

## Chapter 39

### H. MONTAGUE (MONTY) FREY

London

January 6th, 1997

**VM = Vivian Moses; MF = Monty Frey; SM = Sheila Moses**

**VM:** This is a conversation with Monty Frey in London on January the 6th, 1997.

Can I start off by asking you how it happened in the first place that you got to Melvin's lab.?

**MF:** I think it was one of those real accidents. It wasn't absolutely clear that I even was going to be a scientist. We had a family business and I was really expected to go into that. I suspect I kept delaying so that when I had finished my first degree, I had been interviewed for a job with ICI and my father was fairly anti- and said "why did I want a job with ICI?" I said "well, because I'll then be able to afford a car".

**VM:** You had a chemistry degree?

**MF:** I was at Oxford and I had a degree. I was, as it happens, an able student. I didn't find it very difficult. And he said, "well, all right, I think you are being very silly, I'll buy you a car and you can do a doctorate. It will look good on the business letterhead, we'll have at least one director who is a doctor." So, I got the car and I stayed in Oxford and I did a D.Phil. with Hinshelwood. Towards the end of that period, I got engaged. The final year my wife-to-be lived in London and I was in Oxford, not unconventional in those days to have a year's engagement. I suspect that I decided to postdoc. for a whole range of reasons, none of which would be thought to be very valid nowadays. One was, I thought it would delay making the decision about going into the family business.

**VM:** What sort of business was it?

**MF:** Import/export.

**VM:** Nothing to do with chemistry?

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**MF:** Nothing whatever. Secondly, I thought it would be quite fun to go to America and, thirdly, I thought it would be good to start married life well away from both sets of parents, especially my parents-in-law. That's only because my wife, you know, had been a very well-cosseted daughter and all the rest. So I discussed with Hinshelwood where I should go. With Hinshelwood I did gas kinetics. At that stage I think everybody in the lab. thought I was a kind of dilettante scientist so it didn't really matter too much where I went or what I did. Really, the problem was just one of getting grants. In the end, I had three grants: one would have taken me to Washington, DC, one would have taken me to CalTech with Pauling and I then got a Commonwealth Fund Fellowship (they are now called Harkness Fellowships) and that really was, I think to some extent, I had to decide when applying for that where I would go. Hinshelwood, I think, suggested that Calvin would be possible.

I have to say in all honesty: I think I thought that Berkeley would be a nice place to go and who could I go and work with?

**VM:** You were not the only one to...

**MF:** Yes, well, as I say, none of this is academically sound, in a way, certainly not scientifically sound. The other thing about the Commonwealth Fellowship was it was easily the best in terms of the money and the fact that it paid for months of travel and it would pay something for a wife. Altogether, it looked the best bet. I think probably scientifically the best thing would have been to have gone to CalTech and work with Pauling. But — that was it.

**VM:** What did you know about Calvin and his lab. before you went there?

**MF:** Absolutely nothing.

**VM:** You had no idea what they did or what...?

**MF:** When I say absolutely nothing, I knew nothing about it when I chose it. I knew a little more about it when I decided that I would go there. I looked him up and I knew this, that and the other; and one of the reasons that there was a possible niche, though it didn't actually turn out that way, was that I had been using carbon-14, for totally different things, but I had been using carbon-14 as a tracer and he was very heavily into labelling things with  $C^{14}$  for photosynthesis. I think that was a sort of bridge.

**VM:** You hadn't met him?

**MF:** No.

**VM:** Had you met anybody who had been in the lab. before you went out?

**MF:** No.

**VM:** So you really didn't know...?

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**MF:** Nothing, absolutely nothing.

**VM:** Had you agreed with him what you were going to work on?

**MF:** No, and he wasn't even there when I arrived.

**VM:** So what happened when you arrived?

**MF:** It was really quite extraordinary because Calvin was in Europe for three months.

**VM:** This was when; this was '54, was it?

**MF:** '55.

**VM:** '55?

**MF:** 1955; I got there in September.

**VM:** This must have been just at the time I was hearing him lecture here (*in London*).

**MF:** Yes, exactly; that's why he wasn't there! When I sort of think nowadays of people coming to work with me and all the rest, I'm amazed how little disconcerted I was when I arrived. Part of the reason that it was so nice and easy is that we had absolutely fabulous accommodation which no postdoc. had any right to expect and no postdoc. ever had. In fact, the guy from the Commonwealth Fund, who came out to visit us, a chap called Lance Hammond, said it was the best accommodation he had ever seen a Fellow in.

