

## Chapter 16

### RICHARD L. MEIER

Berkeley, California

June 13th, 1996

**VM = Vivian Moses; RM = Richard Meier; SM = Sheila Moses**

**VM:** This is a conversation in Berkeley with Dick Meier on June 13th, 1996. OK Dick: all yours.

**RM:** How did I come to photosynthesis? That's a rather interesting question. It was a round-about process. I was doing a doctorate in chemistry at UCLA and I was telling the faculty that the kind of chemistry they were teaching was going to be out of date very soon. So the faculty challenged me to give two seminars to the department on what I thought would be the chemistry of the future.

**VM:** When was this?

**RM:** In 1942. I gave two seminars, one on the chain reaction and the other one on nuclear reactions. They came together, announced, in '45, and suddenly I was an atomic scientist without ever having been part of the Manhattan District. And, more than that, they had sworn an oath to the FBI that they would keep the secret and I was not supposed to know any secrets. But I knew all of the literature up to the time when they stopped publishing.

I was working at that time in order to avoid the draft at the Standard Oil of California laboratories, now called Chevron, in polymer chemistry (chain reactions, again). In doing this, as soon as the bomb had dropped, I sat down and tried to figure out how it must have been done and then started talking to people around here at the University who might know something about it. Of course, they couldn't talk, but they couldn't object. They could object if I was dead wrong. So, I became quite confident. Then they organised a branch of the Atomic Scientists here in the Bay Area and changed the name almost immediately to the Federation of American Scientists; I have forgotten the exact name of the scientists in the Bay Area but it was a branch of the Federation of American Scientists. I became the chairman of the research committee that we organised immediately; there were about 500 of us in the Bay Area. Later on,

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I became the acting chairman which I was until 1947. So, some of the people are still around here, like Connick and Brewer; others have gone on.

In that particular period the question was: what to do with nuclear energy in terms of international control, or could it be used for power? I had set up, while I was still working at Standard Oil, a study together with economists and engineers here on the campus so we did a study of the feasibility of nuclear energy. We came to a number of conclusions — plants had to be large, they had to be away from the city, and that sort of thing — which were still new. Then we circulated a manuscript around the members of the scientist organisation. One of the copies happened to go to Pacific Gas & Electric. Pacific Gas & Electric had noted, without any signature on the manuscript, as to where I, the chairman of the committee had come from. They tracked me down to Standard Oil and they then approached the president of Standard Oil and said “remember we had an agreement that if you didn’t go into electric power we would not go into energy”. I later on ran into these agreements in Britain, too. This very much embarrassed the President of Standard Oil and he said “Well, it won’t happen”. So he came down to the laboratory, to the director of the laboratory, telling me “it won’t happen”. Besides, I wasn’t competent; I didn’t have any background in the field. I said, “well, I might have agreed with you”. But it just happened that the morning before I talked to him, the editor of the leading journal in America, in chemical engineering, had read the manuscript and he wanted it. He said it was the first manuscript with facts in it. So, here I was — we finally ended up with a situation where it was published, but without any names on it. It was amazing that only the sponsor, which happened to be a foundation that had provided the clerical work, and that sort of thing at the University, and the chairman of that particular fund was on the paper. All the rest was just a committee.

**VM:** Unmentioned? Unidentified?

**RM:** Unidentified.

**VM:** Which was the journal?

**RM:** *Chemical Engineering*. So, it got published this way but I could see from the flack that it was going to be a little difficult to do interesting research. So, I started looking around further. At that time, in fact, I approached Calvin. Calvin was sympathetic to the aims of the atomic scientist group.

**VM:** Standard Oil research, was that in Richmond at the time?

**RM:** Yes.

**VM:** Still the same place?

**RM:** Yes, still the same place, different buildings but same place. So I approached Calvin at that time, hoping that despite interviewing me, he might be able to write a short note recommending me for going beyond. That was the first time I really got to meet

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him. He said he couldn't really do that — I hadn't worked for him or with him. I understood that.

**VM:** Can I ask you a couple of questions about where and when did you meet?

**RM:** In the same building that you are talking about, the ramshackle building. It must have been 1946.

**VM:** He was quite heavy at the time, wasn't he?

**RM:** I got the impression only that he was very active and enthusiastic. I didn't judge about heaviness or anything like that. He was not thin, but he was certainly not heavy from any way that would give me an impression. What I saw around me was very interesting kinds of action and him pushing the postdocs. and doctoral candidates. You always have to look at the latest instruments; that was impressive.

**VM:** Just as a casual visitor to the lab.

**RM:** I had not seen a lab. with that much vitality in it.

**VM:** In '46, that was very early days.

**RM:** Yes. Those were the early days. Actually, later on, I moved on to Washington to become the secretary of the Federation of American Scientists, representing them to the press, to the public, to Congress and that sort of thing, handling the political affairs and trying to get the National Science Foundation started. So I was there (*in Washington*) from 1947-1949. While I was there I began to realise that the secrecy was going to be so high in the future that it did not pay to continue a career that close to nuclear energy. That you had to get security clearance, which I didn't have at the time, and as a result the FBI was on my tail all the time, listening to everything I said in public, a spy at the office door, etc.

**VM:** They were simply suspicious of your knowing what you apparently knew from the public domain.

**RM:** Later on they told me that I was letting out secrets all the time.

**VM:** Although you actually didn't know any secrets.

**RM:** I didn't know what they had classified. We were very careful, too, in the sense that the people that I talked to said well, this part, is worth looking at in the *New York Times*, or sometimes in an overseas publication. One always pieced together what was secret from what was published and made the deductions of what went on in between, how it got that way.

**VM:** If I may harp back, just for a second to the way you started, how long did it take you after the bomb had been dropped to work out more or less what had happened, how it had been done?

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**RM:** Overnight.

**VM:** How old were you at the time?

**RM:** I was 25.

**VM:** I asked the question because I was 17 and a friend of mine were on holiday together, and we spent the whole night trying to work it out, but we failed. Younger than you were and less experienced!

