

## Chapter 8

### EDWARD L. BENNETT

Berkeley, California

May 28th, 1996

**VM = Vivian Moses; EB = Ed Bennett; SM = Sheila Moses**

**VM:** This is May 28th, 1996 and we're talking to Ed. Bennett in Berkeley. When did you first join Calvin and his group?

**EB:** 1949.

**VM:** And how did it happen?

**EB:** Well, I was at Cal Tech at that time and I went to one of these ACS meetings and heard him talk about photosynthesis. This sounded like an interesting place to go. So my professor at Cal Tech, Carl Niemann, wrote Calvin and there was an ACS meeting up here, so Calvin said "come on over and be interviewed". I think I was mostly interviewed by Bert, if I recall. Subsequently, I was offered a job but I wasn't offered a job in photosynthesis. I was offered a job in more the animal biochemistry segment.

**VM:** What was your previous experience and training at that point?

**EB:** At Cal Tech I had done a certain amount of organic...During the war I spent time in Florida in the great swamps (terrible) and then at Cal Tech my degree was mostly making fluorinated phenylalanines.

**VM:** Of course, you were a chemist?

**EB:** Yes, a chemist.

**VM:** Not a bio of any sort.

**EB:** Not a bio, no. I never had a course in biology. My work involved a little bit of what was then called biochemistry because they were enzymatically-resolved into D- and L-forms which was something Niemann had worked a lot on using papain.

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**VM:** And you had just finished: what? Your masters degree?

**EB:** Ph.D.

**VM:** You had finished your PhD. when you came?

**EB:** Yes.

**VM:** What animal work was suggested?

**EB:** I think what I started doing was actually making radioactive aza-adenine, aza-guanine, using carbon-14. I spent close to two years making those darn things. Of course, then, carbon-14 was real valuable. You tried to save every little bit and recover it, and so on and so forth. So I may have done some...I don't know if I ever got around to doing any experiments with them or not. Then, I got a fellowship to Europe after two years and went to Kalckar's lab. in Copenhagen. That was an interesting year. I was kind of naive, I guess, but fortunately I got to come back (*to the Calvin group*).

**VM:** When you were first hired by Bert in 1949 was that an open-ended commitment?

**EB:** I suppose it was. It wasn't anything really spelled out. I guess the other kind of biology-sort of thing that I had done before coming here was working with James Bonner. At Cal Tech you had a major and a minor and the minor, in my case, was in plant physiology. We went out to the desert one weekend and got a lot of plants and extracted them and tested them for plant inhibitors. I found one and identified it, I guess, and unfortunately neither with that nor with the phenylalanines was there any chance of (fluorophenylalanines) of doing any work with them subsequently. I've seen references in the literature to the fluorophenylalanines being used for various things.

**VM:** When you got here, when you got to the group, who was there?

**EB:** I think Martha (*Kirk*) was there at that time, if I remember correctly, obviously Bert (*Tolbert*), Pat Adams, I believe.

**VM:** You are the first one we have talked to who has mentioned Pat Adams. I don't remember Pat Adams.

**EB:** She was tall, slender, blonde.

**VM:** What did she do?

**EB:** She was in the synthetic group. The group at that time had three parts. There was the photosynthesis part that was over in ORL...

**VM:** And they had already gone to ORL by the time you arrived?

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**EB:** Yes, they were over there. And then there was the part down in about the three or four rooms down at the end of the (*third floor*) hall in Donner. One side was doing carbon-14 syntheses and developing things on that; the other side, as I recall, Martha with Bert, were doing metabolism experiments in humans and in rats. I don't know what the years were; one would have to look those things up but I remember it going on.

**VM:** Which side of the corridor did you join in?

**EB:** I wasn't in the carbon-14 synthesis side. My work was supposed to be leading to more biochemical stuff.

**VM:** Who were the carbon-14 synthesizers?

**EB:** The people I remember there; Dick (*Lemmon*) knows dates and so on better than I do. There was Pat Adams; of course, Rosemarie Oswald was involved.

**VM:** Was she one of them as well?

**EB:** She was involved up there. Marilyn can fill you in the years. There was a fellow named Bob Self who died relatively early. Then there was Eugene Jorgensen, who was somewhat afterward and who was unfortunately murdered over in San Rafael, very mysteriously, kind of tragically actually, because he was murdered and his son was under a certain amount of suspicion and I think his son actually died in an automobile accident or something like that. Gene and I used to play tennis together quite often. (*After he left the Calvin group*) he went over to San Francisco (*UCSF*).

**VM:** Did this C<sup>14</sup> synthetic work go on for a long time?

**EB:** I think it went on, probably if I were to pick a year, I would say '55 or so. It gradually got phased out as more commercial places came in (*making labelled compounds*). I remember that Dick (*Lemmon*) got involved, as he probably mentioned, in this radioactive decomposition stuff (*with choline chloride*). I remember I pulled out a few compounds that I'd had around; they ran them on chromatograms and so on and so forth.

**VM:** How did you work at that time? Were you working by yourself or together with other people?

**EB:** I had a couple of people who worked with me. There was this gal named Barbara Krueckel, who was not much good. Ultimately I worked a lot with Ann Hughes. We worked on the fly business (*Drosophila*), the D<sub>2</sub>O in flies. Then this business, I remember we got a big laugh out of it. With the D<sub>2</sub>O we were talking about mice and I guess it was at a AAAS meeting I talked about mouse eggs. Everyone laughed, but in a way that's true. They have eggs...

