

Chapter 24

SAM ARONOFF **(with Edith Aronoff)**

Camano Island, Washington

July 8th, 1996

VM = Vivian Moses; SA = Sam Aronoff; EA = Edith Aronoff; SM = Sheila Moses

VM: This is a conversation with Sam Aronoff — and Edith Aronoff?...

SA: Yes.

VM: ...on July 8th, 1996 on Camano Island in Washington.

You, Sam, I gather, were in Berkeley before the war, from the mid-thirties, and so you can remember what the scene was like into which the Calvin activity later developed.

SA: Yes, indeed. It started with Sam Ruben, you know, and Sam Ruben was the first to receive carbon-14 and to realise its potential for tracing carbon metabolism in plants. He and Kamen (I'm not sure of Martin Kamen's precise role in the undertaking but Sam was the chemist, Martin was a physicist)...As you are probably aware, Sam was carrying a thermos bottle one day which was apparently scratched. The bottle had liquid phosgene in it and it broke, scattering the gas throughout the building in which he and a number of others were working. Rather than just hollering and evacuating, he took the trouble to run in and out of each room, essentially forcing people out into the air, while he himself was ingesting it, breathing it. He lived for three or four days and died.

He'd had a grant from the Rockefeller Foundation to investigate the metabolism of carbon and what was suggested by someone (I don't know who) that Calvin take over the responsibility for that grant.

VM: Did that take place soon after he died?

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SA: Yes, very quickly: days. I had been working in Hilgard Hall with a joint or paired committee of major professors, namely Gordon McKinney (who was a Brit but probably the best chromatographer in the business; he was involved in food technology in the department and the nature of flavours and that kind of chemistry) and with Calvin. I was working on chlorophyll, structure — it had recently, in fact, just been synthesised by Willstätter in Germany and I obtained pure samples of it.

VM: Excuse me: this was as a graduate student?

SA: As a graduate student.

VM: And what had your background been as an undergraduate?

SA: Geology.

VM: So you were learning chemistry, were you, at that stage?

SA: Yes. Well, as I geologist you had to have chemistry but not the same kind of chemistry. I should stress that Calvin didn't know biochemistry either and so my having him and McKinney as joint professors essentially served as an introduction for him to biochemistry.

VM: And when was this?

SA: Where?

VM: When?

SA: When? '36, '37 on. I got my degree eventually in '42.

EA: Now when did Calvin come to Berkeley?

SA: I think a year or two before that.

VM: Calvin came to Berkeley in '37, I think. He was in England on a post-doc. with Polanyi from '35 to '37, and then he was appointed...

SA: But you see, he was teaching organic chemistry and although he had worked with some polycyclic compounds in England, I introduced him to chlorophyll. And so from then on we began to do a combined attack, if you will, on biochemistry.

VM: So you must have been the first, or one of his first, graduate students in Berkeley.

SA: Oh yes. There were a couple of others. One whom you will not meet, because he died, was Wayne Willmot: have you heard of him?

VM: No, I don't know about him.

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SA: And there were a number of others: Lloyd Ferguson...

VM: Yes, we heard about him.

SA: What's happened? Have you seen him, heard him?

VM: No, we haven't seen him but we had lunch last Friday with Gus Dorough.

SA: Where's Gus?

VM: Gus is in Livermore.

SA: Oh, still there?

VM: Yes... and he's retired from the lab. but he lives in Livermore.

SA: He's nigh on to my age.

VM: He told us about Lloyd Ferguson and he thought that he was somewhere in LA.

SA: Well, he taught at the Los Angeles City College for a number of years. He disappeared.

EA: He got old, maybe.

SA: Well, but he disappeared before that; he just vanished and I don't know whether he became involved in something else or travelled overseas. His wife, you remember, was from the South and not completely happy with Los Angeles. She may have introduced him to some of the other possibilities in the South and he may have...

EA: Well, we always thought he didn't know he was black...until he went to that school; what was that school, that black school he went to teach at or something and he met his wife there and she was black, she was very black.

SA: Moorhouse?

EA: No, that's wrong. I think he went to Tuskegee for a short time just after he got his doctorate and he met his wife there. She was a black person from the South and he thought like Jesse Jackson, black. His wife never realised, really, that he was black and that it had any social significance until he went there because he was born and raised on the West Coast.

SA: Well, his pigmentation, mind you, was black enough but none of us ever thought in those terms — black, yellow, brown, whatever: it was meaningless. We socialised in his house and vice versa and there was never any problem.

VM: So all you guys, you and Dorough and Ferguson, were all Calvin's pre-war graduate students.

