Chapter 11

OSMUND (OZZIE) HOLM-HANSEN

La Jolla, California

June 3, 1996

VM = Vivian Moses; OH = Ozzie Holm-Hansen; SM = Sheila Moses

VM: This is a conversation with Ozzie Holm-Hansen in La Jolla on Monday, the third of June 1996. The first thing I have to ask you, then, is how come that you got involved in the Calvin group? How did you ever get to Berkeley in the first place?

OH: I got my PhD in Wisconsin in '54 in chemistry/biochemistry with Folke Skoog, got a Fulbright award for postdoctoral work to go to Norway to work with Brorud (*correct spelling?*) and during that time I shifted my allegiance a little bit and I worked down in the chemistry building at the University of Oslo. During my time there, one of the visitors who came through the lab. and gave a very interesting lecture was (*Arnold*) Nordal, the discoverer of sedoheptulose (*as an intermediate in the biochemistry of photosynthesis*). He had just come from Calvin's group. This was my first exposure to all the great things happening out in Berkeley. I talked to him quite a bit, did a lot more reading. Toward the end of the year I was planning to spend about a month travelling all through Scandinavia on a bicycle. One day I got a letter from Calvin, who had been in touch with Folke Skoog and who was looking for somebody with phycological interests and abilities and Skoog had given him my name. And he said "would you like to come out and join us?". So, I quickly packed my bags, cancelled my tickets for my month's stay Scandinavia and headed for Berkeley.

SM: Which year was that?

OH: 1955.

VM: So, you arrived there at what time of year in 1955?

OH: It was probably August 1955.

VM: You had never met Calvin before then, never seen him"

OH: Never seen him, never met him. We had had probably two or three exchanges of letters while I was in Norway. I remember the first time I met him we were standing down in the ORL which is no longer there — remember that great big table with the white top of some kind, where we used to spread out about twelve chromatograms? We were having coffee and he came in and I met him there. I remember the day well.

VM: By that time, you were, of course, already committed to him, you were there.

OH: Oh, sure. I had been working a couple of weeks.

VM: What impression did you get of him when you first met him?

OH: Not much the first time, because he never said too much, other than a warm welcome. You get the impression of a rather intense — of course, I was aware of the history of the guy, that he was eminently successful. I had read a lot of his books. At Wisconsin I had done a lot of work with chemistry and tried to purify salts. I knew his book on the theory of organic chemistry and all his chelate chemistry, so I was aware of the nature of the man I was meeting. He had a huge reputation and was very accomplished. You can't help but be impressed by that. At that time, he was in good shape. He was, I think, a lot slenderer than he had been some years previous. It was a nice friendly short, not much social chit chat, just a welcome to the lab. and that was about it.

VM: Before you got there (*to Berkeley*), you knew something of what the lab. did because of you talked to Nordal. Had you read up a lot of the papers and were you pretty familiar with the research?

OH: I was pretty familiar because I was interested in photosynthesis, I had always been interested in photosynthesis and photobiology.

VM: Was Andy Benson still there when you got there?

OH: He was just in the stage of leaving. He had actually left the lab. But I met Andy just about the time I came so we've been good friends ever since.

VM: When you first got to the lab. did you have a defined job to do, a defined set of responsibilities? How was it put to you?

OH: I did have the one responsibility of taking care of anything and everything to do with the phytoplankton, the biological collections, which included the succulent plants we were growing for sedoheptulose. I remember we had a little greenhouse in the back of ORL. So, I had to maintain and grow all the mass cultures, chemostats, all this stuff. That did not take too much time so the rest was just free essentially to do any kind of research that you wanted.

VM: By the time you got there the algal culture technology was up and running? People were doing it?

OH: It was in pretty good shape. We had the big mushroom flasks and the big batch cultures, the shakers. We added a lot of the tubes for the chemostat where we maintained the cells in exponential growth.

VM: Did they exist at the time that you arrived there?

OH: I don't think so. In fact, I think this was one of the impacts, if you want to be so generous as to call it impact, that I may have had during my time in that group. Before I came, they were basically chemists working the lab. , with the exception of a few people like Norris (*Louisa Norris, Rich Norris*) that preceded me. He was kind of a taxonomically oriented phytoplankton person and I came with a strong background in some chemistry, biochemistry, physiology, but also a strong biological background. So I was aware of the overall functioning of cells, more than a lot of chemists, I think. I remember talking with Calvin very soon after I came about the nature of his experiments, when he talked about steady-state. By steady state they meant taking a culture of *Scenedesmus* or *Chlorella* in a little flask, or tube, and flushing it with nitrogen for sometime to eliminate all the CO_2 and then shooting in CO_2. I don't think you can visualise that at all as a steady state. Things are happening. I remember when I mentioned to Calvin that this might affect the pathway of incorporation of C^{14} he pooh-poohed it completely and said that that would never be. So we had a dichotomy of thinking. I think I had a better intuitive feel for metabolism of cells and the importance of all the environmental parameters: temperature, light, pH, nutrient concentrations, etc.

VM: And, of course, those were some of the things that we actually worked on in the years that you were in Berkeley, some of the factors...

OH: You remember when I left, Al Bassham had this beautiful lollipop. You remember the old lollipop experiment just with a simple glass vessel? By the time I left, he did this all by himself, with his people. I did not contribute to his instrumentation there. By the time I left, he had the lollipop very nicely instrumented so he could control almost everything, all the environmental parameters.

VM: Eventually, we will come to when you left, and so on. At the beginning, then, you were responsible for the algal cultures and the rest of the plant material. Who did you work with at that time?

OH: Who did I work with?

VM: Was there a technician who did the routine work on it?

OH: There was one person, Althea Vann, and she left sometime after I came in there, some months after I came. And then I had another technician by the name of Pat Smith...

VM: Pat Smith

OH: ...Patricia Smith

VM: The redhead.

OH: The redhead. She stayed for a couple of years. Then, Paul Hayes was there and he helped a lot. He helped a lot with the instrumentation, electronics, etc.

VM: But it was these ladies, Althea Vann and Pat Smith, who did the routine...?