**MF:** What was it and how did you find it?

**MF:** Well, it was just luck. Typically, at this stage, I think I was already relying on my wife-to-be to do all the important things. And I think she wrote to the accommodation office in Berkeley and our letter arrived, as I understand it, as the wife of the head of the Classics Department popped in to see who they would rent their duplex to while he spent a year in Europe. I think somehow or other they must have got a feeling that we were much older and senior and reliable. So, we had this fabulous duplex within walking distance.

**VM:** Do you remember where it was?

**MF:** Yes, I think it was 2554 Virginia Street — it was certainly Virginia and it's the number that I'm not 100% sure of., I think 2554; just next to Euclid, on the corner of Euclid and Virginia. In fact, when I applied for a car parking permit, which they pointed out (a) I wouldn't ever get on the points that I could amount, amass; that that where I lived was considered a prime parking spot to the university and that if I didn't get my car out of the garage and park it outside the house, I wouldn't be able to park it outside the house all day because someone else would be parked there. So, it was

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really superb and he was Professor of Latin. My wife subsequently met his wife but that's another story. There were thousands of books in the house...

**VM:** ...all in Latin!

**MF:** No, all in English, I'm not quite sure why. Not only were there academic books but there was a huge range of novels. It really was great for anybody who likes books.

**VM:** This had already been set up so when you arrived you went straight into it?

**MF:** Yes. It was all set up at a distance, at a very low rent so that we should look after the place. It had a gardener with it, who came once a week. I think those sorts of things made it much less disconcerting that we had arrived and the guy I was supposed to be working with wasn't there.

I think I had discussions — I'm trying to think who I had discussions with; maybe it was Bert Tolbert or maybe it was Lemmon. But anyhow, I decided what I was going to do which was a sort of something with carbon-14.

**VM:** It was pretty much up to you to decide?

**MF:** Entirely at that point, since Calvin wasn't there. Indeed, for the first four months I worked by myself on that. I built my own apparatus, it wasn't too much different from what I had been doing in Oxford so it was relatively easy to do.

**VM:** What was the climate, the local...the laboratory climate in which you were working similar to the one you'd already met in Oxford or were there quantitative/qualitative differences in the kind of atmosphere, relationships, etc.?

**MF:** It's hard to tell at this distance. Most of my experience was, I suspect, a little atypical. In Oxford, Hinshelwood, after all, at that point was President of The Royal Society and a very powerful man, and head of the department, and the people who worked for him, I suspect, had charmed lives in terms of things being done for them. Like, I needed some glassblowing done and it was done within a few days. I only realised a good long time later that people waited weeks and weeks for stuff to get done. Nobody ever talked to me about things like budgets. So I think I had a very cushy existence in Oxford and it wasn't a lot different when I got to Berkeley.

Now, I suspect that if I had been through a normal course of things, Berkeley would have looked like a kind of scientific paradise in that lots of things were available. I remember going to the chemical stores that they had and there were like everything in the Kodak catalogue was there, somewhere. You could just pick it up and take it. Only when I got back to England did I realise how extraordinary this was.

**VM:** In Oxford, had you been working in a little room by yourself or a big room with others?

**MF:** No, I had a room which was about...nearly twice the area of this room...

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**VM:** This room, for the record, is about 300 sq. ft..

**MF:** Yes. OK. It would be probably 450 I should think. When I started, I was the only person in the room and then a postdoc. arrived subsequently and the two of us had it. So it was very spacious.

**VM:** But, nevertheless, it was just you and one other?

**MF:** Yes.

**VM:** What happened in Berkeley?

**MF:** In Berkeley I was given a room which was much smaller but still of adequate size that I had it all to myself.

**VM:** And you worked in that way, all by yourself in a room throughout the period you were there?

**MF:** No, that was for the first four months before I then switched; I mean, when I switched and worked on NMR, the instrument was elsewhere but I still kept the room as an office now and did no lab. work in it. I always was in a very happy state from that point of view.