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SA: Yes. Now with Sam Ruben's demise and Calvin's taking over of the Rockefeller money, the entire direction of research changed so to speak and in a sense that was the beginning of his activities so to speak. He had a consort, whom you've undoubtedly either met or know about, namely Andy Benson. Andy had had some difficulties during the war because he was a conscientious objector and was then rehired immediately afterwards but sought Calvin's understanding and the two of them formed a team. Andy frequently visited a variety of other labs. in order to see what was going on and he and Calvin would then discuss their direction subsequently. When I joined Calvin's lab., it was at the beginning of his receiving of the carbon-14...

VM: Let me get the chronology right. You were his graduate student from the late '30s until '42?

EA: Really McKinney's graduate student.

SA: No; I had dual major professors Mother, both McKinney and Calvin .

VM: And then what happened after '42?

EA: You were a postdoc. for years, though.

SA: Yes, until '43...

VM: With whom?

SA: ...with Calvin and then — Mother, you will have to keep me going on this — I'm a little confused on when we did the war work.

EA: You went off to teach at Boston University.

SA: Well, I took a trip across the country and went to Boston among other places. They needed someone to teach chemistry. They had contracted to teach a large number, two or three hundred of them, beginning chemistry and they urged me to please take this on so I did and I spent that year and a half, two and a half, year and a half (taught the summer session too), to an auditorium of soldiers.

VM: But you were not yourself drafted?

SA: No. I was urged by the Draft Board to take this responsibility as being more important than just becoming a soldier. After this teaching, then, and a bit of discussion with the President of Boston University who was not happy about some of my concerns with...

EA: I think they were the MIT connection.

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SA: Well, you have to remember that President Marsh was a very conservative gentleman and his (*i.e. my?*) pay was in the same range— minimal. It was not enough to live on but I managed to eke out a living by going to MIT where they needed some help in spectroscopy. And so I did this: they had a contract with the army to test a variety of materials for transmissivity of various wavelengths but when the President's bagman, a Professor Alter (*spelling?*), became aware of this additional basis for income, they were quite upset and the President wrote me a letter saying how this must inevitably detract from my teaching and so forth and so on, and they both decided there was really no future in there. And so I left.

SM: Which years were these?

EA: '43, '44. And then you went off to Chicago to Franck's group for two years.

SA: That's right. On the way west, I was on my way home. But I stopped off in Chicago on the way west and there were a pair of gentleman, James Franck and Hans Gaffron, who were interested in a variety of things and discussed some of their problems with me, their chemical problems, and Gaffron was so impressed, I guess is the word, that he urged Franck to please hire me on the spot.

VM: What sort of area were they working in into which you would fit?

SA: Well, they were working on a variety of things but it was primarily carbon dioxide fixation.

VM: And had you had experience yourself in that area by then?

SA: Not really, no. This history of that briefly is as follows. It was done independently in three different labs.: by Wood-Werkman but that was non-photosynthetic so it was irrelevant. Then there was a fellow in Russia, who published an article in a journal *Biokhimiya*, in which he observed the phenomenon manometrically but I don't know that anybody has ever recognised it as being CO₂ fixation. He did.

VM: Do you remember his name, the Russian?

SA: Boichenko.

VM: Oh, that's a lady!

SA: I don't know.

VM: Oh yes, I've met her: she's a lady.

SA: Really? That's interesting because...you can't tell and I couldn't tell from the article and indeed when I wrote the thing up I put "her" in that gender but I wasn't sure that I was correct because, of course, in Slavic it's usually an "a" and if it's an "o" at the end you can't really tell.

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VM: That's right but she is a lady and she worked in the Vernadsky Institute in Moscow and I visited her there.

SA: Well, give her credit. She was an independent and unrecognised scientist in terms of CO₂ fixation. And then third, of course, was Ruben, Sam Ruben.

VM: Did all these three know about one another?

SA: No. It's all independent. I think Sam may have known about Wood and Werkman but it was not photosynthetic, you see, and Sam's initial disclosure involved a finding that the radioactive carbon was in a molecule which he thought to be in the neighbourhood of 2,200 Daltons. It wasn't long after this...This was never...Yes, it was published. There was a publication involving it in the JACS but I'm not sure I recall when. In any event, this was of course quite incorrect; he gave it at a seminar in Chemistry which was well attended. Then, when Sam died, Calvin inherited everything and that was when the work on carbon dioxide fixation began.

VM: But presumably the work of Sam's that you describe was done with C¹¹, wasn't it?

SA: No. You see Sam was the first one to get C¹⁴.

VM: And he had enough of it at that time to use in that way?

SA: To play with, yes. To play with. He received the first amounts and he was the first one to look at photosynthetic CO₂ fixation.

VM: So Calvin, for all the contribution that he made, certainly didn't originate the idea of using C¹⁴ for CO₂ fixation?