OH: They did all the routine culture work, media preparation, things like this.

VM: In that first year, when you got there, who were your friends in the lab. ? (*Laughter*) Well, I know I'm digging.

OH: ...part of my marriage from 40 years ago! I think I remember nearly all my friends, but what year — if they were there in my first or second year, I'd have a little trouble being sure.

VM: But I can help you because I was there your second year. So, if I don't know them...

OH: There was Duncan Shaw...

VM: He was coincidental with me.

OH: ...second year, Utz Blass. You were second year or first?

VM: Second.

OH: Kojiro, from Japan. he German girl Stange...

VM: She was later still, in your third year, I think.

OH: Maybe in the first year I guess I just worked hard. (*Indecipherable*) We started all the camping trips and socialising later.

VM: You mean that you didn't go camping before you spotted my great enthusiasm?

OH: Not too much, no. I was new in Berkeley and was enjoying the life on Telegraph Avenue. We lived just two blocks away from Telegraph.

VM: Whereabouts did you live when you first arrived?

OH: Two blocks off Telegraph...

SM: Channing?

OH: Channing. That's right, Channing.

VM: Then you moved to Spruce, 1218. That's while we were there. We have friends now who live at 1216.

SM: Or even 1220.

VM: Or even 1220.

OH: That's right. The first night we were there we had a big party. We had the big Belgian...

VM: Chris van Sumere.

OH: ...who was very loud and the landlord almost threw us out the next morning. He said we had too noisy friends!

VM: Maybe you can remember the occasion. There was an occasion of a dinner party in which you and he (*Chris van Sumere*) and I all came in tuxedos. You had this lovely white tuxedo with a red cummerbund.

OH: That all happened a long time ago. Don't tell me wife or she'll...

VM: Do you remember there was such an occasion? In fact, it was for Chris' son's christening.

SM: We had him believe that in the west, as it were, on such occasions everybody dressed like that.

VM: So we did. I think we had the right clothes and we were the only ones dressed...

OH: That has faded from my memory.

VM: What sort of research work did you start when you got there?

OH: I spent quite a bit of time, probably a month or so, digesting everything that had been written in the lab. I just went through and read everything I could. I looked at the cultures and did some thinking. I started to do quite a bit of work with the blue-green algae which hadn't been investigated very much at the time; I was interested from my Wisconsin days. I actually did quite a bit of work with the blue-greens during my three years in Berkeley. They aren't too easy to work with as they are not unicellular. Well, there are some unicellular ones but we had filamentous ones, *Nostoc* and *Anabœna*. The reason I got involved with that — there was a spot below alanine on the two-dimensional chromatograms which sometimes got very hot, which we did not know (*anything about*). In fact, that's the one which that turned out to be a carbamyl phosphate. That was Pekka Linko, must have been there during my first year.

VM: He was, yes.

OH: OK. So I spent most of the first year with Pekka Linko, that's right, trying to find what that unknown radioactive spot was. It was carbamyl phosphate. The reaction is

very intense, it's very strong; you get a lot of incorporation of C^{14} via carbamic phosphate synthetase reaction in blue-greens.

VM: That came out of the regular photosynthesis type experiments, lollipops, and all the rest of it?

OH: Yes. That one was also carried on in the dark to some extent. The reaction doesn't drop off nearly as fast as the RuDP carboxylation reaction.

VM: As a plant physiologist you were probably one of the first ones to extend the whole chromatographic thing away from the photosynthesis, as such, into dark reactions and other sorts of things.

OH: I wouldn't be that generous to what I did. There's a huge history on dark fixation and if you look through the old literature — Calvin published quite a bit on dark fixation, mostly in the tricarboxylic acids, organic acids. There was a lot known.

VM: As the time went on into the later period; how long were you there, actually?

OH: Three years, '55-'58.

VM: You left in '58? That must have been soon after we went, in the summer of 1958.

OH: I went back to Wisconsin.

VM: What did you do in the later two years, when you say the first year you had your nose to the grindstone, and it was only when my gang turned up that you branched out socially.

OH: I did less work and more play.

VM: We produced a lot of papers, as it happened, in those years.

OH: I did quite a lot of work with "Kishino" on the effects of salts. This was one of the questions that Calvin and I had about the importance of nutrient concentrations and getting away from the distilled water.

VM: I'm, not sure I know who this guy is.

OH: Kishino , the Japanese?

VM: Is that his first name or his last name?

OH: Nishida.

VM: Oh, yes, yes indeed.

SM: Kojiro Nishida.

OH: This was one of the points of disagreement between Calvin and myself. The importance of the nutrient solution in which the cells are suspended. Nishida and I published one little paper on it, on the different paths of incorporation of C^{14}, depending on whether or not the cells were in distilled water or in some kind of nutrient solution. Then I did a lot of work with Duncan Shaw with the dark fixation, mostly of blue-greens. I don't like to think about those days because we were pretty stupid and I think the DoE, the whole history of the....Maybe I shouldn't say this.

VM: No, no. Go ahead; it's all old history.

OH: I've been involved with the DOE on many other types of investigations, such as working out at Eniwietok where they had about 50-60 atom blasts. I think DOE showed a horrible disdain and lack of interest in protecting humans from radiation. I remember that Duncan and I — this is something that I would never do again — we were working in the dark room. In fact, a lot of our experiments we did in the dark room. We would have a boiling bath, we would have one of these...in fact, I still have one of these little jars over here...with 1 or 2 ml of liquid suspension We would dump, then, usually, 400 microcuries of C^{14}. That's a lot of radioactivity. For field work in the Antarctic or general oceanograph work I simply use somewhere between 1 and a maximum of 5 microcuries. Here we were using 400 in one small volume and then we would dump it into boiling methanol, which means we suddenly get a huge cloud of radioactive C^{14} which you a breathing?

VM: In a hood?

OH: No, in the darkroom.

VM: With no hood in the darkroom?

OH: No hood.

VM: No decent ventilation?

OH: I don't remember any ventilation in that darkroom in ORL.

VM: Was it dark so you couldn't see what you were doing?

OH: We had a little dim red light somewhere in the corner.