**RM:** I had had the chance of looking up all the published literature and the library was fairly good at UCLA. It had no one, however, who knew the field. They'd already been off in the various laboratories, so we had lost half of our faculty actually to different parts of the Manhattan District.

**VM:** Sorry, I took you back from the FBI.

**RM:** I then made a resolve that I had to do something that would not be classified and still would be important and interesting. I decided, because there was a man by the name of Mayer, a Frenchman, who later on became professor of nutrition at Harvard, a very young and very enthusiastic man. We met and talked fast with each other. He was talking about the food problem of the future. He was forecasting by 1990 there was very possibly going to be a protein shortage. So I said, "You know, that's an interesting problem. It takes that long for fundamental research from the forties to the nineties before it could make a difference. So you could do some important work at this stage." With his encouragement I went to the Library of Congress, which is the best American library, and started finding out what there was available. Mostly it was done through *Chemical Abstracts* which was comprehensive. So then I could begin to see that, yes, it was possible. The Germans had been doing some work during the war. It was just being released in '47 and I had identified it. I checked with my friends, who were on the FAS council who were biologists, and they said you can't believe that work because it's German, Goebbels' propaganda. I discovered the next week that it could be reproduced in New Jersey. I could then feel that I was on fairly safe ground.

So, I began to synthesise from food yeast what might be possible. Starting from sugar cane, going through food yeast into protein, etc. And it would be reasonably economic. One of the things I had learned at Standard Oil is a lot of chemical process technology, chemical engineering, and I had also been *the* person in the research group that understood prices, economics, and that kind of thing. So I could do economic calculations: what was economically feasible. So, that's what I could apply to the food yeast.

The difficulty was: Was there enough room in the world for the sugar cane, to grow the protein that the world needed by 1990? I had then decided I had to find out was there a better way yet, and that meant going directly to photosynthesis and trying to

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understand enough about the process to see what would allow one to convert it to this continuous flow technology that the chemical engineers were just then developing.

**VM:** So, you were adopting an engineer's approach to the problem.

**RM:** I was adopting any approach. I also adopted the social sciences approaches and everything else. For instance, I went to Margaret Mead and heard that she had looked at food preferences — would people eat the stuff? — and she would tell me what was needed to get people to eat the stuff, and she doubted it. But she also indicated the kind of research that was necessary. In other directions, as well. I was looking at total feasibility, not just production.

**VM:** In late 1940's terms.

**RM:** In '47.

**VM:** Before the biological revolution, as it were.

**RM:** Yes, that's right. Before the code was broken, before the genome.

**VM:** Before one knew there was a code to break.

**RM:** Well, there was a suspicion. Information theory came out in '48. At the very end of the war, while I was working for Standard Oil, I had been trying to figure out where the breakthroughs would occur next. I decided there had to be some way of translating from biology into hardware-type technology. I found two other people in the Berkeley library looking at various journals trying to get a clue so we could discuss it with each other. But I was unsuccessful; I couldn't find it. Nor were they successful. It turned out to be Shannon of Bell Labs., who was successful. But because I had been asking the questions, somebody whom I still don't know sent me a copy of Shannon's book as soon as it came out, which was only a few months after the paper came out. I and a physicist in my office in Washington saw it and we said "This is it; this works!". So the next year, I took the slow boat to England with a lot of students on it and used the students as subjects and did all sorts of experiments on the students. The idea of a code already existed prior to the time that it was found and how it related to language was relatively easily handled.

About '48 it was then realised that the world population was going to get very likely still larger than there would be sugar cane land. Therefore, we had to do this approach to fundamental photosynthesis. I, during this period, was travelling around to the various centres that the atomic scientists had their activities: from MIT, Harvard all the way over to Berkeley, through the main labs. at Los Alamos and Oak Ridge, Chicago. In all of them I started asking questions about what they know. All the figures pointed to: at Chicago Gaffron and Franck and at Berkeley, here, and one man whose name I cannot recall right now who was simultaneously working but over in the more classical biology department, using microorganisms, and apparently working on some kind of mechanism of photosynthesis.

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**VM:** Here in Berkeley?

**RM:** Here in Berkeley, in the 1940's.

**VM:** Were you thinking of Dan Arnon?

**RM:** Arnon! Yes, Dan Arnon. I couldn't think of his name last night. I tried to visit all of these people while I was making these visits to the atomic scientist groups. Shortly thereafter, toward the end of '48, the atomic scientists ran out of money. I had a family in Washington and they didn't know how they were going to maintain the office so that I looked around, talked to some of the friends that I'd made in Washington, told them I had to live by my wits and run the office with my left hand. One of them was a Pulitzer Prize winner for the major newspapers in Minneapolis, who also published *LOOK* magazine. He said, "Look, I'll hire you as a consultant, just for ideas that might lead to major stories in the future". I then said to him, "well, the real problem is going to be food in the long run, and therefore the only thing that you can take a picture of at the moment are the facilities in Jamaica for making food yeast and another facility run by a parallel producer organisation in Trinidad". So he sent me with a photographer to those two places and I managed to get a little bit of the product. They didn't advance the idea of photosynthesis or anything like that except that I had stopped in here (*i.e. Berkeley*) to test out the ideas of having an open cultivation that would be relatively cheap. Everybody here was very fastidiously trained to keep their algae from becoming infected one way or another.

**VM:** Can I ask you a question about what sort of framework you had in mind? Were you thinking of perhaps improving photosynthesis to give you better yields or were you thinking of better organisation in order to make use of existing facilities and existing abilities — what had you in mind?

**RM:** First I had to have a feeling for what was the simplest possible method of producing protein with photosynthesis and algae.

**VM:** Yes, but why algae? Why not field crops or trees?

**RM:** It was fairly evident from the calculations that you could get 50% to double the yield if you had a simple process.