SA: Oh no. In fact, I don't like to denigrate him to the point that...but Calvin was a first class synthesiser but by and large the initial contributions came from elsewhere. For example, I don't know if you have Alan Brown on your list...

VM: No.

SA: Alan Brown was at the University of Illinois with Ed Fager, a physicist. They were going to give a talk at a Federation meeting on formation of sugar, six carbon sugar, and...

EA: (*Inaudible*)

SA: ...were investigating the question of whether it was a 3 + 3 condensation or a 5 + 1. They had come to the conclusion, in fact, that it was a 5 + 1 and not a 3 + 3. Now Calvin was under the assumption that it was a 3 + 3 until Andy Benson, in visiting Alan, conveyed this idea to Calvin and then on the research changed. Now what Calvin did was to show, to develop the Calvin cycle; that was without question his most important contribution. He showed how the ribose, and specially the ribulose,

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could be regenerated from other compounds in a cyclical manner. But the CO₂ fixation *per se* was Alan Brown's and I don't think he's really gotten the credit for it. I'm not sure, in fact, that Calvin was even aware of the initial information: he absorbed ideas like a sponge and I suppose it's difficult to distinguish between original ideas...

VM: Yes. Well, I suppose if Andy, in the course of his travels, had come across this and it had been a conversational item, it gets incorporated.

SA: That's precisely the way it worked, precisely. And so, I had become familiar with chromatography and had introduced the notion of the chromatography of the radioactive products. I'll back track a bit on this.

VM: Is this paper chromatography?

SA: Paper chromatography.

VM: You introduced paper chromatography?

SA: I want to give credit first to someone else.

VM: OK.

SA: There was a young man in Biology at Berkeley by the name of Stepka, William Stepka: S-T-E-P-K-A. Bill Stepka turned up one day to the lab. and showed my assistant and me (Victoria Haas) this paper chromatography that he had invented. Calvin and Benson were using huge chromatography columns, you know, yea wide and yea long...

VM: You are showing something three or four inches in diameter and several feet long.

SA: Yes — and they wanted to isolate *the* compound in bulk, not with their approach, but Bill Stepka came up and showed Vicky and me the paper chromatography. I immediately saw how it could be used for detecting radioactive carbon and then we made some radioactive algae, I was growing algae for Calvin, and fed them some radioactive CO₂: we isolated the sugar and not only did we chromatograph it but we developed autoradiography because there were some big cassettes around in the Radiation Lab. at the time and I don't know what the Rad. Lab. had been using them for. But there were these large cassettes and I got some film and in fact we developed two-dimensional radio...

VM: And this was you who did this; you were the originator?

SA: Yes — but it was no big deal.

VM: No, but it is interesting to know who actually did it.

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SA: Well, Bill was the originator of one-dimensional paper chromatography. What I realised were two things: first that two-dimensional would be better but that you had to use different solvents in the two directions otherwise everything would lie on a diagonal. And secondly autoradiography, and...so yes, that...

VM: So can I ask you about — by the time I got there some years later, of course, the thing was highly developed with all the equipment in place. What did you do about equipment? What did you use for two-dimensional chromatograms to start with?

SA: Ah! Well — as a matter of fact it was I who put them into the glass jars about yea wide and about that tall, put a bit of solvent at the bottom, rolled them into a cylinder and put them in. Then I ran them in one direction, took them out and to dry them and turned the thing and ran them in the other direction.

VM: I see: you ran them upwards and simply wrapped the paper into a cylinder and put it in the jar to develop in the solvent on the bottom.

SA: Later, later I developed the trough system and then we had the lab. build these troughs and we had little trons on top and put them in with a glass rod and ran them down, took them out and dried them and ran them again.

VM: And this was all your design?

SA: Yes.

VM: Good; I spent many happy hours with those things.

SA: No big deal.

VM: No but somebody has to do it in the first instance.

SA: Yes. Anyhow, what it did was to avoid this really heavy labour that Calvin and Benson were doing with the columns. Quick, simple and when we first showed...one of our first chromatograms, you know, showed a multiplicity of compounds far more complicated. On the other hand, if you reduced the time, which we also did, then we could show that the initial compound, in fact, that we could isolate from the CO₂ *was sucrose; we couldn't, we didn't see the ribose or the ribulose. (Note that the section in italics is missing from the tape.)*

VM: Could I take you back to get your times and whatever. You say after you were a post-doc. in Boston you went to work with Gaffron and Franck. How long did you spend with them?

SA: Oh, was it two years?

EA: You did three years.

VM: Two years. You came back to Berkeley then, in about '46?

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EA: Yes, that's when Glenn Seaborg came back.