**SM:** Just like people.

**EB:** ...just like people, Yeah. That wasn't exactly what I meant. So I worked with her on that. I guess it was much later, after we moved into the new place (*the Round House*), a lot of other things (I worked with Ann [*Orme*]) took place. This business of memory transfer, that was started over in Donner, actually. That was an example of something that so frequently happens. Calvin reads something and he came around and kind of threw out the idea and: "Does anyone want to work on it?". Usually, most of these ideas didn't go anywhere, I don't think he ever held it against a person but ,if you did work on it, he'd certainly support it. That (*the memory transfer work*) started there. We got the two people from Jacobson and Jacobson, who were students with McConnell and they came out. I remember we had a place set up downstairs in Donner (*on the first floor*); it started there. When we moved over to the Round House, I remember what room they went to there.

**VM:** That was a man called Allan Jacobson, wasn't it?

**EB:** There was Allan and someone by the name of R (well, we could look them up).

**VM:** What was McConnell's first name?

**EB:** James McConnell. It's kind of interesting. I kind of had this in the back of my head that somehow or other he he'd been involved in the Unabomber thing. When newspaper articles came out a few weeks ago I was almost right. It turned out that, I think, the first Unabomber bomb sent went to his home and a student opened it up and was injured. So, it was presumably aimed at McConnell but it didn't get him. I remember telling Mark about it that I thought.....and I haven't talked to Mark since. But that memory was right on that.

**VM:** That's Mark Rosenzweig (*in the Psychology Department at Berkeley*).

**EB:** I think that was about '63 when that incident happened. It was the first one up in Michigan.

**VM:** Coming back to the early days in Donner: did the people at that end of the corridor in Donner feel close to the ORL people?

**EB:** Reasonably so. Actually closer than to some of the people in the LBL (*Editor: This is clearly in error. He means “LCB”, the Laboratory of Chemical Biodynamics, Round House*) feel to those on another floor now. We all met together once a week for the infamous Friday or Thursday morning briefings and they were held down in what was then the library (in the Donner Library), still is the library, but didn’t have all the stacks in it. We used that for the room.

**VM:** These were the seminars?

**EB:** Not the seminar room but the room in the library.

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**VM:** These were the seminars you were talking about that were held on Friday mornings.

**EB:** The Friday morning “show and tell”. At that time, early on, you didn’t have the advance notice. You kind of came down with your notebook and were quivering, shall I say, just hoping that he (*Calvin*) wouldn’t look around and see you. Sometimes I think two people got attacked! That way the people from our place and LBL — LRL (*Editor: this is an error for ORL*) got to know each other. Also, of course, one used the paper chromatography facilities over in old lab. We were going across to that lab. quite frequently.

**VM:** So everybody knew everybody pretty well.

**EB:** Yes, I would say that everybody knew everybody. Not like now.

**VM:** What did you do with chromatography? I don’t remember your using chromatography?

**EB:** Oodles of stuff. We studied metabolism of radioactive adenine in mice; I guess Barbara Krueckel was a co-author on that; (that was after I came back from Copenhagen) determining the half-life of various adenine in the DNA and RNA of various tissues, finding that in intestines in had a curve like *that*, in liver then it went like *that*. That sort of thing. There wasn’t a lot of work on that at that time. I don’t ever think I did very much with azaguanine. And then I worked with a fellow that you may have known when you came, Dr. John Weaver who was an MD.

**VM:** I remember the name, not the person.

**EB:** I don’t know where we got it, but we worked with stilbamidine, and did some studies on its metabolism. Those were all back in the early fifties.

**VM:** For these you used the same sort of chromatographic techniques as the photosyntheses.

**EB:** Certainly with the adenine, for example, you would want to know what it was metabolised to, how many compounds it went to in the urine and also to check out the purity of your stuff. We did oodles of chromatograms.

**VM:** Radioautography and counting, and all of that stuff.

**EB:** With the adenine, for example, it very quickly went ADP and ATP and all those kind of things, and a certain amount would get metabolised to other degradation products.

**VM:** As a group, the people who were working on the animal metabolism, did you have as sharp a focus as perhaps the photosynthesis people had?

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**EB:** The reason for it, of course, was that we were interested in cancer and there was actually another person, who hasn't been mentioned, was (*Dr.*) Max(*well*) Gordon (*Editor: He came to the Calvin Lab for postdoctoral studies*).

**VM:** I didn't know him at all.

**EB:** I just don't remember what years he was there and he finally went from our place who. He subsequently went on to Smith, Kline & French (*Smith, Klein, Beckman*) and he was interested in some of the aza compounds. He was there (*in the lab.*) for a couple of years.

**VM:** So the interest in cancer really goes back a very long way.

**EB:** Oh yes, because Charlie Heidelberger was working on that kind of problem there, too. That goes back, might say, before I came. I guess in a way, Calvin suggested: "Well, let's do something connected with cancer research". I remember that I made radioactive adenine and radioactive guanine, probably traces of them still exist, and made this radioactive azaguanine and azadenine, which nowadays would be a cinch but then you had to worry about the yield and all that sort of stuff.

**VM:** What was the relationship between the Calvin group in Donner and the rest of Donner, with John Lawrence's' group?

**EB:** It was moderately close. Bert obviously worked a lot; he worked with a fellow named called Nat or Nate Berlin (*Dr. Nathaniel Berlin, a medical doctor*) who went back to NIH and did a lot of work on blood cell turnover. I don't know who was involved there but obviously his metabolism studies with Martha, he worked on a lot of that to develop this machine, the instrument with the hood on. People were given a shot of this or that and then the excretion was determined.

**VM:** Tell me, I don't know anything about that one.

**EB:** This was probably, nowadays they wouldn't want to know. Bert and Martha were involved in these metabolism studies, using both mice and rats, and ultimately humans. Bert designed and they built this machine that measured the CO<sub>2</sub> output, radioactive CO<sub>2</sub> output, actually both outputs I think continuously.