**VM:** But you had capital expenditure in some sort of equipment.

**RM:** You would have to have capital expenditure to agriculture and, therefore, you look for something simpler than agriculture. The calculations there had suggested what we really need is stirring. Jack Myers over in Texas had indicated that the yield went up quite a bit. The way they were stirring at the moment was something that was fairly expensive in the laboratory so that this began to add a new component in order to get another 20-30% increase in yield per unit area. Another question was about edibility. Nobody ever tasted the stuff except a scientist at Columbia University. He said, "Well, I took some of the product that I had accumulated in test tubes and kept in the refrigerator, and put it together and managed to swallow it". I said, "What

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happened?” He said, “I had to stay within sight of a bathroom door for two days!”. So, it did not seem to be edible. We had to do a search on amino acid distribution and all that sort of thing in the protein, which is pretty good, quite balanced for human nutrition. We couldn’t figure out what was wrong. In the case of *Torula* food yeast the psychology of it was that it really didn’t quite fit human nutrition. People didn’t like it and they didn’t know why they didn’t like it, nobody could figure out why they didn’t like it. I managed to get hold of some and made chocolate milk out of it for my kids; they loved it the first time, the second time, they consumed it, the third time they said “do we have to drink it?”. I could see that there was something in it that was wrong. A little bit later on, in the early fifties, I discovered that there was a product that was fat soluble in yeast that could be taken out of it that made it very compatible; the natives from Brazil, all the way through the Caribbean, had been doing this for a long time. Wherever they could find yeast or yeast-like materials they had been boiling them in oil with cornmeal or something like that. Here’s the way one learns.

The stage after that was one where we started looking, we are now about 1949, and Leo Szilard had always been somebody behind my life sort of pulling strings, and that sort of thing, and he had managed to find some money at Pabst Beer for me to give lectures in microbiology at the University of Chicago in the spring of 1949. Since I had to live by my wits, I had to accept things like that. I gave as thorough an analysis of the way in which the microbiological work might affect world food problems at that time.

**VM:** You had, presumably by then, decided that you were not going to make a career as an experimental scientist.

**RM:** No, I hadn’t. It could be experimental science too; if so, I wanted to do those key experiments. In order to get the “key” I had to take all the relevant factors into account. At that particular time, as a result of those lectures, the Division of Social Sciences at the University of Chicago offered me a position to sort of teach the people in social sciences about the new world of science. They wanted to put me on the faculty in Social Science. But I said that social scientists don’t make the money that scientists to. They said, “we’ll give you the salary of a chemist”. But I said, “Do I have the freedom in the Division of Social Sciences? Suppose there is a new theory on the origin of life that I thought was interesting. Could I do this at the University of Chicago?” I was pretty sure I could, because the University of Chicago was a leading institution at the time around the country; it really stood out as compared to the others in integrating disciplines. They said “yes”.

I said I have one other thing. I want to go to Britain to find out how it was that all these refugee scientists that I have been dealing with here, as a upstart, young Turk leader of the atomic scientists, how they got the way they did? I had some theories already, given to me by a few of them, one of them by a lawyer who is also a political scientist, a social scientist whom they named the (*social science*) library here after — it’s a name very similar to Calvin’s and that’s why it is blocked. At any rate he had some theories that I had to test out. I made arrangements, through an economist friend of mine in Britain, that he would find me the right place to work as if I were a British scientist in a British laboratory.

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**VM:** I'm not quite sure what you are looking for. Could you clarify?

**RM:** There was a special kind of capacity for imagination that I could get in 20 or 30 different scientists who had these European origins which I had not found in any of the Americans up to that date, even Calvin, in that regard.

**VM:** Native-born Americans.

**RM:** Native-born Americans who were captured and held by American culture and these (*people in Britain*) had started out in another culture. So I wanted to start from the beginning and try to find out for myself what was the environment that might be responsible for this. He got me a post as an ordinary bench-type chemist at the Petrochemicals, Ltd. in Manchester, which was a subsidiary of what was called Petrocarbon, a somewhat larger (*company*), that was bringing modern petrochemical knowledge to Britain at the time. At the same time I made arrangements to be with the Manchester School of Political Economy and with the Botany Department in Manchester University and a few others like that. So I was running around trying to be a laboratory scientist four days a week and doing other things. In addition, I was trying to figure out: suppose I were to join this new programme on planning that they had in the Division of Social Sciences (*in Chicago*), what would I do and what would I say?

I decided that I would also like to practice some planning in Britain since they claimed to be doing it. Here was another kind of activity; that started in '49. In the meantime, I had contacted a young man (he was still a graduate student) by the name of (*William*) Oswald. I don't know whether or not you ever met him. He was growing algae over in the engineering station in Richmond.

**VM:** Richmond here? California?

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

**VM:** I don't think I know him.

**RM:** He was in the Sanitary Engineering Department, of all places; he was kind of an upstart in that sense. He was trying to apply real science to sanitary engineering. I could understand that whole area very well because in order to work my way through as a senior at the University of Illinois I was an assistant in the sanitary engineering laboratory. So I had to learn their language and everything else.

Anyway, he was taking chicken shit from Petaluma and doing things with it, like growing algae. Not only that but getting higher yields, too, so if you added what he could do with some kind of tincture of chicken manure and add that to Myers' idea of fairly rapid stirring, then you could begin to see quite high yields accumulating in algae, if only they didn't get sick, if only viruses or bacteria or something like that would not take over. So in '49, just before I left for Britain, I did publish a paper, the first paper, in *Chemical Engineering News*, which was a review of the field up to then



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and with some extrapolation into the future. It was something that I could at least take around to the laboratories in Britain and say “this is the only thing I’ve got in print so far but these are the things I am talking about”.

I had to go to the Seaweed Research Institute in Scotland and I did look around to see if there was any work that was going on in Britain itself. There was a little bit in Manchester but not enough to count. Oxford, Cambridge: nothing. So I didn’t learn very much about photosynthesis in Britain at that time, or about microbiology, really. But I did learn a great deal about just what it was in the milieu of Europe. Because this petrochemicals laboratory was 50% refugees from Western Europe. The Director, by the name of Steiner, was Viennese. He had a number of visitors coming in, so that Herman Mark, when he visited Britain, would stop in at Manchester to this laboratory. He was my idol in polymer chemistry. He could then tell a whole lot of fascinating stories about the bomb secrets that Szilard had before the war, etc., which he was in charge of for a while. So we had very active conversations.

