

## Chapter 27

# BERT M. TOLBERT

Boulder, Colorado

July 17th, 1996

VM = Vivian Moses; BM = Bert Tolbert; SM = Sheila Moses

**BT:** Today is July 17th, 1996 and we are in Boulder, Colorado at the home of Bert and Anne Tolbert at 444 Kalmia Avenue.

**VM:** Bert, can I start by asking what your early scientific history was and how did it happen that you eventually joined Calvin's group?

**BT:** Yes. I went to Berkeley after two years of a junior college in Idaho and took my bachelor's degree there in chemistry, completing it in 1942. Following that, I worked a summer at Standard Oil of California and then I went back to the University to do graduate work in chemistry. After approximately a year of graduate work the pressures of the war had increased and we were moved from our teaching positions in the Department to join the Manhattan District working on uranium chemistry.

**VM:** That was still in Berkeley?

**BT:** Yes, that was still in Berkeley. So I joined the group there and we did research and work in uranium chemistry. I even worked part of the time on a temporary basis setting up equipment down in Oak Ridge, Tennessee. That work continued very strongly, of course, until the atomic bomb was completely developed and was exploded. Then the war came to an end. At that time, then, we did not know what was going to happen and the work was continuing on. We were sort of sitting in limbo.

**VM:** Were you already associated with Calvin? Was he part of that work?

**BT:** No. I wasn't associated with Calvin, but my research professor, Gerald Eyde Kirkwood Branch — a very nice gentleman — was a close colleague of Melvin's and I actually used in my research Melvin's DU spectrophotometer. He had one of the very first of the instruments made by the Beckman Instrument Co. and so I knew not only Melvin but all of his graduate students and the rest of that group; we were in the Old Chemistry Building. I knew Calvin well and many of the other people that were

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in the Department at that time, in the period 1942-1945. What's not clear to me is exactly when, but I can remember that it was either late November or December that Melvin assembled together a group of four of us that had been working in the Radiation Laboratory and were sort of floating on loose at that time. They included Pete Yankwich, Jim (*James C.*) Reid, myself and, I think, maybe one other, but he did not stay with the group. He (*Calvin*) told us that he had talked to E.O. Lawrence and that E.O. Lawrence had given him the authority to create a new organisation (*in the Radiation Laboratory*) which would develop the manufacture of carbon-14 and develop ways of using it, the synthesising of compounds and to apply it to various problems in the biological and chemical world. He offered us basically all jobs, moving out of our old uranium chemistry job into this. Of course, I loved Berkeley very much and I was very happy in the environment and we all sprung immediately to this job. That was really the beginning of the group. So that I was in the initial formation of the group.

Melvin had worked with E.O. Lawrence on this, and John Lawrence had built a laboratory, the Donner Laboratory, and he had quite a bit of extra space in that laboratory. Because he (*John Lawrence*) wanted this development, and it's really not clear whether this was only E.O. Lawrence's pushing or whether John Lawrence also was pushing E.O. Lawrence because both of them recognised the importance of carbon-14 and the importance of developing this technique. So he (*John Lawrence*) assigned us space in the Donner Laboratory. This space had been used previously for the uranium chemistry work. So it had been set up as a chemistry laboratory space and I had worked there. Actually, I hardly had to change my location. I just continued on the same location and we just discontinued the uranium chemistry work and started in on this.

**VM:** This was the third floor of Donner, was it?

**BT:** This was the third floor of Donner Laboratory.

**VM:** How much space did you have?

**BT:** We essentially three-quarters of the main floor of Donner Laboratory plus a couple of counting laboratories downstairs in the basement.

**SM:** May I ask which year this was, when the group got together?

**BT:** I am trying to remember whether it was in December of 1945 so that we actually got started on it in January of February of 1946 — is that the correct time?

**VM:** Yes; that sounds right.

**BT:** OK — so let's go back to it. Therefore, this group was set up. Then there were certain problems that were almost immediately assigned. Pete Yankwich was given the responsibility for developing the counting techniques, the analytical techniques...

**VM:** For C<sup>14</sup>?

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**BT:** ...for  $C^{14}$  which was considered very difficult to assay because it had such a soft  $\beta$ -particle and most of the detectors which they had, which were aluminium-walled or things like this, would not pick them up, and you had to use a mica window one.

**VM:** Question: did you have lots of  $C^{14}$  at that time?

**BT:** We had only very tiny amounts of  $C^{14}$  at that time. I was assigned the responsibility of the synthetic part of the work. There was another person, I am trying to remember his name, who was primarily assigned the responsibility for making the  $C^{14}$ . Essentially what we were looking for was some material that we could put into the nuclear reactors and irradiate and it had to have a very large percentage of nitrogen and it had to have no strong neutron-absorbers in it. Beryllium nitride turned out to be one of the compounds that fitted that material. And so beryllium nitride was actually made over in...Gilman Hall in one of the upper floors and was canned into aluminium cans and these aluminium cans were sent to Oak Ridge where they were irradiated in a nuclear reactor. That doesn't sound right because I don't think there were any nuclear reactors in Oak Ridge. They were sent somewhere and they were irradiated in as high a neutron flux as they could get for a finite length of time. And then they were sent back to us. In the very early days we had the responsibility of taking these aluminium cans, which were very highly radioactive, cutting them open, getting the beryllium nitride out under conditions in which you could catch the gases which were coming out, and then dissolve the beryllium nitride and collecting the  $CO_2$ , precipitating it then as barium carbonate.

**VM:** Two questions. Did you remember who it was in Gilman Hall who made the beryllium nitride?

**BT:** I can remember it but I'll have to think about it. He became a professor over here in Kansas or Nebraska.

**VM :** Was he one of the Calvin group actually?

**BT:** He really was part of the group but not directly. He was much more directly attached to Connick and the people in Gilman. That was more of a co-operative effort. (*Editor: might this have been Denham Harman?*)

**VM:** The second thing. You said that you started out with most of the third floor of Donner. What about equipment? Did you have any, did you have to build it up, did you have to make it?

**BT:** All we had was the standard kind of equipment that was left over from the uranium assay work — that was beakers, pipettes and...

**VM:** You just inherited that?

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**BT:** Yes, we inherited those. Fundamentally, we had to start figuring out how to make these. One of the reasons, I suppose, that I got into the synthesis work was that we were working with gaseous CO<sub>2</sub>. When I had done my (*PhD*) research I had constructed several vacuum lines. That was sort of required of all graduate students in chemistry at that time, to learn something about vacuum line techniques. It was relatively easy for me to start setting up apparatus, working with liquid nitrogen, traps, moving stuff around.

The first efforts were directed basically toward learning how to count carbon-14 or detect it, learning how to make it so that we had available materials, and then learning how to handle it and synthesise it and put it into forms that could be useful.

The photosynthetic work actually didn't start at the beginning of the group. These three groups started and the photosynthetic work didn't get seriously underway, as far as I remember, until Melvin brought in Andy Benson who had been excluded (*from the Radiation Laboratory*) because of his status during the work as a conscientious objector. He joined the group and was assigned, I think almost from the beginning, a space over in the Old Radiation Laboratory.

**VM:** Now Andy had been associated with Ruben and Kamen before the war, or before things got tough in the war, and he had actually been part of the early photosynthesis work. You mentioned earlier in conversation that you also knew these people, Ruben and Kamen.

**BT:** Yes.

**VM:** What is your memory of them and anything of the photosynthesis that they did. Did you observe it? Were you part of it?

**BT:** No. I was not part of it. I knew Ruben because he was my PhD advisor when I was going through that work. I liked him very much; he was a really nice man and wonderful to work with. Unfortunately, he was poisoned by a phosgene experiment that he was doing and I remember very distinctly at the time because we were right there in the building.

**VM:** You were there when it happened?

**BT:** Yes. But I did not actually see it. I was in one of the rooms (*in Old Chemistry*) and was told almost immediately that this flask of phosgene had broken apart and, of course, the warm materials going into the liquid nitrogen caused the phosgene to be blown up into his (*Ruben's*) face, and he got a breath of it. Sam, knowing the toxicity of the materials, knew that he was in very serious trouble immediately. He walked outside, told somebody to get help and laid down on the grass to wait for help and they took him up to Cowell Hospital, which was only about half a block away and, unfortunately, it was a fatal dose. His lungs filled with water and he drowned, basically, as I remember it. They commented his lungs just filled with water, they couldn't do anything about it and so he was lost at that time. That was the end of that work.