**VM:** What was the machine? I don't know about it.

**EB:** It was nothing more than an ionisation chamber, I guess.

**VM:** You moved your hand round your head to suggest...

**EB:** You put a hood on, air came in and air came out. With a rat you put it in a cage, I guess, like a desiccator, but more like a cage; air came in, air went out, flow meters on it so you knew it was 500 cc a minute or whatever it was. So you could determine how much was metabolised from its rate. I don't know who it was that provided the



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**EB:** Yes.

**VM:** When you later moved into the brain studies, did you continue to use isotopes or something which would...?

**EB:** No, I don't think we ever used any (*isotopes*) in it, actually.

**VM:** Did you begin to fall outside the formal AEC guidelines in that sense?

**EB:** Probably. That was what made the lab. so interesting. We certainly wrote about what we were doing and it wasn't until much later, I think, in fact. way after we moved into the Round House, that there was much problem with funding that part of the research.

**VM:** What direction did your work with Rosenzweig and Krech take?

**EB:** More serendipity than anything else. They came over, I guess it was mostly Krech that had the idea that cholinesterase — at that time people did not really distinguish between acetyl cholinesterase and cholinesterase; it was all cholinesterase — and I guess it was primarily Krech that had the idea that different levels of cholinesterase would lead to different behaviour in the rats, different problem-solving abilities. He had worked for years using the Krech hypothesis maze: this is a maze that you can't solve because it is always jiggered so there's no solution, it's random. Rats will adopt a hypothesis and they'll adopt a hypothesis, either I can solve this on the basis of visual cues (if light was right the last time, light will be right the next) or can be spatial (if the right side was right this time, then the right side will be right next). So, you can divide them that way.

They had divided a few of the rats into maze-bright and maze-dull animals. Initially we had, I don't know — perhaps less than 20 animals. At that time, (*Professor*) Joe Neilands, who was right across in the biochemistry building, had a machine called a pH-stat that this very strange character named Cannon, I think, had developed. Joe let us use that to measure the enzymatic activity in little areas of the brain. It turned out that those (*rats*) which were maze-bright, I think (could be turned around), had higher levels of cholinesterase in the visual and occipital cortex than did the maze-dull. We published that in *Science* and went on from there. I think really, in retrospect, it was probably a strain difference involved as much as anything. Anyhow, that's what we did.

Actually, in terms of support that at time — oh dear, I had her name; she married Roderick... What the hell was her name?

**VM:** What did she do?

**EB:** She was an assistant on all this and she was involved in the assays. We assayed oodles and oodles of animals and somewhere along the way, early on, Krech had the idea that maybe if these animals exercised more, shall I say, used their brains more (whatever word you want), that this would change the brain parameters. We got involved in that



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and we had what became very well known as the EC and IC and the SC animals. Indeed, we found that there was a difference in the activity of the cholinesterase. By that time we knew more about cholinesterase and knew that it was acetyl cholinesterase and cholinesterase, and that these two responded somewhat differently. We worked and worked and worked on that.

Subsequently, when we were putting a lot of this data together, we noticed that one group (*of rats*) had lower activity per milligrams, but had more milligrams, so had higher total activity. Then came the idea that the cortex actually changed in its parameters with the environment. About that time Marian Diamond joined us. She was involved, of course, in the anatomical work which she still has carried the theme of the enriched and impoverished (*environments*) very, very successfully since. Unfortunately, she split from us but she has gotten a hell of a lot of mileage out of that idea, applied to humans. A story I like to tell, one night I was listening to the radio in our bedroom, as I often do, and kind of fell asleep, or turned it on, and heard her talking at the Commonwealth Club (*in San Francisco*). I didn't have to hear very many sentences before I knew who it was. Then I fell asleep, it's old hat, and subsequently I heard the applause and oodles and oodles and oodles of questions. The next week there was this guy Smith, who was then chairman of General Motors, got to the end of his talk, (*sound of slow clapping*) little bit of applause, a few questions. Marian, she just retired as director of the Lawrence Hall of Science after five years. We had a very fine retirement party up there with all the big shots — Seaborg, and the Chancellor and all of that — and in April they had a dinner for her as the Alumna of the Year. She has really gotten a lot of honours for both her research and her teaching.

**VM:** How long did you continue to work with her?

**EB:** Probably ten years without going back, many years. We really had quite a productive cycle then, went from the mid-*(fifties)* probably into the seventies, I guess.

**VM:** You mentioned earlier on that you had spent some time with Kalckar's group in Copenhagen, that was after you had been here for...

**EB:** ...two years...

**VM:** ...two years and you went for one year to him?

**EB:** Then I got interested in adenine metabolism over there, as I recall.

**VM:** And that's how you came to do that kind of stuff when you came back?

**EB:** Yes, I believe so. I believe I was interested somewhat before I went but I did more of it over there.

**VM:** At which stage in your career was it clear that you had become a permanent member of the group? Or was it never clear?

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**EB:** It was never really clear! It just kind of went on. I don't think there was any time that I got a piece of paper saying "You are now a (*permanent member*)". It was only after many years.

**VM:** Is that generally the way the group tended to operate, do you think?

**EB:** I would guess so, but you should ask Dick (*Lemmon*) and ask Al (*Bassham*), but I think it was pretty informal in those days. There weren't all the rules and regulations in either our lab or in LBL. Nowadays, you know, after you have been there so many years if you aren't made some sort of status position, why you are probably going to have to go. Along the way, on this psychology stuff, we started writing and getting our own grants so we were partially funded by NSF or NIH, and I can't remember the gal that did a lot of the work early on — Hilda Karlsson. She married Tom Roderick so I was always mad at Tom because he took her away! They are *back at Bar Harbor now. He has had a very successful career. Then Marie (Hebert/Alberti)* came. Somewhere along the way I guess I was involved in hiring Hiromi (*Morimoto*).

