

Chapter 14

MURRAY GOODMAN (with Zelda Goodman)

La Jolla, California

June 7th, 1996

**VM = Vivian Moses; MG = Murray Goodman; ZG = Zelda Goodman;
SM = Sheila Moses**

VM: This is a conversation with Murray and Zelda Goodman in La Jolla June 7th, 1996.

About your time in Berkeley in the “good old days”, how did you come to choose to go there?

MG: I was an undergraduate at Brooklyn College in New York and I had taken an organic chemistry class with a Professor Lewis Sattler and had done very well. As a result of that, he asked if I were interested in going to graduate school; my answer was “indeed, yes, I wanted to go on to graduate school!” He said that he had just read some interesting papers on the use of radioactivity as tracers in biochemical research and he would like me to look at them. I did, and these were early papers of Melvin Calvin and dealt with tracers in photosynthesis, primarily the C¹⁴, and I found this to be very exciting. I indicated that I wanted very much to do research of that sort and among other places I applied to the University of California at Berkeley. Dr. Sattler wrote to Professor Calvin and, lo and behold, I was accepted and that, together with other acceptances, was analysed by me, Professor Sattler and my family and I decided I wanted to go to Berkeley and study with Melvin Calvin.

VM: Which year was this?

MG: 1949.

VM: So, you went to Berkeley. You’d never met him before you went there?

MG: I met him on arrival.

VM: What was it like, meeting Melvin for the first time?

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MG: He was a very active, dynamic man, a chain smoker; he weighed close to 200 pounds, agitated and dynamic, excited and fascinating, all rolled into one human being.

VM: Can you remember where you actually first came face-to-face with him — in which room, what did he say to you, what did you say to him, or is that too far back?

MG: I know where we met. We met in the Old Radiation Laboratory, this rather ramshackle World War I temporary building which was still going quite full tilt in 1949. I came in on a Monday morning and he'd been there for some time; I introduced myself, he was delighted to see me and told me all the things that he expected and wanted for me to be a successful graduate student. It was sort of an intense and exciting entree into the laboratory.

VM: You had to do courses, presumably, at the beginning.

MG: Oh, yes.

VM: So only part of your time was free for lab work.

MG: What had happened is: I was awarded an Atomic Energy Commission fellowship that was funded through the university. These were early days when there weren't all that many fellowships available. This obligated me to do the proper course of study in the Department (*of Chemistry*) and to carry out research. Different from my fellow graduate students, I did not have a teaching obligation which gave me a good deal of freedom at that beginning to sit in on many of the research discussions in the Calvin group and to get to know many of the people in the group since the only diversion I had, and it was a substantial diversion, was to take courses and to do well in the courses.

VM: What sort of contact did you have with the rest of the Chemistry Department, at that time at any rate?

MG: I was one of the first year graduate students and, therefore, I had to have an advisor who would make certain that the courses that I took were appropriate, that I would jump the hurdles which were required of all graduate students.

VM: Who was your advisor?

MG: My initial advisor was Bruno Zimm who then was an assistant professor. During that first year he had decided to leave and to go on from Berkeley to General Electric (that's an interesting story, in and of itself because Bruno Zimm and I are now colleagues here at the University of California in San Diego). But at that point he launched me on my academic career and made certain that I would take the right courses.

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VM: As a graduate student in the Calvin set-up, were you, as it were, at one with the other graduate students in chemistry or were you physically separated and didn't see much of them?

MG: We were separated. My laboratories were in the Old Radiation Laboratory. Calvin did not have many graduate students. Most of the other graduate students were distributed among the organic and the physical and the inorganic laboratories in the department in other buildings. However, I'm not one now, nor was I as a graduate student, hesitant as to go meet people and talk with other graduate students and to deal with the challenges that we had. So that there was a lot of interaction between me and them, although it was primarily in their laboratories and not at the Old Radiation Laboratory because you still had to have (*a security*) clearance in those days to get into that lab.

VM: Did they have departmental seminars at which you and other graduate students were expected to be present?

MG: There were weekly seminars and a colloquium and, of course, within the Calvin group there was a weekly research meeting.

VM: What did you start working on?

MG: Calvin was very much interested in isolating 2-phosphoglyceric acid...

VM: 2-Phospho...

MG: ...and he had the feeling that it was possible to synthesise it and to separate it from 3-phosphoglyceric acid. My first project was indeed to try to synthesise 2-phosphoglyceric and to develop some way of chromatographically identifying it and differentiating it from the 3-phosphoglyceric. The belief then was that the first intermediate that occurred in photosynthesis was 2-phosphoglyceric which quickly rearranged to 3-phosphoglyceric acid.

VM: At that time, was paper chromatography in use in Calvin's lab?

MG: Yes. The large tanks for paper chromatography and rooms which contained these two-dimensional approaches to paper chromatography and radioautography, all the things.

VM: You quickly became involved, no doubt.

MG: I quickly became involved in paper chromatography and column chromatography and the attempt to scale-up from the paper chromatography led me to try much more chemically-oriented isolations of these sugar phosphates.

VM: Had you had experience of biological matters before your involvement with photosynthesis?

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MG: I'd had very little laboratory experience. I had done some work as an undergraduate with Professor Sattler on sugar epimerisations and with Professor Irving Kay on quinolines, but these were simply isolations, typical for an undergraduate. At that time, I did very little in the way of complicated organic synthesis.

VM: In the Calvin group at that time, were there any biologists, or was everyone a chemist?

MG: No, there were many biologists in the group and a large number of chemists. But I would say it was a very catholic group, stretching all the way from botany-types to chemical physicists.

VM: I can't identify in my mind who the botany types were at that time. Can you drag anything up?

MG: The names that I recall include Vicki Haas (*Editor: later Vicki Lynch*), Bill Stepka, Clint Fuller...

