Chapter 37

E. MALCOLM THAIN

London

November 15th, 1996

VM = Vivian Moses; MT = Malcolm Thain; SM = Sheila Moses

VM: This is November the 15th, 1996 and we are talking to Malcolm Thain at Queen Mary College in London.

Malcolm, let's start with that card you just showed us, a card that they gave you when you left.

MT: That's right.

VM: What sort of memories does this bring back to you?

MT: You know, thinking the last week or two, my general impression of the laboratory, it was an extraordinary energetic and happy place. I think people liked working there. The trips out to the Sierras, the camping, the weekend activities — so that everybody was living close by and so it had a splendid social structure to it. Having said that, I think that Rod Quayle and I, as we got to know people better, realised that there were undercurrents which we, as British people, didn't fully understand. For instance, quite a number, we understand, of people actually went to psychiatrists because of tension at work.

VM: Did they?

MT: Yes. I don't know if I could identify (*their names*) on the card. You'd have to remind Rod of this. We found one of our colleagues in the lab. sating "haven't you got somebody to advise you? Haven't you got a counsellor?" Why, we were absolutely astounded. No, we hadn't anybody to give us advice on this.

VM: You came from a "stiff upper lip" country.

MT: That's right. But, you see, a different culture. That was just a very minor sort of undercurrent which was rather strange to us. But generally, a very lively, happy place to work. And, of course, the social life was so integrated with the laboratory life.

VM: Was your wife there with you?

MT: Yes, yes.

VM: So, you were married before you...

MT: ...before we went out, yes.

VM: Of the people who have signed this card: I can recognise Marguerite and Dick...the Lemmons..

MT: The Lemmons, yes. "Cresswell" — that one is Kazuo Shibata; I gave him that nickname Creswell; that was just a personal thing. (*Indecipherable*), of course, is dead. Now I can't read this, who this is, Rick: clearly he must have been on the mathematical side with this sign, (*indecipherable*) sign. Al Bassham, of course; Pat...

VM: Pat Smith.

MT: Pat Smith, yes

VM: A red-headed lady who ran the algae culture. This Power is, presumably, Power Sogo.

MT: I think so, but again, I've only got a hazy memory.

VM: He's a Japanese-American who was an NMR expert.

MT: I think he must have come just as I was leaving.

VM: I don't think anyone else by the name of "Power" has ever been there. Martha would be Martha Kirk.

MT: Yes, indeed. And Norma, of course...

VM: Norma Werdelin. Rod (*Quayle*), I gather, is in touch with her.

MT: And Altha who, of course, was the mainstay for the sink, washing up. Rosemarie...

VM: Rosemarie Ostwald: you know she died?

MT: She died. And Hans (*her husband*), of course...

VM: He died too.

MT: Paul, Paul Hayes.

VM: He died, I'm afraid. A lot of people have died.

MT: This is Hans Grisebach (*we talk about?*): "God save your hair" (*he had written*). Now I don't quite know why...It must have been looking as if it was fading away more quickly than I thought.

VM: Looking at you now 40 years later, nothing much has happened to it.

MT: It has got much thinner; I notice that in gardening. I can't read this script at all, can you read it?

SM: Yes, I can. Would you like me to?

MT: Yes.

SM: ...he is wishing you all the best and strength in your work.

VM: Has he signed it?

SM: It's the same person who wrote this: "God save the King (*indecipherable*)".

MT: I don't know who that is. Who would have been writing in Hebrew?

VM: Was there an Israeli there at the time? We'll have to look up the records and see what we can find.

MT: This one also I can't read.

SM: Well, it's someone whose initial is W.

MT: Iso or...

SM: Something...it's hard isn't it?

MT: This was given to me in a book. When we left, they presented me with a book of Yosemite and the Sierra because they knew how much we had enjoyed, you know, camping and walking up there.

VM: Did you have a going away party?

MT: Yes, indeed. That was held at a park near by as a barbecue.

VM: Ah, yes, in the style of the times.

MT: In the style of the laboratory, in the style of the times, yes, yes.

VM: Can we back-track and ask you how did it come about that you went there, to Calvin's lab.?

MT: I think, in fact, I was working in the early '50s in the Lister Institute of Preventive Medicine with Jim Baddiley. And we worked on the structure of coenzyme A.

VM: In that case, I need to back-track even more. What was your training background?

MT: I did my PhD under Jesse Kenyon on organic chemistry, the mechanism of carbonium ion reactions. When I finished that, which was mechanistic chemistry, when I'd finished that, I said to Kenyon that I would rather like to have a hand, or try a hand at natural products chemistry. He knew Walter Morgan who was head of the Biochemistry Department at the Lister Institute and so I joined a small group, in fact joined Jim Baddiley when he joined the Institute. We had several stabs at things and one of them was the structure of coenzyme A in 1949; that's the sort of main line.

VM: That would have been early '50s?

MT: That would have been in '49. I worked there for several years on, amongst other things, an ICI Fellowship and then Grant Buchanan joined who had known Jim, of course, in Cambridge. Grant Buchanan told me about Berkeley and when I wanted a change of laboratory, and Jim was going off to Newcastle, I thought that I would apply to go to Calvin.

VM: Of course, Grant had already been there...