**VM:** Both of whom have been with the group since the (*late*) fifties.

**EB:** I remember about Hiromi that Bert was very concerned about hiring a male. He didn't think as a technician that that would work.

**VM:** Why not?

**EB:** I don't know. I guess he didn't realise that the Japanese were God damn good. Probably later than that, Hiromi did a lot of work with bungarotoxin that we were involved with. I don't know whether or not the Ferchmins (*Pedro and Vesna*) were here when you were here. They were in the Round House so that is really post the period you are talking about. But then Marie is a superb person, I would say.

**VM:** And she's still in the group.

**EB:** She's working mostly for Hearst (*Prof. John Hearst*) now, I'm not quite sure.

**VM:** Is Hiromi still there?

**EB:** Hiromi is up on the Hill in that isotope thing (*National Tritium Labeling Facility*). I think that he is probably a mainstay up there. He is real competent, real hardworking, probably couldn't ever get him to write a paper; maybe you can now. Marie is one, you know that some people nowadays they've got to have a job description; if something isn't in the job description, then they might not want to do it. Marie always would do anything that you would ask her. If wanted some literature looked up, fine; if you wanted some stuff put into order, she would put it in great order. She and I probably killed and dissected more rats than anyone would ever want to know, probably way over 10,000. She dissected them and did a superb job. Again, I don't know when it happened but I remember once when we were looking for grant support, we were having NSF and NIH teams coming — those days they came to see what you

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were doing, rather than this business of reading and then having questions and misinterpreting what you said, not giving you a grant. So Mark told me who was coming to one of them, and I told Mark, “you can forget it”. There was a guy from the midwest who was coming that we had some discussions with, shall I say. So, they came to watch us do a dissection. The man looked at it: “Terrible, all sorts of white matter, not uniform at all”. About a week later the other team came, looked at her doing dissections: “Beautiful”. She hadn’t changed her technique between week one and week two; and then we did a lot, in terms of chemistry there, we kind of abandoned the acetyl — I guess Hiromi did oodles of acetyl cholinesterase assays, that’s what he did oodles of.

Hiromi worked out a method for doing DNA and RNA analyses which, I think, was better than any in use at that time and may still be one of the best available, using CTAB but the methods used at that time using a spectrophotometer and colour reactions was just no good for brain; it gave all sorts of false answers. So then we did studies on RNA to DNA ratio and found that the ratio of RNA to DNA was higher in certain brain areas of the EC animals. Even though the difference was small, if we were given 12-14 pairs of animals, divided into group A and group B, randomised, we could without fail told you which group was which, it was that reliable. What it meant, we started to think about things to do, but we never got to it. Probably now nowadays with what you can do with RNA and DNA we could have found (*something interesting*). Ann Orme worked for me, too. I guess she started working in Donner.

**VM:** Did you gradually move away from working with other people in the Calvin group more and more to work with Krech and Rosenzweig?

**EB:** Yes, I would say that probably by 1956, I don’t think I did much of any work that was traditional of what the Calvin group was working on.

**VM:** Did Calvin retain an interest?

**EB:** Yes, very much so.

**VM:** Contributed?

**EB:** Yes. He got interested in this worm (*planaria*) business, enough in that to get a trailer put up on The Hill, where Ann (*Hughes*) was. She was a firm believer in it, perhaps still is (I don’t know).

**VM:** Ann Hughes?

**EB:** Ann Hughes.

**VM:** We’ll be talking to her and find out.

**EB:** You’ll find that she was a firm believer, unfortunately. She went back to work with Ungar. Ungar sent us some extracts which never really worked, always excuses. Then

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David Samuel (*Editor: From the Weizmann Institute in Israel*), again in the Round House, got involved in it — this is all Round House — and Bill Byrne was involved in it. So that era was shortly after we moved into the Round House.

**VM:** David Samuel was a bit later; David Samuel was '65-'66.

**EB:** So that worm stuff started over in Donner and was carried over into the Round House.

**VM:** What was the view of the people in Donner about the approaching new building? You had not had to make this move....

**EB:** We had to move down to LSB (Life Sciences Building).

**VM:** Did you me to LSB?

**EB:** Yes, I did.

**VM:** Why did you have to leave Donner to go there?

**EB:** Probably because they wanted our space. I don't know who else moved, but certainly I remember I moved down there.

**VM:** Not everybody moved. There was always a presence in Donner until the round building, wasn't there?

**EB:** Bert was gone, I guess. I don't know whether Dick stayed in Donner until we moved or not. I know why I had to move because we were doing the brain analyses upstairs in ORL so that's why that work had to move. I guess my office and stuff was still in Donner but the brain analyses were upstairs in ORL, in kind of a loft room up there, so they moved down to Donner (*Editor: Error — should be LSB*) in one of the rooms. The thing I remember about that one time in Donner. LBL (*Editor: Error? Should it be ORL?*) had these kind of frosted glass pane doors and one time I opened the door and who did I hit, Henry Mahler?

**VM:** Who?

**EB:** Henry Mahler.

(*Brief discussion about LBL vs. ORL*)

**EB:** That was perhaps the most noteworthy....

**VM:** Was he badly damaged?

**EB:** No, fortunately. He could have been.

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**VM:** So, when the designs began for the new building, what was your interest in securing...presumably you were a contributor.

