrough, a breakthrough that Lawrence had sought for quite a few years. Martin has explained it well in his book. It was uncertain whether it would be a long-lived thing or not, but in the lecture that Martin gave here (which was not too well thought of because it was rather disjointed) he did draw on the board a diagram of the synthesis of C^{14} by neutron capture by nitrogen. I don't remember a discussion of that in any of the papers describing the discovery of C^{14} . I could get Martin to enlarge on that before it is too late.

SM: This was a recent lecture, was it?

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AB: Yes. Murray (*Goodman*) was there and he could give you some feeling that the audience, and Martin too, were quite disappointed in the lecture.

VM: Was Martin still in Berkeley at that time, when you got there? Working with Sam?

AB: Yes. We used to have get togethers in planning experiments. Martin and I and Sam would be in this classroom for 40 students, scribbling on the board. I didn't have much to say because I didn't know much about it. They pulled no punches by telling each other they were absolute idiots, or this idea was stupid, or something like that. But they were very, very good friends and respected each other.

VM: Was it just the three of you working in this area together?

AB: We were the only faculty members who... There were some students, Charlie Rice and Mary Belle Allen, you remember that name?

VM: Oh yes.

AB: Mary Belle, she was a graduate student in chemistry and they finally didn't accept her, something like that, I don't remember, and Sam used to call her "Madame Curie" and she didn't like that too well. She was finishing off some of the microbial work that had been started and I don't remember what she did but she published some stuff. You asked me a question.

VM: Well, the last one was whether Martin was still there?

AB: Oh yes, he was still there, but...I remember I and my wife had been at the Piggly Wiggly grocery store and we (*met*) Martin and he had been kicked out of the lab. as an insecurity risk. He was working for the local shipyard in Richmond, checking the correctness of some kind of welding or something like that.

SM: When was that?

AB: '42.

VM: Had this mood developed rapidly once the US entered the war?

AB: It was a result of the un-American activities nonsense.

VM: That was already active in '42?

AB: Yes. He had been accused of transferring some radiation secrets to the Russians. Actually, he was giving them some music manuscripts of some quartets or something like that (Martin could tell you and [*indecipherable*] in his book). I don't know where my copy is; I bought about six copies of his book (*Radiant Science, Dark Politics*) but now they're all gone and I can't find one. You can buy it in a bookstore here. If you don't have one you should...absolutely. Because it said a lot of things about Miss Kittredge.

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VM: (Could you not cover the mike, I don't know how well it...don't put your hand over it, I'm not sure.) Miss Kittredge?

AB: You don't know who Miss Kittredge was?

VM: No.

AB: You could find out in Martin's book. She was the secretary to the Dean (of the College of Chemistry). The Dean was usually Latimer but it had been Lewis. Lewis wasn't doing any teaching; he just ran the seminar on Thursday afternoon, then he'd pursue his (*indecipherable*). She wrote all the letters. These guys were too busy being scientists and they signed the letters, which was usually OK. But Miss Kittredge didn't like Sam too well, I don't know why, and everybody was afraid of her because she didn't have a sense of humour and she was really loyal to the Dean and to the College of Chemistry but she managed the lives of students (*and faculty*) as a consequence.

VM: So, when Martin left the lab., you continued working with Sam without him, presumably?

AB: Martin was still in town and he was welcome to come to the lab. but probably he was no longer affiliated with the Radiation Laboratory.

VM: He was actually working literally in the lab.?

AB: His work was always with the cyclotron. It was in ORL; you know where the white table was?

VM: Yes.

AB: That's where the (*37-inch*) cyclotron was. It was there when we moved in. The main lab. where Murray worked, and me and Al and everybody, was just a grungy place covered with yellow powder on the floor, uranium salts. So we just had a cheap linoleum pasted on top of it.

VM: On top of the uranium salts?

AB: Yeah.

VM: It (*the radiation*) doesn't come through.

AB: No, it wouldn't come through. If you destroy a building like that now, someone would really scream, for no good reason, of course!

VM: Within about a year or so Sam was killed.

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AB: Yeah, it took a little over a year which was a darn shame. He worked like hell, doing all his teaching and then Sam would start working (*in the lab.*) at 7:30 in the morning and go home at 2:00 a.m. I worked with Sam all the time. I hardly ever saw my wife, and for a newly-wed girl with no great background (*in science*), that was a miserable sentence, it was not good.

VM: What did you do when Sam died?

AB: I was not there.

VM: You were not there?

AB: No. If you read that (*Kamen's book?*), you'll understand it.

VM: So you weren't in Berkeley either?

AB: Not when Sam died, no. When I was at Cal Tech I had many meetings with Bob Emerson; do you know that name?

VM: Vaguely.

AB: The Emerson drop!

VM: Vaguely.

AB: It was a drop in the absorption (*indecipherable*) well under the spectrum. Emerson was the one who with that information led to the two light reactions in photosynthesis. Emerson was in Cal Tech, just a lovely guy, and I heard him give a seminar on this stuff. Of course, I didn't realise how monumental a discovery it was. Emerson was a very thoughtful and gentle guy, not like Sam. British — was he British? He was a descendant of Ralph Waldo Emerson, and he had a brother who was a professor in Berkeley. Bob Emerson became the guru, or leader, of a small group of us who felt that we should be conscientious objectors during the war and he was very supportive and helpful. It wasn't too long before Emerson was killed in the crash of a British turboprop...

VM: When was that?

AB: ...with the round, with the oval windows.

VM: Oh. That was the Comet; that was a jet.

AB: That was a jet?

VM: Yes, that was a jet.

AB: What do you call it?

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VM: Comet.

SM: De Haviland Comet.

VM: That would have been in the mid-fifties, I think; can't remember exactly.

AB: Mid-fifties?

VM: It fell out of the sky because they cracked around the window frames.

AB: I didn't have any contact with him after I left Cal Tech.

VM: So you went back to Cal Tech from Berkeley at that time?

AB: You're getting ahead of us. So I was in Berkeley with Sam Ruben and this trouble with the draft boards and everything, they were after me because they were refusing me the proper 4F status which I could have succeeded had I expressed the willingness to work on defence contract.

VM: 4F was some protected category, specialist category?

AB: 4F was probably conscientious objector. I don't remember which is which but I could look it up.

VM: But you could have got exemption on specialist grounds?

AB: Everybody else was working on (*defence*) projects. By March '43, Sam was overloaded with his research project, defence research project described there. Latimer was the manager of all of this interaction with government research organisations. Sam was working on the movement of heavy gases, gas clouds. Meteorology and...Sporadically we would do carbon-11 experiments but not so many. Sam gave me all of the first C¹⁴-barium carbonate that they had made.

VM: The C¹¹ was made in the cyclotron? How did they make it?

AB: Yes, from boron.

VM: You didn't have much time to use it?

AB: We did maybe a dozen or two dozen carbon-11 experiments. I made phosgene with carbon-11; destroyed it...

VM: But you had not much time before the radiation decayed away?

AB: When you did an experiment?

VM: Yes.

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AB: You could work for about two hours...

VM: ...from the time we got it out of the cyclotron to the time you had to have everything counted?

AB: About five half-lives so five times twenty is 200 minutes (*Editor: sic!*).

VM: What were you able to do in photosynthesis in that sort of time period?