MT: He'd been there and come back. And so it was through Grant Buchanan that I knew about Calvin's laboratory. I got a Royal Society Natural Sciences Foundation fellowship for a year.

VM: Did you make any arrangement with Calvin as to what you would do when you got there?

MT: No, I just turned up with a suitcase and umbrella much to the amusement of the reception committee.

VM: Did you often use your umbrella?

MT: That was Grant again. He said that an umbrella was actually a very useful thing to have in Berkeley. I think the advice, which was individual, certainly created amusement!

VM: You went with your wife?

MT: Yes.

VM: How did you travel? This would have been early '50s was it?

MT: It was a National Sciences Foundation...anyway, part of the finance came from the US and they arranged the travel. We went over on the *Ile de France* and that was a splendid experience. Then we went overland by train from New York to Chicago to Berkeley. When we got off the train we were met by, I think, Ning Pon and Al Bassham.

VM: Really; on that station?

MT: At the station, yes, and Rod Quayle. We stayed in fact, in an apartment near Rod Quayle for a year or two...well, for month or two and then we both had an apartment on the hill, opposite the architectural building, the School of Architecture...what's the name of that hill going up the side of the campus?

VM: Well, there's a road called Hearst on one side and Bancroft on the other side.

MT: I think it was Hearst. We both lived in an apartment house on Hearst.

VM: That was very convenient.

MT: So we just walked through. The amusing thing was, it's a very steep hill, and so all the lorries used to change gear and you were never quite certain whether it was a lorry changing gear or a minor earthquake.

VM: Just outside your bedroom window?

MT: Just outside our bedroom window. There were several occasions when there were minor earthquakes as, of course, the campus sits on a fault.

VM: Can you remember day one, when you first went into the lab. and when you first met Melvin, presumably?

MT: That would have been the first occasion of meeting Melvin. The actual going in, I don't actually remember: no specific memories of the first day.

VM: Or when you first met him?

MT: It must have been very early on because the first person I think we met was Al Bassham because he saw us at the station. And Melvin shortly after. I have no specific memory of that very first meeting. Later on, of course, one formed impressions of how I he would come through in the morning, full of energy and enthusiasm, to see what was going on, and how later in the day he had spent his energy so he would come through a bit deflated, you know, needing revitalising by some new, interesting results.

VM: How did you decide what you were going to work on?

MT: I was only there for a year. I had a leave of absence from my fellowship here in London and so I really fitted into the laboratory. I didn't go... I had read, of course,

the papers about the work they were doing and I wanted to do something on C^{14} tracing work...

VM: To learn the techniques.

MT: To learn the techniques. That was the idea of going over there. I was happy to fit into the existing programme.

VM: And what did you actually do? Which bit was yours?

MT: I was looking, actually, I think it was to see if there were any complications in the phosphoglyceric acid take-up of C^{14}, the usual techniques of using plates and chromatography. Of course, Bob Rabin found the addition of CO_2 to the ribulose. There was no hint of that when I was there.

VM: So the mechanism at the time when you were there, the carboxylation mechanism, was still obscure.

MT: Yes.

VM: Did it break out, did it develop while you were there?

MT: No, I'd say after. In fact, one of the things I was doing, towards the end, was incorporating deuterium into compounds to get the NMR spectrum.

VM: And who were the people who were working on the cycle dynamics at the time when you were there: names that come to mind are Peter Massini but I think he must have been earlier than you.

MT: No, that name is not familiar to me.

VM: Alex Wilson?

MT: Alex Wilson; he had left just before I arrived. I saw his papers, and there's that famous paper of his with the little man with the fishing rod; that was just before I arrived.

VM: We have talked to both the author and the artist.

MT: So the people — Andy was working there for a time, but he then departed half-way through the year to go to the east coast. He departed with his family and his whole tribe of bonsai trees. He was a very keen horticulturist and so I think he sent his family on by train and he went with the car and trailer with these trees which he watered at intervals as he went along.

VM: So you were there when Andy actually left?

MT: Yes.

VM: Was it anticipated, was it sudden, what was the mood in the place like at the time when Andy was leaving?

MT: I think that...Of course, I wasn't privy to the inner circle discussions, but it did come somewhat of a surprise to me and I think it did create a bit of a vacuum to start with. He was missed. Of course, there was Al Bassham there who was sort of continuing...and Bert Tolbert, too.

VM: Did Andy leave shortly after saying he was gong to leave, or was there a long period or a short — do you remember?

MT: I don't think I ever knew.

VM: But from the time that *you* knew, he was virtually going, was he?

MT: I would say — we got there in November and I would guess he left in the spring, I think it was all accommodated fairly quickly.

VM: And so, as you say, there was a vacuum. Take a long time to fill it, did you think?

MT: The dynamics, I think, of the laboratory were very dependent on the great range of scientists coming into it. I think this is one of the characteristics of it. When I was there, there were people from France (Jean Bourdon), from Norway — I forget the gentleman's name...

VM: I forget it: Arnold...(*Editor: Nordal*).

MT: Certainly from Japan (Kazuo Shibata), myself and Rod (Quayle) from England, Switzerland, a Swiss fellow: so that all these people coming and going, spending a year or two and I think that the contributions these diverse backgrounds made to the programme were a bit incalculable in advance. But I think Calvin so of used, or responded to these people and their expertise.