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This was relatively early on, by the way. This was while I was still a graduate student, in my early graduate student days, before I had started working on the other laboratory (*i.e. uranium problems*).

**VM:** So his death, while a personal shock, did not interfere very much with your subsequent thesis work?

**BT:** No, because I was working on absorption of triphenyl methane dyes under Branch, and using Calvin's equipment, and it had really no effect whatsoever, except that I lost a very fine and very nice mentor. I only knew indirectly of his work on the photosynthesis that was going on at that time which was talked about. Then, of course, it was continued to be talked about, because it was a very exciting discovery that he had made, and so then Melvin, recognising the importance (*of that work*), had planned a "do" on that (*i.e. planned to continue it*). He actually didn't discuss that in our initial conversations, that he wanted to go into the photosynthesis work. He merely said that we wanted to develop the use of carbon-14 as a tool in biological and chemical studies, and that we had to figure out how to make it, how to count it, how to synthesise it into compounds, and it wasn't perfectly clear that his long-term objective was to go into the photosynthesis work until he got hold of Andy Benson and brought Andy in and said "OK, let's establish the group to do the photosynthetic chemistry".

**VM:** He must have been thinking about it.

**BT:** I am sure he was thinking about it. But it did not come up in this initial conversation.

**VM:** So there were the four of you, then, the original founders of the group, working in Donner in these various areas. But you didn't stay just four people for very long, did you?

**BT:** Oh no., because there were plenty of funds available and so almost immediately, why we started to get assistants. I wish I had more early Quarterly Reports and that so I could go back and see exactly who were the people in the very, very early days. But, certainly Rosemie was brought in relatively early.

**VM:** That was Rosemarie Ostwald — or subsequently called Rosemarie Ostwald.

**BT:** That's right, and she was a refugee and Melvin brought her in. Melvin had a lot of very good contacts and he was quite perceptive on who was capable of doing interesting and good work in terms of the hiring. During this time I was gradually assuming a certain amount of administrative responsibility with respect to the group and, as you will notice, I was writing the Quarterly Reports in 1948, which is three years later — but I had been writing reports early on — and I guess that I assumed it because it was an activity that I enjoyed and carried on. And so as time went on, I became more and more involved in the administration of the group but, at the same time, I carried on a very active research programme and that sort of was a thing that I liked.

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**VM:** Can we talk a little bit about these Quarterly Reports because, sitting on the table here, there must be two or three dozen of them from as far back...I think the first one says '47 doesn't it?; '47 to-November '49, and then they go up much later.

**BT:** Here's two more boxes over here!!

**VM:** You must have well over 50 of these things. How did they start and what was their purpose and who do you think read them, apart from you guys who wrote them?

**BT:** Well, that was quite clear. They were required by the AEC (Atomic Energy Commission) office in Washington, DC which was supplying the money to us. They wanted these Quarterly Reports. I think in addition to that, E.O. Lawrence himself also wanted the Quarterly Reports so that he would know what was going on. The circulation, as far as I know, was to the administration of the (*Radiation*) Laboratory and to the Atomic Energy Commission in Washington, DC, to the Division of Biology and Medicine.

**VM:** And you think people read them there?

**BT:** Oh yes, I'm sure that they read them.

**VM:** Did they ever comment back to you about anything that you had done, or had written?

**BT:** Not in specific (*terms*) but they did visit the laboratories on a regular basis.

**VM:** Were these classified at the time?

**BT:** No, they were not classified and our work was not classified. Calvin was rather insistent from the very beginning that our work be not classified. It is true that all of us had security clearances and the new people that we were hiring at that time had to have security clearances. But the work was not classified, the laboratory was open, we could have foreigners, as you know, working in the laboratory...

**VM:** Although even there, there was some sort of scrutiny undertaken and they were given special permission...

**BT:** Perhaps.

**VM:** Yes, there was something of that sort.

**BT:** OK, so you remember that. I remember that the security clearances was something that we had to always abide by. In fact, there is almost a funny story on that. We were getting along fine; I had one assistant that was working in the biological chemical area, and, finally, the security people came to me and they said "we don't really like this lady working in your group; we would like you to let her go". And I said "but I don't want to, she's doing OK and what's the trouble?" They hesitated to tell me, but

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finally they did. It came out that she had a lesbian relationship with another lady and they felt like this was a security risk. But I think I was rather adamant and wouldn't fire her. So finally we reached an agreement in which she would have no access to any kind of classified material and we would avoid anything that contacted it and she was retained. That was, for instance, one of the minor incidents that arose.

**VM:** Don't tell us any secrets, but was there any classified material actually involved in the group?

**BT:** Yes. I had a number of documents relating to the uranium chemistry (*project*) and I had available to me a certain amount of information relating to the uranium chemistry. Then, on various kinds of trips that I went on, to see other national laboratories and coordinated with them. I visited Oak Ridge, Los Alamos and all of these place, even up in Washington to the Richmond laboratories. In all these cases, why I heard and got more information about that. So I had documents but not everybody did. Most of the group had no security documents. (*Editor: Note that Calvin himself also had a considerable quantity of classified documents relating to his own war work in the Manhattan District and the plutonium extraction process.*)

**VM:** That was the only classified material that you came across. It wasn't directly related to the work you were doing (*on the carbon-14*).

**BT:** None of the work that we were doing was considered as classified.

**VM:** If I look back at the earliest report that you have (*1947*), I think you mentioned (before we started taping) that this one dated, well '47-November '49 — but anyhow, this particular one is September/October/November '49, and you think this might have been the first or...?

**BT:** I think that there's an earlier one and I would like to find it. I'm almost certain that there is. I'll have to go back through some of my boxes and see if I can find another one of these black binders.

**VM:** You think that these reports started fairly early on in the life of the group?

**BT:** The reports started very early on.

**VM:** They were, of course, hand typed and corrected. I notice they are on the old erasable paper which is no longer used.

**BT:** It was a terrible job for Norma (*Werderlin*) and Marilyn (*Taylor*) to type these things up. They had basically mechanical typewriters and they had to hit them as hard as they could to try to get, I think, the 7 or 8 copies that we could make and if they mistruck one, they had to erase down through the 8 of them, using protectors. It is incredible the difference that photocopying and now the computer has made in our ability to transmit information. I think I kept them (*these reports*) if nothing else because they are really such rare copies. There were 8 copies of these things made and this is one of the 8; where the other 7 copies are, I don't know.

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**VM:** I think it would be very nice if these were properly archived eventually so that they were not lost to history.

**BT:** One of the things that is really relatively interesting is that these documents here, and then a relatively simple document prepared once a year for the Atomic Energy Commission outlining the various groups and what their goals were and so forth, was all that we needed in order to get the money that was being used by the group, which was a substantial sum at that time. The lack of paper work associated with the funds in this work was an incredible pleasure after having lived through a later professional career where you had all kinds of grant applications and an enormous amount of paper work to put out.

**VM:** It tells a lot about the relationship that must have existed at that time between the Washington people and the Radiation Lab. in Berkeley, and, I guess, Lawrence in particular and the hierarchy down from him.

**BT:** One of the things, though, that was very interesting, was that the people in Washington, DC were sophisticated scientists at that time. Many of them were not the classical bureaucrat. They were good scientists. They made sure that they got people who were competent and many of them — I even spent a year there, you know one time, working in the Biology and Medicine Division reviewing grants and contributing to that. They tried to get outside people in so that they wouldn't keep a bureaucratic perspective. They were very nice and they knew what was going on and they appreciated it. And they had the money. Congress had given the money and very generous with it.

**VM:** So, you had easy personal contact with these people, did you? You knew them yourself? If you needed something, you could call them and discuss it.

**BT:** Yes, that's right.

**VM:** The names on this very first sheet, as it were page 1, Act I, Scene I, Calvin and you — those were the first two. Some of these other people; I didn't know them, of course — Pat Adams?