AB: Well, we did an awful lot. That's what Sam's papers with Hassid (*W. Z. Hassid, Biochemistry Department in Berkeley*) and Martin Kamen were about, trying to isolate the products. But they couldn't do any separations, the kind that we normally have done. It was all precipitations and you get co-precipitation and adsorption problems that they never recovered from.

VM: Particularly, I guess, if you're trying to work with...

AB: Later on, when we had the big polemic with James Franck and all those idiots, physicists, in the midwest, Martin was advising them from Washington University, St. Louis, where he was doing very good work but he was giving them the advice based on what he and Sam had done five years, six years before. That was not good advice. It was just unfortunate that he didn't have any of the organic experience that I did.

VM: There were usable amounts of C^{14} available at that time?

AB: Yes. There wasn't very much and Sam gave me the lot.

VM: What did you do with it?

AB: We were looking for the path of carbon in photosynthesis. We never used that terminology but it was exactly what it was. The point of view which Sam and Martin had developed was that the product of photosynthesis is the addition of CO_2 to an acceptor to make a carboxylic acid. That is what it turned out to be but there are all kinds of ways that he could be misled by that. They figured it was not a photochemical reaction but an organic chemical reaction. Therefore, it would happen in the dark. So they were doing dark fixation of carbon-14 dioxide and they did a lot of experiments and started isolating the product. The isolation was based upon the partition coefficient between water and ether, or water and ethyl acetate, of the radioactive product. I altered it by making the methyl ester with diazomethane. This little office I had with a lab., didn't have any hood, it had an open window so you could open that up. I made concentrated diazomethane to make sure the thing got methylated and then measured the partition coefficient and that would tell you it was carboxylic acid for sure. Sam had never done any of that kind of chemistry. That's what I was doing.

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VM: How much C^{14} did you actually have, did you remember? Was it millicuries? It wasn't curies...

AB: No; it was millimicrocuries.

VM: Oh; that small?

AB: Yes. If you will let me get...How far can I go with this cord (*i.e. the microphone lead*)?

VM: You can go quite some way with this cord.

(*Benson retrieves sealed vessels*)

VM: You don't have some of the original stuff still there?

AB: Oh yeah.

VM: You have! Good heavens!

AB: Here's some tritium water from 1939! I don't have any carbon-14.

VM: I'm not going to smell this!

AB: Well, don't open it. This is tritium water: 1.37×10^8 counts per minute per mole.

VM: That's been there for 50-odd years.

AB: Yes.

VM: So you're down about four half-lives for the tritium.

AB: This is irradiated H_2O^{18} ; it's now dry — just leaked out, evaporated out, dried. That was the result of some experiment that they did. I don't have any C^{14} that I know about. We used it all and re-used it; you know, you catch it in alkali, in barium hydroxide.

VM: It was very precious at the time.

AB: That was all there was and they weren't making any more.

VM: What did you find out? I don't remember the early papers on photosynthesis?

AB: I found out that the partition coefficient of CO_2 fixation was, I think, 0.14, or something like that. That is about as far as I went. I wrote it up. By that time I was in the mountains, in the Sierras, in this civilian public service camp of conscientious objectors working for the Forest Service. It was nice place to be, totally isolated, we

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were fighting fires and making roads and things like that. I had this manuscript to send to JACS and I just never did. I got a nice letter from Sam (in here some place) in September 1943.

VM: Just before he died.

AB: Ten days before he died.

It was a very cordial letter, hoping that I was OK and that the family was OK and all that. He was saying what he was doing. He worked up by Mt. Shasta, they had some experimental site. I had visited the Dugway Proving Ground in Utah (you know about that?) and the veterinarian there got him interested in phosgene toxicity. So that's what they were working on. I had also done a lot of other experiments with radiosulphur and carbon-11 on the mechanisms of organic reactions, with Sam and his collaborators. One had to do with (*indecipherable*) green and methylene blue reaction mechanisms in the phosgene project. Various professors in Chemistry and Sam had been recruited to advise their students on how to solve these things and I was helping with that.

VM: While you were up in the mountains?

AB: Oh no, that was before I left.

VM: So during the period you were in...

AB: While I was up in the mountains I had this manuscript I was going to send in but I was too busy and it didn't seem like an important thing to do. As I look back on it, I should have sent it in. It would have made all the difference in the world for my future.

VM: During that period of what: two years, three years you were up there?

AB: Yeah, about a year and a half. Then I was transferred to Stanford University to work on antimalarial drugs where I learned a lot, too. After that, I was transferred to Cal Tech which was the headquarters of the antimalarial project, that was 1945-46. I went from Cal Tech to ORL, to Calvin's group in '46.

VM: How did this happen?

AB: Ernest Lawrence was always interested in photosynthesis for some reason or other. He and Sam and Martin spent a lot of effort making carbon-11 and looking at CO₂ fixation. It was one of the things that Ernest thought was important.

VM: And you knew Ernest and he knew you?

AB: Oh yes.

VM: Did you know Calvin from the days in Berkeley?

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AB: I knew Calvin from the day he came as a postdoc. with Lewis; that would have been in 1937. (*Editor: Calvin came to Berkeley as an instructor; he was never a post doc. there.*). I knew Calvin because he sat over there in the seminar room and I sat back with...the students weren't even allowed in there so I would just sneak in on Thursday at four o'clock and he (*Lewis?*) didn't kick us out, we just had to shut up. The faculty sat around the table down at the bottom with Lewis with his big cigar at one end and as soon as the cigar got fired up, that was the beginning of the seminar.

VM: Tell me, did Calvin used to interrupt seminars in those days, the way he always did in our group?

AB: Bloody right. That was the prime memory I have of Melvin in '37 and '38 was the fact that he was a real good questioner. He had the best questions after every seminar on almost any topic. He was a master at that. You have to give him credit for that. Perhaps that's why he was so successful with Dow Chemical. You know he was associated with Dow Chemical.

VM: I knew that Calvin was a consultant for Dow.

AB: You are going to have to unravel this story. I was working for the Forest Service and fighting fires and working with aerial photogrammetry (mapping) — a lot of field work in the mountains of California and Nevada. Then I was transferred to the chemistry department at Stanford. There were two other postdocs working on the antimalarial project and both of them were outstanding chemists. The professor was student of Franklin's who invented ammonochemistry. Are you familiar with that?

VM: I'm not an organic chemist.

AB: Anyway. This guy was F.W. Bergström.

VM: What was his name?

AB: Bergström. All the reactions were done in liquid ammonia and this was all ammonochemistry. That was fun. One of the guys working with me (*at Stanford*) was Ted Norton (T. R. Norton) and he was an outstanding organic chemist from Northwestern. After the war he joined Dow Chemical in...what was that place that Dow had a lab.? And then he became almost a Vice-President or a top operator in Kalamazoo.

VM: Was it in Concord, Walnut Creek or something like that?

AB: Concord (*Editor: actually it was in Pittsburg*), and eventually at the Dow headquarters in Midland, Michigan, that's must have been where it was. It turned out that Ted had been the one who hired Melvin for Dow Chemical. Ted is or was living in Honolulu; I had a big talk with him a couple of months back. He had a miserable proliferation of cancer in the hips and everything; it's just awful. But he was telling

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me about his experiences in managing Melvin for Dow. He was impressed with a lot of it. He understood the picture pretty well.

VM: So, you went up to Berkeley in 1946?