VM: Calvin and Benson were certainly very close to one another and they had set the thing up together and they were the driving force. When Andy had left, was Calvin's interaction with everybody else any different from what it had been?

MT: No, I wouldn't say so. I think he probably relied on Al Bassham for continuity. My recollection is that Calvin would come through the laboratory and speak to everybody, so that it wasn't, you know, as if he had a mouthpiece in the laboratory. He would come to everybody and walk through talk and hear what you had been doing, making suggestions and, of course, there were the seminars, too, which were held in the Chemistry Department, with Rapoport and...

VM: At eight o'clock on Friday mornings.

MT: That's right; that's it. You remind me now.

VM: I notice you smiling. You have the usual English attitude.

MT: That's right; I'm well adapted. Calvin's interaction with all the group, I'd say...

VM: I know for a fact, and was later part of it, that there was, of course, a management in the group, that the post-docs. were not part of the in-house technical management administration. But I didn't get the sense, all the time, that there was an inner coterie of scientific decision-making. I felt, really as you said just now, that it was an open decision-making thing and it was done all the time with everybody.

MT: Yes, I would agree with that. I certainly didn't get an impression of a group of two or three people with Calvin in it formulating policy and ideas. I would say that's absolutely contrary to me understanding. It was very open. In fact, I thought the graduate students we had, for instance, Ning Pon was a graduate student at that stage, I wasn't aware that they weren't treated as equals. They were in the same laboratory and they might be just the other side of the bench.

VM: You worked, of course, in ORL, on the main floor in one of the big labs. People have offered the proposition that the building itself was a factor, the structure and layout of the building was a factor in the way the group developed.

MT: Yes, I think it probably is. It was compact and the place where you had coffee (of course, we were allowed coffee in the laboratory in those days; unthinkable now) — Altha used to make coffee in the corner there and try to imitate our English accents and we used to imitate hers — the laboratory benches were all clustered around that centre so that there was a community and you interacted with people everywhere in the laboratory.

VM: Have you ever worked in as open a lab. as that, anywhere else?

MT: Certainly not in University College. When I was in the Tropical Products Institute we had open laboratories there as a matter of policy so that people did interact. Also, I think we felt that if you put a person in as laboratory by themselves, the isolation is really not productive to really the best work. We definitely went for open laboratories of that style. I can't say we chose them because I worked at Berkeley; no, definitely I didn't choose them because I worked at Berkeley. I think they chosen by a fellow called Stuart Stainsley who had worked at Mill Hill. He was ahead of me in the Tropical Products Institute and he was the director.

VM: When you went to the Tropical Products Institute you already found these open labs.

MT: Yes.

VM: ...and they seemed natural to you.

MT: yes.

VM: As you say, everybody mucked in and there was no sense of hierarchy in these labs. at all. The building seems quite archaic by modern standards but it did have lots of advantages.

MT: One of the things that I do remember — of course, you had to go down to the cellar to do the counting, the Geiger counters were down in the cellar. At one stage I was doing an enormous amount of up and downing, taking out plates, counting, changing them. At the end of the day I was quite tired from doing all this. So I calculated out how much energy I'd, just raising myself up from that cellar to the laboratory floor level *n* times. It came out to be a 2 oz. bar of chocolate at which I felt thoroughly abashed!

VM: That's all?

MT: That's all!

VM: That early style of counting, with the manual placing, was a bit tedious.

MT: And, of course, the fragility of the mica plates. It was quite an art, making the...splitting the mica plates to make them thin enough.

VM: You were of an era when the mica was split. By the time I got there which was...

MT: You were using mylar; mylar was coming in. Mylar was coming in, but we still used the mica plates when I was there.

VM: Which year did you arrive?

MT: It must have been the autumn of 1954.

VM: OK — and you stayed for essentially a year?

MT: Yes.

VM: In the interim, between the time you leaving and a year later when we arrived, mica had gone and mylar was in. That particular problem we didn't have. It must have made an enormous amount of difference. So you really had a lot of trouble with those mica end-window plates?

MT: I wouldn't say a lot of trouble. One recognised their fragility but they were no more difficult than other things. I suppose the advantage of the mylar was its availability. You see, mica of that size...these counters, you will remember were about 7 cm. across.

VM: Some still exist.

MT: Well, you see, mica of that size and regularity is really a rarity, I should think.

VM: Did you split your own mica?

MT: No, I don't think I did. I suppose somebody like Bert Tolbert did...

VM: Oh; it was done in the lab.?

MT: I get the impression it was done in the lab. I can't be certain of that but I got the impression that it was done in the lab.

VM: So every now and again you would be using one of these end-window counters and you would realise the mica had got a hole in it or whatever.

MT: That tragedy never happened to me but it happened to some.

VM: So you don't know what the mechanism would then have been for dealing with that?

MT: For dealing with that? No. Bert Tolbert might know. Al Bassham would know, too.

VM: It is just that nobody has ever mentioned that (*Editor: but see the interview with Peter Yankwich*) and it never occurred to me to ask. I'll get back to them about this. You think the mica must have come in reasonable size sheets, which they had to split?

MT: Yes. They split it up so that it was fine enough to give, you know, good counts.

VM: And they had to glue it into place, I expect.