**BT:** Pat Adams was a lady who got her degree in Berkeley, was a very charming lady and did synthetic work. I can even tell you a couple of stories about that which might be funny. Because we were working with radioactivity and all that, we always wore white lab. coats, we were required to wear white lab. coats. Quite often I would be down in the laboratory and would be talking about some given problem and we would want to write out some formulas or something, and I might not have a pencil, and they might have a pencil and I guess at least once, if perhaps not more times, why I'd grabbed a pen out of her pocket and helped her write down that. Well, I found out sometime about a year or two later that Pat had made the comment "just as long as he only grabs the pencil!" She was a very charming lady and I liked her. The other thing that was sort of interesting and almost sad about Pat but really, in the long term, it had no (*effect*)... One of the people there is Art Fry; and Art Fry after he finished is a



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graduate student and was working on the decomposition of acetyl peroxide which is a very unstable compound and can explode violently in a detonation type of explosion. He is now, by the way a retired professor of chemistry at the University of Arkansas in Fayetteville. He had used one of the vacuum lines that we had used and had distilled accidentally over, maybe he knew but he didn't realise the consequences, a small amount, probably like a half a gram, of the acetyl peroxide into a trap and then had taken off the liquid nitrogen trap and that was finished for the day. And so the next day, why Pat Adams came in and was going to use the laboratory for something else and she reached up and grabbed the trap to take it off to clean it and the acetyl peroxide detonated. Now, when it detonates there is a shock wave that goes out at greater than the velocity of sound and the glass just shatters into a million pieces. She got a few small cuts here and was not seriously injured but it was really a frightening experience for us. I remember that John Weaver, who was an MD, quickly came up from one of the lower floors (*of Donner*) and administered whatever (*was necessary*). I don't think that the cuts were so bad that they caused any disfiguration. And, thank goodness, none of them hit her eyes; I suspect that she had glasses on but she did have some cut marks on her face.

**VM:** Did people wear safety glasses in those days?

**BT:** They weren't required to. That's why I hesitated whether she had safety glasses on. It was considered good as a chemist to be nearsighted in those days, so that you had to wear glasses all the time, because it was safer.

**VM:** People, of course, in those days were much less safety conscious altogether. They used glass pipettes (mouth pipettes) and nobody seemed to worry too much.

**BT:** Yes and no. In our laboratory whenever we were pipetting with radioactivity, I introduced very early the concept of using a vacuum suction tube or a suction bulb to pipette that. Because when we were working with uranium chemistry earlier, why we never used mouth suction to pipette the radioactive materials. So even at that time we did not use mouth...I mean, we used pipettes but we wither used a suction bulb or we used a vacuum line, closing it off by the amount of air that whistled by.

**VM:** Did you have protective shields when you worked with radioactivity, did you work behind, stick your arms around shields?

**BT:** No, we did not. Remember that carbon-14 is a very soft radiation and there is essentially no radioactivity which goes outside of the flask and only if you have a large, large amount of radioactivity do you get something called "Bremstrahlen" which are soft x-rays which will go outside of the flask. The only hazard, basically, in working with carbon-14 was the possibility of ingesting it or breathing it, so we used gloves to a limited extent but not completely. No. we did not work behind shields; we did work in hoods.

**VM:** What about safety measures? There must have been...No, perhaps I should ask and not just say "there must". Was there a safety organisation associated with the work?

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**BT:** Yes. There was a safety organisation and, after our initial efforts, which were relatively simple and which we had created over the first three to five years (a very extensive network of vacuum lines, and so forth), the safety engineer — I am trying to remember his name and maybe I can't (*Editor: it was Nelson Garden*) was looking at it and he said "I don't like at all, all of these vacuum lines working with radioactivity in relatively large quantities". He proceeded to see what we were doing and he dreamt up a system in which we created boxes, basically on wheels, in which the vacuum line was held into it, it had sliding doors in the front and it had an exhaust fan to exhaust the material out from it. And so then, after about the first four or five years, why we started to work with these vacuum lines in boxes, which was a very good technique. But in the early days we worked with it right out in the air.

**VM:** Were there any serious accidents?

**BT:** No. There were no serious accidents. We never had any evidence of anyone getting any contamination of radioactivity. We tested to a limited extent the urine and the breath of the individuals.

**VM:** Did people wear film badges?

**BT:** Yes, interestingly enough. We wore film badges and they were all negative always because none of the radiation of carbon-14 would go through that. That was considered as a formal precaution against law suits more than anything in terms of a real health safety measure.

**VM:** OK, to come back to this list of people: What happened to Pat Adams, do you know?

**BT:** Pat Adams eventually married somebody, a nice person, raised a family, and then she went back into some kind of chemical work in the area. (*Editorial note about Pat Adams: she was married when she came to the group.*) Winifred Tarpey, who...

**VM:** She's not on that one but she's on some of the others.

**BT:** ...she's on the next one, who was hired at almost the same time, she might have been in the other part of the group here, let me see...do we have Winifred Tarpey?

**VM:** She's in one or other of those sections.

**BT:** Yes. Actually, Melvin hired most of these people (it wasn't until later that I began to interview them) as recent graduates from Berkeley in Chemistry who were looking for jobs.

**VM:** Did he discuss with you, and with the other three of you who started the group up, how it was going to expand or did he just walk in one day and say "this is so and so who is joining the group"? How did this happen?

**BT:** There was discussions about what our needs were, whether we needed more people. It was recognised, for instance, in this period of time, that we needed more people to do

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synthetic work. We have various things that we were trying to make in good quantities and good yields. At that time, the carbon-14 was considered very expensive, and it was; I think we were paying \$32/millicurie when Oak Ridge started to make it. And although \$32 seems like little money now, that was in the days when you could buy a new car for \$600.

**VM:** So, you actually had to pay for this stuff even though it was made internally in some way?

**BT:** Yes, out of our budget. After they started making it in Oak Ridge by radiation, either of ammonium salts or the beryllium nitride irradiation, why then we gave up the effort very quickly and happily. Because we didn't like working with those hot aluminium cans that had been in the reactor.

**VM:** And the price fell a lot, I suppose.

**BT:** It continued at that price for a long, long time. It was a significant item in our budget. In fact, we had to transfer funds to them.

**VM:** Initially, I suppose, all the hot carbon came to you as barium carbonate.

**BT:** Yes, it all came as barium carbonate.

**VM:** You were among the pioneers of converting this stuff to anything else.

**BT:** That's right.

**VM:** And indeed collectively — I don't remember who the authors were — there was a book on isotopic carbon which included a lot of this information.

**BT:** That's right. And I have a copy of the book downstairs if you want to look at it.

**VM:** You were one of the authors?

**BT:** Yes, I was one of the authors.

*Pause to fetch the book*

**VM:** We now have the book ("*Isotopic Carbon*"), and, as you pointed out, \$5.50 was the original price, published in '49! It must have been written...if it was published in '49, it must have been written in the period '47-'48, I suppose.

**BT:** Yes, it was written basically in '48.

**VM:** All of this material, all this experience had already been accumulated pretty quickly.

**BT:** That's right. We moved forward very, very rapidly on all of these phases, as rapidly as we could. We had excellent resources in terms of money and people. As you will

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notice, the authors on it are the three original group members — namely myself, Pete Yankwich and Jim Reid and then Melvin — and Charles Heidelberger had joined the group, after it had gotten started, on the biological end and he contributed enough to it and wanted to be part of the book. So he joined the book group's authors.

**VM:** This book — of course it's in the record, but just to get some information here — this book has a lot of information on technology — how you do things — with respect to carbon-14...

**BT:** Yes, that's what it was.

**VM:** ...and then it also has things on synthesis and degradation methods actually of chemicals, doesn't it? It was hundreds of pages...a major effort and I think it has actually become *the* classic in the field.

**BT:** Certainly for something like ten years after that, this was the standard reference work to it. Within ten years after that there were major new documents produced.

**VM:** Can I ask you something? When it came, a little later on, to measuring the radioactivity on chromatograms, were those pieces of equipment things that you did as well, those Geiger counters and those flow...those Scott tubes (do you remember those?) with mylar, gold-sputtered mylar windows?

**BT:** The gold-spotted mylar windows were commercial developments. The initial developments that we did in the group: Pete Yankwich made them (*the windows*) by splitting mica until it became very thin and evacuating them and filling them with the counting gas.

**VM:** Did he glue the mica onto the..? How did he attach...?

**BT:** He did it with hot Apiezon wax.

**VM:** So he could get it off again if the windows got broken?

**BT:** Well, the windows got broken. And, yes, they could be replaced and were replaced.

**VM:** At some stage you moved then to the mylar windows which were just held on with O-rings or rubber bands.

**BT:** They could be, or they could be also waxed on.

**VM:** When did that happen, do you know?

**BT:** That was later. By the end of the forties, you see, an enormous amount of this technique had been developed and formalised and incorporated into the industrial world. All that mylar stuff was commercial tubes.