AB: Yes. The war was over and Ernest decided they had to get back to photosynthesis. Martin had gone to Washington University with his good colleague who died of cancer.. So he (*Lawrence*) sought around in the Chemistry Department for some organiker who would carry on (*the photosynthesis work*) and Melvin was it. They put Melvin Calvin in their (*Rad. Lab.*) budget and that made it possible for Melvin to invite me to come to Berkeley to start the photosynthesis lab. Bert (*Tolbert*) had already been there and had been working on medical aspects of tracers. And that is described in...I'm sure you know about that. This had to do with John Lawrence's interest in chemotherapy and the mechanisms of all kinds of things.

VM: Melvin describes that in the oral history he did about 1980 which we have.

AB: I read some of that. and we'll discuss that later when this machine is not on.

VM: We've got a couple of minutes to go on this side (*of the tape*).

AB: Some of that is not correct; Melvin's memory is sort of clouded by what he would like to think rather than what it really was.

VM: Well, we're certainly interested to hear what your view of those events is.

AB: Melvin got me there and they said this is where our lab. is, and I designed the lab. and ordered all the pieces.

VM: So you were there at the beginning of ORL before ORL was used for that photosynthesis lab.?

AB: I (*indecipherable*) out of that big room, alongside where the cyclotron was. There was a little lab. there.

VM: Had the cyclotron been taken out ?

AB: No, it was there. I was scheduled to be moved down to UCLA.

VM: I see. So you started to build a photosynthesis lab. in the building while the cyclotron was still there?

AB: Oh yes, I think it was still in use but the 60-inch was already functional.

VM: The 60 was in Crocker (*Lab.*), was it, and the 37-inch was in ORL?

AB: Yes.

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VM: What did you plan to do, as it were? What was in your mind as to the way you were going to develop the project?

AB: I was continuing what I already started; I didn't have any more imagination than that. I started working like a (*indecipherable*). As soon as we got any kind of laboratory we were thinking of (*indecipherable*) every morning.

VM: What? Melvin, perhaps?

AB: No, no, no: Ed McMillan.

VM: Ed McMillan?

AB: Yeah. And I have realised that as far as chemistry was, I had been isolating stuff that I got with the partitioning and I got down to stuff to crystallise so I was trying to recrystallise it to constant specific activity and Ed would come in to kibbitz with me every morning. Little did I know that in the afternoon he was isolating neptunium.

VM: By this time you already had sizeable amounts of C^{14} , presumably, from Radiation Lab. sources, did you, or were you still working with these tiny quantities?

AB: At that time it came from Oak Ridge.

VM: In reasonable amounts?

AB: Yes, we had plenty.

VM: Did you have prior access — did other people have access?

AB: No.

VM: You had all of it, or most of it?

AB: I'm not sure of that. Probably other national labs. had had equivalent access.

(Tape turned over)

VM: OK. So we were talking about in the lab. in the ORL at the very beginning. Who was there working with you; who started; who were the earliest people?

AB: At first I think I was working a little bit over in Donner with Bert and — was Dick Lemmon there? With — who wrote the book on radioactive carbon with Calvin?

VM: Heidelberger and Yankwich and Reid.

AB: Yeah, Jim Reid. Yes, he was there. Heidelberger came later and Pete — ah! One of Same Ruben's graduate students along with Mary Belle was Pete Yankwich so I knew Pete very well back in '43.

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VM: He's now in Washington.

AB: Yes; he's a big head honcho of American education.

VM: Yes. I'll try and get to see him...I'm not sure...

AB: You should; he's a very distinguished gentleman. One of the things you may not know is that Pete Yankwich's father was a federal district judge in southern California.

VM: I've never met Pete and I don't know anything about him at all.

AB: And Dick Lemmon's father was a federal district judge in northern California.

VM: That I did know. So, in the beginning you started off working in Donner presumably because ORL was not yet ready?

AB: We had to pick out the linoleum; it was sort of reddish stuff.

VM: So it was your job, among other things, to get the building ready?

AB: I've built many many labs. That's why I'm so horrified at the way the idiot lab. architects want to do things. They don't know from nothin'. A lot of the things about laboratories were devised in Germany 200 years ago and a lot of them haven't changed one bit, and they should. But neither the architects nor....one of our good friends is the head of a major lab. architectural firm in the world, he designed the Salk Institute labs. and he's been designing labs. for McMurdo (*Sound*) and the University of Riyadh — a million square fee of laboratory space (can you imagine that?) and NIH and every place. But those guys are human and not great.

VM: I guess you really weren't in a position to design a lab. You had a bit of an old building available and you did the best you could with it.

AB: I had had a lot of experience, built houses, I knew plumbing and wiring.

VM: Had you done that by that stage in your life?

AB: Yes. But Melvin had never lifted a finger, maybe a violin, but I doubt it! He didn't understand any of that. The people who knew were Bert and me. Or Bert and I?

VM: Bert and I: "Bert knew and I knew..."

SM: It's not very important!

AB: I've got to use the Queen's English here!

VM: That's all right: this is America!

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SM: She is always “we”; there’s no problem!

AB: Anyway, Bert and I were raised on farms and we understood how things went. I learned an awful lot of technology from Bert. He taught everybody. So you’ve got to give him all the credit you can in whatever you are doing.

VM: As you got into the building, who joined you, who were the first people to work in the building?

AB: Maybe (*Sam*) Aronoff was one of the first major ones that he (*Calvin*) brought in because they figured that Aronoff knew something about plants. My own interactions with Aronoff were on the edge of thorny. That wasn’t because he was good it was just personal. In the last few decades we have gotten along fine. I remember we had some technicians, Gordon Hall and Tom Goodale — you’ve seen that name?

VM: I’ve seen the name, don’t know the person.

AB: Tom was a character, sort of independent character, and he would look all over the place for something that was lost and finally it was on the shelf and he would say “Ah God: if it’d been a snake I would’ve bit it!”. Which means it was awful close.

VM: Do you know where he is now?

AB: No.

VM: I haven’t found him.

AB: Gordon Hall was working with a huge international contractor in Indonesia; he came by one time about 15 years ago. The same name as the people that manufacture water heaters. You don’t care, anyhow.

VM: In the beginning, you started chemical isolations, presumably, of photosynthetic products?

AB: Yeah. Then the concern was whether photosynthesis was truly a light reaction or a dark reaction because it became clear after I isolated crystalline succinic acid that that was what I was trying to find with Sam Ruben, and that’s just not very exciting but Melvin cooked up a cycle, the C4 cycle, so that was C4 photosynthesis but it didn’t have any value in terms of what we were really after. This was disappointing when you look back on it but I developed a lot of techniques.

VM: Were you beginning to degrade products at that stage?

AB: Then Al (*Bassham*) came as a graduate student and I put him to work degrading malic acid. Then Al developed a skill in degrading things so I had to give him the fructose first and but then later sedoheptulose and ribulose and glycolate.

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VM: Was the arrangement in the lab. that these students were formally Melvin's students?

AB: Yes. I wasn't on the faculty.

VM: You were a Radiation Lab. employee, presumably.

AB: Yes. The students were all Melvin's students but Melvin didn't have much to do with signing their projects, especially when Murray (*Goodman*) came — do you remember when Murray came?

VM: It must have been '51 or '52, but I haven't talked to Murray yet.