VM: That wasn't terribly good?

OH: At the time no one was particular. Everyone knew, we used to spend a lot of time arguing after hours about the radioactive safety labels, safety limits, how much radiation you, as a worker, was allowed, and how that compared with workers in Russia. In Russia they had three different levels, remember, depending on the importance of the person: Ordinary workers could get a huge amount of radioactivity, and very important personnel would get probably 1% of that. In our country we only

had one (*level*). The DOE, back in those days fully recognised the problems, but always the bottom line was the financial. The bottom line is that if you make everything safe from the standpoint of health of the individuals then everything gets so expensive you can't afford it.

VM: And that was true, you felt, even in Calvin's lab. ? Did you try to institute safety measures?

OH: No, I didn't, because at the time, I guess you are young and you don't worry about those things. I would not work under those conditions today, though.

VM: Did you have a monitor with you, a Geiger counter monitor?

OH: Not in the hood, no. We wore little (*radiation*) badges. They are mostly for protection by the agency against possible litigation in the future. We still use those things but I don't have much confidence in them.

VM: You didn't have a clicking Geiger counter to warn you if anything got too hot?

OH: We did in the office. You remember you and I would be sitting up on the second floor of ORL looking down on the 37-inch cyclotron. (*Editor: It was the Crocker 60-inch cyclotron*.) Sometimes in the late afternoon, it came to 4:30 or 5, instead of going click, click, click, it would sound like a machine gun. That's when we picked up our coats and decided to head out and go somewhere else.

VM: As far as I know, none of us who were in that lab. in those years have suffered. Obviously, Martin Kamen made a point that he's sixty years beyond us.

OH: That's true. That's one of the things that I don't, one of the reasons that I don't worry about it. Because CO_2 — after all, you breathe out 4% CO_2 so it's a readily exchangeable atom. I'd be much more concerned if I had been working with cobalt-60 or radioactive calcium.

As a matter of fact, we did — I just remembered that one — A very interesting piece of work which I did which was never published. I worked with cobalt gamma radiation (cobalt-60). Was that while you were there?

VM: I don't remember.

OH: It might have been in my first or second year. They had a separate room, up in...I forget where it was; maybe up in Donner...which they had maybe the country's biggest source of radioactive cobalt. You put the sample in and then go in the other room and operate the controls. I irradiated *Chlorella* with huge amounts of gamma radiation, quickly ran back to ORL and checked the standard experiments, the path of carbon in photosynthesis. It was amazing, neither the rate of photosynthesis nor the path of carbon in photosynthesis was affected for hours. Those cells grew, they became giant cells, they were alive weeks later, about ten times the volume and double the yield...the diameter.

VM: It had been a genetic effect.

OH: Sure. The ionising radiation had knocked out the mitotic (*mechanism*) and damaged the DNA, of course, which we know a lot about now. It was interesting to me. In fact, this has carried over into a lot of my present work. I do a lot of work in the photoregions with UV radiation and that only extends, of course, down to 280 nm. We are very much concerned about damage to DNA and photodynamic action of free radicals. This was a very good illustration that strong ionising radiation could knock out the ability of a cell to divide and not have any discernible effect on the path of carbon (*in photosynthesis*), in the rate of photosynthesis or the path of carbon, for hours to days.

VM: I'm not terribly surprised, because DNA is much more sensitive to hits because it's a template rather than the actual enzyme molecule.

OH: One of the biggest areas of research nowadays, since about 1987, has been the biological effects of UV radiation because of the ozone hole. If you look at the big bulk of that work, probably about 95% is being done with looking at short-term photosynthetic rates. I have done a lot of this myself where you measure rates of photosynthesis with or without UV-B or UV-A for anywhere from 4 hours to 12 hours.

VM: How do you measure it these days?

OH: Just CO_2 incorporation.

VM: Hot CO_2? Radioactive?

OH: Yes, C^{14}, sure. Nowadays we use liquid scintillation counters. Back in the Calvin lab. days we had to use Geiger-Mueller tubes and I also used the crystal scintillation counter, the predecessor of the liquid scintillation counters. In fact, that's kind of interesting: we had to go to Crocker Lab. , and they had a big crystal of sodium bromide, or something, and we put a sample into the crystal and count. Now, we have scintillation counters all over the place and when you tell students you were doing work on this problem before scintillation counters, in fact if you are telling them you were working in oceanography before there were any CTDs.

VM: What are CTDs?

OH: Conductivity/temperature/depth, it's a standard. Now they have disposable CTDs: you just drop something over and up a thin copper wire you get a complete profile of the salinity, conductivity and temperature with depth.

VM: All of those things in our lifetime; who uses carbon paper nowadays? (*Laughter*)

OH: I wasn't sorry to see that one go. But this work with the gamma radiation was interesting. Unfortunately, I never published it. I refer to it and I have to refer to one

of the old quarterly ONR reports, which is impossible for anyone to get hold of, I think.

VM: I was thinking recently that one of the things we used to do while we were there, we used to go for lunch somewhere and I was trying to remember where it was. It was some sort of cafeteria somewhere on the south side of the campus. Do you remember where it was? We used to go and line up and get something there.

OH: Are you talking about the old hamburger joint off Telegraph?

VM: I had an idea it was a campus facility of some sort, but I'm not really sure. Part of the student union, perhaps? It doesn't ring any bells for you?

OH: No.

VM: I remember standing in line once with you and Utz Blass arguing about how to pronounce the name of those animals which look like horses and have black and white stripes. I said "zebra" and you said "zeebra". We asked Utz and he said "Of course, it's "tsebra".

OH: It wasn't on the menu, I trust.

VM: No, it wasn't on the menu, but there was some reason why we were arguing about that particular word.

OH: Don't remember that.

VM: You remember the seminars, however, on Friday mornings?