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**VM:** But the design of the Scott tubes? I don't know who Scott was — but do you remember those with gas flow, hooked up to the helium cylinder...?

**BT:** That was a development, I think, that New England Nuclear, no — I am sorry, that's not right. That was a development that was made by some people at either the Argonne National Laboratories or at Brookhaven, or a combination of those.

**VM:** Because the counting chambers themselves were clearly not commercial. They were home-made in some sense, whether in Berkeley or some place.

**BT:** We were still making them. I take it that you used some of those.

**VM:** Oh, indeed I did, yes, together with all the other people.

**BT:** This ("*Isotopic Carbon*") also contained determination of counter efficiency. Pete Yankwich would have written that one, statistical treatment of counting data, Pete Yankwich would have written that. The work on flow and vacuum systems, manometers and gauges, that I did. Stirrers for vacuum systems, which we didn't know how to do, that was my developments there. How we went about doing these various things: I have also some much later publications, big, big books of all the synthetic methods which were accumulated. Those actually came out of the Los Alamos National Laboratories where they accumulated (*these techniques*). Remember, that after we got started on them, it was probably less than a year before groups picked this up at Oak Ridge, at Argonne National Laboratories, at Brookhaven National Laboratories, and somewhat later, but still in the same time frame, it was picked up by work at Richmond (up in Washington) Laboratories — Richland, I'm sorry —and down at Los Alamos.

**VM:** So initially, all the  $C^{14}$  was really confined to government labs., was it? It was only later did it percolate to others?

**BT:** Well, we would synthesise materials and give it to other people...

**VM:** ...in the university structure?

**BT:** ...in the university structure. A lot of the stuff that we made went out to them for their uses.

**VM:** Incidentally, before we finish with the "*Isotopic Carbon*" book, what did Melvin do for the book? He contributed a great deal to it. He wrote parts of it and he was the one who brought the idea to us and said "look, we've got to get a book out covering these various parts". I can't tell you offhand just exactly who wrote all parts of this material.

**VM:** But he wrote some of it as well?

**BT:** Yes.

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**VM:** So back to these names, then; we've got a lot of them. Bartsch: who was Bartsch?

**BT:** (Bob) Bartsch was a graduate student and I don't know where he is now.

**VM:** Art Fry?

**BT:** Art Fry I just described.

**VM:** H. Gray? Well, if you don't remember, you don't. The next one is a D. Hauptmann?

**BT:** As I remember, Hauptmann was an older man who was visiting the laboratory on sort of a special postdoctoral type of status or a sabbatical status. I don't remember more about Hauptmann. (*Editor: He was from São Paulo, Brazil*).

**VM:** That brings me to another question. This group started with four of you and then it grew to start with by Melvin hiring people. When did the graduate students and the postdocs. begin to be part of it?

**BT:** They were all Melvin's graduate students because none of us had academic status and so all the people who came in as graduate students were associated with Melvin. Melvin outlined the projects for them. They, in general, did not do the synthetic work which was considered not worthy of a thesis necessarily. It was necessary work that had to be done. And Bob Bartsch and Art Fry and the rest of the people were Melvin's graduate students.

**VM:** But working in Donner with you?

**BT:** Yes, working in Donner with us. I think that my name is even on one of Art Fry's papers or something else like that because I worked with him rather closely.

**VM:** Since you were there all day every day, as were the graduate students, you in fact must have been working with them.

**BT:** In a way, you know, we were like super postdoctoral assistants working with the graduate students.

**VM:** Incidentally, one of the things was, when you were hired by, I guess, the Radiation Lab. was your formal employer,, you were hired simply as a permanent employees without time-limited things, as far as you know?

**BT:** I was hired by the head of the Chemistry Division, as I say, and I think it was in January of — let's see, I started my graduate work the fall of '42...in the fall of '43...in January of '44 I was hired by a guy by the name of Prescott into the Chemistry group at the Radiation Laboratory. That was an entirely different group, that was a group that was working on the uranium chemistry.

**VM:** This was just an open-ended employment.

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**BT:** It was an open-ended employment. We actually didn't ask whether it was permanent or anything else. It was a wartime job. You see what really happened was that we were graduate students and we were exempt from being drafted at that time. As the war efforts intensified, and as the need for chemists in the Manhattan District increased, why, then there came a time when they said "OK, you can't be a graduate student any longer; you've got to go to work full-time" for the Manhattan District...

**VM:** ...even though you hadn't completed your PhDs?

**BT:** ...even though I hadn't completed a PhD. Actually, it was interesting that from that time I worked two full-time jobs. I worked a full 55-hour job in the Radiation Laboratory on the uranium chemistry and in the evenings and on weekends I would work on my PhD thesis research down in the Chemistry Building. That was really no problem because there was nothing else to do. There was a war on, there was a limited amount...you couldn't travel, you couldn't go anywhere and it was exciting.

**VM:** Other people were presumably, were in a similar position, so there was a group of you.

**BT:** That's right.

**VM:** OK; so that's as far as Hauptmann. Do you remember any more of these? This one: S. Hughes? (*Editor: Probably Dorothy (Hughes) Johnson, BS chemist in the Donner Lab. who did synthetic work prior to Pat Adams.*) If you don't, let's not waste time.

**BT:** Saburo Ikeda. I just remember the name. Jorgensen (*Editor: Eugene Jorgensen had the distinction of being the only member of the group who was ever murdered!*), Bernie Neivelt was a postdoc.-type of gentleman from Switzerland and I'm sure that you could run him down. Rosemie (*Rosemarie*) Ostwald was a very nice lady who had taken a PhD in organic chemistry from Karrer in Zürich and was a refugee from Vienna where her family had been. Melvin had met her somewhere or other and she joined the laboratory and she was a great contribution. Bob Selff, I think was another graduate student, but I would have to look. (*Editor: Bob Selff was a BS chemist like Pat Adams.*) Yvonne Stone, I can't remember her. (*Editor: She was a BS chemist who worked with Pete Yankwich in the counting room and on counting procedures/developments.*)

**VM:** The next thing that occurred to me that I would like to ask you is the communication inside the group. By the time I got there, eight years later, the Friday morning seminars were well developed.

**BT:** They were developed then; they were going.

**VM:** From the beginning?

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**BT:** From the beginning. From the beginning we had weekly seminars and everybody met and Calvin would ask various people to talk about what they were doing and give reports. You didn't prepare for them at that time; why, he just went wherever he wanted to. That kept the group together. That meant that everybody knew what was going on and heard what was going on.

**VM:** By that time, by the end of a few months at any rate, you had, what?...a dozen people, or some such number as that? It had grown from your original four.

**BT:** I said that we had this organisational meeting in late November or December (*of 1945*) and we got started on this thing here at...I would say that, yes, we probably had...maybe we had a dozen people. After all, we had Calvin's graduate student group...

**VM:** ...and there were several of those?

**BT:** Yeah, there were several of those. And as the new graduate students...see, at that time, there were a lot of GIs coming back coming back from the war, and they were very good, and very hard-working and older people. So Calvin was adding new graduate students to his group at a pretty good rate.

**VM:** If you try to remember what one of the early seminars looked like, did you have a room full of people or were there just a few people sitting around the table?

**BT:** OK, well that's a way of finding out how many people...my memory of it is that there would be the equivalent about two or three library tables and they would be pretty much surrounded. I would say that there must have been by the end of the half-year at least a dozen or more people.

*(Tape turned over)*

**VM:** We talked about the period which must have been the beginning of 1946ish, after the group started and there these dozen-ish people. And you were all in Donner. Eventually there was a second leg to this group developed in ORL.

**BT:** That's right.

**VM:** How did that happen?

**BT:** Basically, as I remember, Melvin hired Andy and they had to find some additional space for it ( *i.e. for the photosynthesis work*) and the cyclotron group had been contracting over there and there was space in ORL. And so E.O. Lawrence gave that space to Melvin, and Melvin and Andy then started to rebuild it immediately and equip the laboratory and get started on the photosynthesis work. Andy spearheaded and drove that effort.

**VM:** So Melvin had the resources, presumably, at that stage, to equip another building.



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**BT:** Yes, no problem.

**VM:** And put people in it as well?

**BT:** And put people in it: yes.

**VM:** Now, once that began to happen, what happened to the relationship between you and the others in Donner and this other group in ORL?