SA: Glenn Seaborg was working in both labs., in the Radiation Lab. at Berkeley and the one in Chicago, and on his way through the Chicago one he stopped in to say "hallo" and I pled with him to get me out of that...

EA: Germanic atmosphere?

SA: We did not get along, Professor Franck and Gaffron and I. In all fairness, they weren't happy with my point of view about a variety of things. They were distinctly Germanic and I think that (as long as we're writing history we might as well be truthful about it) he and Rabinowitz, who was at Illinois, were once speaking in the hall, in German which I understood perfectly well, saying that they thought they would develop Germanic education in the entire US graduate schools. This would become a model or a focus for...they weren't happy to have anybody listening in on this. And so...

VM: So you came back to Berkeley in about '46; and what were the circumstances in which you were able to join Calvin's group?

SA: What do you mean by "the circumstances"? I asked "could I join?" and he said "sure". Money was no problem.

VM: He gave you a job?

SA: Yes.

VM: You were an employee of the Rad. Lab. or whatever?

SA: Yes.

VM: On a permanent basis?

SA: Well, there was no end in sight.

VM: I see. And by that time, who was there? What constituted the group?

SA: Oh, there were quite a few.

EA: Bassham? What was his name?

SA: Alan? Alan wasn't really part of that group. He was working on...John Weigl and Alan Bassham were working on their Ph.D. theses but I don't think it was related to...

VM: He was working on succinate, on dicarboxylic acids.

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SA: I don't know what he was working on but I don't think it had anything to do with that group.

EA: (*Indecipherable* — *White?*) was there, wasn't he, at that time?

SA: I'm a little bit confused as to the initial time and the subsequent time.

EA: We all played bridge at noon.

SA: Ah. That was — was that in '36?

EA: I think that was in '46; '46 to '47.

SA: Was that the bridge school? Melvin was unhappy. He would come down at noon and see us playing bridge and would do his utmost to...

EA: ...break it up!

SA: He was a verbal whip and say you mustn't waste your time wasting your time that way when you should be at your lab. bench making big discoveries. He asked Lloyd Ferguson and Wayne (*indecipherable*); you know, if you were to name the others I could remember them but I...

VM: Well, I'm not sure who was there at the time. I guess Andy was there.

SA: Andy was not...

EA: ...a bridge player.

SA: ...involved in our...Andy stuck pretty close to home meaning he did his bit of teaching and he made his journeys around the country to find out what other people were doing.

VM: Well, the other people that I know were there: there were Donner people — there were Dick Lemmon and Ed Bennett.

SA: Dick Lemmon and Bennett were over in the other building.

VM: Yes, in Donner. They weren't playing bridge?

SA: No.

VM: Really, who was there? Was Murray Goodman there at the time when you were there?

SA: No; doesn't ring a bell.

VM: I'm pushed to know who was there, actually, at that time.

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SM: I remember hearing about this bridge game but I can't remember who from.

SA: Gus was not involved, Gus Dorrough wasn't involved. But Gus would know if his memory is better than mine.

VM: Well, we could call Gus. Do you remember a guy called Elmer Badin?

SA: No.

EA: No.

VM: Because I don't have a good chronology of when everybody was there. Not yet — Marilyn, Marilyn Taylor is getting it for me but she hasn't done it yet. OK: so you came back as a regular staff member in the lab.?

SA: Yes.

VM: And you worked in ORL?

SA: Yes. Never up on the Hill.

VM: Right. So all the things you developed down there, the chromatographic and the radioautography and the...What about the counting stuff? How did that come about?

SA: Well, the way we first did it was to have a little counter tube and we'd stick — I did the autoradiography so I knew where the spots were — we'd put the tube over it and count it.

VM: And these were conventional commercial counters with mica windows?

SA: Yes; from Nuclear Chicago. And indeed they were so pleased with it that I recall that they invited me to come to Chicago to see their newer equipment and I was there for three lovely days and saw my first night-club events. Who were those...?

EA: (*Indecipherable*)

SA: No, no, no! They were three men and a lady and I heard them perform not too long ago.

EA: You don't mean Peter, Paul and Mary?

SA: Yes. Two men and a lady; Peter, Paul and Mary.

VM: Peter, Paul and Mary were your first night-club entertainers?

SA: Yes!

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VM: Where had you grown up?

SA: Berkeley. But it depends on what you mean by “grown up”. I was at university in Los Angeles but I couldn’t stand LA.

VM: You went to high school in Los Angeles?

SA: Yes. Venice High School.

VM: Venice?

SA: Yes. Well, if you’re familiar at all with LA...

VM: We have a son who lives in Los Angeles.

