

Chapter 30

MARTIN GIBBS

Woods Hole, Massachusetts

July 22nd, 1996

VM = Vivian Moses; MG = Martin Gibbs

VM: This is a conversation with Martin Gibbs in Woods Hole on the 22nd of July, 1996.

Of course, you were never a member of Calvin's lab. but you were aware of what was going on and were looking at it from the outside and it is in that context that I would like to talk to you, in the early days. So can we start by you telling me how you became involved in photosynthesis and what you were doing in that area around the late '40s and early '50s?

MG: In the late '40s I was still finishing my doctorate degree.

VM: Where did you do that?

MG: At the University of Illinois in 1947. And since post-doctoral experience was rare in those days, because the moneys were not there, I immediately took a job, and that was at Brookhaven National Laboratory. The other great event on the Illinois campus was that I met my wife there, over 50 years ago. And so if you look at that article I wrote for *Plant Physiology Newsletter* it indicates I went directly to the Brookhaven Laboratory from my degree from the Illinois campus. It is interesting that Bob Emerson came to the campus while I was there but I had already selected a major professor and therefore I couldn't work with Bob Emerson. I was finishing off my degree when Bob arrived. And so I left and went to the Brookhaven Laboratory.

At that time it was an old Army fort, or base, which was an induction centre for both World War I and World War II. So on arrival there were no laboratories, just barrack buildings, and no library. The nuclear reactor was being built. And one had to wait for about a year and a half before we had laboratories and so one could do no more than read the literature and, reading the literature, that is where I got acquainted with the research in the Calvin laboratory.

VM: What had been your background before that?

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MG: I was a botanist. I took my degree in the Botany Department at the University of Illinois. I was trained as a more classical botanist and so when I came to the Brookhaven Lab. (as) the plant scientist, I was *the* plant scientist, my job was to do service work for the mammalian physiologist on campus and what they wanted was radioactive sugar which they could use in their experiments. So they approached me and said, “There is a process called photosynthesis. Why don’t you try and make us some radioactive sugars?” So my first assignment was to make radioactive sugar. I read the Calvin reports on what they were doing with higher plants and with algae. So I built an apparatus of my own and gave labelled CO₂ to higher leaves to isolate sucrose and hydrolyse to glucose and fructose. At the time I was doing this I think a paper came out with Greg Kracow (*spelling?*) and Zev Hassid and (*H.A.*) Barker in which they had taken *Canna indica* and had isolated radioactive sucrose, which was what I was doing at the same time. So after I made the sucrose and broke it into glucose and fructose and crystallised it and gave it to the mammalian people, I became a factory source. And so I gave radioactive sugars to I don’t know how many people around the world, mostly in the United States.

Then I became interested in where this sugar was labelled. The only process at that time had been designed by Harland Wood in Cleveland. I went to Cleveland and Harland showed me the procedures and brought them back to Brookhaven. I decided to degrade the sugar and find out if it was uniformly labelled. As usual, as an isolated individual without any support, anything I had done was done by the Calvin lab. at least months before I even got ready for publication. I think the Calvin lab. did it by chemical means because Andy was such a superb chemist, and so was Melvin, and so they had chemically degraded the sugar, in pairs again, and so I did the bacterial procedure and I just confirmed what they had already done. So I just dropped out of photosynthesis completely; and so I got involved with the pathways in which sugars were broken down by plants and by bacteria because by then I had a very good supply, basically one of the world’s supplies of labelled sugars — glucose labelled in the 1 position, glucose in the 2 position, glucose uniformly labelled — and so I decided to feed these sugars to microorganisms to determine whether or not the only pathway known then, which was the classical glycolytic Embden-Meyerhof pathway, was universal. It was a stroke of good luck that I C. Gunsalus came from the University of Illinois to spend the summer with me. Here was a very highly established investigator and I was still three or four years past my Ph.D. degree, and he said, “I want to use your sugars with my organisms.” So Guny came for the summer and lo and behold he brought this organism called *Leuconostoc mesenteroides* and there we discovered a completely new pathway of sugar degradation. I gave a seminar a year later on the University of Pennsylvania campus and I met a person who was a boyhood hero to me and that was Meyerhof.

VM: Oh, was he there?

MG: Yes. When he left Germany he came to Pennsylvania. And he listened to the seminar (I don’t think he believed a word I said). He believed there was only one pathway for the breakdown of all carbohydrates...

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VM: His pathway.