VM: Clint was there when you arrived or did he come later?

MG: He came later. But these were people who were there during my stay in the Calvin group.

VM: You didn't overlap with Sam Aronoff, did you?

MG: I did not. And, of course, Andy Benson who really was a chemist and a plant biochemist and a major force in the actual functioning of the Old radiation Laboratory.

VM: Where in the building did you work? Where was your bench? Who was next to you? Who did you see through the rack? Can you remember all of those things?

MG: If you were to put the Calvin laboratories in the following axis: On one side the lab. pointed toward Gilman Hall and Le Conte Hall, the major chemistry and physics building. On the other side it pointed toward the Old Chemistry Building and Lewis Hall. My desk was the furthest in the Calvin group toward the Old Chemistry Building and Lewis Hall so I was against the wall.

VM: Who was next to you?

MG: Vicki Haas was just opposite me and back-to-back with her was Bill Stepka.

VM: This was in the big lab., the one with the big white table in it?

MG: The one with the big white table in it. When Professor Calvin would come into the laboratory from his office, which was in the Old Chemistry Building, the first bench he would pass would be mine. That was both a blessing and a curse!

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VM: So you saw a lot of him in those days.

MG: Well, when he would come in, very often it would be to talk about recent chromatographic discoveries and everybody would gather round the big white table. The big white table became sort of the focus of everybody's attention when Melvin came into the laboratory.

VM: When Melvin was there, did he tend, naturally, to gravitate to the big white table and cause a group to develop there or did he go and talk individually with people at their benches?

MG: Both. But somehow, very often, we wound up at the big white table.

VM: So, there was a lot of back and forth chat, presumably, between you in a relatively small room. You could hear one another, I suppose?

MG: That was probably one of the most exciting of the learning experiences because you couldn't miss if you wanted to hear what was the latest, what was the conjecture, what was the abandoned hypothesis, it was all sort of put out there in a very open way for everybody to consider. Of course, behind us as we talked were the "gloop-gloop" machines which were harvesting *Scenedesmus* and *Chlorella*. I would go to bed at night very often hearing "gloop-gloop", "gloop-gloop" which was simply the motion of the shakers to keep the algae suspended and, therefore, growing well. But the noise became a rather onerous burden to take home every night.

VM: You were right close to them.

MG: Yes, very close.

VM: Who was tending them at that time?

MG: This is before Paul Hayes.

VM: I didn't know that Paul Hayes had tended them but...

MG: He was sort of a factotum in the laboratory. Martha? And...I am blocked. I can see the faces but the names escape me.

VM: I'll throw one at you; it's someone I didn't know myself. Someone called Anthea (*Editor: actually Altha*) Vann.

MG: Yes. She was also the dishwasher and a person who kept much of the equipment and glassware in good shape.

VM: What sort of difficulty did you have — or perhaps you didn't have any — in distinguishing your thesis work from the general work? Your thesis was to have been your work, presumably, and yet in such a melee of a lab. was it easy to separate out what was yours from what was other people's?

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MG: Clearly, what Melvin wanted me to do was to try to isolate chemicals, molecules that were important for photosynthesis, and to do this in a way that was basically organic and analytical chemistry. That was very different from the focus of many of the other people who were far more involved in the biochemical processes. So, there was a relatively easy way to identify my focus and I stayed with that while doing other things in the way of learning and carrying out photosynthetic experiments.

VM: So, you started off isolating 2-PGA...?

MG: Which I found I never could make because what I isolated all the time was the cyclic phosphate. When that cyclic phosphate opened, it opened primarily, if not exclusively, to the 3-phosphoglyceric acid. That sort of thing went on for a while until it was clear that 2-phosphoglyceric acid was not the key intermediate in the earliest stages of photosynthesis. Then my emphasis changed to go toward knowing what array of organic phosphate molecules exist in photosynthesis and when do they appear and how do they change with the growth cycle of the algae.

VM: Can I pursue some of the points in relation to that. Were you there when PGA was recognised as being the first fixation product, or was that before your time?

MG: It was before my time. When I came, it was already known that 3-phosphoglyceric acid was a very early product in photosynthesis.

VM: After your failure to synthesise 2-PGA, was there any more suggestion that anything other than 3-PGA was the first compound?

MG: This sort of happened over time. More complex pathways were being considered; what ultimately did work out to explain the carbon cycle, but at the beginning many, many different molecules were looked at for carboxylation and cleavage to give 3-phosphoglyceric acid and other products.

VM: But you weren't part of that?

MG: No, no. I was not part of that. I have the distinction, together with Dan Bradley, in that a major part of our work was to begin — this is later in my career — the path of phosphorus in photosynthesis. We actually published a paper with Professor Calvin, of course, in the Journal of the American Chemical Society entitled *The Path of Phosphorus in Photosynthesis. I*. There has never been a II.

VM: They allowed you to publish a number 1 without there being a number 2?

MG: I think that Professor Calvin certainly had the platform to carry that forward and we did that. I think in the paper actually showed why there could not be a simple path of phosphorus that could be traced because there were too many reservoirs of inorganic phosphate and the dynamics of phosphorylation and dephosphorylation to prevent that kind of analysis — very different from the path of carbon.

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VM: When you explored the various phosphorus compounds that you said you went on to after the 2-PGA story, you got them from the paper chromatograms?

MG: Or from ion exchange chromatography since I was primarily asked to obtain larger amounts of these materials. The great mystification of my PhD thesis was the existence in *Scenedesmus* of a huge storage of polymetaphosphate. We really didn't know why it was there. It was by far the largest reservoir of phosphate, and certainly high-energy phosphate, and it just overwhelmed everything else that was there. Melvin speculated that this could be the kind of origin of high-energy phosphates and their ability to phosphorylate and it was kept ready to go, with proper co-enzymes and enzymes, as a mechanism for rapid phosphorylation of all kind of molecules.