OH: I do. One point I wanted to make earlier; I remember — well, you were involved with this, too. We worked with Ning Pon isolating various cell organelles and looking at compartmentalisation of reactions and enzymes within the cell, which is still the most interesting problem. In fact, one of the reasons I was interested in that, and it's still important in a lot of work, is interpreting the data from C^{14} incorporation experiments. If it's newly-incorporated C^{14} it is deposited in the chloroplasts as a storage product and the respiration is in the mitochondria and you liberate the cold CO_2, then it's hard to determine the net rate of photosynthesis. Trying to correct for respiration is still a problem for which we have no easy answer in biology.,

VM: We did two papers on that. We did that one that you just mentioned. We also did another one in which we tried to look at the dynamics, kinetics, of hot CO_2 in and out from respiration and photosynthesis in dark and light. You remember: that was in the *Journal of Molecular Biology*?

OH: With the big lollipop set up. That's when you and I worked late one evening.

VM: Probably, yes.

OH: In fact, that night...I think about that set-up because it's a period when we were blessed with beautiful facilities. I remember for that set-up we had the C^{14} by ionisation counting counter, the oxygen was O^{18} circulating gas phase, something which I never had the access to that kind of equipment since. I remember one interesting thing (you might want to delete this from the tape). Remember, we were measuring CO_2 with the infrared gas analysis and there's a little intake in that thing. We were using CO_2 for cooling. We had the IR down below the level where we were using the dry ice. We had the CO_2 flowing down, like the fog coming over the San Francisco hills, and it was affecting the CO_2.

VM: I don't remember that; maybe I've wiped it.

OH: I remember it very distinctly. I have always remembered that CO_2 flowing down.

VM: You couldn't actually see it, could you?

OH: Yes.

VM: You could see the (CO_2) fog?

OH: Yes.

VM: We used to live in an office, didn't we, on the second floor under the roof of ORL.

OH: That's right.

VM: I don't remember now whether it had a pitched roof, did it, in the office?

OH: I think it did, yes. It was overlooking Crocker Lab.

VM: It was remarkably untidy, I remember. There were three desks.

OH: Your desk! Mine was....I think mine might have been even better than yours.

VM: I think I have a photograph somewhere. Who was the third person who shared the office? Do you remember?

OH: I don't remember.

VM: Was it Nishida?

OH: I think there were rotators in there. Do you remember who occupied, who was in charge of the Crocker Lab. at that time?

VM: No, I don't.

OH: Hamilton.

VM: Hamilton?

OH: Doctor, MD. He had his own operation there. He was the one who died of thyroid cancer. I think that's a very classic case. His family, as I recall, took it to court and he was declared "death from accidental..." He died because of some job-related fooling around with radioactive iodine.

VM: And we were, of course, pretty close to that thing.

OH: Oh yes.

VM: When we used to go down into that underground counting room, the basement room, where we had all the Geiger counters, did the background rate shoot up there in the late afternoon like it did in the office?

OH: I don't think so. I think it was shielded.

VM: There was a lot of concrete round it.

OH: Yes.

VM: You were the guy who taught me the Calvin technology when I first arrived because you were a year ahead of me. We worked, I think, on the second floor and we had a bench in a big lab. outside the office, didn't we, as far as I remember. For general work. Obviously, we used the big facilities whenever we needed them but I think we had our work benches (everybody had their own work benches), as far as I remember this was on the second floor. I can't remember what the lab. was like; can you remember it, what the room was like up there?

OH: I remember we had all the apparatus. We had all the shakers and temperature-controlled growth facilities up there.

VM: Was the upstairs divided into rooms or was it a big open space?

OH: Upstairs, that's where we had the chromatography room with all the tanks where we used to walk around with the hood. We did have good precautions for the phenol and the propionic acid. We put on a helmet with air being circulated, compressed air.

VM: Did you use that? I think I never did.

OH: Of course. Phenol is nasty stuff.

VM: I think I've survived it; that's been a long time.

SM: You used to come home smelling of it.

VM: I remember one of the things that we worked on in the early days was on D_2O, growing *Chlorella* in D_2O.

OH: In fact, that effect was inhibiting the rate of sterilised mice, or something.

VM: We tried to grow it (*Chlorella*) in pure D_2O and we had great trouble and we never succeeded in getting more than about 60 or 70% as I remember. I think somebody else later on did.

So you were there...What made you leave, in the end? You were there only three years on a job which you presumably you could have held you indefinitely if you'd wanted to.

OH: Not really, no. I always visualised this as a postdoctoral training period, not a permanent thing. I had the offer to go back to Wisconsin as an assistant professor, where I had a very nice laboratory, so that was very enticing to me.

VM: When you were hired in Berkeley in Calvin's lab. , was there any discussion between you and Calvin as to how long you would stay?

OH: No. I assumed it would be of limited duration, two or three years.

VM: He never said that.

OH: I don't think we discussed that, no. The initial correspondence was such that — I don't think I saved the letters — but I went there with the full intent of just trying to do a good job, learning as much as I could, getting a good foundation in that work, and then moving on to a teaching faculty position. I have always liked to move around, change one's area of research, I think it's very stimulating. From my experience, as I looked at people who had stuck at research in one lab. in a non-teaching capacity, and a lot of the Calvin's group were researchers without any direct contact with the faculty at Berkeley, I think there are diminishing returns after some years.

VM: Why?

OH: For one thing, to get back to one of the points we mentioned earlier, the Calvin group had existed for such a long time and was so good for such a long time because it had a strong, very strong leader. I think if you are a good scientist I think you decide on what you want to work on yourself, you want to approach a problem yourself, you want to think for yourself. Once you are a member of a big group, like working in industry where they give you monthly work assignments in your mail box, you lose that individuality and imagination. In the long run, it's a great period for learning and broadening yourself but I don't think it's in the best interests of a potentially productive scientist to be a kind of a hired hand or to be subservient to a distinguished person for more than some three or four years.

VM: Did you feel — clearly you did. You felt that Calvin was a great influence in what you did?

OH: Of course. I learned a lot from him.

VM: You learned a lot but did you feel that you could have done whatever you wanted to or did you feel constrained?

OH: No, no. That's one of the good things about Calvin. He never negated or expressed dissatisfaction; he was very critical, so you always tried to do your best job. I don't think he ever said "don't do this or don't do that". He was always very receptive to any ideas.