MG: His pathway. And I tried to explain to him that the isotope indicated a different pathway. It didn't indicate the intermediates or the mechanism, it was just that it had to be a different pathway. That eventually came out in various laboratories. One of the major players was Seymour Cohen. (He just lives around the corner here now. He is retired and is over here.) Then, after I published that work, I got a phone call one day from Severo Ochoa saying he had a visitor from Munich, Feodor Lynen and he wanted to make some radioactive...at that time they worked on the condensing enzymes, they called it, and they were looking at the mechanisms by which acetyl CoA combined with...gosh...

VM: With oxalacetate.

MG: with oxalacetate to make citrate. And they had labelled acetyl CoA. They decided to come to the laboratory. And they get here, these two very famous, established men, Ochoa and Lynen, to work with me to make their acetyl CoA. It was a very funny incident because I said to Ochoa and to Feodor, "Gentlemen, please put on gloves because we are working with radioactive materials." And they said, "Young man, we are established chemists. We will never get radioactivity on our hands." After about half an hour I said to them, "Now we just test the hands under the counters. Well, the hands just blew off and they just dived for the sink, and they were scrubbing away and scrubbing away, and finally Ochoa's hands were down to around 40,000 counts a minute and Feodor's down to 10,000, and Feodor said, "You know, Severo, it is because of your hot Spanish blood and my cold Prussian blood that you are running 40 and I am only running 10!" (*Laughter*) After a few days we made the compound and so off they went.

VM: All your syntheses were enzymatic syntheses, you were not a chemist?

MG: I was not a chemist at all. And so after I did the work with *Leuconostoc*, I went to the 1954 Botanical Congress in Paris; Feodor Lynen asked me to come to his Institute and give a seminar, which I did in German. At the end of this lecture in German a young grad. student said, "May we have the questions and answers in English, please?" But in that room was Otto Kandler. He came to listen to the seminar. And he and his wife invited me to their apartment for lunch and during the lunch they approached me with respect of coming to the Brookhaven Lab. because they said "We would like to take your method of degrading sugars, where you get individual carbons instead of pairs of carbons", and because Otto became interested in photosynthesis at that time. So they came with the express purpose of feeding labelled CO₂ to algae for short periods of time to make use of the *Leuconostoc* procedure to find out how the sugars were labelled.

VM: They already knew what Calvin's group was doing or had done?

MG: I assume they did. I didn't know that. So that was my introduction to photosynthesis. It came because of Otto and the *Leuconostoc* procedure.

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VM: And that was in about '55?

MG: In '55 when Otto came with Traudl and their child Maya, the three of them. And Otto spent a half a year and then they moved on to Berkeley.

VM: Do you remember why they moved on to Berkeley?

MG: I just assumed it was pre-arranged.

VM: Because the story that I heard, and see if it rings any bells, was that Otto was on a Rockefeller grant, as I remember...

MG: I think that's right.

VM: And the Rockefeller man, whose name I vaguely remember as Pomerat, but I'm not sure, actually asked him to go to Berkeley and he was reluctant but that's what I remember. Do you remember that?

MG: No, I just took it for granted that they would spend half the time with me and half the time in Berkeley. And so they bought an old car and Otto had never had a driver's license so she did all the driving from the East Coast to the West Coast with their little daughter, Maya. I just took it for granted this had been pre-arranged. I was never consulted, no.

VM: At that time had you...were there any discrepancies in your and Otto's degradation data and that of Calvin's? Because when Otto got to Berkeley there were furious debates between him and Melvin.

MG: As I remember it, the only degradations that were carried out in the Berkeley lab. were done where they got pairs of carbons, that is carbons 1 and 6, 2 and 5, and 3 and 4; and they then concluded that the radioactivity in the two halves were equal: $1 = 6$, $2 = 5$, $3 = 4$. And the *Leuconostoc* procedure showed that was incorrect, as you well know. The 4 was labelled the highest, then the 3, and so on and so on. That was published and then after that was published Otto just took off and went to Berkeley.

VM: Because Calvin would no doubt have seen that. That was really a tautomeric argument because if they measured it in pairs then it was an assumption to assume that 1 was equal to 6. I don't remember the details but ,if you are correct, then they were simply making that assumption and hadn't demonstrated it.