MT: And, of course, then the gas was led through the chamber.

VM: When you got there in '54, the chromatography was already well in hand; the whole technology was...

MT: That was well developed.

VM: And there was that very smelly room upstairs under the eaves.

MT: Yes, the butanol and acetic acid; that's right.

VM: Got your dose of liver poisoning.

MT: Fortunately, butanol isn't so bad.

VM: The bulk of your work, then, was devoted to the PGA problem. I don't remember your papers, I have to confess.

MT: In fact, I don't think it...the point that Calvin was looking for was an unusual distribution of C^{14} in the PGA and it wasn't there. So I don't think it ever led to a publication.

VM: Did you not publish from there?

MT: I think the only thing published from there that year was on the early stages of NMR. So as far as I was concerned personally, it was not a productive time for publication.

VM: But, you learned, presumably, how to...

MT: Yes. I went to use a technique which I then used, you see, when I came back on pyrethrum biosynthesis and was able to set up a laboratory to do that.

VM: Using C^{14} and the technology you learned there?

VM: You were, presumably, part of the group which was continually talking about what was happening and the significance of the results day by day in the lab.?

MT: Yes. Grant Buchanan came back. He had attended a Christmas party with...in Calvin's in Melvin's home and he had produced a very strong alcoholic drink, and Grant had made the quip that this must be the active C_2 fragment! Now, that was the phase that was going around — that was the sort of the last phase: the active C_2 fragment. Of course, this was before we appreciated that it was the addition of CO_2 to ribulose. They were looking at that stage for an active C_2 fragment.

VM: That's interesting because you say that Alex Wilson had already left by the time you got there and yet his data really indicated quite strongly that there might be a C_5 acceptor. I'm interested to hear that even after he had left, people were still talking...

MT: I think you've got...this is where my memory...Grant Buchanan making that quip sticks in the memory, and it may be displaced a bit in time.

VM: Of course, in his (*Grant's*) day would certainly have been true but a year later, by '56...

MT: Ribulose was certainly the sugar of the time.

VM: And indeed Rod published a paper. Rod was there at the same time as you?

MT: He arrived a bit before and left a little bit earlier.

VM: I think he had a paper, an early paper, something on ribulose; I can't remember the details now. Who were the people with you at the time? I see these people on the card that we talked about earlier. Who were the people who you remember as your friends, or closest colleagues in the place?

MT: I suppose the closest friend was Rod Quayle and Yvonne, who lived in the same flat and worked in the same laboratory, and we went camping together. Rod was, of course, is a delightful raconteur and he tells the story of how he went on a camping

trip (we weren't there) and they were going up with a group from the laboratory — I don't know if he's told you this, and...

VM: You haven't told it to us yet so we don't know!

MT: He badly wanted to have a pee but he didn't know the other so well and had the usual English reticence. So he said "that's a wonderful place to photograph — let's take a photograph there". Somebody said "it's much better later on". He was dying for a pee. They said "it's much better to take a photograph later on." When they finally did let him out of the car to take a photograph, he couldn't get his zip down and he swore he would never have zips again; he would always go for buttons! (*Laughter*)

VM: Well, you've left us without the denouement. What happened?

MT: Well, I think he did actually get the zip down but Rod himself didn't develop that too well.

VM: No. He didn't tell us that, actually.

SM: Was Yehuda Hertzberg there, could he have been the Hebrew? Because he was there...

VM: Hirschberg.

SM: Yehuda Hirschberg.

VM: I rather think he came at the same time we did, but I'm not absolutely sure.

MT: It doesn't ring a bell.

VM: I'll have to look up the list and see who might have been there from Israel at the time.

MT: You asked who were the main links. I mentioned Rod Quayle and he was working on the adjacent bench. Of course, Al Bassham had really taken Andy Benson's place and he was the person who knew the background if you had any problems on supplies or discussions. He was the person who knew the techniques, supplies and things about the laboratory. Bert Tolbert, to a certain extent, but my links with Bert weren't quite so strong.

VM: He was in Donner, of course, together with some of the other people. I noticed that your card has been signed by people who were in Donner.

MT: Marguerite and Dick (*Lemmon*)...

VM: ...and Rosemarie (Ostwald) was in Donner and maybe some of the others.

VM: Martha, I think, at that time was in Donner as well.

MT: Might well have been.

VM: She was working, I think, with Ed Bennett but I'm not sure.

MT: Yes, Ed Bennett — you mention Ed Bennett but he's not down here, is he? I wonder unless I can...

VM: There are names all over the car, aren't there, and it's difficult to spot them.

MT: Clint (*Fuller*); now, of course, that's another name. He left about the same time, I think, or a little after Andy I should think.

VM: So he overlapped with you in the lab.?

MT: Yes, he was there in the lab. He was really in charge, as I remember it, of looking after the algae collection; and he was (*taken?*)...who was it? Norris?

VM: Rich Norris.

MT: Rich Norris. Yes, it's Rich. When I see a Rich here (*on the card*), I think it must have been Rich Norris, a very nice man indeed, I liked him very much and he was always very helpful.

VM: I have never met him and I have an idea he died; I'm not sure whether he did. I'm getting a bit confused now about what we've been told. His wife, whose name was Louisa...

MT: Yes, Louisa, that's right.