The next stage after coming back...

**VM:** Excuse me. What was the answer about the European scientists, or are you coming to it?

**RM:** One was a kind of camaraderie based upon being a gentlemen, not quite the nobility, but there was a level below the nobility of professionals that would achieve this. There was a participation in the culture of the city which was an intellectual culture as much as anything else. So that the opera; for Franck (James Franck) to describe how he had gone to the opera in the evening almost three or four times a week after he had been to the laboratory all day is something that nobody in America would have done. But he felt it was necessary; it was part of life. It is this participation in the life of the intellectuals as much as anything else that had counted. But also the intriguing idea, high competitive intellectual activity between themselves, that is, to get the really advanced idea first. We had it in this country, too, but the idea of being the first man there was not as important as the one of being there “solider”, in America. As I had moved around America I could see that difference fairly strongly. A whole lot more is difficult. Later on I looked at the elites of Japan and China and India and other places. India had a little bit of that British (*attitude*), very little. China and Japan were so utterly different that I had to recast myself in order to understand it.

**VM:** Looking at the scene, 45 years or so later, have things changed in your view?

**RM:** Between America and Europe?

**VM:** Has America changed? Has Europe changed?

**RM:** I would say what America has done is it has brought in the finest minds of the rest of the world as postdocs., doctoral students, etc. and that the interaction is fantastic. If you look at the journals right now and look for the ideas in *Science*, or any of the other major scientific journals, you’ll find a high fraction of Asian names. Yet, when

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you go to Asia you can't find any productivity in those schools. America has not only attracted some of their best but it has given them (*the Asians*) the freedom and the interpersonal contacts, the equipment, the laboratories, the library and everything else and made it easy for them to shine earlier, I'd say. In Japan you have to wait until you are 50 and you have to retire at 55.

**VM:** But are you saying that changed American science now is reflecting the Asian component in the population?

**RM:** Also European. Previously it was heavily European. Now there are even more Asians than Europeans.

**VM:** But, the Americans are still not going to the opera, are they?

**RM:** The others are really Americans. That's the important thing. They think of themselves as American, they can't go home again. They are Americans. We are an open society. We can absorb these populations quicker than the Europeans. I had two room mates at UCLA who were Japanese and we had others that came from Europe, refugees. They were no different. We were already co-opting at that time and it's much more intensive now.

**VM:** What do you see nowadays, looking at the European scene?

**RM:** I haven't been trying to look at it for quite some time. I did look at Vienna about six or seven years ago and I forecast that Vienna should be coming back in vitality. It was a dead city and they are all growing old together, including the buildings, and I didn't see any hope in the mid-eighties. But when I got back in 1991, or so, I began to see that there really was hope; that the old families, the business families such as the Stoner family that I had married into, if those families remade the connections that they had had in Eastern Europe that there was a job to be done and it was likely to be done. The one thing that I could not foresee in 1991 is the Russian Mafia moving in, into the banks and other facilities. There is now an infection, you might say, that's come along with the dynamism that is slowly, it's not intense or anything, that has made a future for Vienna.

**VM:** Interesting what you say about the attraction of Europeans and Americans in the forties and fifties because Calvin's lab., I suspect at that time, was unusually full of European postdocs. and graduate students. Maybe that was one of the contributing factors.

**RM:** I am sure it was. Also, they were recommended to him by people whom he trusted.

**VM:** Yes. He makes a point of that in various statements that he has made.

**RM:** This is true in every institution which I talked to which tended to get outsiders. I had been one of the corn belt type, coming from Illinois. In fact I had managed to earn my way into college by picking tassels out of hybrid corn when it was just invented. Otherwise, I was far too poor to go to college. Being the corn belt type, I had done

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studies on why the bright boys chose chemistry. It was because it was so arcane that nobody in the family would understand and they could then proceed without any limits. For instance, my family had a number of school teachers in it before the depression and, therefore, they said school teaching is safe and you should be a school teacher. Of course, I would resist that. They were saying that you ought to be a farmer; farming is safe, etc. So we had a number of them that would be good in any subject. University of Illinois at that time was sort of the world capital of chemistry and as I interviewed the honour students senior level at the at the University of Illinois I could see that these were people who were going to establish themselves in the American fashion but they didn't have the verve or the imagination that I discovered among the Europeans that had come here.

**VM:** In the early days of the Calvin group, in the forties and fifties, when there was such a high European component, was this unusual? Was Calvin much of a pioneer in recruiting people of that sort, or was it, in your experience, happening everywhere in this country?

**RM:** At that particular time, after World War II, the University of California in general was sort of leading the pack. I visited maybe twenty universities between 1946 and 1949 in America and I was very inquisitive in more than one department. I could say that in that sense the University of California really had the capacity to accept and utilise the brains and fit them in various ways.

**VM:** That's an interesting connection. The fact that Calvin was both a professor at the University but also involved so heavily with the Radiation Lab., which, by its very nature anti-foreign because of the security considerations that they had, meant that Calvin's students and his postdocs. had to get security clearances of some sort from the Radiation Lab. to work in his building. It was, in fact, not that easy for students from other departments actually to gain access literally through the front door.

**RM:** I worried about that. I knew about the "Q" clearance problem and things of this sort, working for the Atomic Energy Commission. I thought Calvin had some extra money around that he could spend outside of the AEC money.

**VM:** I think that's true. But somebody or other was telling us that it was literally difficult to be a casual visitor in the building as you could into other departmental labs. because of the security consideration, because it was an AEC building. It may be the case that in Calvin's lab. the attitude of the University of California, in spite of the Radiation Lab. bureaucracy, plus the Radiation Lab. money produced this very large and very interesting and unusual group at that time.

**RM:** Well, it certainly produced the equipment. After that, Calvin had to be the selector of the personnel and now to get them through the bureaucracy was something else again, which he apparently took the effort to do. So that the idea of creating a kind of world team was something that I most admired. For instance, (*the University of*) Chicago had an awful lot of the refugees from Europe but they were quite elitist; they did accept some of the people from outside, very seldom Oriental, however — only Hungarians and people like that; Hungarians and Poles — so they were somewhat

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exclusive. They worked hard to make sure their people could get into the laboratories, but the laboratories were smaller at Chicago and the pace was much slower.