**EB:** We all had our input.

**VM:** What was your input? What were you looking for in the new building?

**EB:** I guess I was mostly interested in the room that was kind of “my” room, shall I say, that was going to be my rooms on the first floor. As you go around to the left there is an office and then there are two rooms, the next two rooms. Initially both of them were assigned to me and then, I think I didn’t use the first one of the two too much and gradually had to give that up. Then the second room, that’s where Hiromi did a lot of work and that’s where our snake venom work was done, and then, of course, I had a couple of lab. benches upstairs and had a couple of graduate students. I was kind of involved with a guy named Simpson (*Lance Simpson*), I think, and also with a fellow who went to England and has done real well and is now up in Davis (*Michael Hanley*). For a while there, that again was in the Round House, we had a really good group going, with a graduate student and with Vesna and Pedro (*Ferchmin*). When they left we got another person is as a postdoc. but he didn’t accomplish a hell of a lot. That program kind of lost its momentum. It was a good programme. We did a lot of things then and we are still equal to the time.

**VM:** What was your view about the proposed design for the Round House with these big open labs? Did you like the idea?

**EB:** Yes. I liked it then. I think it has kind of gone downhill since then. It’s been cut up a lot more, a lot more stuff (*equipment, desks, etc.*) put into it. I think on the whole that lab. has adapted pretty well when you consider we have been in there 33 years and it is still pretty functional without a lot of major changes. I think the idea of having an open lab. where people could interact was really great.

**VM:** You found from your own experience and from your people that it worked like that, as it should have done?

**EB:** Yea, yeah. That I liked, I know that some of the others don’t do it that way, I had an office there on the second floor on the south side, one that looked out into the lab. — I never pulled the blinds down the whole time I was there. What I liked about it: You could sit in your office and see if someone was out there, if you wanted to go out and talk to them, know where people were and what was going on. Some people go into their offices and pull down the blinds, there was all this talk about having windows or not having them (*in the offices*). I thought it was great being in a glass house.

**VM:** Everybody we have talked to so far has been very (*enthusiastic about the building*).

**EB:** Some of the newer people in the building have the blinds pulled down all the time.

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**VM:** Perhaps you needed to belong to the earlier generation of people who had grown up with the idea. How about the social side of the lab.? Have you always been a participant in gatherings and parties?

**EB:** Some. You know as one gets married with more responsibilities you become less of a participant. But certainly Martha and, to a slightly lesser extent but not a lot, Ann Hughes did a lot. Martha used to have a trip or two every summer up to Wrights Lake, which 10 or 15 people would go up; they used to organise ski trips up to Yosemite and a moderate number of hikes up to the mountains. I don't think there are so many of those now.

Of course, as the lab has gotten bigger it's harder to do. There are 70-80 people in there now. Some of them I probably wouldn't recognise if I saw them. When we were in the lab., one knew everybody. Absolutely. There was nobody in the lab. that you didn't know their name. Another way that the lab. has changed a lot, in a different context, is — Calvin was always visible. He was visible in ORL because, again, that was open and, as you probably know, he was always out there looking at a chromatogram. That was the idea he carried over to the new building. You might say he wasn't as visible over in Donner but, since we had meetings once a week, he was visible then.

**VM:** When he came to Donner did he do the same thing as he did in ORL, actually talk to people in detail about their work?

**EB:** Yeah, I think so. I think he knew pretty well what was going on. Certainly you felt like if you wanted to talk to him about your work, I think sometimes, I don't remember, but I think sometimes would call us over to his office, about something, and say "Hey, Ed, I'd like to know a little more about what's going on, what you are doing". Not in a threatening sense, just in an interest sense.

**VM:** Did these social activities go on from the very beginning? When you joined were they doing trips up to the mountains?

**EB:** Some of us, yes. Certainly, in part, because a few of us had rather similar interests — Bert, Al, Dick and myself. I don't know if Dick mentioned but recently he got interested in the registers off the various peaks in the Sierra. He went down to the Bancroft Library where they retrieve the registers from a lot of these places and maybe put new ones (*in place*). Dick's very organised, you know. He knew that on August 6, 1949 he had been on a trip to this place or that place, Mt. this and Mt. that, so he found the registers, when he could, and he Xeroxed the pages from them. Some of the pages had Al and Dick's name, maybe mine on, maybe another one with Andy's (*Benson*). He had about half dozen or more of these. He just did this recently.

**VM:** I can guess what they are, but I have never seen them. These are registers kept on the peaks?

**EB:** They vary. More formally they were maybe a notebook, something like what you have or even smaller. Some of the more formal places would have a metal box that they

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were in. So, when a person got up to a 14,000 foot peak they would inscribe their name and the date and maybe some comments — “Beautiful view today” or “Hell, I don’t know why I did this” or “A storm is coming”. Some of them were less formal, nothing more than Prince Albert cans with pieces of paper in them.

**VM:** It shows you that we never got to the top of one of these peaks!!

**EB:** You should have. I remember some of the Swiss fellows that came over that went up there with Dick, Bert and myself, the Schmidts, Kathi and Hans Schmid.

**VM:** How about the wives? Did they go along too? Your wife? Dick’s wife?

**EB:** I guess most of the trips I was referring to were those climbing trips were before we were married, but not all of them. I think one of them afterwards; I’m pretty sure that Millie went on that one, up in the area of Mts. Williamson, Barnard and Kindall (that was probably before we were married). Bert was intent on climbing all of the 14,000 foot peaks in California and those three were three you could get in one weekend. The notable thing about that is going up Shepherd Canyon and I swore that if I went up that once I would never do it again because you go up over a ridge and down some, then you go up this long, long canyon in the sun, the stream is way down there and there’s no water coming in the side streams. We got up to where we could camp overnight by a little stream; there was a little bit of water there, enough to cook with. The next morning it was all dried up. We then went and found a camping place on some bare rocks. Andy and Al, I think, went quite often to the White Mountain because there was some research going on up there. Al may have been involved in that as part of his navy stuff. I don’t know, you’d have to ask him. That would have been before the 1950s or so.