AB: I think it was earlier; You'd better nail it down. Anyway, Melvin had a coronary in '49, at the age of 37 (*Editor: actually 38*) — you know that?

VM: Yes, I knew that he had one.

AB: And his brother (*Editor: Calvin never had a brother*) and father had died at age 37, of coronaries.

VM: No, I didn't know that.

AB: It was a grim prospect; it scared the bejesus out of him...

SM: Luck he had...

AB: ...and Genevieve gave him the right kind of diet and it was lucky for Melvin that the guy in the next lab. (*Editor: in Donner, where the heart attack occurred*), and the guy who took care of him when he was struck with this coronary, was Jack Gofman. You know who that is?

VM: Yes.

AB: Jack was the only one in the world who knew anything about HDLs and LDLs at that time.

VM: Melvin was very fat then, wasn't he?

AB: Yes. He was built like Isaac Stern.

VM: And did he smoke as well?

AB: Constantly. Oh cripes — that was the worst part of working for that guy. It was just awful. He'd come in the morning when I was trying to write something up (I had a little desk in an office in ORL) and all around my chair were these cigarette butts, all over the floor. I didn't have enough sense to kick him in the rear end and tell him to get out of there.

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VM: He chain-smoked these things, did he?

AB: It was the pits. My father never smoked, he was a doctor and he wouldn't allow anybody to smoke when they came into his office. My dad was six foot six and he could tell people "don't" and they didn't. So I was always prejudiced against smokers and here was Melvin, smoking like a sieve, and he was a likeable guy and I didn't mind his personality too much, but he was a (*indecipherable*) so I had to clean up after him every day. And then after he had his coronary and got so scared that he quite smoking, he never apologised once for having stunk me up and cluttered the floor to pick up after him for several years. I'm still bitter about this.; you shouldn't put it on your damned...

VM: It's not terribly sensitive, I don't think. Were you there when Melvin had his coronary?

AB: Yeah, sure. It was at an AEC review committee were there and they had to present their budget and the reasons for their existence. That was held in the little seminar room at the end of Donner hallway. I don't remember the details of him grabbing himself because the next thing I knew they had him lying down on a table, or couch someplace, in a the room next to Gofman's lab.

VM: Was he making a presentation at the time?

AB: I don't remember whether he was making a presentation or whether it was just after or just before it but it was just then. So that was the beginning of what must have been several months, two months in the hospital to level him out and get the heart back. That was a real shock. We had agreed to write a review for *Annual Reviews of Plant Physiology* or was it *Biochemistry* (you can look it up) and there I was stuck clutching the burlap, having to write the whole review, which I did, while Melvin was incommunicado in the hospital for a long time.

VM: Was there concern about whether he would survive at all at the beginning?

AB: It was pretty serious in those days. He realised how serious it was. He was scared stiff and so was Genevieve but she solved his problem.

VM: So no more smoking and no more eating!

AB: He lost 50 pounds immediately and he looked almost the way he is now. But I do have a photo of him, I wish I could put my finger on it, before the heart attack. He's in swim trunks, wading into the water in New York at Cold Spring Harbor. You could tell what shape he was in, total pale. But it's a superb picture and it will surface sometime; I certainly put it some useful place but I don't know where it was. I've looked for it a couple of times unsuccessfully.

VM: When you set up this lab. in '46, by then, presumably, you were beginning to use ion exchange separations.

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AB: Not really. Ion exchange came a little later. That came by way of Dow Chemical. We wouldn't have known anything about ion exchange were it not for the fact that Melvin was consulting for Dow.

VM: Right at that early stage he was consulting?

AB: You can find out from Marilyn maybe when he started but I think it was certainly '48, maybe '49 that he started with Dow. Before that he had been working with Dow on his chelating business which may have started in '46, I don't know. He didn't communicate with me on any of this stuff. He would bring home resins later on from Dow.

VM: What were you using as a technology way back in the beginning of the ORL days?

AB: Just partition between organic solvents. I knew a lot about that from having taken chemistry in Berkeley. It was important for doing iodimetry kinetics and so forth.

VM: I remember the stories about the first isolation of PGA. You took that out, eventually, on a column didn't you? There is this story of Melvin in the red zone who suddenly realised what it was while the car was parked.

AB: That doesn't have anything to do with PGA. That had to do with the cyclic recarboxylate (*Editor: no doubt this means the primary CO₂ fixation reaction*). It had to do with the carboxydismutase involvement of ribulose diphosphate.

VM: That's what he dreamed up in the red zone when he was parked?

AB: I think so. But we had discussed dismutation reactions for ten years and this was nothing new. I was never very excited about that idea.

VM: Presumably this whole development of the carbon cycle came about interactively and in bits and pieces as you went along?

AB: Oh sure. We were in the dark, but Melvin never admitted that he was in the dark. He would publish papers and they were only half-truths. But one of his great attributes is that he never worries about being wrong. It doesn't bother him at all. He just goes ahead. You got my little paper on thioctic acid?

VM: I have got *your* paper on thioctic acid.

AB: Where have you been, Vivian? Make sure you get one. Anyway, Bob Buchanan, do you know him....

VM: We are going to see Bob Buchanan in a couple of weeks time.

AB: He's a top-notch, absolute top-notch. He is now the President of the (*American Society of Plant Physiology*). He decided that we should have a certain little page in the (*ASPP*) Newsletter of Plant Physiology dedicated to people writing... So he got

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me to write the first one, which you will get, and Melvin wrote the second one and Marty Gibbs wrote the third; I don't know who's on deck now.

VM: The Gibbs' one I've seen but I haven't seen yours and I haven't seen Melvin's.

AB: Melvin's was OK, but it's not anything you don't know. But the one I wrote about thioctic acid is something you know but it's from my point of view.

VM: Sure; I'd like to see that.

AB: I thought it was fun. Everyone seemed to enjoy the heck out of it and Buchanan said people in his class were really eating these things up.

VM: Sometime around then you had the introduction of paper chromatography.

AB: Yeah, Stepka came from Cornell. I don't think I knew much about paper chromatography until Bill came.

VM: Stepka had apparently been with Steward in Cornell.

AB: Steward had a postdoc. from England...what's his name?

VM: Dent.

AB: Dent and Stepka learned it all from Dent. As you well know, it was a very toxic kind of paper chromatography and didn't work very well with phosphate esters, especially with any salt. Maybe that's one of my major contributions in ORL was devising a functional paper chromatography method that works, and still works better than anything else.

VM: Can we think about that for a bit. You presumably were aware of the invention of paper chromatography, were you, in those early days...

AB: Oh yeah.

VM: ...but between you it didn't seem appropriate for you to use it at that time in photosynthesis. Why didn't you start using it, once you knew about it?

AB: We did. We started immediately.

VM: Before Stepka came?

AB: No, no, no.

VM: Stepka brought the idea to you?

AB: Yes.

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VM: So you weren't aware of the Consden (*Martin*) and Synge paper chromatography which came out in, I think in '44?

AB: No, I don't think so. But, as you well know, paper chromatography is just a matter of partition between water and organic solvents and that's what I had been doing. The partition column came out with Bob Holley's separating RNA, aminoacyl RNAs.

VM: When Stepka brought this around, when? '48 or so, do you reckon? How did Stepka come to the group?

AB: He was a graduate student, if I'm not mistaken, in Soil Science — trying to think.

VM: But not in Chemistry?

AB: You're going to see Stepka; he'll tell you.