VM: Were you using phosphorus-labelled materials for isolation or did you use C¹⁴?

MG: Yes; these were all ³²P labelled materials so that I could follow them quite carefully and completely.

VM: You did P³² photosynthesis and then isolating the compounds in the usual way and chasing them up.

MG: Isolating large amounts of the material. So that paper chromatography didn't work but ion exchange chromatography did.

VM: Your job was to identify these materials?

MG: As best I could.

VM: Which ones did you succeed with?

MG: There were all kinds of hexose phosphates and even more complicated phosphates that we could identify, triose phosphates and even, I believe, 7-carbon sugar phosphates and things of this sort were easily identified. Some of the diphosphates were also apparent and the sort of build-up of these molecules made sense as one looked at what was developing as the path of carbon in photosynthesis. Finally, this enormous reservoir, which was probably 80-90% of the labelled phosphate in *Scenedesmus*, turned out to be this polymetaphosphate.

VM: When you arrived in that lab., who else was there?

MG: I can complete the big lab. I was at the end, and Vicki Haas, then Bill Stepka, then Al Bassham. In the small lab. was Andy Benson, Dick Lemmon — there were some other people there and it sort of all turns into one.

VM: Was the upstairs of ORL in use while you were there?

MG: Upstairs was only used for paper chromatography. Downstairs in a very carefully shielded room were the Geiger counters. There's an amusing story about that.

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VM: Please.

MG: When I arrived at the Radiation Laboratory, in those days everyone had to go through a physical examination. In the course of that physical examination the physician who examined me noted that there was a small growth on my thyroid. He said it was likely to be a benign adenoma but that he would urge that I see the specialist at the Medical School in San Francisco and have it out at my earliest convenience. That turned out to be over the Christmas holiday in 1949. I went to the university hospital and was operated on by Dr. Nash, who was a world-renowned thyroid surgeon, and he took out that adenoma and indeed it was benign. However, in the course of determining that I was asked to consume a small cocktail of iodine-131 to see the take-up of the iodine-131 of the adenoma and the thyroid and the whole general aspect of that. It was from that test that the surgeon believed that it was benign and that it would be an easy thing to take out.

Now I had that operation and returned to the laboratory and, of course, mingled with everybody and, of course, also had my opportunity to do some work with radioactivity in the counting room and every time I would appear in the counting room the Geiger counters would absolutely go berserk. Nobody knew why. I had not connected the two, that my gamma-emitter was really sending the background up 5-10-fold at least, if not more. This kept going on and one day it was Andy Benson sitting and counting and I walked down in there and suddenly the counts went wild. He looked at me and he said: "Why don't you leave the room for a second". I did, and then he motioned me to come back in. I came back in and then they put the Geiger counter to my throat. Of course, that was the origin of the problem and I was told very firmly to take a vacation from anything near counting in that laboratory and handle my research responsibilities distant from them as much as possible. My colleagues in the graduate school came to believe I did this purposely to get out of all the things that were involved in hours and hours of counting. After a while, even the iodine-131 disappeared and I was accepted back into the fold.

VM: I think you mentioned earlier on that you were there when Calvin had his heart attack.

MG: I believe it was in November 1949.

VM: Soon after you got there?

MG: Yes. I arrived on the 7th of September, so this is perhaps 2-1/2 months later and Melvin had come into the lab. and was complaining of indigestion and talking with many of us, we were around the white table. He left us to have a scientific meeting with Dr. John Gofman in Donner Lab. The next thing I heard is that he was in the hospital and he'd had a coronary. The story was that he'd really had the coronary while meeting with John Gofman who was a medically trained researcher and understood the symptoms and immediately recognised what was happening and had Melvin taken to the hospital.

VM: What effect did that have on the work in the lab.?

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MG: For the next six months Melvin was basically at home. He would call into the laboratory and spoke primarily with Andy in the Old Radiation Lab. (by that time, the other person who was there with Andy in that small lab. was Nate Tolbert) and with Bert Tolbert in Donner, and with Ed Bennett who was also in Donner, and with other folk. He kept in contact with many of these people and I would write brief reports on my progress in terms of the synthesis and some of the isolation of the organic phosphates, give them to Andy and that would be sent off to Professor Calvin who then would send back notes of things he thought I should be doing. It was six whole months before I saw him again after that November morning.

VM: Was he very changed?

MG: I saw him in November. He was still smoking and he was close to 200 pounds. When I saw him in spring, approximately six months later, he was about 145 pounds and, of course, was never to smoke again. He looked like half of Melvin Calvin that I had seen before and much better looking and healthier looking person than the person who was so heavy the previous autumn.

VM: When he came back into the lab, was it the same man, doing the same sort of things, acting in the same sort of way?

MG: Pretty much. You could see the same sort of love and excitement and intensity of reaction. He was nowhere near as agitated; he was much more methodical and he had a regimen enforced by his wife, which he followed. He had an eating regimen and a time and rest regimen which he also followed.

VM: In the lab. generally, in the group generally, was there much sense of competition between individuals?

MG: It was a very large group and, as I said before, there were very few graduate students. Among the graduate students, there was really very little competition. We were all sort of overwhelmed by the large number of highly talented and successful postdoctoral researchers, visiting professors and research giants who were in the laboratory. I remember, in one case, Hans Schmid from the University of Zürich who succeeded the great organic chemist Paul Karrer as professor at the University of Zürich, was in the laboratory and was full of incredibly useful knowledge and important ideas about natural products and chemical reactions. Just being with him was enormously rewarding to me as a graduate student oriented toward organic chemistry.

VM: All of these people were interacting freely with everybody else; there was no sense of hierarchy inside the lab. was there?