**VM:** Sorry; I've interrupted your main theme.

**RM:** That's all right; this is fun!

**SM:** As an aside, it may be of interest to note that, of course, Calvin spent some time as a postdoc. in Manchester.

**RM:** I didn't know that.

**VM:** Between '35 and '37 he was there working with Michael Polanyi.

**RM:** I worked with Michael Polanyi later on; I didn't know that.

**SM:** Obviously you both appreciated the same sources.

**VM:** Calvin himself was also, as you know, from the midwest, from Minnesota.

**RM:** From the corn belt.

**VM:** So, he was under the same influences.

**RM:** It's amazing. Michael Polanyi at the time I arrived in Manchester had just published a paper proving that planning would not work. Here I was, thinking of joining a planning department. I looked carefully at the paper and said "Hey, you know, he's using the same kinds of models that we use for polymers. By using his logic I can prove that rubber won't stretch!". When I pointed this out to Polanyi, he said, "Let's not talk about planning after this." We talked about everything else in the world but not planning. He had this feeling primarily because of his European experience. Thank you for telling me about the background of Calvin.

**SM:** It seemed oddly coincidental.

**RM:** He was certainly a kindred spirit, there's no doubt about that. I can recognise that. So now we are returning from England, many other things were happening in England at the same time. I did have a chance to practice planning, working with the Scottish Council which was an official group in Scotland, and tried to figure out how they could get high tech. industry into Scotland and a few other things. Nobody would believe me, but it came and it came in the places that I said it should be put. So Scotland did win out in the long run but it didn't come as a result of planning so much as taking advantage of opportunity that they'd heard about. I had spent half a dozen trips to Scotland working on Scotland.

Coming back, I then knew a little bit more about what I had to learn to teach (*at Chicago*). This particular department was taking on the developing world and they had a special little experiment going on in Puerto Rico where the chairman of the

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department was Professor (*Rex*) Tugwell who had been a New Dealer under Roosevelt, one of the main New Dealers. The person he picked as a right-hand man had gone down to Puerto Rico to think through a post-war plan for Puerto Rico. So here we had Puerto Rico as our “experiment” and we were using it for laboratories, as we called them.

(*Tape turned over*)

One of the first things I got into at Chicago was what would one do about taking a place that was as poor as Asia and pushing it ahead, using modern social science and that sort of thing, plus anything I knew about science and technology. So teaching was that way until '52 when the budget cuts at the University of Chicago had forced somebody to get off the payroll. So, they decided that for six months I would get off the payroll and go to Puerto Rico and get on the Planning Board's payroll in Puerto Rico.

I was shipped down the river, so to speak, as they call it in America. The result of that is that I'd gotten well started. The Puerto Ricans taught me all the things that were not in the textbooks about development so that I had to go back and write a book on how development, based upon the things that they had invented, how development might proceed in the Third World. I had looked over very carefully their own technology and biotechnology, and they (*the Puerto Ricans*) had done advanced work in yeast but they had not gone into food yeast for reasons that were already well understood. They also had the very first automatic factory in canned orange juice. It's very surprising that an automatic factory would be down in that backward territory. Just at that time there was an article in *Science* indicating that possibly the world population problem might be solved by a special product in the citrus rind that could be extracted, a very simple phosphorylation could be undertaken, and it could be “the pill”.

**VM:** The “food pill”?

**RM:** No, in this case, the pill for population (*control*), anti-fertility pill.

One thing I had left out: In 1951, Szilard and I and Gaffron and some of the postdocs. in the institutes at Chicago had had an evening discussion group. It's now evident that the scientists are relatively free to choose their own direction and responsible scientists like “us” would try to choose a direction that might make a difference, recognising that it'd take thirty years for research to have an impact. We decided to look at all of the things that were going to be scarce in the future — food was obviously one, another one was water, still another was minerals, another was energy. We could then ask ourselves what could scientists say about these things that might restrict further social and economic development? When we did population, Szilard had discovered that the Rockefeller Foundation had had a study going on about the feasibility of an oral contraceptive. All the scientists at that time had thought that oral contraceptives should be the solution — it might be a liquid, it might be a solid. So the Rockefeller Foundation was investigating it. It had been coming to the conclusion, with all the doctors and sociologists, that it was impossible. That got

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Szilard mad and so he looked around for five top-ranked chemists, they were all easterners, and he discussed the problem with them. Within a matter of six weeks or so he found five different potentially feasible chemical paths to an oral contraceptive. He showed it to the Rockefeller Foundation committee so they didn't publish the report. Then, a year or so later, about '52, one of the persons he'd sought at Worcester Polytech. had gone a little further and he'd discovered that the tools were already there, and he did a few experiments, so they were getting ready to do field tests on an oral contraceptive and they chose Puerto Rico for these field tests.

Several things were coming together on the biological side. On the food side, we were involved in this but, of course, the Korean war was on. Then a notice came to the University through the Office of Naval Research that if ever we could have a Manhattan District in protein that would really be a great thing because at that time MacArthur was being pushed back to the end of the peninsula and we might have to do a Dunkirk. The Navy was thinking through how it would defend Japan against submarines from the Soviet Union when all the protein in Japan — not all it but a very large share of it — was coming from fish protein in their waters and they couldn't protect the fishing vessels. So here you'd have an American army, which would have taken leave of Korea, in a starving country. What could be done? They needed a Manhattan District in food.

After having that set of discussions we said that in food the highest priority would be trying to find a way of getting mass production of *Chlorella* probably. The Navy said "see what you can get up; we would like to have it as soon as possible"; the Office of Naval Research. We had to immediately contact the top man: the only man who could understand us in Japan was Hiroshi Tamiya. Did you ever meet him?