VM: Anything you wanted to do, any bright idea that you developed or thought up, and you decided you would like to run with it, you could have done it?

OH: You could have done it, right. I think that's a mark of a person....I'm not surprised. I think any good scientist would have this inclination.

VM: One of the characteristics about him was that, once the main path of carbon was resolved, I think he was not after any particular target anymore and he was willing, maybe even before then, he was willing to entertain lots of other ideas on how the subject could develop. Maybe in the early days, and I'd have to talk to other people about this, maybe he was more single minded about what he was looking for, certainly not towards the end. Did you miss the fact that you didn't have any of your own graduate students in Berkeley?

OH: No.

VM: Did you miss that?

OH: No, because that wasn't my role there. We were strictly hired researchers and as such when Calvin got other visitors like Nishida from Japan: I remember Nishida's English was very poor and Calvin saw him once, I think, and then handed him over to me to take care of and then the next time I saw him, I think, was at his farewell party! (*Laughter*) We never had any students but we got visitors to work with, to help start on their research projects.

VM: You didn't miss the absence of students?

OH: No. I had just come from my PhD at Wisconsin with a year of (*postdoctoral*) research (*in Norway*) and I wasn't in the mood for trying to teach or educate graduate students.

VM: You wanted to spend all day every day doing research.

OH: I was still in the 100% learning mode myself.

VM: What about the social life as it developed in the later period, in the second and third years that you were in ORL: the parties, the camping trips, and all the activities outside the lab.? We remember you as being *very* involved in all of that. Do you remember it like that?

OH: I do, I do. I look back upon those days a lot. Now I tend to work here in the lab. about six days a week, including almost every evening. I remember working in the evenings a few times with you when something really demanded extra effort. Most of the time we did a lot of fun things.

VM: You used to play a lot of sports.

OH: A lot of sport, a lot of camping, a lot of social life.

VM: How about the parties in the lab. , do you remember anything in particular about them?

OH: Not particularly, except that we always...we had the usual Christmas party. We had very good attendance at Christmas parties, we always had nice farewell functions when people left, Calvin used to have social functions at his house. I think, in looking back upon it, it's probably about the best functioning large group I have ever seen, very diverse personalities, different ages from senior research people to very young students.

VM: How do you account for it?

OH: I draw the analogy to this other group I was involved with for 22 years, the Fuching (*Editor: spelling correct*?) Research Group which was world famous.

(*Interruption*)

OH: The Fuching group, the leader of that was originally John Strickland who was a brilliant guy who came from England during the second world war, trained in chemistry. After the war he went to Nanaimo and he and Tim Parsons revolutionised biological oceanography through their expertise in biology, chemistry and biochemistry. He died when he was 49. We had a big group of about 40-45 people and we were left with about 7-8 senior people, like myself, so we ran it for one another for 18 years but never had one person who was **the** boss. We rotated chairmen so it was always musical chairs. We rotated the chairmanship and all the other functions of running a group. Eventually, the thing kind of just fell apart, even though we were all world known scientists with a lot of graduate students. It fell down from the conflict and problems — interpersonalities and it also fell down socially. Calvin's place, I don't think you ever had the possibility of this happening. I think everyone intuitively recognised Calvin as the dominant person and everyone had direct lines to him. You might not like everyone in the group but you never let it interfere with the overall functioning of the group.

VM: Were you ever aware of him not communicating with people except in the Nishida type of case where he felt there was a language difficulty. Did you feel — don't mention names if you don't want to — that there were people that he didn't like or that there were personal conflicts in the lab. ?

OH: He threw out one student, I forget the guy's name, who had done something bad, he just threw him out and told him never to show up again. Don't remember the person's name. Let me tell you one thing that bothered me a little bit about that time. It had to do with radioactive isotopes and you'll be talking with the important people so you can correct me or correct any mistakes.

VM: Get their point of view, anyway.

OH: Right. We had a graduate student in there by the name of Karl Knut Lonberg-Holm who was very bright and you'll be seeing him, I trust.

VM: I'm afraid we won't be seeing him this year but I am in touch with him.

OH: This was at the height of the Cold War and the Russians were setting off bomb blasts in Siberia. He took it upon himself to make wipe tests on automobiles in Berkeley and report the data to the local public broadcast radio in an effort to inform the people of what was happening worldwide. The story I got is that he was called into Calvin's office and Calvin was acting on behalf of higher-ups in Washington. He essentially gave him the order either cease and desist from any such activity or get out of my lab. He could not do both. This was one thing that I thought was very bad on Calvin's part. It's another illustration of however strong and powerful you are in the world of science, you are subservient to the people handing out the money in Washington. He had a huge lab. and he needed the money. That's one reason why he was so productive. Most of us write proposals, spend half of our time nowadays writing proposals. There, none of us worried about money. Calvin got all the money for all the equipment we needed, all the salaries, all the support facilities. He could not jeopardise that even for a good scientific public relations reason. I found that very disturbing. I thought he should have been big enough to tell the AEC that there was no interaction between what the graduate student did on the outside and what was done in the lab.

VM: Unfortunately, I think it's too late to ask him about that.

OH: Oh I think so.

VM: I don't think he would remember.

SM: He doesn't remember most things, unfortunately.

OH: Sure. But even today, pressures of all kinds, political pressures — I see it around Scripps (*Scripps Institution of Oceanography*) all the time. However high you go, the Chancellor's ear is going to be sensitive to the demands of the Regents who are very

economically successful business people. It's a continuous circle, like the food chain of one person biting the other.

SM: I think, apart from anything else, until his (*Calvin's*) daughter, Elin, was arrested in the student revolts, I think it was in '68, (*Editor: it was 1964*), I don't think he really had any sensation of political awareness. He just did what was convenient for him and really seemed to have no political opinion. That hit him.

VM: Can I correct that? Those revolts were actually in '64 over the Goldwater (*Republican convention in San Francisco*). Rather earlier.

OH: I guess with that....I thought you might ask me my general impression of Calvin. You asked me my initial impression.

VM: Please; go ahead.