MG: I don't think so. Again, thinking back to those days, the overall impact is one of enormous and free exchange. I'm talking primarily about the Old Radiation Lab. group. The other group in Donner Hall were doing different things and were much more involved in things like nuclear aspects of medicinal chemistry and some other

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work in biochemistry that was quite different from what was going on in the Old Radiation Laboratory. ORL was completely focused on photosynthesis.

VM: How much contact did you have with the people in Donner?

MG: Every week we had a research group meeting and I would hear what they were doing and I would listen to some of the work, primarily that of postdocs. and staff scientists. I don't recall any graduate students over there. There was another group working on R_h factors in the Old Chemistry Building, that was a small group...

VM: This was part of Calvin's overall empire?

MG: ...part of Calvin's overall group. Elmer Schallenberg was one of the people working there (*Editor: actually, Schallenberg was working in Old Chemistry*); he was a graduate student at the same time I was there.

VM: Tell me also about the group seminar, the famous Friday morning seminars?

SM: You mentioned Elmer Schallenberg, was part of a third group? I'm sorry; I didn't catch what the third group was.

MG: He was doing work, I believe, on the porphyrin world in terms of structure and biochemical and organic chemical aspects. It's a long time ago and I don't remember specifically what he was doing. It was something to do with haem and haem proteins.

SM: This was not with the photosynthesis group in ORL.

MG: This was not with the photosynthesis group in ORL...

SM: And not with Donner?

MG: ...and not with Donner.

SM: Where was he?

MG: It was in the Old Chemistry Building in a lab. in the basement, I believe.

(Editor: The work on the R_h factor in the Old Chemistry Building was done in collaboration with a bacteriologist, Merwin Moskowitz, and a medical doctor, Dr. Robert Evans. They were attempting to isolate the factor from blood that causes the R_h incompatibility. This project was initiated by Professor Calvin after the death of his first son from R_h incompatibility. He received a grant from the Rockefeller Foundation. However, the work was inconclusive. One of the people who worked in the lab. on this project was Genevieve Calvin, Melvin's wife.)

VM: To get back, then, to the Friday seminars: had you ever met phenomena like this before?

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MG: It was totally new to me. It was a meeting of everybody, beginning no later than eight in the morning and no one knew who was to be called on. It just turned out that we all came, either prepared, or prepared to explain why we were not prepared.

VM: So some of you actually came in there carrying stuff, carrying data or materials?

MG: Yes, that was not unusual. If one presented results which were quite positive and new, then things went famously. If one didn't accomplish what was expected, clearly there were frowns on Melvin's face and very simple admonitions to get going, "to get off the dime", to make progress, to do the right thing.

VM: Presumably, there was some sort of approximate rotation in which people would come round every so often and give a talk. It would be their turn again periodically.

MG: In a global sense, you are right. You would say, "Well, I spoke last week, it's not likely I will be called on again this week", although there were some exceptions because Melvin turned out to be very interested in a particular area and wanted to hear more and more about it. Most often, if you had spoken and it went all right, you could think of about a two-month hiatus before you had to begin preparing again.

VM: Presumably, if Melvin heard something interesting, he would be close on the heels of that person for the succeeding days and weeks.

MG: That's right. That's exactly how it worked. If it was interesting, that afternoon it was likely that he would be at your desk and your lab. bench, saying "let me see what you have done; let me understand, let me see if I have it right". If you didn't explain it correctly, I remember the famous strong rejoinder was "I don't understand". That meant you had better stop, slow down, pick up the pieces and come back with an explanation that he could follow and pick up very quickly. He was so fast and so rapid in understanding so many things that if it went in a direction it didn't mean that you were being too complicated, it means that you were not being clear.

VM: He was clearly the outstanding person in the group, but he didn't do everything.

MG: That's right.

VM: How can you evaluate his contribution versus other people's — can you? Maybe it's too difficult.

MG: After so many years of being a research director, I realise that he played many, many roles. He was a teacher, he was a resource person, he was a knowledgeable fountain on a wide variety of aspects of the science that we were interested in from physical chemistry all the way to botany. He was knowledgeable about what was in the literature, he was up-to-date about recent discoveries, he had ideas about new directions. All of these things made him an enormously successful research director. Now most things that were analysed by him and undertaken as a result of his suggestion did not work out. But the few that did were the hallmark of the laboratory and the great success of the laboratory.

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To do that, he had to have people of great ability with him, in both an anticipatory sense and in a follow-up sense. That if the ideas were somehow out there, how do you put them into practice? That was one of the exciting aspects of being in that lab. Because if something were suggested, which came out of discussion around the white table, let's say between Nate Tolbert, Bill Stepka, Al Bassham, Andy Benson and Melvin Calvin, there was an immediate plan of how to do a series of experiments, how to do this rapidly. That was a very clear characteristic of the lab. at that time. You had to have those people!

VM: So I suppose, that although he may have been the single most prolific source of new ideas and inspiration, he didn't think up everything that went on. Other people made enormously valuable contributions to the development of the research.

MG: Absolutely and prime among them was Andy Benson, who was very much a broadly based scientist in this area of photosynthesis and technically highly accomplished. He was in the laboratory at all times, so that he had that sense of how to make it happen in the lab. And, of course, discussions always involved him. He was, and is, a relaxed, contemplative person who is very easily accessible and in the day-to-day kind of world of being a graduate student or in functioning in ORL, I would say that Andy had most of the responsibility for reacting to what was going on, analysing what was going on, suggesting routes to new experiments. That was a major emphasis of each day in the lab.

VM: One of the things that particularly interests me, and I've really little idea of how it happened and who was involved, was the working out of the later parts of the cycle with the heptoses and the pentoses, and so on. Were you there at that time?