**VM:** No, I don't think so.

**RM:** He had a biological institute. When we contacted him, I was the person that contacted him, he said, "I am sorry we just can't collaborate in any way whatsoever. The food problem is so severe in Japan at the moment that all my postdocs. and graduate students are out in the yard cultivating vegetables in order to keep us alive. We do have equipment but we can't use it: we have to use our efforts to stay alive."

**VM:** Can I ask a question: at this stage, during the Korean war, therefore early fifties.

**RM:** It was 1951 going into '52.

**VM:** What was your contact with the photosynthesis research activities around the country at that time?

**RM:** Every summer I'd be coming to Berkeley, as well as some other places, but at least Berkeley, from Chicago (this was where I was located) because my mother-in-law lived here and had a house on Warring Street and we could leave the kids with her and my wife and I could go and to the High Sierras for a major holiday. While I was here (*in Berkeley*), I would finish writing a paper or something. At the same time I would drop into see Calvin's lab. to see what was going on now, and see any other

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labs., including the engineering lab. Also, I'd go over to Stanford and the Food Research Institute. They had two sides: one side you may have visited but the other side was in economics. Fortunately, I could talk economics by that time so I would then talk economics of food scarcity with the Food Research Institute at Stanford. The connection was primarily that I was free during the summertime.

**VM:** Therefore, you were aware of the advances being made in Calvin's lab., at least in a general sort of way.

**RM:** More than general. I was mapping out now what were the specific enzymes and what were the amino acid constituents and what were the mechanisms that were involved and what were, more important for any kind of chemical technology, what were the rate-determining steps in reproduction and that sort of thing?

**VM:** Your view and your interests of Calvin's work was how you could use this information to increase food production.

**RM:** Absolutely. That's the reason he let me in.

**VM:** Presumably you were also in contact with Gaffron in Chicago.

**RM:** Yes, but he didn't have that sympathy. I could tell him these things, and gave some seminars in his institute, but it was something, it was applied research, it wasn't "pure", which is another feature of the European attitude.

**VM:** He wanted to keep his operation "clean", did he?

**RM:** That's right: pure. Calvin didn't care. Calvin came apparently from the midwest, so he was interested. He thought it was a socially responsible thing to do, if ever one could do it, to transfer the know-how to somebody who could use it.

**VM:** So Gaffron was a much purer scientist, as it were?

**RM:** Much more classical in the European sense.

**VM:** Calvin and Gaffron presumably knew one another and had contact at some point.

**RM:** I was around virtually at the time that the break had occurred between them. You must have been around also, I guess.

**VM:** No, it was before my time. I never knew Gaffron.

**RM:** Apparently what had had happened is that (*William*) Lawrence of the *New York Times*, the science editor, had come in, was introduced by Berkeley's Lawrence (*Ernest O. Lawrence*) of the Radiation Lab. who just left him with Calvin. Calvin didn't have anything else to say but what he was doing. The science editor could understand a good share of this. So wrote it up, in quite long stories, in the *Sunday New York Times*. That caused the whole contingent at Chicago to blow up. "Calvin is

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a publicist and he wants to cop the credit for himself”; this is the sense of priority in the European. Calvin didn’t even know what...it came as a surprise to him. For several years, no matter what I said when I was talking to the Chicago people, it’s just incredible that anybody would behave that way, I was the only person that knew what was going on in both laboratories. I would convey what Calvin was doing before it was published to Gaffron and the team that he had and I would convey Gaffron’s material to Calvin.

**VM:** Gaffron himself was Austrian was he, or German? I don’t remember.

**RM:** He had worked in Germany and I don’t think he was Austrian. He could have been something else. He could have been right on the French border.

**VM:** He was European, anyway.

**RM:** He was definitely European.

**VM:** Were the bulk of the people in his lab. also Europeans?

**RM:** Half something like that; they were fewer in number.

**VM:** But there was an American influence in his lab. as well?

**RM:** Oh yes; there was definitely some American influence.

**VM:** Would they not have recognised Calvin’s legitimate interest in the practical consequences of what he was doing?

**RM:** No, that had nothing to do with it. What really happened is that they felt that he (*Calvin*) had gone to the press and made these exaggerated claims.

**VM:** The antipathy was really all on Gaffron’s part and not on Calvin’s at all.

**RM:** That’s right. I knew it all the time and I acted as a kind of an emissary, being in Chicago and coming usually more than once to Berkeley each year.

**VM:** As someone who, as you said, knew what was going on, perhaps the only one who knew what was going on in both labs., what sort of progress was Gaffron making compared with what Calvin was doing?

**RM:** About one-third, one-quarter the rate.

**VM:** Along similar lines?

**VM:** They were looking at enzymes, and that sort of thing, looking at mechanisms.

**VM:** So Calvin had the livelier and larger organisation.



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**RM:** Absolutely. He had a larger group, he had the better instrumentation, although I must admit that some of the things that came out later on that became extremely important, such as the acrylate type gels and that sort of thing for separating out enzymes, I first heard about it in Gaffron's lab. But a year later I saw that Calvin was having great big print-outs of it so, in other words, Calvin ran faster.

**VM:** I think that one of the things that Calvin must have learned as a result of the introduction of paper chromatography to his lab...

**RM:** In fact, paper chromatography came first and then came the acrylate.

**VM:** I think that paper chromatography must have taught Calvin the benefits of being alert to what's going on in the latest technical developments. Because he was always, at the time when I knew him later than that, he was always very receptive to (*new technology*).

**RM:** Yes, ever since I knew him. He was really preaching that to the people around him.

**VM:** So once something happened, then he had the ability to capitalise on it. He would, as you say, invest in large measure in what he thought was good for the work he was undertaking.

**RM:** That's right.

**VM:** And Gaffron was more cautious, was he, in that sort of regard, or perhaps he didn't have...

**RM:** He was more sceptical.

**VM:** Maybe he didn't have the resources.

**RM:** Even when he had some of the key things (that turned out to be key later on) he didn't push them the way Calvin did.

**VM:** There's no point in trying to make a comparison between the two men, but you have told us interesting things about the two of them.