**VM:** Then there was the lab. picnics — Tilden Park?

**EB:** Sometimes in Tilden. Later on, when Calvin got his ranch, there were a couple of picnics up there. They were often in Tilden. Nowadays we seem to have them more at the back of the lab. They had a few of them recently, during the last five years or so, up where the Kerr Campus is now, back in a field up there.

**VM:** Were those centrally catered or did everybody bring stuff?

**EB:** People either brought stuff and then somebody would take responsibility for getting the main things.

**VM:** They were usually one-day things or afternoon things?

**EB:** Afternoon. They still have so-called picnics or barbecues outside the lab. in the back there now. Vangie (*Peterson*) is a great one for organising things like that. If she had her way she would probably have a picnic every month.

**VM:** And then I remember Christmas parties.

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**EB:** Yeah.

**VM:** Presents? Were there presents?

**EB:** At the early ones there were. And I recall there was some poetry or something that went along with them. They were all over in ORL.

**VM:** Who were the poets?

**EB:** You know, if I gave a present to you, I'd want to make up something about it.

**VM:** I remember that Dick was a bit of a poet at one time. Did you write poetry too?

**EB:** Hell, no. Again, there are so many people, they (*the Christmas parties*) kind of disintegrated once Pimentel came, partly because he liked to have loud, loud music. Even now they have music. They try to put 60 people into that seminar room, 60 people start talking, I give up on things like that.

**VM:** It gets more difficult as you get older to hear all these things.

**VM:** Can I stop the tape? It's almost at the end.

*(Tape turned over)*

**SM:** I'm wondering what your first impression personally was of Melvin was, when you first met him.

**EB:** Well, I guess one was very excited, you know. I went to this ACS talk and heard this discussion of all the work going on in photosynthesis; at that time it was pretty well developed and it sounded like something that would be a heck of a lot of fun to work on. I don't think I ever felt that he was unapproachable or anything like that. I was elated when I got a job up here, jobs weren't than easy at that time. I had one other offer, from Lederle. I had the choice of going here or going back to Lederle; there was no competition and I recommended the Lederle job to a good friend of mine, a fellow by the name of John Brockman, who took it and was very successful there. He worked with Tom Jukes for many years; unfortunately, he's gone the past year or two. If I hadn't gotten this job I might well have ended up at Lederle, I don't know.

Another place I was interviewed for was this Baxter Laboratories, which, at that time, was a small outfit, now it's a huge outfit.

**VM:** From what you have been of the Calvin group over the years and what you knew of other groups before you came and of your time in Copenhagen, is it a very different outfit from other places?

**EB:** I think it was, particularly up until the time that Calvin phased out.



**EB:** Kalckar's lab. is the only other lab. that I had close experience with. That was a relatively small lab. Kalckar worked right in the lab., he was around all the time. It was great if you could understand what he was saying but apparently his English and Danish weren't too different. Finally, I didn't get along that well with him because I had different ideas of what I wanted to do than he seemed to have, even though we had spelled them out initially. It was a small lab. with 3-4 other people, couple of lab. technicians, and, if you removed all the walls, it would have fit into this place — in an old Danish building and, of course, much, much less in the way of equipment. He obviously was a very brilliant person but he was a kind of hard person to understand just what he was thinking. I enjoyed my year there, not so much because of the lab. (I didn't mind the lab.), certainly worked hard there. But I liked Copenhagen and all the things that it offered. It was a great city.

**SM:** So the dynamic of the group in the Round House, and before the Round House, was different from any previous experience you had had or any other group that you knew of in the matter of working.

**EB:** As a graduate student at Cal Tech, maybe there were, my lab. experience after leaving Reed College in 1943, I went to Cal Tech to join the war effort, shall we say. There was a group of us who were working on the analysis of war gases; I worked with Niemann and Swift. Niemann was very well known for his analytical procedures — inorganic analysis — so our job then was to phase down what he had designed for macro, down to where you can analyse 3 or 4 milligrams of some unknown war gas which he had perhaps collected on a carbon filter, or collected a drop or two. So we worked out an analytical scheme which I think was perhaps pretty good, I think. It was used for a long time. We developed a method of putting the sample, mixing it up and putting it in a little tube and heating it, as I recall. I remember Joe Neilands, some years later, said he still used that in his laboratory here (*at Berkeley*) for people to analyse stuff; he thought it was great. That was before all the days of modern instrumentation.

There were half-dozen of us working on that and a few of us got sent off after a year and a half to Florida, to a place called Bushnell. What we did there was to analyse bubbler after bubbler of samples that were collected out in the field: in other words, they would shoot off a mustard bomb and then they'd collect a bubbler for the first half hour, the next half hour, and so on, and bring them to the different stations; you would have to analyse them. My job was mostly to titrate, I guess it was with bromine these things, and you used an indicator. Finally, we noticed that the first samples were easy but the later samples got harder and harder; the end point wasn't as sharp. Then it was realised that what was happening: the first samples had one sulphur predominantly and then the polysulphur ones began to come off which was one of the things which led to the idea that the mustard was very persistent. In a way it wasn't that persistent; it was the impurities that people were discovering, detected six hours after the bomb. In humidity like that it was all gone. So, I spent too God damn long down there.

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**VM:** When you were at Cal Tech, was that in Pauling's department?

**EB:** Yeah, yeah.

**VM:** So you knew Pauling well?

**EB:** I wouldn't say I knew him well. I managed to survive two floodings of his office.

**VM:** You flooded his office twice?

**EB:** We had a big electrophoresis device, about this big around, and electrodes here, put your sample in I forget in where. The tubing broke or something happened in the middle of the night; it went down to his office. Some time later, a similar sort of thing happened. But he didn't kick me out.