**BT:** In the first place, the relationships were very cordial. We were all part of the same group. Marilyn and Norma and me in the office and Melvin integrated the entire group together. We met weekly together for the research conferences so they knew just as much what was going on and listened to the conversations as that. The only thing was since we were not specifically excited by that work, because we (*in Donner*) were trying to solve our own problem, we listened but didn't absorb it on the level that we would (*have done had it been our research problem as well*).

**VM:** The group remained, you think, one group, administratively and financially?

**BT:** Yes, not only did it remain one group administratively, socially, and remember that that group (*in ORL*) was calling on us for counting techniques and for the barium carbonate or the CO<sub>2</sub> that would be prepared in our group (*in Donner*) and all the rest of the activities like that.

**VM:** There was a period, which must have lasted for some 16 years or so, from the beginning of ORL, which I guess was '47-ish, or something like that, until they all moved into the round building together in '63, when there were actually two groups, or, at least, two locations. By '56, when I got there, there was some feeling of difference between the two groups. The Donner people...you didn't see them so often, if you were in ORL, you knew them, but you didn't know them as well.

**BT:** That's right.

**VM:** Did this feeling start earlier? Did you feel the same thing from Donner?

**BT:** Well, we never felt like they were a separate group; we merely felt like they were a separate phase of the research. After all, by that time, Ed Bennett was going his way relatively strongly and other kinds of work was going on (*in Donner*). I was working extensively with the MDs in Donner, trying to do respiration measurements of CO<sub>2</sub> with a guy by the name of Nathan, N.I. Berlin. We were using labelled glycine to determine the turnover time of the red blood cells in the body. We were just different types of approaches. But Melvin, by holding the research conferences and everybody going every week, made sure that all parts of the group remained adherent. I felt even at the time that I left, which was in 1957, that although there was distinct groups, that socially they were highly integrated.

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**VM:** From your position in Donner, and conscious of the fact that you were one of the founders and therefore you were more likely to have known everybody than the later joiners...

**BT:** That's true, that's true.

**VM:** So, the young people who joined (*Donner*) in the fifties, say, would not have known the ORL people as well.

**BT:** That's right. They would have gone over there and they would have had relatively little contact with them. My wife, Anne, was there (*in ORL*) as a graduate student and I think that she hardly knew the people over in Donner because she was a beginning graduate student and didn't go over to where we were. She was having strange enough surroundings to be in where she was without coming over to this other building full of MDs.

**VM:** As you say, socially they were very well integrated.

**BT:** Yes. A lot of that was due to the efforts of Gen Calvin and Marilyn who was a very strong force in keeping them integrated, and Rosemie (*Ostwald*). There were organised skiing parties and other kinds of parties and Christmas parties and everybody got together for these social interactions which was very important in creating a continuing bond between the various segments of the laboratory. I have lots of pictures of some of these old parties and they are a pleasure to behold.

**VM:** Calvin was clearly the scientific originator and leader of the thing, but he was not really a manager, was he?

**BT:** First, I think he really didn't want to waste his time doing the managing. I think that he could certainly have done it. I had an aptitude for handling the management affairs without getting into too much trouble and so I gradually assumed more and more of this kind of responsibility. It was relatively early, almost from the beginning, that I started to write the quarterly reports...or not write the quarterly reports — I got pieces of paper from each one of the various segments of it (*the group*) and then tied them together, put introductory notes on them. You notice I say here (*on the quarterly reports*) "edited by me (*B. M. Tolbert*)". In other words, I'm not claiming that I wrote this thing here but I merely edited it and the source of the various pieces of information came from the individuals.

**VM:** What about things like the budget. Did you have to...you said that there was a relatively simple way of getting money from the AEC, but nevertheless it had to be done; was that your responsibility?

**BT:** Yes, that was my responsibility. I handled the budget and basically, between Calvin and I, why we handled the salary increases and adjustments to the salary and all the paper work that went on the administrative level basically went through my office. Melvin was smart enough to know that he wanted to concentrate on the science and I

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was willing and able to handle this other kind of work without getting into too much trouble.

**VM:** He signed things that you put in front of him, presumably, which were relevant?

**BT:** Yes, that's right.

**VM:** What about equipment and supplies? How were they purchased; who was responsible?

**BT:** They were purchased by purchase authorisations which went through our office and which I signed.

**VM:** It was a relatively simple matter, wasn't it, for small amounts of materials.

**BT:** Oh, yes.

**VM:** On the nod, more or less. What did you do about bigger pieces of equipment?

**BT:** I don't remember where it was that we couldn't (*sign things ourselves*), but I don't remember having any problems at all with buying almost any kind of a piece of equipment. Geiger counters and things like that were some of the major pieces of equipment; vacuum glassware, there was more than adequate funds to take care of everything. I think if we had gotten into trouble and needed more money that E.O. Lawrence would have footed the bill and said nothing about it. So at no time do I feel that we were really constrained on the basis of budget.

**VM:** It was a most favourable situation which hasn't lasted, unfortunately.

**BT:** (*Laughter*) It was a wonderful situation in that respect to have interesting scientific problems to be tackling and sufficient resources to develop them at the maximum rate.

**VM:** Do you think that sort of group — that group — could have developed if the money not flowed as easily as it did?

**BT:** I think so. I think it would have been somewhat different and it might have been distinctly slower and things like that. But I think that the breadth of interest that Melvin brought to it...remember it took a wide breadth of interest for Melvin to be interested in everything all the way from the biological type of work that Heidelberger and Ed Bennett, and really interested in what they were doing, and then the synthetic work which we were doing and then the photosynthetic work on Andy's part. At that same time, may I remind you, he was also deeply involved yet with some aspects of chelation chemistry and had written some books on it and was continuing to be active in that. That was going on not through the group but all but through his chemistry (*department activity*).

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**VM:** One of the points in relation to this — we were talking a few days ago to Sam Aronoff who said that he came back on visits to the group fairly late on in his period, that is to say, I suppose, late forties and maybe even early fifties — (*and Calvin was*) still working, I think he said, on cobalt oxygen chelates.

**BT:** Yes.

**VM:** Was that going on that late? That was, I think he said, for possible submarine use to supply oxygen?

**BT:** Yes, that was probably going on. But this part of Melvin's (*work*) had no activity in that (*i.e. no connection with the Bio-Organic Group*). That was strictly over in the basement of the Old Chemistry Building. And I didn't think that very much of that was going on, any more at that time. It was mostly paper work.

**VM:** So Calvin had activity in the (*Old*) Chemistry Building which was separate from ORL and Donner?

**BT:** I don't know the details of it but I suspect there was some. A graduate student, a few graduate students over there. After all, during the war effort he had a relatively large ONR contract on this work and had quite a large group of people that were engaged in synthesising all different versions of these (*chelate*) compounds and looking at their oxygen absorption properties.

**VM:** That was in the Chemistry Department in Berkeley?

**BT:** That was in the Chemistry Department in Berkeley. In fact, down in the basement of the Old Chemistry Building, because that was where Gerald Branch's laboratory that I was occupying, was down there too.,

**VM:** Did you see a lot of Calvin? Did Calvin come into Donner very often?

**BT:** Yes, he came into Donner quite often and then I would go to his office.

**VM:** Did he come into Donner as a breeze in and breeze out, or did he spend a lot of time, did he go through people's individual results?

**BT:** No, he didn't. That basically was done at the weekly research conferences when those things were covered. In terms of our work, no. He might come through occasionally to see what we were doing and keeping track and give whatever suggestions that he could, but basically we were carrying that work on ourselves.

**VM:** When it came to questions of publication in the open literature, you made decisions yourself, did you, about publishing, that this was the time to write something up?

**BT:** Yes. And if we wanted consultation and advice, why we'll talk to Melvin about it, too.

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**VM:** Did he insist that his name went on everything as a matter of course?

**BT:** No, not at all. If you will look over here, for instance, in this volume I have "Synthesis of Labeled Compounds 1947-1950", here are a great many of them. You will find that although here is one that Melvin Calvin's name is on, you will find that Melvin is not on most of those names (*i.e. publications*).

**VM:** These were things that he didn't therefore insist on getting himself involved with things that he actually hadn't contributed to.

**BT:** That's right.

**VM:** You were very involved: there must be hundreds of things, I guess, in these things (*i.e. the Quarterly Reports*) that you were involved with here (*in this volume*) and there are lots and lots of people involved. I can see that.