**RM:** I could also compare with Jack Myers. Jack Myers is a middle western type too, no European contacts, and he was a pragmatist in the sense of, well if something didn't work, try another way. He was the one finding out the very significant effect of stirring and quite a few contributions to what might become commercial later on came out of his way of cultivating algae. Again, it was a matter of style.

**VM:** But Jack Myers was never the breakthrough scientist that Calvin was. He never had the breadth of knowledge or experience or vision, I think.

**RM:** He didn't have that European contact. I would say he could have been a Calvin if he had had the European contact and exposure.

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**VM:** That's an interesting comment.

**RM:** I was really impressed with what he was able to do with what he had.

**VM:** I'm sorry, I keep diverting you.

**RM:** This is fun. I'm interested to see what it is that you connect with.

**VM:** As you know, we are interested primarily in the history of the Calvin group and therefore I'm trying to bring into contact and make it explicit because the record that this is, we have to make it clear for those who may listen.

**RM:** You're absolutely right. I'm doing my best. I have to introduce context.

**VM:** Right. So where were we?

**RM:** We were back around 1952, end of the '52, the Navy says "no"; it isn't going to be in time. We have turned the Koreans around, the North Koreans, so we don't have to worry. Actually '53 they suddenly hit the Chinese, but that was something else again. Certainly no Manhattan District based upon protein was going to help at that time. In effect, there was no real demand. But by that time, some food — in fact, one of the things that had happened, we had passed the hat around among the scientists and we got maybe \$500 and we sent it off to the starving Japanese scientists. Because we had done this, the Rockefeller Foundation was shamed and sent them \$5,000. So, they could eat again and they could go back to work! They then set up what we thought was the highest priority and that was ponds, open ponds, and cultivate the algae. As soon as the algae were growing rapidly, they outgrew everything else around and everything else starved, even the bacteria starved. It was easy and here people in the laboratory had been spending all this effort trying to grow algae rapidly and keeping them sterile and everything else.

I had decided, when I was at Chicago in 1951, maybe I might go back to a laboratory and do what seemed to be the obvious thing and set up an ecosystem. open to the atmosphere, and try to find a way where algae would become dominant and, therefore, it would not be any kind of secure, sterilised kind of process. This was submitted to Washington — at that time the National Science Foundation had not yet been founded — but whatever source it was sent to. And the referees sneered at what I had proposed: "Everybody knows that you have to keep these things sterile; therefore, this is an incompetent kind of proposal".

**VM:** Well, I think there were really two attitudes. You were interested in the food potential and you didn't mind if there were bacteria...

**RM:** I told them I was.

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**VM:** That's right...but people like the Calvin, and no doubt the Gaffron, group who were using them as experimental material really had to keep them clean so they dealt with one system at a time.

**RM:** What I am trying to say is that their results would have been obtained even if they hadn't been clean.

**VM:** There would have been more argument.

**RM:** They would have been doubted.

**VM:** Was this food situation in Japan at the time public knowledge?

**RM:** Public in the US?

**VM:** Yes.

**RM:** Not very much information about it. It was known to MacArthur and other people in Japan. Things were very tight.

**VM:** I was wondering whether the photosynthesis research people (Calvin, Gaffron and the others), knew then of the possible significance of their work in terms of food production, whether this was and issue...

**RM:** They did know it. In fact, at Stanford in either '52 or '53 (more likely '52), the Stanford people brought in Tamiya, his wife and one research assistant and they brought all the people who were growing algae in America in laboratories together at Stanford, and they exchanged information with each other about techniques that they had learned. They were thinking very much about algae as food. Then Mrs. Tamiya would take the product out of the laboratory and each time she would make a new dish out of the laboratory product and serve it at the tea seminar. I got in only for one of the seminars, it was held during the summertime at Stanford, but I got the reports now. What was she doing? I knew there were problems in the consuming of algae. They said, "Well, her cakes were pretty good, she had a bread that was pretty good, she had a flavouring for rice that wasn't bad, but that algae stew, or so", one algalogist a specialist in Pacific algae) made a terrible face. He said "that was the most awful stuff". He said they had a Japanese name for it, and he gave it to me. "Oh, you mean she could do *that* with algae?" That would have made algae acceptable in Japan. This is the extent to which, I think one of the postdocs from Calvin's lab., was in that same group, (*algae culture evolved*). We had this feeling that "Gee, it looks now as if it ought to work for places like Japan", except that you had to find a market. You now had to think business-wise.

I didn't ever get a chance to see Tamiya again, although I corresponded with him several times, until I got to Japan myself in '66 and went over to visit him. He said, "You know what happened as a result of that? My students went into commerce." That was despicable because he had the European tradition. He said "And not only that: they are competing with each other which is un-Japanese". I said "What are they

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making?” He said the most successful thing so far is *Chlorella* yoghurt because they have heard from the western world that yoghurt is very good for people who have upset stomachs or want to keep slim, and green is a lucky colour and the two fit together very well. So, they made yoghurt and that was the first commercial product. That must have been sometime around 1960 or so that it came on the market.

**SM:** Was it actually green?

**RM:** Yes, it was actually green. It starts green and unless they do something to the chlorophyll, why it will stay green. So here was a product which he was basically responsible for in teaching the students but the students didn’t have any jobs and they had to sell their careers to entrepreneurs and they had to invent things at that period in order to stay alive.

On the same trip, about 1966 it was, I was in Taiwan and somebody was knocking on my door early in the morning and it was an old friend from the sanitary engineering side who had been visiting. He said “Dick, you’ve got to see this. You know, there is the biggest damn *Chlorella* culture in the world here and it’s ten hectares and ten stories high”. I couldn’t believe him, of course, and they’re doing something to the algae that I can’t make out because the translation problem from Chinese to English was so bad: “You’ve got to find out. How long are you going to be here?” And I said “three more days”. I didn’t even bother to see it. I got a story as to what it was. The ten-stories high was only a drying unit; algae contain a lot of water and they will spoil if you don’t dry them very quickly, so they were drying it. The ten hectares was correct because the Japanese in order to make their *Chlorella* yoghurt had decided that the weather was better in Taiwan than in Japan and that they would be safe to have ponds in both places. They got the central committee of Taiwan to actually invest in this and, I couldn’t figure out how, but they got the highest level support. I then had to do detective work. It turned out that the person I could meet was the planner, the chief planner in Taiwan, and I finally got him to talk. He said, “Look, I’ll tell you what went on. It’s so fantastic, nobody will believe you. Therefore, no matter where you tell it, you know, it’s incredible”.