OH: I'll give you my summary after having spent three years there and I left with the firm, firm realisation that he was a brilliant man when it came to chemistry, a very hard worker, a man who was dedicated and, I think, focused on only two main issues in his life: his science, which was very encompassing (hard core science) and his family. He was a very devoted family man and very hard-working imaginative scientist. But you could never get him to think, or discourse, or show any enjoyment of any other activities which many of us enjoyed...sports, relaxation, social life, opera, music, baseball. I remember having lunch with him at these weekly organisation meetings over in the Faculty Club; the only thing he ever showed any interest in was science and then family. He is a complete blank to me in terms of all the other interests which many of us have in life.

VM: He did change a little after he got his Nobel Prize and was drawn into the Washington circuit. There he learned about another style of life and about other sorts of people and he became involved in politics, not party politics, of the Washington advisory groups. He changed a bit.

OH: I'm not sure that's a worthwhile goal.

SM: He probably learned how to handle it, rather than to have opinions.

OH: A couple of other impressions that I have of Calvin...one of the things I learned from was never to go far beyond the knowledge that you are sure of. He was absolutely, what's the word?, not tough, but he would cut anyone down. He was certainly smart enough and this was certainly true in the Friday seminars we had. There might be a professor from Germany speaking on chemistry but if he ever made a little mistake in his interpretation, then Calvin was really ruthless in chopping people down. I don't think he showed any compassion or any understanding of human nature or sensitivity. He'd just whack them in terms of faulty thinking. So, you learned to be very careful with everything you said in terms of argument or discussion.

VM: But he also came out with some pretty way-out ideas which you could criticise to him.

OH: Oh sure. He liked that pretty well. (*Indecipherable in terms of meaning*) One of the good things about him was that he was very receptive to all kinds of ideas. This was exploring things. I remember the one time that Ning and maybe you were with a group, and Duncan Shaw was certainly in the group, we were sketching on the board the soot cycle — were you there?

VM: Yes, yes.

OH: ...which was a spoof on the old carbon cycle. I was very embarrassed and thought he would give us hell. I think he read it...

VM: We were about to erase it and he said we shouldn't., there might be something in it.

OH: I thought many times in the past that that's indicative of the man. He could, his imagination was very good and he did not immediately dismiss anything like that.

VM: Would I be correct in summarising your view that the success and the character of the lab. depended clearly on the leader...

OH: Of course.

VM: ...on the character and the intellect and the dedication of the leader, but also on the support?

OH: Well, sure. I think a group like that...he was always in the lab. and he'd come around at seven o'clock in the morning, he worked long hours. Another facet of his life, he always knew. I think, what everyone was doing. He would come around and talk to you and ask for the minutest detail. I remember on the experiments on the culturing stuff, if he ever saw a rubber tube rubbing up against the moving mechanical parts, you knew you would catch hell. I think he really got the best out of everyone through kind of fear of being chewed out. But he kept track of what you were doing. This is certainly one of the reasons why he was so successful in getting good work out of people. He showed an interest in you and he was there to talk to you and you often learned something from talking from him.

VM: Do you think that another important factor was the nature of the problem itself, it was such a nice clearly defined exciting problem? You knew where you were going and what you were trying to do.

OH: That's particularly true when he first started, for The Path. When he started there were all kinds of conflicting ideas. In fact, it led to people like Fager from Chicago quitting science, or quitting that kind of science, and becoming an oceanographer because they got on the wrong path. Once the PGA got realised that that was the first stable product, then the problems became much more diffuse. This is a very good point, Vivian. Having been an oceanographer for about 35 years, oceanography is so

different. Nine out of ten times I hear an oceanographic lecture I keep thinking, what is the nature of the problem, what is this person trying to get an answer to? Most people don't have a good question or a good problem. In this case, there was a very sharply described problem, namely, the first stable product of photosynthesis and what happened to it, the path after that.

VM: That provided an opportunity to develop the momentum for a group which could then run and become more diffuse without falling apart.

OH: One thing I wanted to say before, about the structure of the group and why it was so successful. Not only did you have a very strong man at the top who was thoroughly familiar with everybody and with the details of the day-to-day work, but you also had a small group of permanent people, like Al Bassham, Dick Lemmon and Paul Hayes, who maintained the lab. in terms of everyday functioning; they kept all the machinery running so Calvin was separated (*from that*) and he was the intellectual head. All the other details of running a lab. were essentially taken care of by this second stratum. Again, to go back to one of the other questions of why I left the lab. , I don't think the second stratum, it's hard to be your own imaginative scientist and being in the second stratum.

VM: I think it was at the time when you left. I think later on it became much easier because people had developed their own careers and their own reputations and because Calvin was much less in control later on because he was spread more and more thinly.

OH: But then the nature of...the work in the group, was much, much broader. Rod Park came, after I left (we overlapped for a couple of weeks or so), and he expanded the work into the biophysical structure of chloroplasts.

VM: Rod actually came before you left, did he? Was he nominally your replacement?

OH: Yes.

VM: I don't know where he is now, I must see if I can look him up.

OH: He's a big shot on the Berkeley Campus.

VM: No, I think he's left Berkeley.

OH: He's left?

VM: Yes, I think he's gone somewhere else.

OH: He might have retired, but he's a big shot sailor. He likes sailing big vessels.

VM: Marilyn will know where he is.

OH: I bumped into him here about ten years ago and wasn't he dean or something?

VM: He was. I had lunch with him in Berkeley two or three years ago, four years ago, but I think he has moved on. I'll find him.

OH: By the time you left, was there any talk yet of a new building?

OH: Oh sure.

VM: What did you feel about the role of ORL itself in shaping the group? Do you think the structure of the building was a factor?

OH: (*Laughs*). It was an old building. Wasn't it Al Bassham who had a little sign in his office about a young person who was doing Nobel Prize winning work in an old wooden laboratory, and they become famous, gets lots of money, building big new labs., and then they never do much after that.

VM: The last bit was that the third stage was that he showed visitors around his new lab. Al Bassham did have such a thing in his office.