VM: From your point of view, did you bring Stepka in or did Stepka find you? How did you make the contact?

AB: I think he found us. I don't know...you'll have to find out from Bill how he was engaged because he was not a student in our place. But he may have been a graduate student in Plant Nutrition. I'm pretty sure of that. He was working with Overbeek; he'll tell you. He knew the English literature, what there was, very well but he wasn't enough of an organic chemist to devise a novel method, which I did.

VM: When he came in, presumably it was new to you guys and you had no set up for it, no equipment for it and you had to build something. How did you start, glass jars or did you build wooden tanks at the beginning? How did you get going to the extent...?

AB: Stepka only knew glass jars but I wasn't going to put up with this nonsense. So I made it the way I thought it ought be and it worked pretty well.

VM: Presumably you tried it out in glass jars and convinced yourself it would have some value.

AB: At first we had the glass troughs, glass weight rods. But since we were in the Radiation Lab. and we could have anything built we wanted to of stainless steel, by the best guys in the country, I got troughs that were masterpieces and you still can't buy anything that good.

VM: Did you design those troughs and the sleeves and the stainless steel clips the hold the...?

AB: Sure. Of course. I designed everything. Talking about design: when I was working in my little office with Sam (*Ruben*), we had to spread the samples from the dark fixation so that we could count them, count the C^{14} (there might be 100 counts or 200 counts, or something like that) with some reliability. All I had at the time was Libby's screen-wall counters. The wall was a screen with holes, some of the beta (*particles*)

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could sneak in and get measured inside the Geiger counter volume. The way Sam had been doing it, he had sort of wrapped the thing on a paper or something, or cellophane or something around the screen and then sealed up the counter. I said heck, we've got to have ground joints. So I made such a counter with ground joints, the first one, that you could take apart and put the sample in. The sample I mounted in glass cylinders, about that long, just the length of the screen. The counter had to be twice that long so you could put the sample over here for the background and slide it over the wall for the counter. First you have to evacuate it, fill it with counting gas and then count the bloody thing, and to put the sample inside of a cylinder required inventing a sample roller. So I had two rollers that rolled the cylinder and with heat flowing through this thing and you would dry the sample inside the cylinder so it would be thin and uniform, and then you'd put it into the counter. That was the precursor of the disc thing that I designed, the rotating disc where you could blow and get a nice sample spread over a disc for counting a thin organic sample. Bert advised me on a lot of this.

VM: Bert had experience of course with the use of...

AB: Yes. They invented a little cup for getting barium carbonate. And Sam knew all about self-absorption of barium carbonate. I don't remember if I used...I must have made barium carbonate discs, too. Maybe I did that with Sam, I've forgotten.

VM: When it came to the two-dimensional paper chromatography, did you start out with two dimensions?

AB: Yes.

VM: There was that little frame thing that held the paper while you blew air onto the spot and ran the liquid out of a (*pipette*).

AB: I invented the little folded stainless thing, you just hold the paper, let the paper go where it wanted to go.

VM: For drying of the solvent or putting the sample on .

AB: Putting the sample on.

VM: You invented that little arrangement with the blowers top and bottom.

AB: I put just one blower. I'm the guy that put the big Bunsen burner in the blower mount so you could get real heat. I could run circles on anybody in boiling water or evaporating anything. I still can. To do all these things, both with Sam and at Cal Tech as an organiker, all the evaporations were done in vacuum with a water aspirator in the big flask. We didn't have rotary evaporators which are still not as good as what I was doing. With five minutes a litre for water, I could do pretty well. So I don't think anybody could beat me boiling water!

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VM: I remember those rotary evaporators were always giving trouble when you did it under high vac. or even on the water aspirator they would tend to bump.

AB: I figured all that out.

VM: How about those end-window Geiger counters that we used which had the mylar windows. How did they come about, the gas-flushing ones?

AB: I think Jim Reid and Bert had a lot to do with the design of those things. They had the thin mylar and so I guess they used what they had.

VM: This technique of using the radioautography: that must have come about the time you were introduced to chromatography.

AB: I did all that, yeah.

VM: Were you actually the inventor of that, I don't know what the history of radioautography was.

AB: I invented the drying racks and the boxes, you know you pull out the rack and you put all the papers in there, and they have to be sucked from the bottom or else they would flutter around, you can't dry them in the hood.

VM: Did you invent the concept of those big film radioautograms that went on the chromatogram?

AB: Yeah, sure.

VM: Had anybody used them before? I don't remember.

AB: I suppose.

VM: They were used for medical X-rays, weren't they, for chest X-rays?

AB: People before us would only have one-dimensional things and put a piece of film on the strip. But since I knew a lot about X-rays since I was that big...my dad had a portable X-ray machine so he could go to a farmhouse and make an X-ray of somebody who'd broken something and take it back to his office and develop it. This was a beautiful oak box with a Tesla coil inside, which weighed about 40 pounds, and you screw in a couple of things with brass balls and pull out copper wires and you hook this onto the X-ray tube which is held with a wooden oak clamp, and take the X-ray or else use the fluoroscope/fluorescent plate. I spent a lot of time watching my fingers with this unshielded X-ray tube.

VM: You remember when they had that stuff in shoe shops and you could look at your feet?

AB: I wish I had this thing. It would be worth a fortune now, I was just too busy...

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VM: The one that my father had?

AB: It had big glass tubes. So I'm the first one to use, I bet you, the 14 x 17 (*inch*) X-ray film (*for radioautograms*).

VM: I've seen some of your original chromatograms up in Berkeley.

AB: I still have thousands.

VM: You have a museum full of stuff here, have you?

AB: More or less. I refer to them once in a while. These films and chromatograms include all the compounds and all the information. There's no lab. notebook. I never kept a lab. notebook. Why keep a lab. notebook? You ought to have everything on the paper chromatogram, the details of the experiment, when it is and who done it. The fact that everything in the algal extract is on the paper chromatogram, nothing else, either at the origin (maybe it blew away), at least they are all there and most of them are separated from polysaccharides to phospholipids and triglycerides. It's a fantastic separation technique and nobody really recognises the fact that it is a gradient elution. The composition of the solvent system changes with how far it has gone. I published some stuff on this once and nobody really realised the pH changes that occur.

VM: In fact you have chromatography of the solvent itself as part of the process.

AB: Yes. Part of it evaporates and the aqueous part is adsorbed on the water (*Editor: this should be "on the paper"*) so the solvent is more organic as it goes. This had been recognised slightly by some people but not to the extent it should have been. It runs circles around most other chromatographic techniques in spite of HPLC.

VM: And you have stacks of the chromatograms still around the place?

AB: Yes.

VM: You still using it?

AB: Yes. I dig them up once in a while when we need something special.

VM: Do you use paper chromatography as an analytical technique?

AB: Oh yes.

VM: You are all set up to use it?

AB: I have not been using it the last six months but my boxes are down below by the lab. that I've got. This was my lab., but these other jokers moved into it and I've got the drying rack in the hood, and so forth. That's me. I'm just counting some C¹⁴-methanol. The first methanol that I used was made by Bert.

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VM: C¹⁴-methanol?