MG: Yes, I was. This was a very exciting time.

VM: Who were the participants, where did the ideas come from?

MG: That's hard to say. Certainly it was quickly recognised as something of substance at the discussions around the white table. People were at this particular point thinking of other routes beyond the simple 2-carbon mechanism. I think there were all sorts of analyses of what kinds of enzymatic processes could go on and, without being able to say who mentioned ribulose diphosphate or sedoheptulose diphosphate first, it was there, as I remember it, that part of an active series of discussions. Almost in a sense of discovery, they were talking about these kinds of reactions and suddenly everybody was talking about them. My recollection is that this was something that made sense in terms of suddenly beginning to think of aldolases which were then somehow enzymatically being looked at in other contexts — nothing to do with photosynthesis. The aldolases were the entree into carboxylation of these keto sugars that then led to the unraveling of the path.

VM: As far as you know, was there any direct contact with Racker and Horecker who were the other people obviously working in this area?

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MG: Not that I know of, although their names, of course, were then known in the laboratory after that; but not that I know of.

VM: There were also other external influences and discussions, debate, arguments that I know little about. The relationship with Arnon, with Gaffron, with Gibbs and people like that. Did they impinge at all on the internal life of the lab. as you, a graduate student, saw it?

MG: There were competing laboratories and there was a real sense of primacy of our group over them. There had to be a concern of the laboratory with information that would of necessity keep us ahead of the competing laboratories. It was sort of a distant sense of competition and not something that day in and day out was a topic of conversation. There was Fager and Gaffron and their work but it was somewhere near Chicago or in the midwest and that was far distant. The Arnon era and that was all after I had left.

VM: So people didn't come in and say "My God, one these other people are getting very close — we had better hurry up".

MG: Not that I recall.

(Tape turned over)

VM: Carrying on the discussion: you actually finished your work in about '52 or something like that?

MG: Yes, in September of '52.

VM: How was your thesis produced? Did you have to type it all yourself or did you get help? What happened in those days?

MG: The initial draft was typed by Zelda, my wife of one year at that time, and then, after it was proof read and corrected from the standpoint of content, it was sent up to the Rad. Lab. on the Hill. They typed it as a Rad. Lab. document and duplicated it, and that was my thesis.

VM: And a copy or several copies were bound from that print-out?

MG: Yes.

VM: Who was on your thesis committee?

MG: Jim Cason, Chet O'Konski, Melvin, of course; I don't remember the others.

VM: Melvin, presumably, would have been well aware of what was in it as it was being produced. What about the others — did they want clarification, did they want to talk about it?

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MG: Oh yes. I think that there was discussion with each of them and modification before the final draft was typed. We didn't have formal defence of theses, the University of California doesn't have that. But it was read by the people and critiqued and then submitted in partial fulfilment, etc. and the degree was awarded. I guess the official time would be June of 1953 but it was in September of 1952 that we left, toward the end of September.

VM: You've actually kept in very close touch with a number of people from that lab. ever since you left?

MG: Yes.

VM: You have been aware of the developments that have gone on and much of the progress which has been made. To what do you ascribe the success of the group, particularly in the early days, particularly in the photosynthesis era?

MG: It was a confluence of the timing, the energy and talent of Melvin Calvin, the funding through the Atomic Energy Commission, the bringing together of very highly motivated, talented researchers in a truly interdisciplinary function. It was an interdisciplinary laboratory in the days before "interdisciplinary" became popular. And the fact that the problem chosen, the path of carbon in photosynthesis, was solvable by the techniques of tracer chemistry. All those things made for a special character and productivity in the laboratory.

VM: The individual personality and abilities of at least the leading people were critical to the success...

MG: Oh, absolutely.

VM: ...and the funding, presumably, was an important factor.

MG: That's true. One could set up a kind of permanence in Berkeley which was different from the administration or the faculty and this, of course, worked very well in the Bio-Organic Group, the Biodynamics Group and also, of course, in the Radiation Lab. as a whole. That meant that people were not concerned about where will I get a job next week, that this was something they could count on in a way quite different from ordinary postdoctoral researchers.

SM: Before we say more about this, I was thinking that we had Zelda come and go, as it were, to Berkeley, and clearly this happened in 1952? 1951. I think it would be interesting to have Zelda's impressions of what she came to and how it seemed and how welcoming the group was and how she felt about it and how different it was from things you'd known before.

ZG: This was completely new because I had lived at home. I had never lived away from home before, and here I was married, away from home, but it was probably a welcoming group. I think that one of the things that was impressive that the Calvin's had lab. parties, had the group over at least once or twice a year and they were, Gen

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particularly was very mother-hennish about all the graduate students and all the people in the lab. and treated them very much as a family. I think that was a very important influence on my life. I certainly cannot judge anything that went on the scientific level. I know that I learned to wash glassware, I think I did some counting with the Geiger counter at one point and used to be in the lab. late at night frequently. But what went on scientifically, I have absolutely no way of judging or assessing in any sense. I don't know what it was all about.

MG: I do remember that there would be small dinner parties at the Calvin's and often Zelda and I were included, and that was supportive and reassuring to me as a third-year graduate student. We got married in August of 1951, at the conclusion of my second year as a graduate student. Now the problems of completing a thesis and settling in to married life really needed some help from the Calvins and I think what Zelda alluded to was quite correct. She (*Gen*) was supportive and did help. In his own way, of course, Melvin is detached from things of this sort and as long as he was sort of guided by Gen, it made our first year more pleasant than it would have been ordinarily.

VM: Zelda, what did you see of the social scene, as it were, a lab. spouse; how did you inter-act with other lab. spouses?

ZG: I think the only lab. spouse I really remember was Trudy Bradley, wife of Dan Bradley who was a graduate student with Murray. (*To Murray:*) Dan came after you came?