VM: ...who was in charge of the algal (*cultures after Clint Fuller left*) and, I think, worked with Altha to some extent on the algae. She lives somewhere to the east of Seattle and when we were there, she simply couldn't get to us and we couldn't get to her; it was some hours drive away. I know about them (*the Norrises*) but I have never met either of them. Of course, Rich Norris is in your deerstalker photograph.

MT: That's right, yes. In his quiet way Rich was very able, providing the *Scenedesmus* cultures — invaluable. I do remember thinking at that stage what can easily happen is when you have a group like that, which are mainly chemists-biochemists, you bring in a biologist and, if he is, say a soil (?) botanist, it is very easy for him to lose his botanical sort of basis and become one of the others. I must have discussed that with Rich Norris because at the back of my mind there is that sort of thinking and it's probably from speaking with Rich Norris.

VM: Was Rich a biologist?

MT: I think he was a botanist.

VM: You think he lost his botanical flair?

MT: I think we were discussing the hazard in that situation of losing it. You've got to take positive steps to keep it, to keep your identity, your discipline identity so that you can make your contribution.

VM: You think that a botanist would wilt under the pressure from all the chemists rather than influence them?

MT: I think it's a possibility. I think also Clinton (*Fuller*) had the same feeling. Have you spoken with him?

VM: Yes.

MT: Do he never feel that he might become less of a biologist in that situation?

VM: I can't remember that he said so but I have to say that we heard so much from so many people in a short time that until we go back and play them all over again, I really won't remember.

When you went there (*to Berkeley*), you told us that you had already had the experience of doing a PhD in England and you had been working in the Lister for a bit. What differences did you find in style between your previous experience here and going into this American context? Do you think what you experienced was American or Calvin's group?

MT: I think at that stage in the '50s there was an international approach to laboratories. There wasn't all that great difference between the laboratories I had worked in UK and in America. I can believe that certainly in some UK laboratories, they were still very patriarchal but I hadn't actually worked under such a regime. To me, there was no great revolution or revelation. I had always been in close touch, daily conversation, with the head of department, whether he was leading me to a PhD or with Jim Baddiley working on coenzyme A, so that there was no great change there. I do think that any head of department stamps their individuality on a group. And I think Calvin stamped this conviviality that I mentioned before. So that, if you work in London, the chances are at the end of the working day, you spread to all quarters of the compass. Social life is made that much more difficult. Whereas in Berkeley, you were really within sort of walking distance of one another. This was the great enjoyment that we felt there. The social life, the conviviality, the friendliness which spread from the laboratory through to personal activities.

VM: And the convenience allowed you to pop in at all hours of the day and night.

MT: Yes, that's right. You could just go across (*the campus*) and continue something just half an hour in the evening and it means you got 24 hours more time in.

VM: Was your wife working while you were there?

MT: No, she's a teacher, and she didn't have a work permit so she couldn't teach professionally, but she used to help with one or two of the families round about who had young children.

VM: You didn't have any children at the time?

MT: No.

VM: You were able to go in and out of the lab. as often as you needed to.

MT: Yes.

VM: I remember it was the sort of place that people would come in at odd hours, to put chromatograms on...

MT: ...that's right, or change counters or something like that. It was just the guards. Perhaps that was the thing that did surprise us, the guards with pistols in the holsters.

VM: The campus police.

MT: Yes. That was a novelty.

VM: You remember there was all the palaver about getting clearance to work in the building.

MT: That's right.

VM: For UK citizens there wasn't any problem but for some of the Eastern Europeans, I think they were simply not allowed to work there. It was a long time before they got any...

MT: I know that to get clearance was important. They actually did keep tabs on you. I was very surprised because, you see, we had a permit to go in and when we left we went via a Southern Route through New Mexico down into Mexico. We crossed the border at El Paso. That was an amusing occurrence because we were about to go through on a bright and sunny day when suddenly the sky became dark and I realised there were four enormous military police stationed just round me. I was wearing, for the convenience of travelling, an ex-US Army haversack. They said "Hi, bud, where do you think you are going" in their inimitable southern drawl. I responded with one word which was sufficient: "well, actually..." and at that they pealed with laughter and I was let through. We left El Paso on a certain date, which is in my diary. About four years later, in the UK, I received a letter from the US Immigration Department saying they had no record of me leaving the States and could I provide them with details. I thought what an extraordinary machination of paper, going on so long. In fact, because of my diary I was able to supply them with time and date accurately.

VM: So you went into Mexico and came back to Britain from Mexico?

MT: We went down through Mexico and flew from Mexico City to Cuba and from Cuba to Jamaica and from Jamaica on a banana boat home.

VM: I see. Well that was an interesting route to travel.

MT: Yes, Greyhound, you see. We went by Greyhound bus all across California down to Mexico City. We thought "oh, splendid". We didn't realise that there was a quantum change between the Greyhound in California and the Greyhound in Mexico! Very interesting experience.

VM: What sort of contact have you kept up with the Calvin group since you left at El Paso?

MT: I saw Calvin several times when he came to the UK, when he collected an honorary degree at Nottingham. When some people like Al Bassham came over — saw him. Ning Pon; Ning Pon, of course, came over several times, sometimes unexpectedly. On one occasion in Sheffield he turned up in our drive in a camper. He and his family. On another occasion on a Broads holiday, I wasn't present at the time. Again, on the edge of the water, a camper arrived and there was Ning Pon waving to my family in the boat. We exchange Christmas cards but that's about all.