**BT:** He was a very good director in that respect. I mean, when it was his graduate student that was doing (*the work*) like, for example, Art Fry, why then Calvin's name would go on it because that was a different situation. But when it was Charlie Heidelberger's work or when it was Jim Reid's work or my work, and if we were just carrying it on and doing it, why we were treated as mature scientists in our own right.

**VM:** The graduate students probably welcomed having his name on their paper because it probably helped their careers.

**BT:** Yes.

**VM:** In those days, at the end of the forties and beginning of the fifties, there were the more senior people who were, I guess by then, you and Dick Lemmon and Andy, Al and Ed. That was the group of senior people who essentially worked there permanently.

**BT:** Yes, that's right, as long as they wanted to.

**VM:** There were clearly some support people: there were secretaries and so on who had the usual type of secretarial employment, and then you had technical assistants. They were also on permanent employment (*status*)?

**BT:** Yes, really, very much so.

**VM:** As well as support staff — dishwashers, store keepers and...

**BT:** Typical of that were, for instance, Martha Kirk and Ann Hughes. Martha Kirk was hired... Now, you see, the support people I might have helped hire or I might have hired myself, but even if I did decide to hire them, before I would formalise it I would take them over and Calvin would meet and talk to them and then give his opinion on where it was. For instance, Martha Kirk was hired and she worked for several different groups. For a long time she worked with me doing animal work of various

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kinds. After I left the group, then she went over and joined Al Bassham. There are a lot of papers of Bassham and Kirk in the later stages of the photosynthesis work.

**VM:** The group was always a group of the Radiation Lab., wasn't it?

**BT:** That's right.

**VM:** And so you were answerable to the Radiation Lab. administration in whatever form.

**BT:** That's right.

**VM:** So that means if you and Melvin between you agreed to hire someone, you then had to hand it over to the Rad. Lab. people who did the paper work on that.

**BT:** That's right.

**VM:** That went for all of the things, although you were always based on campus, it was always a Radiation Lab. group.

**BT:** It was always a Radiation Lab. group. and not part of the Chemistry Department, which is interesting.

**VM:** What do you recall as being the relationship between this activity and the Chemistry Department? What did the Chemistry Department think of it, as far as you could tell?

**BT:** That's a very interesting question. It was obvious that they approved of it and had it but it's not clear that Melvin had as much support in the Chemistry Department as he necessarily wanted. I always felt that one of the reasons that he supported Rapoport's work all through these years, providing him with at least a postdoc. and an assistant or two and a certain amount of money, was that Rapoport as a colleague gave him support within the Chemistry (*Department*) on the professorial level. Melvin actually wanted faculty members involved but there was not a lot of them that got involved with his group. It wasn't until later periods when you got...

**VM:** Tinoco and Hearst.

**BT:** Yes, and these people involved...that there became more faculty involvement. In a way, you can say that these people got involved because here was a source of funds and this was important for the continuation of their research because as funding got tighter...In the early days, everybody in the Chemistry Department really could get all the funds that they wanted to carry on their research. You might call those "golden years" from about 1945 up until about 1965.

**VM:** If they participated with Calvin, it was because there was an academic benefit to both parties. What about people from other departments? Clearly Calvin as a member of the Chemistry Department would have his closest links with them. Was there any...and I don't recall any of this but you would know. Was there any attempt to

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involve the Biochemistry Department or the Botany Department or any other academic groups on Campus?

**BT:** Yes, there was, and that was engendered basically through the individual people. Ed Bennett set up an extensive collaboration effort (*with the Psychology Department*). Many of the people didn't know enough of those people. On the other hand, we did contribute to their research by supplying labelled compounds and techniques and information, but none of us set up close relationships with these people. I knew and interacted with a number of people down in LSB. See, at that time, Stanley's biochemistry building hadn't been built yet, although it did get built before I left...

**VM:** ...the one that's called the Virus Lab?

**BT:** Yeah, the Virus Laboratory. That was the first biochemistry building that was built on the campus.

**VM:** One of the things that I have always found a bit puzzling, and maybe you can help us understand a bit, is the relation between Calvin and Arnon. (*Laughter*) By the time I joined, we knew that there was a stand-off relationship but I don't actually know why. Do you?

**BT:** I actually do not know why. I cannot answer that question adequately. They were doing research in the same areas and they were competing. Arnon was a good researcher and he could produce some very interesting results, and he did. But Melvin obviously had more people and more group and more money at his command. I can remember that Al Bassham, who was very good at singing songs — an excellent voice — had written a number of long ditties about Dan Arnon and the photosynthesis story; perhaps you have heard some of those.

**VM:** Well, if so, it's a long time ago and I don't remember.

**BT:** I don't know the origin of that, except it was just they were both working in the same field and competing and each one felt like they had special things.

**VM:** You were aware of this antipathy or whatever it was?

**BT:** Yes, that's right.

**VM:** I don't know whether they actually disliked one another or they were simply scientific competitors.

**BT:** What they might have done was, instead of remaining sort of secretive with respect to each other, they might have collaborated more closely. But I don't think that they wanted to collaborate more closely. I think that each one wanted all the glory and didn't want to have any ideas that might be inferred came from the other person.

**VM:** They were a bit touchy about one another's possible encroachment.

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**BT:** Oh yes, they were quite touchy about that. As far as I know there was relatively little communication. I don't even remember, for instance (which I would have), that Al Bassham would have gone down and sat in on Dan Arnon's seminars and things like that which he probably should have.

**VM:** Did any of Arnon's people attend Calvin's seminars?

**BT:** No.

**VM:** So, there wasn't that much collaboration.

**BT:** There was no collaboration; and it might have been that just from the fact that there was no collaboration resulted in this feeling of competition.

**VM:** As the thing (*i.e. the Bio-Organic Chemistry Group*) went on through the fifties, it got considerably bigger? I don't have a chart showing just what the rate of expansion was, but it was, I guess, by the mid-fifties, it must have been 50 or 60 people strong already.

**BT:** Yes, that's exactly right.

**VM:** Filling all the available space, presumably, that was available; it was fairly tight.

**BT:** Yes; in fact, squeezing very much over in ORL where we didn't have as much space. And even in our area, why we were relatively tightly squeezed because John Lawrence's group by that time had gotten large enough that he didn't want to give up any additional space.

**VM:** Bearing in mind that you left in '57, was there any consideration before you left about getting other accommodation?

**BT:** There was no formal activity at that time. Nothing had been discussed on a formal level that I know of about building another building...

**VM:** Or even moving into another building?

**BT:** Or even moving. It was certainly recognised that there was not enough space there.

**VM:** Calvin's group continued to burgeon all along through that period. People were pushed in. In fact I seem to remember that somebody or other said about the mid- to late-fifties he was beginning to have to curtail the number of postdocs. who came because there simply wasn't enough space for them.

**BT:** I am sure that is correct. He had created a sufficiently great name and there were that many people (*who wanted to come*). One of the things that was always very interesting I remember during this period was the relationship to foreign postdocs. He had a large number of applications from people all over the world. He said "I have one criteria that I can select these people or choose them. I won't take anybody who



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doesn't get money from his own country, or his own government or from his own area". It wasn't that the money was what was important but he said "I want the decision that they think this person is important enough and good enough to come over here and the only indication that I can really get on it is the granting of partial support".

**VM:** It's not a bad idea, when you have lots of people that you don't know, coming from places, from labs. that you don't know either. You have to have some way of sorting them out.

**BT:** That was the criteria that he used.

**VM:** I have to say, I think that worked rather successfully. People who came there were good quality people.

**BT:** They were excellent.

**VM:** There were very few who were not, if any. I don't remember anybody who wasn't. Did you feel that the postdocs...Let me ask a preliminary question. Postdocs. came to Donner as well as to ORL, did they, from all over, the international crowd? Did you feel that they contributed interestingly to the group, just apart from their scientific work, but the fact they came from foreign countries and different cultures.

**BT:** Incredible contributions. The conglomerate of the various people and their viewpoints and everything made the group exciting all the way through. They were welcomed and there was no discrimination against them and they became parts of the group. It was absolutely marvellous. They were very important.

**VM:** I suppose one of the things it has done is to produce an international network of these people now, at least through Western Europe, partly into Eastern Europe and in Japan where there have been X-members of the Calvin group, as well as, of course, all over the United States.