MG: That's right. Dan came one year after me, when I was in my third year he was in his second year.

ZG: We became good friends, and Trudy and I used to spend the evenings in the laboratory. She was also working for a PhD at the time at the University in another field, I believe psychology. She was busy with her own "thing" and it was interesting because I think that even their friends were probably more from chemistry than from her field, which is quite interesting in retrospect. There were other graduate students we were quite friendly with, but I don't know that I think of it as a spouse kind of thing. It was Berkeley. Berkeley is Berkeley. It's very different, everyone is a student, even when you are in your sixties. It's that kind of life.

MG: Let me add something to what Zelda is saying. There were very few graduate students. I think Dan and I were the only married graduate students. The others — Alex Wilson, Elmer Schallenberg, Anne Zweiffler at that time — were not married. We did socialise somewhat with graduate students and certainly with Dan and a bit with Anne. The postdocs. and the senior people in the group were a good deal older than us and, therefore, it was not usual as students to be in their milieu, although they were friendly and we did have all kinds of laboratory events, dinner parties, we would be at Andy Benson's home, and things of that sort. Most of our social activities turned out to be in the community outside the lab. (we can talk about that) within Berkeley.

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VM: Do you think that was typical of most of the lab. residents, that the lab, although it had a strong internal cohesiveness as far as professionalism is concerned, it was not a social centre? Am I right in that?

MG: I believe you are

ZG: But it was also very social.

MG: Oh yes, but it wasn't something that people-in-the-laboratory's total social experience were with people in the laboratory.

VM: In talking with people about the whole experience, many comments have been made about the nature of the building in which you worked and the effect this had on the way you interacted with one another. How do you remember it?

MG: Well, the Old Radiation Laboratory was a wooden building, one storey — actually there were two storeys...

VM: Actually there were three storeys.

MG: Yes, yes, yes! But from our standpoint it was that one-story that we lived on and worked in. The building was sort of ramshackle; it was certainly no architectural wonder. We had the chemical laboratories on one side of the building and on the other side was the big machine shop. There was also, I believe, a glassblowing shop there also. There were environmental health and safety (or its precursor) were also in that building. In the basement were counting rooms and on the top floor we had the chromatography rooms. It wasn't a place that had compartments. Within the lab. it was all essentially open. There was nowhere you could hide from anything. Andy's office was glass enclosed, the secretaries had an office (glass enclosed), so you could see everything and everyone. If I wanted to see or talk to Andy, it was a very simple thing to do. There were no barriers.

VM: Were the doors left open, incidentally? Did he have a door and was it left open?

MG: He had a door, I think it was always left open. This created an atmosphere of openness in the laboratory. It makes it much easier to do things together. It also makes it all-knowable if you goof. So it's a two-way thing and I think the net effect is very positive.

VM: Did you tend to stay together during the day, did you tend to eat lunch together?

MG: I tended to each lunch with the graduate students and seek them out because we had exams to take, we had courses to take, so I most often would lunch with them. After Zelda and I got married we would have lunch together in Faculty Glade almost every day with many of our friends.

VM: Did people work late at night in the building?

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MG: Many did, yes, certainly I did.

VM: Zelda, I wonder if you have any comments about Murray's working late at night.

ZG: Yes, I do. I think for a while I would go to bed, wake up in the morning, go to my job, but frequently I would also go to the lab. with him and stay up until three o'clock in the morning and try to work the next day. Murray assured me that this was going to change after he got his degree, that this was the life of the graduate student. For many years, probably 30, I believed him. I no longer believe him, because it never changes.

I think there's another point that I would like to include. I think that the life in Calvin's lab. and the way Calvin ran his group was a very, very strong influence on the way Murray has run *his* group and the fact that there are people from all over and that there is a somewhat interdisciplinary (*atmosphere*), never a concentration on only one direction, that there is an openness, and certainly the group meetings, which are not Friday morning but Friday afternoon!

MG: But for many years it was Friday morning and it was at eight o'clock in the morning and the difference is that I would make certain that people were prepared to talk. I wouldn't do the whole Calvin scenario, but only part of it.

VM: For an hour, you have talked only in positive terms about this experience. Was there nothing negative?

MG: Oh, there were many difficulties and problems. I think one of the aspects of such a big group that many graduate students felt was that Calvin was unreachable, that he was sort of up there on Mt. Olympus and it was difficult to get to him for free and open discussions or advice which graduate students typically come to mentors for. I think there were difficulties certainly, or problems for me, that came from Melvin's heart attack. His departure (*from the lab.*) was not easy for me to take because it was some vacuum and void in terms of the *faculty* mentor. Now Andy Benson did step in and do a wonderful job in trying to help but it never really was complete because I would look at the other graduate students whose mentors, whose faculty mentors, were there with them and for them. I didn't have that.

VM: So Andy didn't quite fulfil that role?

MG: He couldn't. He tried to do all that was useful for me in the laboratory and that was excellent and I appreciated that. But when it came to all kinds of hurdles within the academic requirements — which courses to take, when to take them, what kind of research report was in my file that could be used, let's say, for the committee to evaluate my progress — that's where Calvin's absence really did hurt.

VM: Was it difficult for some graduate students to get to him? Was he very taken up with the latest discovery and the person who made it, to the detriment, at least temporarily, of some other people?

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MG: I think I would put it in another way, in a kind of statistical weight-fraction of the group. There were so few of us who were graduate students that we didn't really make a great impact on what were the discoveries day in and day out, what coming out of the laboratories. There were many postdoctoral people and visiting professors and people on sabbatical and things of that sort, that they would be creating aspects of research which were very professional and at the cutting edge. Graduate students were less involved at that, even if we did make some important discoveries and Calvin would sit and listen and explain and catalyse new experiments, they were so filled with the others that we tended to feel a little bit left out.