MG: Part of it came because Andy had worked out, I recall, maybe it was Al, an elegant procedure for the breakdown of the sedoheptulose and in fact when we saw the sedoheptulose data and the ribulose data it was clear to us that the *Leuconostoc* data were correct. Because, if you remember the sedoheptulose data, there was a hole in the middle of the labelling and that was explained very beautifully by what we saw in the sugar, in the glucose. But because of the degradation procedures they had used, they couldn't take advantage of that and so we saw the ribulose data where the 1 was

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more than the 2 and the 3 was far more active than the 4 and 5. The interesting data was sedoheptulose. The glucose fit in perfectly with their own data.

VM: Were you in touch with them at that point?

MG: No, I had never met Calvin at that point, I'd never met Andy and I didn't know Al and I never in my life had been there.

VM: When did you begin to know them?

MG: Probably not until the '60s.

VM: Oh, really; as late as that?

MG: I was an isolated individual working by myself on the East Coast. I didn't travel very much, you see. I don't think I met Melvin until at least into the '60s and I don't recall meeting Andy until that time, or Ed Tolbert or any of these people. Because outside of the work I did with *Leuconostoc* and the degradation of sugars with Otto, I went back to plant science, plant respiration which was what I was doing. So I really backed off of photosynthesis.

VM: So you didn't have any great interest in photosynthesis at that time?

MG: No. It was a means to an end with the *Leuconostoc*. And also because people like Harry Beevers came to the laboratory, B.L. Horecker came to the laboratory and we put to use the *Leuconostoc* because at that time Bernie Horecker was involved in the conversion of pentose phosphates to hexose phosphates. And so we were synthesising labelled ribose 5-phosphate, giving it to liver extracts as well as plant extracts, crystallising out the glucose 6-phosphate, degrading the sugar to show the mechanism both in liver and in roots and in leaves.

VM: I'm trying to remember when, this would have been in the mid-'50s?

MG: In the mid-'50s.

VM: Because that's when Horecker and Racker were identifying the enzyme for the pentose phosphate cycle.

MG: That's exactly right. And so Horecker came and spent a summer as a means of confirming what he and Ef Racker had written down as to be the intermediate pathways.

VM: And, of course, their information was entirely relevant to Calvin and, indeed, they used it in Berkeley as leads to what might be happening...

MG: That's correct. I don't think Horecker and Racker looked at it that way. I think they were interested in mammalian tissue and it just happened to fit in with what Melvin was doing.

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VM: Oh absolutely. But I think the fact that they discovered those enzymes gave him support for the mechanisms that he was playing with.

MG: I think there was no doubt. Unfortunately, I felt neither gave enough credit to each other. That is, I don't think Horecker and Racker really credited Calvin for what he had done and vice versa. I think they had gone on their own ways and never communicated.

VM: As you saw it from outside these groups, but aware of them, were you conscious of a lot of competition between them?

MG: No.

VM: They just ignored one another?

MG: I can't answer whether they ignored one another. They certainly didn't have much communication simply because, remember, Ef Racker was an MD who came through the field of psychiatry.

VM: I didn't know that.

MG: In fact, he came to our home. He was a very professional artist and when Ef came to our home in Ithaca, New York, he did portraits of our children which hang on the wall in our home. And Bernie Horecker, again, was in the NIH and his interest was in, really, liver and Bernie had never used leaves and roots until he came to Brookhaven. So I just don't think they communicated much with each other. I think they were aware of each other, naturally, but I don't think they even went to the same meetings. I don't think Melvin came very often to the biochemistry meetings and, if he did, I don't think they were friends.

VM: And you were in very much the biochemistry circuit and the plant physiology circuit at the time?

MG: Yes.

VM: How long was it before there were other people in Brookhaven working alongside you?

MG: When I left Brookhaven I was replaced by Clint Fuller.

VM: When was that?

MG: In 1956.

VM: Were you still alone in Brookhaven?

MG: Yes. I was still alone.

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VM: Not even a technician?

MG: Oh, I had a technician and I had an occasional post-doc. who would come but most of the folks who worked with me came in the summer.

VM: What did Brookhaven see as the purpose of employing you? To service the mammalian people?

MG: In the beginning, to service the mammalian people just to make labelled compounds for these people.

VM; Where did you go when you left Brookhaven?

MG: I went to the Department of Biochemistry at Cornell University, in the College of Agriculture, and I left Brookhaven because they didn't grant me tenure so I just left and became a tenured professor at Cornell University. I stayed there ten years before I came to Brandeis University.

VM: Where you then stayed then for the rest of your...

MG: For the rest of my career.