AB: Yes. I fed it to algae and they fixed the methanol into sucrose and every other normal intermediate so it was clear that methanol was readily metabolised by algae. When my colleague Arthur Nonamura, who worked with Al Bassham ten years ago, was trying to grow his alga which produced oil (isoprenoids) at the time when oil prices were high, he was trying to get these algae to grow faster,. And I suggested that he give them some methanol, they love it. I had done this with Bert's methanol a hundred years ago. Sure enough, the algae grew twice as fast. Then Arthur became a farmer outside of Phoenix with 1,000 acres of cotton, and he sprayed his cotton with 30% methanol and it produced a crop a month ahead of anybody else's around there. He made a lot of money as a consequence of not having to pay for all the water, insecticide and labour. We published this and this developed into something that will be worthwhile.

VM: If I can take you back 40 years to ORL...

AB: I'm not interested in what's 40 years ago. How are we going to do this experiment to understand how methanol works? I'm trying to get some radioactive benzoic acid, right now, and I asked John Foster how you make it. He said it's very simple. You start with pyridine, or piccolinic acid.

VM: You can't buy radioactive benzoic acid?

AB: You can, for \$1,000 per millicurie. But it's made by neutron absorption by pyridine, or pyridine carboxylic acid, which gives you very hot stuff. Harwell charges an arm and a leg for anything. Those SOB's in your country: they got a closed contract with the Russians for their cheap barium carbonate and they charge \$200 a millicurie for it instead of \$4.00.

VM: That's business, isn't it?

AB: It's robbery.

VM: You don't have to buy it!

AB: You capitalist, you.

VM: That's what it's about. That's how they do it!

VM: When we were talking before lunch, you said to remind you to talk about Bersworth.

AB: Ah yes. About 1947 George Bersworth came rumbling into the lab. I don't know why he found Melvin but there must have been a reason: you could ask Melvin...

VM: I don't think you could ask Melvin.

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AB: ...or you could ask (*Art*) Martell (*Editor: now at Texas A & M University*). Bersworth introduced me and Melvin to chelates. He came because Melvin had a metal chelate, cobalt chelate compound for his oxygen production during the war, that's why Bersworth came. I learned a lot from him. He came through four or five times, talking chelates, because he had invented the versenes, as he called them. Lately I have been dealing with a chemical company near Boston for which Bersworth had been a consultant in designing of their chelate compounds. I was amazed to hear something about him. I don't know if he is any longer alive. He's the father of chelates.

VM: You also mentioned Philo Farnsworth. Who was Farnsworth?

AB: Ah, yes. That was a story about Farnsworth and Earnest Lawrence coming in every day with this complicated glass apparatus with a lot of wires dangling out.

VM: Who was Farnsworth?

AB: Philo Farnsworth invented television.

VM: Invented it?

AB: Yeah. Television was not a British invention, it was American.

VM: Oh dear!

AB: I can show you a US postage stamp with Farnsworth's picture on it. Another one with Tesla, another one with the inventor of the electronic vacuum tube. There were four electronic inventors in this country.

VM: And he came through the lab. with the complicated...? What was he doing there?

AB: A complicated thing; they was going into the glassblower's shop.

VM: To get it fixed?

AB: To get it readjusted, or something; change the position of some electrode. This was the forerunner of the trinitron three-colour television gun that SONY developed for (*indecipherable*) production.

VM: That was remodelled, or fixed up, in ORL?

AB: Yeah. I forget the glassblower's name; I've got a photo or slide or something.

VM: So, before too long, presumably, they got the 37-inch cyclotron out of the middle of the big lab. You had then essentially the whole building except for the shops which remained there?

AB: No, we didn't have the whole building, but at least the 37-inch room was ours.

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VM: Where did that big white table come from?

AB: I built it, or organised it.

VM: It wasn't really a table, was it? Didn't it have sets of closets or drawers (*under the table top*)?

AB: Yes, it did have drawers in it for storage space but we wanted some big white thing to lay down the X-ray films. It needs to be white or you don't get the contrast that you want.

VM: So tell me: how did it begin to work out that you began to realise that there was a cyclic effect in the path of carbon?

AB: Anybody knows that there has to be a cyclic regeneration of an acceptor but it wasn't clear at first that ribulose di(*phosphate*) was the acceptor. I looked all over the lot for C₂ acceptors, so that's why we had labelled glycol and acetate and glycolate and all those possibilities — and we sort of gave up on that. About the time that we realised that I was doing the experiments where you withhold carbon dioxide and look to see what's happening, and it was the ribulose di(*phosphate*) spot that was piling up. That made it clearly the acceptor. But that didn't tell you how. Maybe that's the idea that Melvin got at the red light, in the red zone; I don't know. But we had been talking dismutations for a long time as a way of gaining free energy from a reaction. And it's still amazing that nature invented a process which organikers had never dreamt of. In all of the other enzyme reactions, there's an organic chemical model for it. It may be impossible to do in a biological thing but there's always a model reaction. But not for ribulose carboxylation.

VM: I guess nature has been going a lot longer than organic chemists, and given time the chemists would have come up with it.

AB: I don't know, but maybe some well-read organiker would discern an organic reaction that's analogous, maybe in ammonia chemistry or sulphur chemistry or something like that, but we didn't know about any and I still don't know. I talked with Stanley Miller every week or so about these things. Stanley was educated in the Berkeley Chem. Department and he is very familiar with what we were doing.

VM: He's down here now, in the Chemistry Department at UCSD. He's still there, is he?

I think the last things that really...a couple of things I want to talk to you about. You eventually left, when, in '55?

AB: Yeah. But we didn't do the PGA (*Editor: phosphoglyceric acid*) bit; you jumped.

VM: No, please tell me. I'm sorry.

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AB: Because then we were fiddling with ion exchange resins and Melvin got the Dowex 1 resins from Dow and also there was some precursor of Dowex resins, what were they?

VM: The Amberlites?

AB: No, before that. It was a local product, made in Berkeley or El Cerrito or some place. Probably written up in our paper. Anyway, in the very short-time photosynthesis the main chunk of the radioactivity was much harder to elute from the anion resin than the activity from longer photosynthesis. Those phosphate esters came off much more easily than the one from short, short times.

VM: You knew they were phosphate esters.

AB: Yes, I don't know how, but we ran some controls. It was Melvin that recognised the fact that if it's harder to elute from a resin it means it is bound by two anionic groups, not only one. Therefore, it's bound to be PGA.

VM: Why was it bound to be PGA?

AB: Well, it could have been fructose diphosphate.

VM: You knew it was a sugar at that point?

AB: Yes.

VM: Or something like a sugar at that point?

AB: Yes.

VM: So you isolated that, I remember, didn't you, by mixing the hot stuff with a lot of cold.

AB: I hydrolysed the phosphate off and then made the pure fenasyl glycerate with fenasyl benzoyl chloride; it's written up. That's a derivative of the glyceric acid. And then co-crystallised that to a constant specific activity and that sort of nailed it down. In there (*Editor: in Benson's filing cabinet*) is a bottle of the original Kohlbaum glyceric acid that I used. Our chemistry storehouse was fantastic. They had a standing order for everything from Eastman Kodak and then they had 100 years of Kohlbaum and German preparations and preparations by faculty members and whatnot. It was a real museum piece. That kind of thing people don't keep any more.

VM: To tell you the truth, I just don't go into chemistry storerooms. I don't know what they keep in chemistry storerooms; they've probably cleaned out all the old stuff.

AB: Now they clean it out and order things like a cook, go down to the grocery store and buy what's necessary for dinner, and that's it!