VM: Have you been back to Berkeley?

MT: Yes, I went back several times. Because I used to sit on recruiting boards for the British civil service, finding jobs for British scientists who wanted to get back to England. And so I used to interview, was one of a panel of interviewers and San Francisco/Berkeley was one of our stations. So I think of two or three occasions I went back there.

VM: Presumably you saw people when you did?

MT: Indeed, yes.

VM: On some of your later visits, the new building was there?

MT: The new building was in operation. Yes, I saw that; the round building.

VM: I am sure somebody must have told you about the philosophy that went into the design of that building to replicate ORL. How did it strike you? You know, one can never recreate an environment because it's got so much...

MT: I think that my impression, and this must only be taken as an impression, is that they tried to recreate something which was small and enlarged it and retain the quality. I think the enlargement, actually, led to a loss of a particular quality and this was the impression I got from the people speaking there. That's just an impression.

VM: It's a difficult situation. How else would you have designed a new building? I was part of the design team and remember the difficulties we had.

MT: The challenge.

VM: But ORL was rather a unique structure which we subsequently found was built in 1885, much earlier than most people thought.

MT: It was really a wooden structure, wasn't it?

VM: It certainly was a wooden structure.

MT: I can remember, we used to watch with amusement: you remember, out by the greenhouse, there was the laboratory dustbin, and high school students used to come along there surreptitiously and riffle through the dustbins to get out bits of equipment which they could build into their own experiments. I don't know if you remember that or not. I can remember the ambivalence towards this, thinking that the dustbin might have something dangerous in it but, on the other hand, these eager young scientists could have the encouragement of finding bits and pieces which would be useful to them.

VM: You didn't put out a tray of non-dangerous things?

MT: It wasn't sort of systematised such as that.

VM: It was rather an interesting building. I think...I'm trying to remember back myself. It didn't strike me as being that interesting when I first went into it. It's only, I think, with the passage of time that you realise...

MT: And also the comparison with other laboratories in which you worked.

VM: I both went to and came back from much smaller rooms than they had there and so I was impressed at the time. And then I went back to stay for a long time in these big room environments. In the interim, when the group was in the Life Sciences Building, it was very different. There was one big lab. What I meant to ask you earlier on (I remember now): there was, of course, the group in Donner which was separate from the ORL group — Bennett, Lemmon, Tolbert— were you in close contact with them?

MT: No. I knew them and they attended the seminars. In fact, I did a bit of work with Bert Tolbert. They certainly took part in the social affairs but I personally wasn't very much aware of their scientific programmes.

VM: So, it was a bifurcated structure, was it?

MT: I think it was. Or, shall we say, it appeared to me to be one.

VM: As a lab. worker, and not in any sense an administrator, you were conscious of there being another lot, whom you didn't have a great deal of contact with.

MT: Yes. I do remember at that stage, we are talking about diversification now from the photosynthesis as such, Al Bassham and Andy Benson were getting interested in photosynthesis in the oceans and the uptake of CO_2 in the air and the rate of photosynthesis. I think Al Bassham had been in the Navy, and had got naval contacts so work was, I think, starting to move...I don't know how far it got or what they did. But when you consider now the interest in that subject, the importance of the oceans in the CO_2 buffer situation, it's interesting that they were thinking on those lines at that stage.

VM: That was before '54, wasn't it?

MT: About '54, yes.

VM: Well, Al didn't do that but, of course, Andy did. Andy eventually ended up at the Scripps Institution of Oceanography at La Jolla and he has essentially been working in those sorts of areas now for many years.

MT: He must have moved back from the east coast to La Jolla.

VM: He did; when we saw him in the summer of '96 he said he had been there for 33 years.

MT: My impression is that Andy actually had a very innovative brain. I think that...I don't want to...subsequent (?) comparisons tend to be invidious...I think that Al Bassham was very much more a nuts and bolts person and day-to-day working and programming. I think Andy Benson had more vision and so that he took on the CO_2 in the oceans and so on doesn't surprise me. And, I think, because of that vision he probably made a very great contribution to the Calvin group.

VM: I think everybody who knew him in that context recognises that. You must be aware that for a long time it was often called the Calvin-Benson cycle or the Benson-Calvin cycle.

MT: I didn't realise that.

VM: There was a time when it was; some people used the term more than others. I'm not quite sure how it's referred to now because, in a sense it's old hat, and textbook material.

MT: I certainly had that feeling, that gut reaction, that Andy was a person with imagination and vision and, therefore, could make a great contribution to any programme he joined.

VM: I think that my experience (and I'm interested to hear yours) was that most of the people who were there were actually rather good, the people who worked in the lab. Was that your impression?

MT: Indeed, yes. I think that certainly I would say so. I think that if you looked at subsequent careers this has probably been borne out.

VM: What was your subsequent career? What happened when you left Berkeley?

MT: I resumed an ICI Fellowship in London. Jim Baddiley had left the Lister Institute and gone up to Newcastle, so I went to University College, the Chemistry Department. I was trying to resolve, optically, phosphate esters.