VM: ...professional life.

MG: That's right. So I didn't move very much.

VM: But you did become involved in photosynthesis in various ways later in your career?

MG: I became involved later but mostly at the level of the organelle after that, just to determine if the sugar is made by the same pathway as the intact plant using the *Leuconostoc* procedure and then I got involved in the regulation of the pathway, mostly at the level of the chloroplast and got involved in (*what I*) call "chloroplast respiration". Chloroplasts do respire and they break down sugar by the alternate pathway and by Embden-Meyerhof to some extent. I was really never that deeply involved in what you call photosynthesis. Clearly if I had started with Bob Emerson, probably I would have been. But I didn't come from that background.

VM: But from your background, as you look at what went on in the '60s, how do you view the work of Calvin and his group in contributing to that resolution?

MG: Well, clearly it was the key work; it was the basic research of that period and all of us were just followers because they were the leaders and we just followed along. All we could do, really, was to fill in the little holes that fell through the network out there. I always envied them — this huge group of obviously very competent people — and clearly working alone as an isolated individual trying to compete with them was impossible.

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VM: Did it ever occur to you to join them?

MG: No, I'm an Easterner. I am a Philadelphian by birth and I have made my whole life here on the East Coast. I was never invited.

VM: Many people more or less invited themselves and were more or less accepted. There was a time when the group was expanding quite quickly and Melvin would bring in good people who showed an interest. I can't predict what might have happened 40-50 years ago but it 's not out of the question that had you expressed an interest you would have gone.

MG: Well, but I didn't know these people, you see, I had never met any of them. We might have attended common meetings but I didn't know them so we were never on speaking terms at all. So as absolute strangers it was difficult to pen a letter saying, "I am interested in what you are doing, would you mind having me in your laboratory?"

VM: I was able to do that as a post-doc. but then you were beyond that when...

MG: I was beyond that. I didn't have tenure at Brookhaven but I had a laboratory array. And I really wasn't interested that much in photosynthesis *per se*.

VM: So your connection, your only connection for a long time, was Otto going from you to them?

MG: That's about it. When Otto left I went back to working on plant respiration in intact plants and extracts, on the pathways of sugar breakdown. So when Otto left my deep interest in photosynthesis went.

VM: Well, I am sure we'll pick up Otto's story, at least I hope we'll pick up Otto's story next year when we get to see him. Because when he arrived in Berkeley, as I remember it, there ensued months of vigorous exchange of views with Melvin, to put it mildly. It led almost to a crisis of discussion in Melvin's lab. because there was a conflict of interpretation of data and there were a couple of big internal meetings to try and resolve it and collectively everybody agreed that he was essentially right. I don't remember now what Otto felt at the time but perhaps Otto will remember that.

MG: When Otto left Berkeley he came to Cornell University where Karen (*Martin Gibbs' wife*) and I were now living and spent a few days and he told me about this compound, hamamelonic acid which he felt was the critical intermediate, and that's about all.

VM: He didn't discuss the big arguments?

MG: No, Otto never discussed the arguments that went on in the Berkeley lab. Well, Traudl occasionally came into the lab. to do the degradations and she also volunteered to do the washing of the dishes since I didn't have a dishwasher. I washed dishes as well as anyone else in the lab. did. She was very helpful in that respect.

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VM: Was she a scientist?

MG: I really don't know her educational background but she seemed to be competent because the things we were doing were rather routine.

VM: Once you worked it out ...

MG: Once you worked it out it was a routine matter. She was just as good as I was at doing these routine things. She was there quite often once she could get a baby-sitter. In those days you couldn't bring a child into the laboratories of the Department of Energy so from time to time when she had get a baby-sitter she came in and worked alongside of him. It is a pity, I have some great pictures of that period, some big ones of that sort showing Traudl in the lab. and Otto in the lab. and so forth. They aren't here they are somewhere in Lexington, I suspect.

VM: Well, sometime, if you can be bothered to make a decent copy of one of them which shows you with them that might be interesting to have.

MG: I think we have one on a slide which shows us at a big lobster party because we always enjoyed lobster and champagne parties in the lab. and in those times 10 and 15 lb. lobsters were rather inexpensive — they were like a trash fish at that period — so we would make the lobster in the autoclave...(*indecipherable*)...a pot that large for a 10 or 15 lb. lobster. So we had parties and I think I have a picture on a slide of the whole lab. with Otto and Traudl and myself standing along this huge lobster after it was cooked in the autoclave. I don't think it has been made into a positive.