SA: ...there used to be a large fountain in front of the... Venice High School doted itself on art. The major instructor there felt that art was so necessary for human existence that he had a huge fountain built in front of the school and at the very head end of it was a statue of a lady, a nude lady, young lady, turned out to be Myrna Loy the actress who had been a student at Venice High and this art instructor was so taken by her lovely figure that he had carved a statue of her and made it into a fountain.

VM: And you stayed there until you went to Berkeley as an undergraduate?

SA: I went to UCLA...

VM: I see, yes.

SA: ...which was most unhappy...

EA: It was very new and (*Indecipherable*)

SA: They had just moved from downtown...

EA: They had a teachers’ college mentality — still.

SA: Yes...

VM: That’s how UCLA started, was it, as a teachers’ college?

EA: You see, it was a teachers’ college downtown and then...

SA: ...and they were given land out in Westwood.

EA: What was the name of that developer?

SA: Well, it was in Westwood but that wasn’t the name of the guy.

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EA: He had all that land, he bought up all that land, and he donated...

SA: ...the land to the university so he could develop Westwood.

ES: Yes: then he developed Westwood.

VM: Ah, clever, yes!

SA: It worked., it worked. In any event, the university at that time was not only immature in terms of teaching but as history showed it was a hotbed of Nazism and indeed much, all of the faculty in German language were fired because they were so anti-Semitic and replaced so that it was unpleasant, it was unpleasant, and I did not do well at all. But there was a lovely teacher in geography; in fact, I did so poorly that I was on probation and the head of the committee on students who were on probation was a Professor MacDonald who taught geography — a very kindly-looking old lady and when she spoke with me she said that I was probably in the wrong area and would I take her geography course so she could evaluate me? And I took her geography course and did quite well as a result of which she wrote a letter to Berkeley to the office that provided employment for students and said “please give this young man a job”.

So when I got to Berkeley I was able to go to seismology and was given the job of sorting out a charcoaled piece of paper with a needle scratching through the charcoal and my job was to change it every day and file it. I got into the basement where this seismograph was and found an absolute jungle of records that had been dated but not stored in any way. So my first job was to organise these and it took quite some time to arrange them in sequence and I then took care of the seismograph and noted when there were...

EA: Significant changes.

SA: ...events. Yes.

VM: Were you being a student at the same time?

SA: Oh yes, of course, that was how I was able to live because the teaching assistantships were very few in number and generally given only to students in their final year. This was a very difficult thing. As a result of Professor MacDonald, I had this job which saved me — at 25¢ an hour!

EA: Yes — it was big money!

SA: Well, it was enough for a very healthy bowl of soup which my wife and I would eat for dinner at a Chinese restaurant.

VM: You were already married at that point?

SA: We married. Yes, indeed, she came up with me.

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EA: There was a little lag time.

SA: Oh, we've been married now sixty years and so...

EA: But you must remember that he graduated from high school when he was fifteen so he went on into UCLA at a very young age and I always felt that that accounted for his doing poorly. He wasn't mature enough yet to settle in.

SA: Well, there were fifteen of us who were fifteen years old at the time and I was the youngest of the fifteen. So she may be right; we were certainly conferred no advantage.

VM: But by the time you went up to Berkeley you were already married?

SA: Yes.

VM: So you must have been married pretty young?

SA: We married...

EA: I was twenty.

SA: I don't recall whether we actually got the marriage certificate...

EA: We got married in Berkeley.

SA: Yes, I thought (*indecipherable*) in fact or in Oakland.

EA: I think there was a lag time. I think you went up first and then I...

SA: Yes, you're right, quite right Mother, you're quite right. And I think the actual marriage certificate was issued from Oakland.

EA: Yes.

SM: And which year were you married?

SA: '36.

EA: '36. I was born in '16 so twenty was right.

VM: OK. So as somebody in the lab. when you rejoined it in '46 and you had a job with them and you worked in ORL, you said there were several people there working in the group at the time.

SA: Yes, and apart from Ed (*indecipherable*) I'm darned if I can remember who they were.

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EA: Wayne (*indecipherable*).

SA: Yes, Wayne didn't — Wayne was in the lab., in and out for a while. You see, the war was beginning...

EA: ...to wind down.

VM: In '46 it was over.

EA: Not with Japan, I don't think.

VM: Well, the bomb was August 8th, '45.

SA: We were working on cobalt...

EA: yes, of course.

SA: ... compounds which would combine with oxygen in the hope that they could be put into submarines and which would provide a source of oxygen in the event that the oxygen tanks had to be thrown away; because the additional speed that could be attained by that was meaningful. On the other hand, they were then bereft of their oxygen supply underneath so this ability of this cobalt chelate to assimilate oxygen reversibly meant that it could conceivably be a source for them.