OH: ORL was very good, I think, for producing work. You were in close proximity to people and it worked well. Everyone in the group, that included chemists, physicists, biologists, biochemists, were together essentially in one little old building. That's one of the objectives of Calvin's whole philosophy was to have people working closely together, increased communications.

VM: It's an interesting thing, I feel, of this question of the space in which you actually do your work and whether the space as it were comes ready-made or whether you have to build the space yourself, as they did in ORL. It was a very personalised building. They had arranged things very much to suit themselves in that building. It was very flexible: you could knock things down.

OH: Sure, this is one of the advantages of an old building — and old houses: you can move walls around. You can't do that in these modern buildings.

VM: When they built the new building...you've been in the round building, have you?

OH: I've been in it, never worked in it.

VM: What did you think of it when you visited it? Obviously, this was an attempt in a modern building to recreate the spirit of ORL. Did it leave any impression on you?

OH: I was there just for the two-day ceremonies up there four or five years ago. It was so big that I got no feel for it, really, about how well it might function in the same way that we visualised ORL. It would be very hard to maintain some of the attributes of ORL, which was much smaller.

VM: That's something that we will have to think about and we ask everybody what their impressions were of the new building. They vary, as you would expect. Do you think that given the structure of the earlier group that it could have lasted indefinitely, or do

you think that sort of structure, with a great man and the growing group, has a limited life of necessity and sooner or later it must break up into something else?

OH: Like everything else in life, I think there's a productive period and then a demise and ultimately a disintegration. I can't think of any group that has really gone on for a long time. You get institutes, of course, where the directors die and new ones come in. But for a group, I visualise that there's a birth, a young developing period, a highly productive period, and then change.

VM: Do you think that the Calvin group went on for a good long time in its productive phase compared with others?

OH: I think it would probably be about the best I know about. After all, it lasted from the mid-forties to...I guess it's still going on.

VM: Well, it's no longer his lab.

OH: It lasted until the time Calvin retired.

VM: That's a period of forty years near enough.

OH: Oh sure. That reflects that one dynamic man who was recognised as the authority.

VM: It did change very much in character by the end of that forty-year period. It was no longer funded from a single source. People were already having to get their own grants, etc.

OH: I guess if you want to get another lab. , I guess something like the Salk Institute here (*in La Jolla*) might be comparable. It's one man, Jonas Salk, who was famous and built up a huge institute.

VM: But he populated it with stars, didn't he?

OH: Yes. He brought in stars.

VM: Calvin didn't do that. Whether he wanted to, or didn't want to, he didn't. He did it with much younger people.

OH: There's a big difference. Salk really became involved with art and philosophy and integrating religion and everything else into one functional system.

VM: Of course, Salk did that..

OH: Salk thought he was the spokesman for God.

VM: As far as I remember, Salk developed that lab. after he had his Nobel Prize. And, of course, Calvin did it long before he got the Prize. Can we finish by....

OH: Having been at Scripps Institution of Oceanography for 33 years and living and experiencing some of the politics of UCSD (*University of California at San Diego*) and SIO, I look back upon the time up in Calvin's group and I marvel at the good relations between however many we had, 30, 40, 50 people in the group which included a lot of social functions, camping trips, cross-country hiking trips and all, and in all that time I don't remember any bad personality problems anywhere in the lab. which interfered with any aspect of the work. Which is very rare. Everywhere I go now I see the personality conflicts and political gains and things having very adverse effects on the organisation of the institution, on productivity, how you spend your day, how happy people are. But thinking back on my three years in Berkeley I don't recall ever feeling frustrated or being annoyed or feeling that my work was suffering because of any personality problems with anyone in he lab. This is a most remarkable thing.

VM: If I may say so, I think it reflects the lack of hierarchy. I think there was the "boss" and there was everybody else. And as far as everybody else was concerned, there were the visitors who were not there for very long and there was a small group of people who were the permanent ones but there was no competition between them and Calvin. They were never in line for his job, as it were, and, therefore, they were not jockeying for promotion or position. I think that sort of absence of competition for status inside the group must have been important.

OH: One little chapter, which we have not mentioned on is: we also had Otto Kandler there during that time and we had the German couple, man and wife...

VM: Metzners (*Helmut and Barbara*).

OH: Metzners. Remember there was a lot of controversy for a short while on the possible C_1 product. In fact I think Calvin published a paper on that which he had to withdraw. Or he should have: I don't know whether he ever did it but it turned out to be a chromatographic artefact. I imagine you will be talking with some of these people.

VM: We would like to talk to Kandler and to Metzner, if we can get to them, and we are going to talk to Martin Gibbs (where Kandler had been) before we go back to England.

OH: Another interesting chapter was that during that period we had some pretty heated discussions with Calvin in his office about these results. Even though it contradicted what he felt was right, he again showed a very good open minded, scientific approach where he listened to it and did not tell us to just forget about it. It was a good open scientific discussion.

VM: I suppose as a result of the Kandler involvement, there was one meeting where all of us did gather in his office to thrash it out once and for all.

OH: I remember him coming up to me after a meeting and putting his hand on my shoulder and saying something about "it was a good meeting and I'm glad we got together". It continued on for a bit but again this was typical of Calvin. He's a true

scientist. He wanted to get the answer and I don't think he cared whether he had been right or wrong. This is an important point. If you look at all of his publications, he made a lot of errors in interpretation. But they are not stupid errors. They were errors which were based on the best thinking of the time on the data available. And there's no harm in that. In fact, it's a good way of science progressing. He was not afraid of making an error, which a lot of people, these days, are afraid of making errors.

VM: I think the worst you can say is that he sometimes went into print a little prematurely.

OH: Oh, sure.

VM: I think he felt that there was competition from other people working in the same area and I remember at the time that there was concern that other people shouldn't get there first. But that's natural in scientific work. By and large it worked very well, I must say.

OH: During this episode with Otto (*Kandler*) and the Metzners, and even though it was against what he thought, he was very open minded and really showed that he was interested in the science and did not care about personalities. He took no personal involvement with this.