VM: Sorry: which esters?

MT: Phosphate esters; phosphorothio esters. In fact, I ran into great difficulties there and never achieved that. Subsequently I realised the things I got had such low optical activities that it would have been difficult for me to make the measurements reliably. I also got collaborating with Charles Vernon and his group on enzymes and so kept in contact up until his recent death with Charles. We had a great mutual interest in the phosphate-rich energy bond, the triple bond of ATP.

VM: He and Barbara...what was Barbara?

MT: Barbara Banks...Well now, that went back to a paper which Charles and Moore...George Moore — have you ever come across him?

VM: No, I don't think so.

MT: ...and Ron Gillespie, professor of chemistry out in Canada somewhere? They wrote showing that the concept of the energy-rich phosphate bond was a mistake, a misunderstanding in thermodynamics. The phosphate bond, any chemical bond, is an energy minimum, not a maximum. I read this when I was at the Lister Institute and as a chemist immediately saw the sense of this. I got in touch with Charles Vernon — used to attend some of their seminars at UC — and we realised we had a similarity in outlook. I became, I think I can say, notorious at the Lister Institute because I used to champion the fight against the energy-rich phosphate bond and Charles Vernon used to like to remind me I was the only person he ever knew who had been eliminated from a dance because they didn't believe in the energy-rich phosphate bond!

VM: Eliminated from a dance?

MT: Yes. The Christmas Party at the Lister Institute; one of the eliminations was anybody who does not believe in the energy-rich phosphate bond had to leave the floor. I was the only one. (*Laughter*)

VM: Oh, I see. it was an elimination dance!

MT: An elimination dance, yes.

VM: I see. Did they know that you were the only one?

MT: I told Charles this and he was much amused. But of course, Barbara Banks still has continued writing on this subject so this misconception still carries on. But anyway, I had these links with Charles Vernon and worked there for a couple of years; published some papers with him on enzyme chemistry. Then I left to go to what was then the Tropical Products Institute.

VM: In what capacity did you...?

MT: I went as a section leader. I was in charge of a group working on the biosynthesis of pyrethrins. Of course, my work with C^{14} came in.

VM: So this would have been in the late '50s by then, would it?

MT: '57, yes. At that stage, I was in just charge of a small section working on the biosynthesis of the pyrethrins and their chemistry and discovered a new pyrethroid which was based on jasmone; chemistry as well — organic, classical chemistry. Then one began to get other responsibilities for pesticides in general. I became a member of WHO Committees on Pesticides, FAO and you gradually ascend the ladder and you get responsibilities for pulp and paper, and became deputy director and you have the lot.

VM: Eventually, you were director...

MT: Yes.

VM: ...by which time it was called what?

MT: Well, we went through various names. Tropical Products Institute for a period and then the Institute was amalgamated with the Anti-Locust Research Centre in South Kensington.

VM: These were all, of course, government supported...

MT: They were government supported by the Foreign and Commonwealth Office from funds devoted to aid. You clearly couldn't have a laboratory which had big entomological section on locust control called the "Tropical Products Institute". So it was called the "Tropical Products and (*indecipherable*) Natural Products Development Institute (TDRI).

VM: One of these snappy civil service names!

MT: One of these different names. It has gone through several names like that. Then we added to it a group of agricultural economists who were concerned with assessing land use management and we became the Overseas Development Natural Resource Institute (ODNRI). And I think it's something like that right now.

VM: This has been funded under the Overseas Development Administration programme?

MT: Yes, that's right.

VM: As an aid activity?

MT: As an aid activity. Now, of course, they are seeking to privatise. Subsequent to my leaving the Institute, it moved down to Chatham in Kent. One of my last jobs was gearing up for that move.

VM: You eventually became the director of the whole organisation? How long were as director?

MT: I think...let me see; I forget; I suppose about four years. I'm guessing four years.

VM: And you retired in about '85?

MT: '85, yes.

VM: And live a life of luxury in Norwich since?

MT A happy life. Retirement can be very rewarding if you plan it and don't seek to do exactly the same things you've done in the past. I think it's a time for taking on something new, (*using*) new talent.

Tape turned over

SM: You mentioned your culture shock when you arrived, at seeing the Campus Police with guns in holsters. Can you tell us something about how you found living in Berkeley at the supermarket-domestic level as compared with what was very much like post-war England still at that time?

MT: There was a contrast, a quite definite contrast, between UK and Berkeley, apart from the weather, of course — everybody, I suppose, comments on that. The campus had got, at that time a lot of students in the "flower power", so there was a lot of student activity, one was aware of the political feeling arising fairly strongly. The laboratory, whilst aware of it, was not part of it, not being at sort of student level. I think also the equality of it struck us on occasions. For instance, the swimming party. We went swimming there. You stripped naked and you had a shower and put on a grey sterilised costume. Rod Quayle and I were amused that you might be standing next to a student one time or a Nobel Prize winner the next, but you were all naked in the same shower. Now we felt this would not happen in UK, there would be segregation of the ranks. It's like the officers and the men! So that was a distinct change.

Of course, San Francisco itself was an enormous shock because that's an entrepôt for things we had never seen in the UK. I first got interested in the differences in Chinese jade through the shops in San Francisco and, of course, the museum on the campus — there is some lovely jade in the campus museum. Also, I enjoyed access to the stacks (*in the library*), again being on the site, to be able to go down to the stacks in the library and, no matter what the subject, pick out books at your choice.