VM: But you weren't doing that, surely, when you rejoined Calvin in '46?

SA: Oh yes.

VM: You were still working on oxygen chelates?

SA: Oh yes.

VM: I see. So you weren't totally dedicated to photosynthesis when you came back to his lab.

SA: Oh no.

VM: But you were nevertheless a participant because you designed the chromatography...

SA: Yes, and we grew the algae. My official job was to grow the algae.

VM: When you say "we"...

SA: Vicky and I. And I developed a tank with fluorescent lights which were fairly novel.

VM: And those flat-bottomed flasks, one of which still survives?

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SA: Well, that was my design.

VM: Yes.

EA: When you were with Franck you were working on photosynthesis, too.

SA: Oh yes. But that was in a very different way. Franck was not happy with that — I sort of had to do that on the side in a sense. You see, Boichenko had developed this manometric method for CO₂ fixation that was not known. I mean, you had to be able to read Russian...

VM: And you didn't know of it?

SA: I did know; I found it in the library and I immediately saw its virtue and so I resuscitated some manometers and all of my work with Franck and Gaffron was manometric.

VM: Do you read Russian?

SA: I did at the time.

VM: So your work with Franck and Gaffron was all manometric following Boichenko's example?

SA: Yes.

VM: But when you went to Calvin...

SA: I spent much of it setting up the lab. You see, there had been no fellow there before me — (*indecipherable*) Davis; I don't recall his name any longer but he was unhappy with a number of problems so I inherited his lab. and spent a good deal of time cleaning it up and getting the manometric equipment to function so that was the basis of my activities there.

VM: How long were you there roughly?

EA: Three years.

SA: Yes, three years. Three years of Chicago is five years of anywhere else.

EA: Very urban.

VM: So when you came back to Berkeley, to Calvin and you were part of your time working on the cobalt oxygen chelates and partly you were working on photosynthesis. And the big breakthrough was the arrival of Stepka with the chromatographic paper...

SA: Yes.

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VM: And you were the one who began to run with that first and to develop the thing?

SA: Yes.

VM: And how far did you get? How long were you associated with the work after the chromatography started?

SA: Oh gosh. Where did we go after that Mother?

EA: We went to Iowa State in '48.

SA: I was wondering whether — yes. There was a fellow in agriculture — I think I mentioned his name: J.P. Pennant — an elderly gentleman who...

VM: That was in Berkeley agriculture?

SA: Yes, from Missouri. Who had become aware of this position that was available at Iowa State and asked if I would be interested. It involved radioactivity because there was an institute for atomic research there headed by a Professor Spedding (*correct spelling?*). Spedding and his group had developed the material for the bomb. And in fact, not only developed, it was produced there. And so I was asked would I be interested in joining the group as a biochemist and I said I would. When I got there (I had been preceded by a certain amount of bargaining) and the Dean of Agriculture was interested in introducing the use of radioisotopes into agricultural research so I was put into Botany much to my...

EA: Horror.

SA: ...disgust and so I spent the initial years in Ames in the Botany Department, complaining all the time that I taught plant radiobiochemistry and I must admit I learned a lot; I had small classes because there were not many interested in it and had ample opportunity for research.

VM: So that's why you left Berkeley then in '48 in order to take up that position?

SA: Yes. It was a tenured position.

VM: So by the time you had left, had there been much progress made with the radioactivity and the chromatography that you had introduced?

SA: No, not much except that I had convinced Calvin and Benson to go in that direction. So they gave up their columns then and went on to paper chromatography themselves after that. It was the obvious way to go.

VM: Do you remember anything about your social life in Berkeley? (*This question was not recorded on tape.*)

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SA: Occasionally we would go out and play soccer, or touch football it was, touch football.. And I remember Lloyd saying that he didn't realise that as quiet a person as I was in the lab. could be so aggressive on the field. But that was the extent of our social activity.

VM: Did you not — well I guess you were married, and maybe some of the others weren't, so you had a domestic base; did people not tend to act in a social group, people in the lab.?

SA: I don't understand what you're...

VM: Did they spend their spare time together, did they socialise out of lab. hours?

SA: I wouldn't know.

VM: You didn't?

SA: No.

VM: Did they have things like Christmas parties or trips to the mountains or that sort of thing?

EA: That's due to institutional evasion in his head.

SA: It just wasn't done in those days.

EA: Well, it wasn't institutionalised. (*Passage here indecipherable*) ...and they worked late and independently and it would be difficult to have sort of social routines.

VM: Right.

EA: You know, it was in transition, Berkeley was. Before the war it was kind of a family institution vaguely. You know, you were part of a family. It was big but it was still family, you know, still people-oriented. But after the war they got these big grants and it turned into an institution. And so this was the end of the era, '46 to '48, before it became big business.