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VM: Yes, you're probably right. So I was opening up the question of your eventually leaving there. You tell me that I was going too fast and there were other things you wanted to discuss.

AB: I backed you off to PGA.

VM: Yes: do you want to back me off to anything else?

AB: I'll back you off to where I was stuck writing the *Annual Reviews of Plant Physiology* or *Biochemistry*; *Plant Physiology*, I guess, a review on CO₂ fixation. And that was an OK review; Melvin read it over and that's about it. At that time I don't think we knew what the carboxylation reaction was. After that, he went on a vacation trip to Norway with Genevieve and her mother, visiting their family and all that. A professor of chemistry in the Agricultural College (*of the University of Norway*) buttonholed him and begged him to send somebody over to set up an isotope laboratory. So I was awarded a Fulbright grant to go to Norway for a year. That's when (*Grant*) Buchanan came to the lab. and what's that other guy with the Italian name?

VM: Massini; Peter Massini.

AB: They did a good job.

VM: Buchanan worked on sucrose phosphate, I remember; that's what he told us.

AB: I was on the verge of identifying UDP glucose and if I had stayed there I would have recognised it before Leloir, but Leloir took over. Leloir was a very fine man, there's no question about that.

(Tape change)

VM: OK: Andy Benson tape 2.

The question I was about to ask you: when you did those really classic experiments of the kinetics, when you turn the lights on and off and dropped the CO₂, did you know what to expect, were you looking for confirmation of an idea, or was that rise and fall of ribulose and PGA unexpected?

AB: I don't think that was expected. We knew that something had to rise and fall. The best work was done by Shinichi Kawaguchi who came, maybe in 1949. He was a professor of kinetics actually in the University of Osaka. He became later president of the Japanese Chemical Society, a lovely guy. He was very meticulous and we did a time series of CO₂ fixations from 5 seconds up to 10-15 minutes. He and I made chromatograms of the whole series (I've got all those). They were beautifully done and the separations were excellent. At that time, we knew something about hydrolysis of the phosphate esters, so the phosphates were eluted and hydrolysed to recognise which ones they were so we could get a kinetic rate diagram for all of the intermediates of CO₂ fixation. It's wishful thinking but both Melvin and I thought we could learn something from this. The only thing that we can say we learned is that

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malic acid production is very fast, almost as fast as PGA. It makes me think that by some mechanism that the phosphopyruvate carboxylation is an important aspect of photosynthesis that still hasn't been clarified.

VM: Was it those sorts of experiments that gave you a handle on the concentration of compounds in the cell and that you could then use these to look at the variation when you altered the external conditions?

AB: Yeah. We could deduce from that what the concentrations in the cell were. But at that time I don't think we did that. That came later when Alex Wilson was working and Al and a number of people. But Alex did it on a grander scale than any of the others.

VM: We'll see Alex tomorrow and talk to him about that. There is, of course, that thing in Alex's paper, of the little fisherman on the...

AB: I have a good copy of that.

VM: Who did that? Alex himself?

AB: Alice Holtham drew it. But it was Alex's nonsense. This is akin to dipping the drinking milk straws into honey and sticking it to the ceiling. He was a total fox, that guy. He was just delighted that this sailed through the JACS and got published. It was a very small diagram in the journal, you know. You could just barely see this little fisherman.

VM: You need to know where to look for it, don't you?

AB: It's there. I showed it in my talk in Grenoble last year in January. We had a chloroplast-photosynthesis meeting in a ski resort town above Grenoble. I gave the historical talk in that. I spent a lot of time about Alex Wilson, who was such a character.

VM: The other name that came up before lunch was John Weigl. You had something you wanted to say about John.

AB: Actually, it was about his wife, who gave me the title for an article I wrote, "The Green Secret".

VM: What was the article?

AB: Something about radioactive carbon in photosynthesis but we didn't know the cycle at that time, that was 1948. It was a reasonable article but it's hard to find in the journals nowadays because it was published in the Yearbook of the Grolier Society.

VM: One of the things that certainly struck me when I first became involved with this was, in a sense, how wise, or if you like to say how fortunate, you were that you were working on a problem where you get rid of the unused substrate. When you put the chromatographic products from the algae onto the paper, all the hot CO₂ which hadn't

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been used was lost, and it didn't foul up the rest of the experiments. If you tried virtually any other substrate...

AB: You have had to get rid of it.

VM: ...you'd have a great glob of the stuff sitting there which is likely to foul up anything else.

AB: Good point; write that down in your article.

VM: Well, it's on the tape now. Had it not been like that, you would have had a hell of a lot more trouble sorting stuff out because you were dealing with very faint compounds.

AB: The whole sequence is a bit of fortunate choices, like me being educated in Berkeley and knowing who these guys were, and going past the 60-inch cyclotron every day while it was being put together, when I'd go to class right past the Anthropology Museum because I knew something about that; then getting to work with Sam Ruben and Martin at the right time — nobody else had that opportunity. And then the first shot of radioactive carbon us.

VM: The combination of all those things really rather suggests that you cannot design deliberately organisations to be successful like that. They happen because things work out by properly chance.

AB: Yeah.

VM: Can you put your finger on what you think are the most important reasons why that group was so successful in what it did?

AB: Maybe because Melvin was so darn clearly oriented in which way he wanted to go and he refused to let anybody distract him with their own ideas; he only wanted to push them for what he could glean that would help *his* ideas, which is sort of disgusting but in other ways it was very successful and it's not very considerate. Not to pay any attention to what a visitor wanted to tell us but needling the visitor for what you could get out of him.

VM: When visitors came into the lab., did he tell them what to do or rather suggest?

AB: He'd angle around to find out what they knew that would do him some good and then keep asking questions...

VM: In order to provoke them to go in the direction he wanted?

AB: ...in order to get them in the direction that he wanted. That was the secret of his success. He was excellent at asking questions!

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VM: What about the building? People have very fond memories of that shack. Do you have fond memories of it?

AB: Oh, yes. I have memories of the Rat House, too.

VM: I can't share those with you because I didn't see it. But the ORL I certainly can.

AB: But the Rat House didn't include much of a group. It was mainly Sam and I who were involved. Bill Libby's lab. was right next (*to ours*), in the same room, practically and Barker I knew and Hassid and so forth (*in LSB*).

VM: What do you think was so special about ORL?

AB: Calvin was darn good. He'd get there before eight in the morning because he always had an eight o'clock lecture or something like that. He would always come in and ask "what's new?" Before he went home in the evening, 5:00, 5:30, he'd always ask "what's new?". I learned after a while not to tell him everything that was new because we had to have something in our pocket when we really have to show up with something new. Once in a while — I think Al understands that, too.

VM: The people that we have already talked to mention that the physical structure of the building, for all that it was an old crummy building, it was a pleasant place to work and it was socially very integrative, people were pushed together and they liked working together. They liked the central feature of the big white table.

AB: That's true. It wasn't so bad because it had new shiny floors and new benches and the best white porcelain lab. sinks in the world because everybody that had these grey old duriron filthy things and had lousy faucets. I had the best chrome-plated faucets in the world. Over every lab. bench was a line of two-lamp fluorescents, solid, so it was brightly lit and that makes a lot of difference. Most people don't realise that.

VM: Had you ever worked in a building before with big labs. like that, or was your experience in small labs.