I can give you one example in oceanography where some person about 20 years ago was trying to prove that these little yellow-green cells in deep ocean water were alive. I was talking to him and he said if they proved not to be alive, he would be so depressed he would probably kill himself or something. He got emotionally involved. Which is not the true scientific... As a scientist, you should try to find a scientific answer; you don't want to get emotionally involved. In this I thought Calvin was very good. He was always trying to get *the* right answer whether or not he was right or wrong.

VM: But it was important to him, like for any scientist, that *he* wanted to get the right answer. He didn't want the next guy down the block to get the right answer. It was important that they came from him.

OH: I don't know how much interaction he had with (*Daniel*) Arnon.

VM: There were people whose publications concerned him and, if you read the literature and read some of the reports that people have written, there was obviously concern about being beaten by others the need to get there first. But that's commonplace.

When you left Berkeley, you went to Wisconsin as an assistant professor?

OH: Right.

VM: How long did you stay there?

OH: Four and a half years, five years.

VM: So that takes you to about ’63?

OH: Yes.

VM: Then you came here to Scripps?

OH: Then I came out to here.

VM: And you have been here, as you say, 33 years.

OH: Yes, more or less.

VM: What relations have you maintained with the Calvin group, with the people in the Calvin group?

OH: Not too much. We corresponded and sent Calvin Christmas cards for many, many years. Some of the people we corresponded with and kept in contact with the Moses and a few people.

VM: And, of course, Andy’s in the building.

OH: I keep close contact with Andy and I see people like (*Paul*) Saltman who had some involvement with the group at one time. No — Paul never did. He was one of the people I used to read about, working on succulent plant metabolism. We have Murray Goodman and I see him once in a while down here.

VM: We’re going to talk to Murray at the end of this week. OK; thank you very much. It’s very much what we hoped you would tell us.

OH: Do you want me to add something about what I have done since then?

VM: Yes, please do.

OH: Off the tape?

VM: Record it; I think it’s interesting for people to know what happened to those who were in that lab.

OH: OK. I’d like to say one person I met at Berkeley at the swimming pool was Dr. Ellsworth Dougherty who was head of the, absolutely a brilliant guy, he was head of the laboratory for gnotobiology up in Strawberry Canyon; he was an MD/PhD, absolute genius. When I went back to Wisconsin he had a grant to go down to the Antarctic to investigate terrestrial and freshwater life in the Antarctic and he invited me to participate. I went down there, that was in ’59. On the way I stopped in Berkeley and saw Calvin and told him I was going down to the Antarctic. He really expressed great envy, I think.

VM: Envy?

OH: Envy. He realised that, he said something to the effect that he wished he could down and do some biological investigations on the forms of life that were endemic, that survived in these harsh environments. We were not looking at the ocean, we were looking at the terrestrial and freshwater streams. He showed a real interest, particularly with regard to possible photosynthetic pigments effect at low temperature and of light, and everything. I went to the Antarctic (this '59/'60) and I got very much involved. I had never been interested in ecology *per se*. Back in Wisconsin I was in the physiological-biochemical area under Skoog and the other major component of the botany department there were the plant ecologists under people like Clark who has a very famous ecology section. But there was a big barrier between the two and so there was hardly any conversation. We never took courses in any other discipline.

But then when I went to the Antarctic, I became very much interested in the effects of low temperature and phase changes for the transition between liquid water and the crystalline state. In the Antarctic all the terrestrial life: mosses, liverworts, a few mites and things, but the plant life is basically algae and liverworts and mosses. It's usually in crevices which are very protected from the harsh environment. Sometimes you have to look down about five inches in the rocky stubble before you find a layer of algae which have enough protection to survive.

VM: Excuse me: this is life on a rocky substratum?

OH: Very often you will find.. round the (*indecipherable*) there's a lot of rubble and small stones. If you just walk, you won't see anything. If you get down on your hands and knees and start pulling away and uncovering a few layers of rock, you find layers of green algae and mosses.

VM: Is there anything on the ice itself, on the ice sheets?

OH: There are snow algae, sometimes. Most of the life is microscopic and you have to get down on your hands and knees and look for it. It was pretty obvious that if you are looking in a little niche of a dark volcanic rock (I have some over there on the window sill), you are exposed to repeated cycles of freeze-thaw in terms of minutes, many times during the day. I became very much interested in the survival mechanisms of water relations and went back to Wisconsin and changed my major interest. I spent a couple of years just studying effects of freezing and thawing in repeated cycles and of freeze drying and removing...and the degree to which you removed the bound water of cells and the effect of viability. I got interested in exobiology at the time. In fact, I have hundreds of samples of freeze-dried algae from the Antarctic in these little tubes (should have one here somewhere); yeah, these little tubes which are sealed in high vacuum and which are probably good for decades or centuries, maybe. If anything is going to survive geologic time, it's that. At the time in Wisconsin I was interested in space travel and if you are ever going to send a man about 100 light years away, you will probably have to free dry him. My research had changed a lot. During my trips I gave some lectures here at Scripps and I was invited to stay and I have been here ever since. So I became an oceanographer in contrast to my formal training at Wisconsin. You might even call me an ecologist

now. But I'm afraid I don't think too much like an ecologist at the present time. I'm still interested basically in the response of organisms to environmental stress and how they adapt. The last three or four years I have been spending half my time on the study of the effects of ultraviolet radiation, particularly under the ozone hole in the Antarctic.

VM: Last question: What did those three years in Calvin's lab. do for you?

OH: Probably gave me a very good springboard to a good academic life. Far more important is the intellectual development and seeing good science at work and the functioning of a great mind and having it kind of infused in you. I think that carries through everything I do now to some extent.

VM: Had you met an exciting situation like that before?

OH: I had met famous people. In Norway I had worked with Brorud who was one of the most famous biological oceanographers (phytoplankton). That's a different kind of science, very descriptive, some physiology of the coccolith formation, but it doesn't have any dynamic approach where you really intertwine chelate chemistry and physics and biology. This (*the Calvin lab*.) was a qualitatively new chapter in my education, I think, which made a great impression on me...

VM: I think as it did to everybody else.

OH: ...and it affected my thinking of science and it affected the approach I had to thinking about problems and how you attack them.

VM: I think we'll close it there. Thank you very much indeed.