VM: After you had been there a couple of years, you were almost as experienced in the technology of photosynthesis research as anybody else.

MG: Not so, because there were changing aspects of the photosynthetic experiment that were continuing and continuous throughout the time. What I was used to six months before was no longer being done; the lollipops continued to change, the radiation detection, the whole development at that point post-Beckman DU into the area of the Cary spectrophotometers, which were a total revolution in measurement; the coming into the laboratory of infrared spectroscopy; these made everything change so rapidly that graduate students just could hold on to try to keep up.

VM: So graduate students, or at least your view as a graduate student, were really you were at the junior end of things.

MG: I did have that feeling and I was a learning vessel into which lots of knowledge was being imparted. What my role was, was to learn how to do research. That I learned in the Calvin group with many, many miscues and many experiments that didn't work out. But out of it came a thesis and papers and that ability to say I can work in the best of laboratories.

VM: As we've mentioned earlier, you have kept in touch with a lot of the group, including Calvin on and off. Therefore, you are familiar with the round building, although you've never worked in it, as I understand.

MG: That's right.

VM: What do you think...? Well, let me phrase it in this sense: obviously, the group, any group, including that one, changes in character as it goes on and as it develops and so forth. Accepting the fact that a new building was to be built in the early 1960s, what's your view of the way and the concept of that building, and do you think that its attempt to recreate, in a sense, the atmosphere of ORL worked?

MG: I was at the dedication of the Round House and I remember all of the explanations. I could see that the laboratory was designed with the idea of openness and there it was an attempt to recapture and to stimulate the activities that went on in ORL. It was a much bigger building, it had many floors, and it attempted to do much more than what went on in ORL. ORL was focused on photosynthesis and photosynthesis only. Here there was everything that at that point was of interest to Melvin Calvin...

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VM: Or had been of interest.

MG: Or had been of interest to Melvin Calvin, and because of the size of the building there had to be alliances and obligations and interactions with the Chemistry Department which brought in people of great talent who were really totally independent of Melvin Calvin. So I could see the activities in the Round House as being much more complicated and really not in any way able to recapture that open simplicity of ORL. It looked too formidable an edifice to do that.

VM: Do you think they should have done something else?

MG: It's not for me to say.

VM: It's too late to change the issue, the building is there.

MG: I can see why they did what they did. I wonder, after all these years of being in science research in the university whether any golden era can be recaptured. It's golden because it probably cannot be recaptured.

VM: The problem is that the people survived the golden period with many years of work still ahead of them and they want somehow to prolong the aura of that time; the "aura" is the right word, isn't it?

MG: I think that if I were in that position, what I'd want to do would be at the time when the change was necessary, to change not only the building but the direction of activities completely. I would look to do things that were not comparable. I would look to try to make an impact somewhere else.

VM: I think Melvin tried to do that while at the same time recognising that there were a whole set of obligations to people who were part of the group who were not necessarily able, or willing, suddenly to change direction as a body in some young, new way. By that time, there were many of them, there were eight or nine almost independent or semi-independent research groups. It was a consequence of the way the group had developed later on that the position was arrived at.

MG: He couldn't give up the commitment to many of the research activities of the ORL and Donner period because the people who were doing that work, many of them came with him to the Round House. The funding remained in a major sense the same as it was in Donner and ORL. Therefore, although he did try to do some new things, a central emphasis had to be what went on in ORL and Donner. I think that had built into it the seeds of failure. Not that people didn't try. But the questions became different. Suddenly protein chemistry was interested in the photosynthetic centre, the whole trans-membrane proteins, and the structure and the chemical physics of the process. Melvin had really solved the organic/biochemistry interface beautifully and now there were other questions being asked. And the people who were in the group weren't ready to go in that direction. They weren't the X-ray diffraction specialists,

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they were not the chemical physicists who had come into the field. So, I feel that was the problem.

VM: What happened to you since 1952; it's now 1996? Can you very quickly recapture the last 42 years?

MG: I'll try. What happened was that through Melvin's efforts I obtained a postdoctoral research fellowship in the laboratories of John Sheehan at MIT and I wanted to go in that direction because after my experience in the Calvin group, what I wanted to do is more synthetic organic chemistry. Sheehan gave me that opportunity. I worked in the area of peptide chemistry with John Sheehan and also in the area of amide bond formation as a general phenomenon. I stayed with him two and a half years and from that laboratory I went to Cambridge, England to work with Lord Todd and George Kenner, again in the field of peptide chemistry. I decided that peptides and proteins, or that interface, was the area of research I wanted to follow for my own career. In Cambridge, England I learned a good deal about the kinds of chemistry involved in peptides, nucleotides and conjugates of the two.

VM: Was that the occasion on which you were accused of obstructing the Queen's highway because you opened the car door in the face of a cyclist?

MG: That's correct. That was a particular unintended event, of the fact that I had a car with the wheel on the right-hand side and I don't think I was completely used to getting out of the car on that side into the roadway.

From Cambridge, England I was offered an assistant professorship at the Polytechnic Institute of Brooklyn.

VM: Is that where you did your undergraduate work?

MG: No. I was an undergraduate in the City University of New York and the campus that I was at was Brooklyn College which is different from the Polytechnic Institute which was a private institution. I began my career there in September of 1956. I rose through the ranks from assistant to associate to full professor and I worked with a group in another golden age, this in the area of polymer chemistry. The director of the laboratories and the chemistry effort on polymers was Herman Mark, a renowned figure, and he really pulled together a great and distinguished group of polymer chemists. I joined them and emphasised the bio-organic and biopolymer area of research and continued that when Herman Mark retired. His successor as director of the Polymer Institute was Charlie Overberger; and Charlie Overberger was a very successful organic chemist in synthetic polymers. After one year he was hired away by the University of Michigan and went to Ann Arbor as chair of the Chemistry Department and Vice President of Research.