AB: Cal Tech. They were big labs. but there was not the interaction with others. Every student had his own independent project; I knew what they were. The interaction there was with the postdocs. That's darn important. In ORL there were a lot of postdocs. and the experience for students was extremely valuable as you well know.

VM: There were probably more postdocs. through that place than there were students, weren't there?

AB: Yes. It was a real...Murray will attest to that.

VM: What do you think of the Round House that was built obviously much later, a decade nearly after you left?

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AB: It was just too much stuff! It was spread out in too many directions and I don't feel that anything significant developed out of it.

VM: As a building, given the opportunity, the fact that ORL got demolished, that they were pushed down to LSB in not very satisfactory accommodation. And the opportunity arose..

AB: I don't think I could knowledgeably make a comment that. It may be OK but the people who worked there will know best, not I. I just saw it. There are these fancy EM and NMR, and all that stuff in different places around the perimeter. That was a nice idea. We spent most of our time around the big white table; there was enough room for everybody.

VM: Well the big white table was still there but, of course, the group had become much bigger and I think it grew to nearly ninety or so in the Round House.

AB: I think Melvin's Friday morning seminars were very effective in getting bashful people to get up and tell their story.

VM: As I started to say earlier, you left in '55, was it?

AB: The end of '54.

VM: Where did you go from there?

AB: I didn't know where I was going for a while, except that I had to go.

VM: Can you tell us why you had to go? OK.

(Benson shakes his head)

VM: Where did you go?

AB: To Penn State. I had a brother-in-law who was a Professor of Geophysics at Penn State and he and I were classmates at Cal Tech before that. He married my sister, and she came to my graduation. Millikan gave us our PhDs. They were settled in State College, Pennsylvania, and I got an offer to go there so I went. It turned out to be a very good opportunity because the chairman of my department, Agricultural and Biological Chemistry, was very generous. He gave me a decent lab. and I didn't have to do much teaching. They were real bright students because there was a very strong Chemistry Department there. I made a lot of good progress in lipid chemistry. Our article in B&BC 1000 is on the discovery of phosphatidyl glycerol which is the main phospholipid (*indecipherable*).

VM: How long did you stay here?

AB: Six years or seven.

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VM: And then you came here?

AB: I decided I didn't want to stay forever in Pennsylvania; it was too hot in the summer. It was OK but we invented the sulpholipid and a lot of neutron activation chromatography which, I think, was clever, but nobody uses the (*technique*); stupid people. One of the guys next door has got a job in the University of Rhode Island Marine Center and they've got a nuclear reactor right there and he could do fantastic things. As long as you have a free nuclear reactor, you can run with it. Boy, it's just terrific. See that tube? It's yellow because of the radiation? You put a chromatogram in there and hot-up the P^{31} ...

VM: That's right. You were doing this...

AB: ...to make P^{32} . So we got a way of quantitative analysis of phosphate compounds, phospholipids, doing kinetics of a lot of things.

VM: I remember your work on that.

AB: You just saw of the top and take the chromatogram out and put it on a film after the silica decays, aluminium or whatever with a short life.

VM: How long do you have to leave it in there?

AB: A few hours to overnight.

VM: And the paper doesn't deteriorate significantly?

AB: That's the limiting factor. I got the smart idea of rolling up, ironing mylar polyethylene sheet onto the paper and you can roll that up and activate that and the polyethylene holds the paper together. The only trouble is that the SOB, at Dow Corning?, who invented Mylar...found a long-lived...very dastardly interference from some radioactivity. It turned out to be...not scandium...a catalyst for the polymerisation of the mylar, and it was the secret part of the patent. I tried to get information. I got German Mylar, Japanese Mylar; it's all made by the same du Pont patent.

VM: And it interfered with your phosphorus?

AB: So that was impossible. You're right — the fragility of the paper...What we learned out of this was that the compounds whose spots are radioactive, but it's no longer the original compound, it is just recoiled phosphorus, so we got into hot atom chemistry for quite a while and did some important things that are now turning out to be pertinent to the origin of life kind of stuff.

VM: When you left Penn State did you come directly here?

AB: I came to UCLA. My classmate from Cal Tech, Jim Mead, was the director of a major lab. in the med. school there and he offered me a job at reasonable income.

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VM: You weren't there very long, were you?

AB: Just a year. Then I had a choice of staying at UCLA in their biology-botany department and the med. school, or else coming here. Back in '54, when I was in ORL, two characters from Scripps here invited me to come down and try carbon measurement of phytoplankton productivity. So I came down with some counters and we went off on the ship chasing algae in the ocean. That was in '54. The guy who invited me is being posted some place on Friday so we have a big party coming that.

VM: Who was that?

AB: His name is Bill Thomas. He is still going. He is studying the ice algae up in Yosemite Park; these are photosynthetic algae that live in the ice.

VM: Are you still doing experiments here?

AB: Yes. Not any good ones, but that's what this is all about, I guess. I bought \$1,000 of radioactive methanol and I just dilute it. It came in one lambda of methanol in a Gregovsky ampoule and I had to transfer it to a couple of cc of water because I'm going to use it in water, anyhow. Dick Lemmon pointed out that it will be killed by radiolysis.

VM: Do you complain?

AB: People complain about paper chromatography being slow and tedious and all that and it's not. The amount of time you spend on a single analysis is very little. You just have to wait for a couple of days.

VM: Do you do lots of them at the same time, don't you?

AB: Yes. You can think about something else! One important part is making every minute count and not let anything hold you up, like going to dinner, so you can turn the chromatogram at the right time of the night. That way, you can get things dry in a hurry. We have speeded things up a lot. I can come up with a product maybe twice as fast as anybody else.

VM: In the 15 years after you invented that paper chromatography technology, we haven't improved it a great deal. We have a bit. We got some alternative solvents because the original solvents were not good from every point of view, we have improved the counting techniques somewhat, on paper we have an automatic counter and things of that sort.

AB: I didn't like that. You cut up the paper and then you ruin the whole advantage. You can't store these pieces.

VM: But we had a lab. notebooks; you didn't have a lab. notebook. You can store the film.

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AB: No, we don't have a lab. notebook. Who needs a lab. notebook?

VM: I tell you that when you are faced with counting thousands of these spots, making the thing automatic was a revolution.

AB: Maybe.

VM: There was no way any of us were going to do it the other way.

AB: Did Alex Wilson do that or was that done after Alex?

VM: The automatic thing? I did that with Karl Lonberg. That was my contribution. And that machine, in a version that Al modified, is one of the few relics that remain in the AEC/Radiation Lab. storage warehouse in Emeryville. That's there.

SM: But it's mislabelled.

VM: It's no longer mislabelled, I hope because I told them built it and who designed it; I hope it is no longer mislabelled. They originally had it listed under the name of the technician who modified it. So I sent them a copy of the original paper and showed them that wasn't the case.

AB: The glassblower's name was Harry, somebody. That part's right.

VM: I'll ask other people around and see if anyone can remember the glassblower's name. Marilyn will probably remember.

AB: Oh, I've got it. You have never seen my slides? They are labelled like that, thousands of them.

VM: My God!

AB: You don't have to look at the pictures. You just read the titles and you know exactly what's on the film.

VM: "John Lawrence, '45"; God, it must be marvellous looking through some of these.

OK, well I think it remains only for me to thank you very much indeed for all the time you have spent with us. And shut the tape down.