**VM:** You knew him personally, saw him in seminars, etc., stuff like that?

**EB:** Yeah, yeah.

**VM:** Very different from Calvin?

**EB:** I would say quite different.

**VM:** They were both remarkable men. How would you differentiate?

**EB:** Well. I didn't interact with him, of course, like I did with Calvin, so it's a little harder to tell. But obviously he was a man with lots of ideas and some were right and some were wrong.

**VM:** That's true of Calvin as well.

**EB:** That's true of Calvin as well but I think maybe more of Pauling's were right, I don't know. It's hard to know.

**VM:** Did he bubble out, the way Calvin does (did)?

**EB:** Yes, I think he threw out ideas, yeah.

**VM:** Receptive to criticism?

**EB:** I don't know. I never criticised him.

**VM:** Do you find Calvin receptive to criticism?

**EB:** Well, I suppose so. It would depend who it is coming from. I think if it were more suggestions, he would probably be receptive, but if you — I've never ever tried it — point blank said "You are all wrong in this". Now, (*Mina*) Bissell would be a good one

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(*of whom to ask that question*); she was one of the few people in our group who would ever tell Calvin to his face that what he was saying wasn't right or something ought to be different.

**VM:** But if you gave him a reasoned argument, he would...

**EB:** Yeah. I don't think I ever had much reason to differ with him on things. There was a time when one went over papers somewhat with him. He would make suggestions and that sort of thing; there wouldn't be any problem. Later on, of course, he would look at them less carefully.

**VM:** In the early days, when presumably he had more time, did he write papers or was it people like yourself who always wrote the papers?

**EB:** No, he wrote a lot of his papers, I think. Yeah. Certainly he would talk. Lots of times he would give a seminar, kind of a lab. seminar before a talk, and he would go and give the talk, and putting the two together those would become at least a paper, maybe not quite as formal a paper as some others, but he would test out ideas. Marilyn would know better but I think early on he personally probably wrote quite a few papers. Some of the papers certainly had his name alone on them and I think he was always generous about co-authorship. I don't know of anyone that complained "Hey, I did half that work and I didn't get my name on the paper". I never heard of anyone making that complaint.

**VM:** Not at all, On the contrary. We have heard one or two cases when he felt his name....

**EB:** If anything, I think he would lean the other way and your name would get put on a paper. That was probably less prevalent then than it is now. I think nowadays the system seems to be put everyone that even washed the dishes on a paper, almost; if they type the paper up, put them on, almost too much.

**VM:** Going back in time, again, what did you do about actually writing them? Everything was written longhand, or did you all have typewriters?

**EB:** We had typewriters. I tended to write papers longhand and type them; I could type.

**VM:** You would type them yourself?

**EB:** I typed the first draft. Then, of course, we had people that would type the final draft either in our lab., or when I was working with Mark and Krech, why they had secretaries (*in the Psychology Department*). A lot of the papers that had my name on with Mark's I probably didn't write that many words on; he was good at writing and this worked very well. He would write a draft and I would make suggestions on it, we'd work it over together. The chemistry part I would usually have to write. The more general part, he was a much better writer than I was.

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**VM:** I was thinking of the days before word processors and memory typewriters and photocopiers, it was more difficult, wasn't it?

**EB:** I guess we had photocopiers, fairly early. There was one in Donner, not in our lab., but one in the library.

**VM:** In the old days?

**EB:** Yes. I remember there was an early Xerox down there, before it was a household name; it would be interesting to know when it was. I asked the broker about, would that be a good company to buy; he didn't know anything about it. I didn't buy any a — that was probably a mistake; that was when it was 10 and it probably went to 100 or more. That was before it was a common word like Kleenex or Coke.

**VM:** Generally speaking, did you have good equipment?

**EB:** I think we had pretty good... For better or worse, I wasn't like Mel (*Melvin P. Klein*); I never wanted a lot of high tech. equipment but we managed to buy a couple of pH-stats of our own, after using Neilands', bought from Cannon; he came in and worked on it. It seemed like a sophisticated piece of equipment at that time. We had a Beckman (*spectrophotometer*) that had a sample changer on it that went back and forth. In fact, I think I may have made a suggestion that they make something like that. When Hiromi was first doing (*this work*), taking a reading here, then pulling it there, pulling it there, running it down, then doing it again. We then got this one which was on a recorder, and you could get the slopes. That was tremendous. Nowadays that wouldn't be considered high tech. at all. Then, the Beckman was a work horse at one time.

**VM:** What did you do about library facilities? Donner was adequate for your needs and you didn't need to go much elsewhere?

**EB:** Donner and things down at the other end of the campus for things that were more psychology. There was never any problem with the library. Donner was pretty good in those days. It's only in more recent years that they've gotten this policy of journals on more than one place on the campus, probably on too many places. I maybe overstating it but they are so expensive. It used to be that you could find JBC (*Editor: the Journal of Biological Chemistry*) in Donner, the Main Library and might even have found it down in the LSB library, several places at least.

**VM:** So in those early days, you really didn't need to go down to the other end of the campus much.

**EB:** I did, because I was working with Krech and Rosenzweig. People would go down to LSB quite a lot, I think, and use that (*Biology*) library. That wasn't that afar.

**VM:** Did other people in the group form associations in other departments, the way you did with Rosenzweig and Krech (in Psychology)?

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**EB:** Some did. I don't think, I don't know how many did, you'd have to ask them. I think it was certainly encouraged. Al may have formed some with the people down in the photosynthesis area; I think he had a joint appointment for a while. When it was in terms of the time frame that you are talking about, I wouldn't know.

**VM:** So you felt by the time you had been in the pace a few years and were well established that you were pretty much an independent operator, you did whatever you thought fit? And he encouraged you?