VM: At that time you couldn't do that in UK libraries?

MT: No, you put in...unless, of course, it was a reference library. But in Berkeley all the literature of the world was there on stacks down below and you were allowed to peruse it, I was allowed to peruse it and students were as well. There was a great freedom of access there which the system allowed.

VM: Did you find anything strange and unexpected when you got there that you can now remember? To prompt you, the example that I can think of in our own lives was when we once went into a post office to make a telephone call only to find, of course, that post offices were not the same as the telephone company as had been in Britain.

MT I think we were warned about certain sort of differences in terminology, so that didn't come as a shock. Something is flitting through the back of my mind which was a bit of a surprise but that's gone again. It was all probably the freedom, I think, and the interchange, perhaps, was a bit freer than in England although I had been fortunate in working in departments which were friendly.

VM: It was all very easy, of course, wasn't it, because the language was the same and you rapidly found out how everything was done and things worked.

MT: Slight differences in usage but nothing that caused great shock.

SM: How did your wife feel about it?

MT: She enjoyed it. Of course, she had travelled on the Continent before going out to America. As you say, it was easy with the language travelling there. We much enjoyed the camping and being able to get up into the Sierra.

VM: So it seems that you have pretty fond memories of your time?

MT: Yes. Filled with admiration for the individual Americans although I think sometimes the collective Americans...

SM: That's lovely!

VM: One last thing I remember, and I think, it may be something Rod said. Did Rod come and work in Tropical Products?

MT: Yes, that's right. You see, he came...

VM: Rod Quayle. I should say this is Rod Quayle for the record.

MT: He returned, you see, to England a bit before me, probably six months before me; I forget the actual time. He came back to a job in the Tropical Products Institute and when I came back he moved on to Oxford. And so the job he was doing became vacant and I applied for it and got it. I followed on directly from Rod Quayle.

VM: I See; but there was no...it wasn't the fact that he had been there that led you specifically to that place?

MT: No, no, it wasn't. In fact, I had worked — I shall have to backtrack now quite a long way — I was a student during the war, and as such we were under the auspices of the Joint Recruiting Board. When we graduated, the war had just ended — this was in 1945 — and so as students we saw the VE celebrations in Trafalgar Square and somewhere have got photographs of being hauled off by policemen from the tops of air raid shelters we were clambering on, so there was no need for us in the Armed Forces. So the Joint Recruiting Board could allocate you to a job in a factory, or in essential work, or in the forces. I went first of all to work with a consultant chemist...I got a job with a consultant chemist which was very wide experience. We did all sorts of analyses — for instance, greyhound doping in dog vomit, arsenic poisoning in elephants' intestines, some very amusing anecdotes from that period. But I got a knowledge of essential oils there. Again, under the auspices of the Joint Recruiting Board, I applied for a job at what was then the Imperial Institute in South Kensington. I worked there on essential oils. And then I was allowed back to become a full-time PhD student.

So I had behind me from early days — I graduated at about 19 on one of these accelerated courses during the war — I had behind me analytical experience and a knowledge of essential oils. So I was able to fit into the Imperial Institute. Now, the Imperial Institute ultimately became the Tropical Products Institute. So, you see, I went back again.

VM: So you had contacts and you knew...

MT: I knew quite a lot about the work and the subject matter. By chance, Rod Quayle — it was purely accidental that we followed one another.

VM: Good. Well, I think we have explored your California era extensively. Shall we call it there?

MT: If anything occurs to me I can get in touch.

VM: Indeed. Well, it's been a great pleasure and thank you very much for your time.

Can we revert for just one moment to the deerstalker hats? You got them, did you?

MT: Yes, I ordered them through a shop, I think it was in Regent Street. I wrote off and we had to find out how to measure head size. I don't know if you have ever gone into measurements of head sizes but we did this and I think we got four or five.

VM: How did the deerstalker idea arise?

MT: I think it must have been Rod and I joking about Sherlock Holmes; I think this is how it arose. Then Calvin or somebody else, Clint (*Fuller*), asking what deerstalkers were.

The situation built up finally to ordering them; I think it was from Lilywhites; Lilywhites I believe I got them.

VM: I think it might well have been. It's just such a famous picture.

MT: I can tell you because I've still got mine and I can look inside and see if there is a maker's name.

VM: Anyway, we look forward to getting a photograph of you in your deerstalker.

MT: You asked about cultural shock. Well, I can remember two instances in this. First of all, the hospitality of the people in the laboratory was immense and I think all of us enjoyed parties, outings and so on. But shortly after arriving there we were invited to a party —it might even have been a Christmas party — and our host, whose name I forget at the moment, threw open the door and said "Hi folks! Here's Nancy and Malcolm, Mal Thain from London". Now, I had never had that sort of introduction before, neither had I ever been called Mal Thain! This was a bit of a shock. But the other one, another Christmas party, we were chatting away, and the son of the host was sitting there. And I thought what an intelligent child, sitting there and lapping up the conversation, not asking any questions, but taking a great interest in everything I was saying. He finally revealed it all when he said "Gee, you do talk funny!".