I succeeded him as Director of the Polymer Research Institute in 1967 and had the opportunity of hiring Dan Bradley who had left the Calvin group, gone to the National Institutes of Health and then I brought him back into academia as professor of physical chemistry and polymer chemistry in the Polytechnic Institute of Brooklyn.

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That year was 1968-69. But, by that time I realised that the golden age and the golden era that Herman Mark had created at the Polytechnic Institute of Brooklyn could not be recaptured or recreated. And I decided to leave.

In 1970 I accepted a position as Professor of Chemistry here at the University of California at San Diego and in 1971 we moved, and I have been here ever since.

VM: And lived happily ever after!

MG: I have been Chairman of this Department of Chemistry and Biochemistry, I have been Acting Provost of Revelle College, I have chaired the Academic Senate, so I have done all the things that are asked of a professor in the University of California. But most of all I enjoy being just a professor and doing the research and teaching that brings me together with my research students, my graduate students, my postdocs. and the undergraduates.

VM: And, of course, remembering the good old days!

MG: Yes indeed.

VM: One last question. Unfortunately, we can't talk to Dan Bradley because he died. When was that and what were the circumstances?

MG: During the late 1960's Dan had been diagnosed to have high blood pressure. He was on medication and he didn't like taking the medication. It interfered with his thinking, as he would tell me. I am only supposing that he went off the medication and was actually working with his wife, Ria, in a Montessori school, painting the place in October, with all the parents, in

ZG: 1970.

MG: In October of 1970. He was on a ladder, painting, collapsed with a massive stroke and died.

VM: While he was still in harness at the Polytechnic Institute?

MG: Yes. He was 41 years of age.

VM: It's a sad note on which to finish what has been an otherwise entertaining hour. Thank you very much. And we've recorded it for posterity.

MG: I hope that it has been useful. It's difficult to remember it all factually, and what I've described I hope was true and what I forgot I hope you'll forgive me.

VM: OK, we will. And thank you very much.

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MG: We talked about the collection of organic phosphates and storage phosphates in *Scenedesmus*. This took some time and my harvesting of *Scenedesmus* continued over a period of weeks. When I had collected a sufficient amount, I concentrated the algae and then, with acid extraction, took out as many of the organic phosphates and other molecules as I could and then undertook a very careful ion-exchange separation and isolation of individual compounds.

Now the compounds were very radioactive because they were loaded with P^{32} and therefore, whenever I ran such an experiment, I put up signs around my lab. bench saying “Danger — Radioactivity; Be Careful”, all the things that were in use to alert people to the fact that we were working with extensive amounts of phosphorus-32. On this particular day, I was undertaking my hottest separation of the radioactive organic phosphates and I’d set up a very careful automated delivery system of the extract to the top of a rather substantial ion-exchange column and also had worked out, after much effort, a fraction collector that was really controlled and oriented to work directly with a specific volume of eluent that would come through the column.

I set this up in the late afternoon and decided I would check this after dinner sometime. Zelda and I were invited to have one of our typical spaghetti dinners with Larry and Lee Schechter (Larry was a graduate students in physics and we were close friends with the Schechters) so we went to their house. We had dinner and we were chatting a bit and we were trying to decide what to do — whether perhaps to go to a movie or something of that sort in the evening and I indicated that I should go back to the lab. and check this elution of highly radioactive organic phosphates. And Larry came with me and we came to my lab. bench and to my horror noticed that the fraction collector was moved rather substantially out from under the dripping ion exchange column. And a very large puddle had accumulated on the lab. bench and with a simple Geiger counter I could see that it was very radioactive. After blanching and sort of gasping, what I did was, of course, stop the experiment immediately and survey the scene; and it was clear to me what had happened.

Every night in those days, the security at the Radiation Lab. checked all of the windows. And even though I had the sign which indicated that there was radioactivity, the guards were used to those signs and, as the guard checked the window behind my lab. bench and desk, his gun, which was in a holster on his hip, must have hit the fraction collector and moved it. He never realised it, checked the window and left. And here this experiment which took several months to prepare was in danger of being destroyed simply on the basis of this accident.

Well I was so stunned that I didn’t know what to do. I suggested that we go back to the house and take stock. We did. We decided that there was nothing to be done then. I had roped off the area and made sure nobody would go anywhere near that and we decided we would go to the movies, at least to give me time to think before going back and deciding what to do. We went to the film “High Noon” which was, I guess, with Gary Cooper and was a western adventure film. And this, of course, held my attention and riveted my thoughts on what was happening in the imaginary world and did the job of at least giving me the time to make a plan.

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Following the movie, Zelda and I went back to the lab. and I cleaned up and, of course, alerted the health and safety people so that they would be able to come in the morning and check out and decide how to further secure the area and make it habitable. What we did — what I did — was to work up the rest of the experiment completely and what came out of that was a sufficient success to go forward and complete my thesis. But if it wasn't for the accident of the gun, probably the gun hitting the fraction collector, I'm not sure that I would have found that huge reservoir of polymetaphosphate because it came in so much later after the rest of the radioactivity that it gave me the time to continue the experiment and to find this rather substantial reservoir of high energy storage phosphate.

VM: So all was well that ended well.

MG: Yes, and it taught me that it was probably not necessary for the security guards to check the windows that frequently and without being concerned about what was going on around them.

VM: OK.

MG: By the way, that experiment did take me all night to work up. Zelda and I finished in the laboratory about six o'clock in the morning and went out driving back home as we watched the sun rise. So it was not only a reasonable success but it was sort of a very nice feeling to watch the sun rise over the Berkeley hills as we went to our apartment.