**EB:** Yes, we wrote grants, and Calvin had to sign-off or write a letter supporting it. There was no problem. It wasn't quite the...Most of these grants went through Campus rather than through The Hill because there was less red tape involved at that time. Subsequently, there got to be some problems. Again, the time frame is somewhat different, but it seems to me that one of the things that we used to do that has been subsequently lost was there was usually a certain amount, a pot, of money for postdocs. They were kind of passed around to different people from year after year. In other words, let's say Dick had a person the last two years and I hadn't had anybody, then Dick wouldn't get someone and I'd get someone, or Al would get someone. The other thing you were asking about, equipment: there was usually a general sharing of ideas of what kind of equipment was needed and what the budget was going to be, it wasn't like now, I think: there are more unilateral decisions made to buy us \$60,000 piece of this or that which may or may not do more than gather dust. I know there's more than one piece up there (*in the attic*) that hasn't done much more than gather dust over the years. Somebody thought it was real cute, certainly a few years ago.

One of the things, again, post-moving into the Round House, that I always felt was kind of a decline, was when we to give up the downstairs storeroom. Then, everything was all parcelled around upstairs (*and in the halls*). Up until that time, everything was downstairs; if you wanted a beaker, you went down and a beaker, if you wanted this or that, and we had storekeeper; and we lost a certain amount of cohesiveness when that happened. That was partly to make room and partly because of budget, and so on.

Then, of course, with this Tiger Team; I don't know if anyone has told you about the Tiger Team recently...

**VM:** No, I don't know about the Tiger Team.

**EB:** Yes, you probably don't. A few years ago the Department of Energy, of course, was coming under great criticism for all their contamination. So the then, guess it was Watkins, the ex-Navy guy (*who was head of DoE*) decided he would take charge. So he had these Tiger Teams that went out to various places, including ours, and how much money it cost the lab. and how many days to clean it all up? Some of it was necessary, of course, but they had to throw away oodles and oodles of stuff. Talking about Marie (*Alberti*), she was kind of cast in the middle of that. One day they would come along and tell you that these things should be on a shelf separate from these things, or they could be on the shelf with these things, and then they would come around and say no, they can't be on the same shelf, they have to be on a different shelf.

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I think I never saw Marie come as close to breaking as with that. She worked real hard on it (other people did to) but her efficiency, she was the one that got the brunt of it, and, fortunately, she has come with us on —Millie has organised a number of trips to benefit Mono Lake and fortunately one of these trips came up so she got away from it all, near the end.

**SM:** During what period was this?

**EB:** This was in the last few years.

**SM:** That recently?

**EB:** But now they expanded the Health Safety group up in the lab. (*LBL*) to 110-120 people up there (a huge number) and they have developed all these protocols of how you gotta do stuff and, if you have something coming out of the system and if your procedure says to mix A and B, and you have it written down, you are probably all right. But, I may be exaggerating a little bit but not too much, but if your protocol doesn't say you mix A and B and then you go ahead and mix them, you may be in trouble. And, of course, the storage business: of course, some of that's legit, but it seems like lately they've slacked off. I don't want to say anything to Marie but her desk is somehow the messiest lately. They came into your office and you had to make sure that you didn't have anything up high, like those books that might fall on your head. That wouldn't be safe up there. Mel (*Klein*) managed to somehow escape the... The problems with his office — I guess he is just moving now, or has moved his stuff, because today when I was poking my nose in there, I think I saw Marie and someone else looking into what was Mel's office. I didn't look but maybe they are painting it or something, I don't know.

**VM:** In the days when you started and for many years after that, presumably safety and things of that sort were differently regarded?

**EB:** Differently regarded, yeah. Nowadays if you should throw one atom of carbon-14 down the sink you probably would have problems.

**VM:** But there probably weren't too many accidents, were there?

**EB:** No.

**VM:** Nobody died?

**EB:** There was a lot of — a few months ago there was a lot of concern about experiments that may have been done unknowingly to people without their permission at LBL, with radioactive isotopes. I don't think carbon-14 ever came out, but if it did, I can see Bert maybe having problems. I think they were more interested in those with the heavier isotopes. In retrospect, it's easy to say, "Well, you shouldn't have given that person 10 microcuries of plutonium", but you didn't know at that time.

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**VM:** After you came back from Copenhagen in '54 you actually spent the whole of the rest of your experimental life in the lab.? Did you ever go away again?

**EB:** No.

**VM:** And now you've retired? When did you retire?

**EB:** I don't know. In terms of pay, about 8-10 years ago. But then I had part support from down in Tolman until two or three years ago.

**VM:** Do you still retain an office in the building?

**EB:** Yes, I'm getting ready to move out of it, though. I have taken out some stuff, and as I was telling Sheila we have been involved in cleaning out the downstairs, so when we get that cleaned out I'm going to bring this stuff from the office home. Then I'm going to hear some more static.

**VM:** Did you ever think of moving to one of the academic departments?

**EB:** The opportunity never came along and I never looked very hard. This was nice, you know, until 10-15 years ago. You could get a grant and, between that and the support you got from the Department of Energy, you could run a pretty nice program.

**VM:** Aside from grad. students, have you done any teaching, or much teaching?

**EB:** A little bit. Occasionally down in Psychology there were a couple of semesters when I taught a "chemistry" course or whatever you want to call it, for psychologists: talking about receptors and things like that there. That was probably 20 years ago.

**VM:** You haven't missed not having a lot of teaching?

**EB:** No. I kept busy enough without it.

**VM:** You know, that's excellent. If one can continue to be active all the way through, that's great.

Well, shall we call it a day?

**EB:** Yeah, I gotta go.

**VM:** Thank you very much for coming and thank you telling us so much.