What was happening is that the Taiwanese government claimed to be the legal government of China and, therefore, it had to maintain in its cabinet, departments for each province of China. There was a bureaucrat there, a mandarin, who was collecting all the information possible and this information, because China was communist, had to be used by the US government which was using some of the territory of Taiwan for its troops. He said what happened was that we weren’t getting enough information, because China was a closed book. We had people in Taiwan from every province in China who spoke the dialects and everything else and knew their way around, but we could never get them there. And if we could get them there, we couldn’t get them back. So, then the CIA and the Taiwanese government got together and said we will provide you with aircraft for drops in empty spaces, we will give the parachute-dropped people about five days of provisions based upon American K-rations, and then they have to be on their own and they have to move through China back to the coast and we give them special radio signal to send and we will pick them up by submarine off the coast of China. This way the Taiwanese could

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fill in data about the respective provinces and their duty was fulfilled and the Americans would get their evidence.

One of the things that happened there is that the people managing to get back (more than half of them did get back) was that they felt sick. The more they looked at it they detected it was the milk powder in the K-ration because many Chinese don't have the enzyme that allows them as adults to digest it. They said "What if we put in algae?" They got some of the Japanese algae and it was great. Immediately there was a military use for algae for the survival of these paratroopers. Then, the Japanese said "Look, you've got to find your own markets for this because whatever we do with this kind of thing we want to reduce the risk and broaden the market". The Chinese looked around for what kind of markets they could discover and they found that in the Chinese pharmacopoeia all the secret dried mixtures that are sold by peddlers and through the grandmothers of households and the grandmothers then decide which one fits what ails you, that some of them required iron, others required minerals and others protein. Here, was *Chlorella* that could be used for all three. So they did the extracts of algae and provided them with different-looking materials for their respective ones (*requirements*) and they looked similar to what the regular pharmacopoeia would indicate. They then used this material for the Chinese pharmacopoeia. The man, the planner, is now the president of Taiwan! He managed to survive all the politicking and everything else. He was a very competent, very serious guy.

We then took people from the sanitary engineering lab. who were going to bring in microbiology and waste handling and a few things like that to the new Asian Institute of Technology in Bangkok. They were getting a new location, out on the edge of the city, so we very carefully instructed everything that might be known from Calvin's work and Myers' work and the Stanford work and everything else to this man who had a good background in microbiology. He then took it to this area outside of Bangkok and he set up ponds, and found very economical methods — he found low land area that he could defend against the water that would wash in off the hills, with plastic sheet bottom for the ponds, and they would have sides, they call them "buns" there, maybe three feet high, and they would scrape the bottom for stirring — that had been learned again here in Berkeley in sanitary engineering — and they could then produce a lot of algae. The head of the algae group (*in Berkeley*), Oswald, got a cable that "we're having trouble with our algae. Our chickens are getting spattled-legged (they were feeding it to the chickens) and we don't know whether it's our algae or whether it's our chickens. So, send us five kilograms of Berkeley algae as quickly as possible." Which took a little time to prepare.

**VM:** How did you get five kilograms of Berkeley algae?

**RM:** Because they were growing it out of chicken shit in the experiment station.

**VM:** Oh, I see. This wasn't Calvin's activity?

**RM:** No, no. This is more about what happened as a consequence. So he (*the man in Bangkok*) then received this algae, compared the Bangkok algae with the others, and

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the answer that Berkeley algae was great and Bangkok algae was bad. Now, they were grown by the same formula, the same timing, continuous flow process and everything else. But, they had forgotten that in Richmond that they were in the blast of the cold air that came through the Golden Gate. You had real tropical background so that the problem that turned up then was that the algae that they grew in Bangkok were already old, given the conditions. As soon as they grew them faster and got a higher yield, then they were the same as the Berkeley algae. So we began to understand more about the physiology of ageing. It also confirmed this Columbia scientist who ate a couple of test tubes of stuff that he had been consuming old algae.

**VM:** Can I ask you at this point, since your story is getting on into the middle of the fifties, I guess, some such period...

**RM:** This part is already in the sixties, a kind of sequel to it.

**VM:** Calvin's work on the path of carbon in photosynthesis was well over by that time.

**RM:** It was well over. We were continuing on the momentum — the Japanese plus American work.

**VM:** What would you evaluate the significance of Calvin's work, or Gaffron's too come to that, for the sort of thing that you are now talking about?

**RM:** It was absolutely essential.

**VM:** Can you explain how?

**RM:** Let me give one example only and that is that he had identified one of the key enzymes that would be necessary, and then somebody else a year later or so determined the fact that it was maybe 20-25% or so of the total and it was beautiful in terms of amino acid distribution for something like fast foods. We could imagine right away that the Mexican foods brought into the taquerias in California might very well use an extract of algae the way it was being done in Taiwan. The rest of it, the chlorophyll itself, was not very digestible, etc., so it was a way of separating out the major component of the algae by rather quick...They had already identified the methods of purifying or separating out the key component.

**VM:** Did their work enable you to get better yields of algae? As I recall by that stage, nobody had really gotten very far in getting photosynthesis to work any faster or anything of that sort.

**RM:** Jack Myers in Texas did some. The addition of the chicken manure added 30% to the output.

**VM:** Sure, in terms of culturing techniques, but not in terms of implying that the fundamental biochemical understanding to knowing how to organise the system...

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**RM:** What happens as soon as you go into technology you're confident that you can begin to control things because of the background. You know which enzymes work, you know the temperature effects, you know what's the limiting reaction. Because of all this background you then don't make mistakes in technology, and mistakes are expensive.

**VM:** Can I stop this tape here?