VM: What about one in the lab. — do you have one of them working in the lab.?

MG: Individually, but not together. I'm pretty sure it's individual.

VM: We have an odd picture of Otto, not of Traudl, of Otto in Berkeley at one stage. We are taking pictures of everybody now. I don't really suppose we can do a then and now in anything we publish but anyhow it is interesting to see what people looked like then.

MG: Otto then, as he is now, was a very intensive man and he was very devoted and loyal to the subject. He was essentially a 24-hour worker because he was there only six months and he accomplished an awful lot in that six-month period, and Traudl was very devoted to Otto and came to the laboratory, as I said, to assist him when he felt he needed the help. But then after they returned from Berkeley to Cornell when we were there, and he went back to Germany, I didn't see Otto for many years. We've had contact but very limited contact in the last 30 years or so.

VM: Well, the last time I saw him was on a bus in Moscow in 1968, I think — that's nearly 30 years ago — at a microbiology conference or something of the sort.

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MG: Yes, Otto, you see, switched, as you well know, out of plant science and so we didn't go to common meetings after that. And I've been in contact mostly with Erwin Lanscow who...and also I had other post-doctorals from Otto's lab. that came to me.

VM: So there was some sort of contact.

MG: Again they came basically to learn the *Leuconostoc* technique and to also apply the sugars to microorganisms to see if the pathways were there. And we had just worked out the procedure for what became the Doudoroff-Entner pathway because we gave labelled sugar to an organism, a pseudomonad, which showed that the *Leuconostoc* pathway, the Embden-Meyerhof was not present and we had just done the isotopic work and within a year Doudoroff and Entner came out with the pathway.

VM: I had realised you'd been involved but I hadn't ever heard your story put together in a coherent fashion to realise how it had started and why you became.....

MG: We were 3,000 miles away and, as I said, I had not even letter contact with these people or telephonic communication with these folks on the West Coast.

VM: Things are very different now from the way they were then.

MG: That's right. Travel money was not as abundant as it is now and the annual meeting was the Federation Meeting, which was in Atlantic City, and therefore a local meeting for me, basically, so that I had never been to the West Coast in all that time. I guess I haven't seen Melvin Calvin, even now, in 20 years. I see Andy to some extent at the Academy meetings and haven't seen Al Bassham in 10-15 years. Ed Tolbert I see at the Academy meetings annually. The other people like Bert Tolbert or other folks in the lab. I don't know.

VM: I am sure they would be delighted to see you if you were in the same place and looked them up.

MG: Well, I did see...the last time in Berkeley I spent the day with Dan Arnon and with Bob Buchanan. That's right, I was on the Visiting Committee so I did see Melvin at that time — the Department of Energy Committee to assess the laboratory.

VM: That's when you went into the round building.

MG: That was the first time I'd been inside that building — that's the only time I was inside that building. And Al was kind enough to show me about the building and show me how the thing was cut up into wedge-shaped pieces of a pie and I had never seen the laboratory. I had never been invited to see the laboratory either, by the way.

VM: There was an older building.. and they probably told you at the time that the philosophy, if that is what one can call it, for designing this was derived from the old wooden building in which the early work was actually done, which by chance, was a very open building, one in which the group had grown up, and Andy has nice stories about how he organised that building for them. It was a building that E.O. Lawrence

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originally had his 37" cyclotron in and when that went out it left a building with a big hole in the middle and they just took that space.

MG: I would suggest you visit Allan Brown, who was a very close associate of Hans Gaffron, and he was actively competing against Melvin and Andy in that period for the first labelled compound in photosynthesis.

VM: Which department was he or is he in?

MG: I think at that time he was in the Department of Botany at the University of Pennsylvania but when he did the work with Hans Gaffron it was done at the University of Chicago.

VM: OK. But I could presumably reach him through...

MG: At the University of Pennsylvania, Botany Department. Because he can fill in that period in the late '40s and that was hotly contested: what was the first labelled product of photosynthesis? Melvin obviously was pushing PGA; at that time I don't think Allan Brown and Hans Gaffron were. I think that's a critical period. I don't think I would depend entirely on information from the Calvin laboratory. Sam Aronoff was associated with that lab. and I know Sam is a very even-handed man but he was in that laboratory.

VM: OK. We'll do that when the opportunity arises. Meanwhile, many thanks; nice to see you again after so many years.

MG: You're welcome.