VM: I see.

SA: I'd have to ask you something — she reminds me. How did Koshland Hall get named? Did they — Dan Koshland was a staff member.

VM: I don't know. The short answer is that I don't know. There is a plaque in the hall which has got his picture and...

SA: Dan Koshland's picture?

VM: Dan Koshland's picture.

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SA: His family must have a tremendous amount of money.

VM: I think that may be the case. You know that he's Levi jeans?

SA: No.

VM: Does that answer your question to some extent?

SA: That tells me how he...

EA: ...where the money came from.

VM: Well, where it *may* have come from.

SA: Yes.

VM: Yes. I think that may be the relationship.

EA: There was a fellow — was he down there? There was a fellow whose family were in politics in Los Angeles. I think his name began with a "Y". I don't think he was part of that group in Berkeley. I think he was always...

SA: No, but there was a graduate student, Ralph Yount, who went to Pullman. He had nothing to do with our group.

EA: No, no. I think that the one I'm thinking of never left Los Angeles. I think you knew him down there.

VM: So after you left and you went to Iowa, did you continue working in photosynthesis?

SA: Oh yes.

VM: What was — in the same sort of way, in radioactive carbon?

SA: Oh yes. Of course, much of the field had been exploited so one could only do accessory things.. One of the things I did was to develop a method for degrading glucose to show the distribution of radioactivity within the molecule and thereby imply its mechanism of formation.

I think the most meaningful experiments were in transport. I think it's been assumed then, and probably rationally, the sugar made in the leaves was transported in some form down to the rest of the plant but my experiments were the first to actually show the transport. In other words, fed the C¹⁴ to a small area of a leaf and show how the sugar moved out of that area, in fact, to show indeed it was sucrose that moved and that it was transported from the leaf to the rest of the plant. Anybody who's ever gotten sap out of a tree would say "so what?" But this was actually the first demonstration.

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VM: So from being an original geologist through a chemist and a biochemist, you became close to a plant physiologist? And is that how you developed the rest of your career, in plant physiology?

SA: No. There was a gentleman from MIT by the name of Bear, Richard Bear, who took over as a Dean of Science for a year and who was aware of my complaint of residence in Botany. In all fairness, they had done all they could to make me happy there. They had rebuilt the lab. in Botany, made it specially suitable for work with radioactivity because there were faculty members in the department who were concerned about my use of radioactivity and that it would affect them in some disastrous way, so they were actually anxious to get me out.

VM: This was in Iowa?

SA: This was in Iowa State and so this lab. was built for me to allay their fears. But Bear saw the virtue of developing a Department of Biochemistry and he, in fact, initiated this department. I was one of four or five original members and we were stationed in Gilman Hall in the Chemistry Building. So I moved out of Botany at that time and Spedding, Frank Spedding, who was Head of the Institute for Atomic Research, then rebuilt part of Gilman Hall for use in that manner.

VM: But you didn't stay at Iowa State very long, did you?

SA: Twenty years.

VM: Twenty years, you said; oh really, twenty years?

SA: That's not too long.

VM: I'm sorry. So that was taking you now to the '60s, the late '60s. And where did you go after that?

SA: Well.

EA: He was seduced.

SA: There was a Professor of Economics who had a son in the west and who somehow arranged to get hold of the administration at Simon Fraser University and I received a phone call...

EA: You went to Boston College for two years.

SA: Oh, excuse me. Yes.

EA: As Dean of — what was it? Science?

SA: I was Graduate Dean and Dean of the Faculty of Science.

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VM: Boston College was in Boston, was it?

SA: Oh yes, and it's a Jesuit college. And we were never really quite sure why they wanted a Jewish lad to be their Dean of Science and so forth but it was never a problem. Never. They had taken down all of the religious items in the room, and you could tell from the shadows on the wall where they had been, but we were on the best of terms. And so, yes, I spent a couple of years there although I felt that I was really not quite part of the scene.

You know, in Boston you are not...you are a newcomer throughout your life, regardless of the length of time you spend there. Edith and I felt that it would be at least two generations, probably three, before you were accepted as belonging to the area and so when I received this phone call from out of the blue asking whether I would be interested in coming as a Graduate Dean to this new university in Burnaby, British Columbia. I was aware of Simon Fraser as an explorer; I didn't know that he had this university named after him. But Edith and I went out to check it out and decided to go there.

SM: This was in 1971, was it?

EA: Yes, '71 I think.

VM: And you stayed there for the rest of your academic career?

SA: Yes.

VM: And have you retired from it now?

SA: I've been retired. They changed the retirement rules after I left but if they hadn't I might still be there. You had to retire at 65.