**BT:** Remember at this time the world was shrinking because of the airplane had made travel relatively easy and fast and it was becoming rapidly less expensive. I remember that in 1952 I took a sabbatical or a postdoctoral (I got a NIH special postdoctoral fellowship) and when I was going over and back. I said OK, I'm going to go by boat, it is going to be the last time I will ever be able to take the time to go by boat. I went across on the Ryndam, one of the Dutch all one class boats, and it took us 9 days to get over to Rotterdam. It was absolutely a delightful trip but it was a pleasure that I knew I would never be able to afford again; 9 days over and 9 days back, can you imagine giving up 18 days sitting on a boat in the ocean?

**VM:** I can imagine, because that was the boat we first came to America on...

**SM:** in '56.

**VM:** Where were you heading?

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- BT:** I was going to the Eidgenössische Technische Hochschule in Zürich, working with Ruzicka's group. I took over a very simple project in which I wanted to...he was an expert in cholesterol and I wanted to study the radiation chemistry decomposition of it. I prepared very pure cholesterol and then attempted to learn methods of how to assay it for impurities that could be formed by radiation.
- VM:** Your saying "radiation decomposition" then reminds me of Dick Lemmon's work on choline chloride. That, presumably, stems from the same origin, does it? You were, both of you, interested in radiation decomposition of organic compounds
- BT:** That's right. And you see: what we found was that this compound, acetylcholine chloride, which we were making, very rapidly decomposed. Yet, if it was not labelled, it did not, it was chemically stable. Since the effect was so marked, we ran it down. In fact, I think I was on the initial publication on that because I was involved in that very closely. Then Dick proceeded to study what was going on and why and published a number of papers on that. Then, having done that work, we said if this compound can decompose by its own radiation at a very significant rate, then perhaps all these other compounds are decomposing. And so we went back and started to look at the rest of the compounds and realised that as we were making higher and higher specific activity materials, why they were self-decomposing at significant rates. In fact, by that time compounds that we had made two or three years before and they were beginning to look pretty sick.
- VM:** Before I go on the other compounds: did you look at ways of storing compounds so that you would minimise their radiation decomposition?
- BT:** Yes, of course, and many of the compounds were in sealed ampoules without any oxygen. I think that we even thought about trying to store them at very low temperatures. But that doesn't stop radiation decomposition.
- VM:** I seem to remember, but you will know better, that if you dissolve these compounds in alcohol, don't you tend to get an absorptive effect because some of the radiation hits the alcohol molecules and is therefore...
- BT:** Yes, that was one of the things that certainly was attempted. But when it hits the alcohol why it produces free radicals and those free radicals are reactive and can go back and attack the compound. The compound is more susceptible to radical attack than is the alcohol, you will, in fact, almost intensify the decomposition.
- VM:** We have talked a lot about the work you and your colleagues did with  $C^{14}$ , what about the other isotopes which were important at the time, first of all the radioactive ones of tritium and phosphorus, then there was work going on with stable isotopes. Were you involved in any of these other activities?
- BT:** Yes, I was greatly involved in the carbon-13 work but that occurred at a later date. We did not use much tritium in our work. We did use deuterium. There was a bunch of experiments that were going on with deuterium which Ann Hughes was running on

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the toxicity of deuterium in whole animals, I don't know whether you have that in the record or not.

**VM:** I know about that because the first thing I did when I got to Calvin's lab. was work with Ozzie Holm-Hansen on deuterating algae. I was part of that scene. In fact Calvin suggested to me that was the way I cut my teeth on chromatography.

**BT:** Anyway, Ann Hughes found out that when you go above about 22-25% deuterium, why sterility resulted in the mice that she was working with. We did no  $C^{13}$  work and we had no mass spectrometrists in the group and we had enough questions that we could answer using the carbon-14 that it didn't become of interest. But in 1966-67 I went to Washington, DC for one year as a scientist in the Division of Biology and Medicine, and one of the things I took with me was a desire to develop carbon-13 as a tool. I began to push that back there and I found out that the Los Alamos National Laboratory was quite interested in carrying on a series of activities. And so I pushed through a programme of development of carbon-13 in which they set up a still for distilling...was it carbon monoxide? I think it was carbon monoxide, CO, to isolate the carbon-13. They became a major producer of  $C^{13}$  that still ended up, because of the oxygen-18 in it, at about 95.5%  $C^{13}$ . One of the reasons that I was pushing it was because the NMR techniques had been developed and they were obviously tremendously important because only  $C^{13}$  has an NMR spectrum. The carbon-12 and carbon-14 do not have an NMR signal. One enhanced the sensitivity of the NMR spectrum work and then provided a very interesting tool. So that there were then two tools to study carbon-13, both the NMR and the mass spectrometry work.

That programme then developed very strongly and went ahead and eventually commercial companies said "we're going to set up a still and make carbon-13" and you, the Atomic Energy Commission, can no longer make it because you can't compete with us. So, the laboratories were forced to stop it. By the way, that's the same thing that happened with respect to carbon-14 labelled compounds. Several places, but especially Oak Ridge, started to make labelled compounds and sell them on a commercial level. We didn't want to do it. That work continued and was very useful to the scientists of the United States until a number of commercial companies said "we want to make these labelled compounds" and they actually forced them (*Oak Ridge*) to stop selling carbon-14 labelled compounds, saying you can't compete with industry.

**VM:** The AEC couldn't compete with industry?

**BT:** Yes, that's right. It was not allowed.

**VM:** Why was it that the group chose not to become involved with tritium and phosphorus very much?

**BT:** There aren't as many good problems to be answered using phosphorus as there are with carbon-14. After all, phosphorus metabolism is relatively straightforward. John Lawrence used a lot of phosphorus but he used it because of its radioactivity and in the treatment of cancer and leukæmia patients. We did do some tritium experiments.

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It wasn't easy to detect. We didn't have good methods of assay for it and that probably was a big handicap.

**VM:** Then later on in the late fifties, I don't remember whether it was there before you left or not, there was some interest in using  $O^{18}$  as a tracer. Were you part of that?

**BT:** Yes, I was.

**VM:** There was a woman from Sweden, called Ingrid Fogelström-Fineman who did some neutron activation of oxygen-18 to make fluorine-18. Do you remember that?

**BT:** I do.

**VM:** I don't know whether you were part of that.

**BT:** I wasn't part of *that* experiment, but I was part of an earlier experiment because, being somewhat interested in the NMR work, I co-operated with a man by the name of Harry Weaver who was down at Stanford and at the Varian Laboratory, and we were the first ones who demonstrated the  $O^{17}$  signal by NMR in an organic compound. We took an organic compound and prepared it and put it into the NMR.  $O^{17}$  has a rather broad signal — it's not a very sharp signal like the carbon-13 signal is — and got a very nice band and published it. That was the first time that anyone had published something on  $O^{17}$ . Now  $O^{17}$  since then has become particularly from the work that came out of the distillation of the CO at Los Alamos much more available and both  $O^{17}$  and  $O^{18}$  are in their own rights very interesting isotopes. To the people who were working in photosynthesis these were very important. Remember, that in the photosynthesis work that the enzyme, ribulose biphosphate carboxylase oxygenase, is both an oxygen  $O_2$  enzyme and a  $CO_2$  enzyme.

**VM:** The last question I think we might discuss in this phase is: eventually you left.

**BT:** That's right.

**VM:** In the beginning everybody was very young, weren't they. Even Calvin when he started the group was in his early thirties, 34 or something like that. I guess you were all younger than he was.

**BT:** Oh, yes.

**VM:** You must have been in the late twenties — ish.

**BT:** Since I was born in 1921 I would have been 25.

**VM:** Well, there you are. That was the typical age of many of you at that time.

**BT:** That was the age of Pete and Jim and all the rest of us.

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**VM:** And Andy was a little older, perhaps, but not very much. In the course of years of being employees in the place, you were also getting older. In 1957 you left and by that time you must have been — born in '21 — you were 36. Why did you leave?

**BT:** Sometimes I ask myself why I left because I was afflicted with a disease which is called “Berkeleyitis”.

**VM:** Oh yes! It's well known.

**BT:** Anyone who has lived any length of time (*in Berkeley*) gets this disease and you recognise it. It has absolutely magnificent summers and winters and is an exciting environment and why should you leave? I think I left for basically two reasons. One of them, I was seriously concerned about Melvin's health. I estimated, and at that time he had already had two heart attacks and there was good indication that he would have more, and I thought that there was a high probability that his life expectancy could be as few as few years. I had seen that in the laboratory when a really strong senior leader like this disappears that the group just eventually, although it may continue on for a few years, dries up and is terminated. I said (*to myself*): if I have an interesting opportunity. I probably should leave it this time.

That was one reason. I think there was another problem which was much more subtle. There was a fundamental conflict in the laboratory concerning the photosynthesis work. Basically, the photosynthesis work was the development of Andy and Melvin, and they worked closely together on it, in developing it. Of course, Al was an important assistant on it but he was really the junior member by far and away of this group. As it became apparent that this was a very important discovery and very important work, why Calvin eventually — and this was discussed rather openly between Gen and Melvin and I, and even Anne was part of some of the discussions — that there really wasn't room for both Melvin and Andy in the same group. They were both people of stature and they both wanted, and Melvin wanted all the credit and it was his group and he had started it and so he essentially said to Andy that he had to go. I think this did not sit well with me. I think that there was a little bit of reluctance on my part that I didn't like the situation. I felt a great deal of sympathy for Andy. As you know, he left and went to a very excellent position at Penn State, which he was not happy at. He had Hans Ostwald build him a very beautiful California-style house back there. But even that, in the middle of Pennsylvania, was not enough to make him comfortable. So eventually he left and went back to California which was his home.

**VM:** Did you also yourself hope for an academic position, did you see advantages of having an academic position in your own right, rather than (*be a member of a group*)?

**BT:** It's interesting. A number of people in the group, such as Heidelberger and Yankwich and all these people, had a very clear-cut idea when they joined the group that this was sort of a postdoc, experience, that they were going on as soon as they got a good academic position. In fact, they did, all of them. I did not. I did not have that level of ambition. I was not necessarily ambitious on that level. I wasn't bothered by the fact that I didn't have an academic position. In fact, when I joined academia and went

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here (*to Colorado*), and I came here as an associate professor without tenure (and two years to tenure), and saying “well, if I don’t get tenure, I don’t really care. It’s not important to me necessarily whether I’m in academia or whether I’m in industrial research”. I like research, I enjoyed research and would have been perfectly happy at that. So I continued here and so, of course, I got tenure and continued on here. But I have always felt that I could be just as happy in a place where I had adequate research funds to do interesting research and doing research.

**VM:** Had the atmosphere in some senses been different, and Melvin’s health been different, you happy actually with the day-to-day, month-to-month, year- to-year environment?

**BT:** Oh yes. If that had been different, why I probably would not have left and given over my job to Dick Lemmon and would still be in Berkeley. Heavens knows who I would have married then what my children would be!!

**VM:** You might have married Anne because you met her, presumably, in Berkeley.

**BT:** Yes I did. I met her in ’58. That’s certainly possible.

**VM:** Looking back on it now, nearly 40 years later, regrets or no regrets?

**BT:** No regrets.

**VM:** You’ve had a happy life in Colorado?

**BT:** Absolutely. It’s a delightful place to live. The climate is delightful.. The university has grown. During the period since I have come here this has changed from a...from not so...well, it was a good university, but it wasn’t distinguished...to one of the very good universities. I’m pleased to say that I think I’ve had some part in it. I certainly was the one that pushed this group here in the Chemistry Department into accepting biochemistry as part of their regime. Of course, the department is now called the Department of Chemistry and Biochemistry. I can, in fact, remember the first real biochemist that we hired, was a guy by the name of Pete Albersheim, a PhD in biochemistry from CalTech, and I spent part of a year being a visiting professor in Argentina with a person by the name of Dr. Enrique Strachman (*spelling?*) who was an MD in Donner Laboratory who came from Buenos Aires. And I had been down and visited him and when I came back I said to the department that the one thing I wanted to do was to hire a biochemist and I want to be the chairman of the search committee for that biochemist. They said “OK”. I was really almost surprised that they gave me the go-ahead. We hired this man Pete Albersheim, who is now a professor down in Alabama, and he was a very dynamic young man. From that start, the biochemistry division grew to the point where it is now a very famous and a very strong department. And it creates strength within the department with the diversity which one really needs in chemistry in the broad level.

**VM:** And you have also broadened out and have some industrial activity as well in chemistry?

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**BT:** Yes, although that stuff started later.

*(New tape)*

**VM:** This is Bert Tolbert Tape 2.

One of the things I'd like to get your views on before we finish is the building. Many members and former members of the Calvin group have spoken in glowing emotional terms about ORL and the round building was, in a sense, an attempt to recreate the ORL philosophy in modern terms. What do you think? Do you think ORL was special?

**BT:** Yes: ORL was special, and just as many old buildings are, they are special. They are very nice to do research in because you can do whatever you want to them, you don't have to worry. They have wooden walls, you can nail through them, you can cut through them, they are easy to work in. ORL was also very interesting because it was one big room and the laboratory benches were put into there and small cubby holes were put around the periphery for the offices, as you remember. This meant that everybody was working in the same room together. They communicated then and used the common equipment. So, ORL was a very special laboratory in that respect. We didn't have it over in Donner. We had one big laboratory but then we had three or four small laboratories. And that splits people up.

**VM:** And you noticed the difference, did you, between Donner and ORL?

**BT:** Yes, that's right. The other thing, of course, that made ORL very special was that they were working, and were highly successful, on a very fundamental problem, namely, one of the photosynthetic pathways of carbon.

**VM:** And well focused.

**BT:** And well focused and directed. So that everybody's contribution you felt moved science forward in a significant way. Unfortunately, you succeed in making a synthetic compound in good yield, and so forth, then it goes to somebody who is going to do some major experiment. Although you have succeeded in making the compound, it isn't the fundamental discovery that you make if you discover a new ribulose biphosphate or something like that.

**VM:** As you know, the ORL came to an end, they demolished it, and another building was ultimately designed. I know it was after you left and we have already discussed that there was no discussion of this apparently before you left. But you have seen that round building and you know the idea on which it was based; what did you think of it? I don't know when you last saw it, and since Calvin retired from the directorship the whole character of the place has changed a lot, but presumably you saw it while he was still director.

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**BT:** I have seen it at intervals every few years. I saw it in the early days, when Calvin was the dynamic force and it had the community effect that he wanted, and I have seen it since then when it has become basically a series of individual laboratories for professors in the Chemistry Department, which is what it is now.

**VM:** What's your view, what did you think of it really? The interesting thing is what did you think of it in the early days because if it gets degraded...?

**BT:** Absolutely delightful. The whole concept was delightful, in which essentially all the equipment and everything was community equipment. It accelerated the pace at which you could do research, you didn't have to go find a storeroom, to get a beaker or pipette, and there was always somebody around who could instruct you on how to use this counter, or something else. The communication was very good. I just think that it was great. But, it requires a strong director and it requires lots of funds to keep it going. What really killed this thing was that as funds became tighter there wasn't enough money to take care of the overhead. That was a major problem.

**VM:** Then, in addition, it lost its unitary character.

**BT:** It lost its unitary character because the people that were coming in were in themselves individuals who wanted to do their research and they didn't care whether the group corresponded to the others and they didn't care about the other research that was going on.

**VM:** It is difficult to see that type of building being built now, isn't it, a building for a man, as it were. Unless it's an institute; well, even if it's a dedicated institute, do these things really survive the first generation in the form and with the satisfactory character that they may originally have been designed for?

**BT:** I don't know. It all depends. If there is a proper succession done, a unified laboratory can do fairly well. But, not like Melvin's laboratory...

**VM:** ...as it's now become.

**BT:** Well, as it was, that does not survive. It might as well be a number of small buildings as that Round House design which Melvin put together. I guess the answer is "no". I don't think they really survive the one strong director. Unless a new director comes in which is strong enough and can fill the vacuum and can take it over. The other thing that I see that makes it very difficult today is the matter of funding.

**VM:** Even if a new director takes over an existing place, he has the problem of inheriting the people of the older regime and they may not be his choice of personnel unless you have an internal promotion to the leadership which may or may not work.

**BT:** Most of them do not work. I have not seen any of them work, so I will say that. That's not the way. There has to be another method of getting that level of creativity that you want.



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**VM:** It may very well be that the people who do work as the regular staffers in a place like that are simply different people from the leadership people, the great man who's the founder.

**BT:** That's right.

**VM:** OK. Well, I think we ought to thank you very much for having talked so far. We're not finished yet because there's a mass of material for us to look at and, having looked at it, we may very well want to open up the discussion again for a bit.

**BT:** All right.

**VM:** But let's call a halt for this...