EA: You retired at 67.

SA: Yes, I talked them into the additional two years.

VM: Well, I was part of an analogous system and I had to retire at 65. By the time I retired they were no longer interested in being talked into 67.

SA: You could only work part-time after that. In decreasing years, so that by the time you were 72 you could only teach a summer session.

VM: Oh I see — and they faded you out.

SA: Yes.

VM: It was like old soldiers fading away.

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SA: That's right.

VM: I think it's time to turn the tape over actually. So thinking back now after all these years — after all you left Calvin in something like '48 or so, that's a long time. How does the whole activity strike you now, looking back on it and what a group like that was able to achieve in the fifteen or so years that followed?

SA: I'm not sure what you're asking. I think there are many problems which are so complex that they have to be solved by groups rather than an individual doing it over a lifetime. In those days, this was the simplest approach: Calvin had a very good set-up and so the lab. work, I think, was highly successful and the group was congenial so that one could ask no more. I had no complaints whatsoever about that period.

VM: Do you think it was the sort of work, the sort of exploration and development, which required unusual understanding and perception or once the initial ideas were in place was it reasonably straight forward and all sorts of people might have done it?

SA: Oh, I think science is sufficiently logical so that if one is well-trained there are obvious paths to follow. One of the paths which we didn't follow, for example, which Calvin was able to perceive immediately when I thought of following in that direction, was to do the crystallography of the cobalt compound. I had learned how to crystallise the material and had taken a microphotograph of some of the more perfect crystals and when he saw them it was quite clear to him, as it was to me, that would be the next stage to follow so that one could have detailed knowledge of how the cobalt and the oxygen combined — what kept it. That may have preceded some of the work that is now routine but it wasn't in those days. So that I think that well-trained minds — and we were well-trained — have an obvious scenario.

VM: So once the original steps had been taken, really by Ruben in the use of radioisotopes to follow carbon fixation in photosynthesis, the idea was already around and available for people to run with.

SA: I think so. Mind you, there was a tremendous amount of work to be done...

VM: Of course.

SA: ...and it was not common in those days for chemists to work with living material, biochemical material, but they were getting into that system. It was, in fact, the beginning of real biochemistry.

VM: It's interesting that none of the plant biologists, or botanists in those days, actually did it: it was the chemists who did it.

SA: That's right.

VM: The botanical ideas of photosynthesis at that time were very primitive — of the carbon pathway, fixation.

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SA: Oh, I would say they were essentially unknown.

VM: Yes.

SA: As I mentioned earlier, what was being done in those days at the time by plant nutritionists were to ascertain the nature of the elements which were required simply for plant growth.

EA: (*Indecipherable*) ...Fay Morgan...in some little venture. I don't remember what it was.

SA: Really, I don't know about that.

EA: Yes he for a while was...

SA: Agnes Fay Morgan was an economist, a home economist.

EA: A nutritionist, I'd forgotten about that. But you see I was taking some courses in epidemiology and the best epidemiologist was in this home economics set-up at that time at Berkeley. She, by the way, had only a bachelor's degree but she was a full professor. Veedy (*spelling?*) I think the name was and a leader in developing epidemiology which just shows that (*indecipherable*) a doctorate. Yes, but you see that Berkeley as a whole, again, was a family — you know, the people mixed. They weren't compartmentalised as much as I think they are now.

SA: Well, Berkeley revolved around some institutions. One prominent institution was the Faculty Club. Business of all kinds was done at the Faculty Club and if you wanted to get something done that was where you went if you were allowed in. The centre of much of the activity was the bridge game and the bridge game was dominated by G.N. Lewis and Gerry Branch and whoever else they might invite to play.

VM: In the Faculty Club?

SA: In the Faculty Club. I don't mean this in a demeaning way; it was simply a social atmosphere for much of what went on. My benefactor was the head of Botany.

EA: Hoagland.

SA: No — Hoagland was Plant Nutrition. And for the moment his name escapes me. I was a reader in Botany, meaning that I read the examinations, and one day he received a complaint about my grading so he asked me to come in and explain to him how I had come to this grade. I showed him the examination and how I broke down the answers into parts and the numbers which I assigned to those parts, and he was thrilled that somebody had taken the time to be that concerned with grading and thereafter I could ask anything of him, so to speak. But he was responsible for my getting a Teaching Assistantship and for showing my value to chemistry and all of that.

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VM: Well, I think that's a very interesting story that you've told us. We had not been able to go back quite that far before we talked to you.

SA: Indeed, it goes one step further: I almost became the Provost of Santa Cruz — but that's another story.

VM: OK. That is for the next set of researchers. Thank you very much, both of you: we very much enjoyed being with you.