**VM:** Coming back to the beginning, what did you start working on?

**MF:** I was looking for an isotope effect on the pyrolysis of pyruvic acid.

**VM:** As a pure chemical problem?

**MF:** Yes.

**VM:** Was this related to other things they were doing?

**MF:** I think they had got interested in isotope effects because they are quite important in terms of mechanism because you get fractionation and you may get very misleading things. That was one of the reasons why I thought I would do an isotope effect, it would fit in. I think Dick Lemmon had...I think it was he who had pyrolyzed pyruvic acid. They had been trying to make a labelled compound, I forget now exactly why. It was according to what was in the literature, the pyrolysis was totally atypical in that it led to the elimination of carbon monoxide. Now there are a large number of isotope effects involving the elimination of carbon dioxide and this would have been one of the first, if not the first. That's what attracted me. It doesn't pyrolyse homogeneously; it pyrolyses on glass surfaces. The sad thing at the end of the day is that Dick got it wrong. It actually gives off CO<sub>2</sub>, so it isn't terribly interesting. The literature was wrong and, although I put it right. it became a very uninteresting study though it had some fun.

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**VM:** That was what you were doing in the first few months?

**MF:** Mm.

**VM:** And that was, as it were, a separate piece of work; it was not part of some great team effort?.

**MF:** No. None of my work was ever part of a team. So in that sense I was quite isolated. I was doubly isolated in a sense that I was very much a physical chemist, indeed a gas kineticist, whereas most of the group, of course, were not. They were much more knowledgeable in areas where I knew very little.

**VM:** What sort of interaction did you have with the people in Donner, members of the Calvin group based in Donner — and I don't know: there were ten or twelve of them there at the time?

**MF:** It was more social than scientific though at coffee times inevitably things were discussed. It seemed to me a lot of discussion was on technical things to do with apparatus functioning or equipment and how it functioned and not...well, not in my presence, anyhow...so much on scientific problems as I would see a scientific problem.

**VM:** In the absence of Calvin in Europe for those three months, was there much in the way of scientific leadership as you saw it?

**MF:** Clearly the Donner group was one group and the ORL was another group. I don't know. We still had joint seminars, both groups once a week. Though it was quite fun in a way because we would get tape recordings from Calvin in Europe which were often played at the beginning (*of the seminar*) of what he was up to in Europe. For someone I hadn't met, it was kind of amusing. And then there would be some seminars. I never felt really out of depth in them so they weren't so focused in an area I didn't understand that it wasn't worth going to. What I did remember is that it started at eight o'clock in the morning.

**VM:** All the British contingent...

**MF:** ...noticed that. It wasn't quite so bad in one sense for me in that for the first two or three months my wife had a job in a firm that also started very early in the morning. I'm trying to think whether she had one or two jobs; one, at that stage, which started at 8:15 or something so I had to get up anyhow.

**VM:** She had come on an immigration visa so she could change her job?

**MF:** Yes. She subsequently changed her job so she could start at 9:00 o'clock in the morning at the YWCA, which we thought was rather a hoot.

**VM:** In those first four months you were really working in a room by yourself and really on your own little project...

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**MF:** Yes, absolutely.

**VM:** ...with only tentative connections with anybody else?

**MF:** The connections in a scientific sense were more “where do I get this from?”, “how do I get that”, one or two rather extraordinary things in that...I mean, on the technical point of view, I knew how I would have done a separation in Oxford which would have used liquid oxygen as a coolant. That was thought to be incredibly dangerous there where liquid nitrogen was available. In order to get liquid oxygen I had to go over to Giauque’s lab. where, in fact, there was someone working who I had known in Oxford, a girl, the first girl who ever worked with Giauque (he didn’t believe women should be in science). They would give me these 5 litre globes of liquid oxygen which they were distilling and I could tap it out of the bottom of the still.

**VM:** Remind me: how much temperature difference is there between oxygen and nitrogen?

**MF:** Oh, I don’t know: perhaps 15 degrees or something, critical for the separation I was doing. The thing that I wanted would not freeze in oxygen, and so I could pump it through, and it would freeze in nitrogen. The nice point was that they told me to be very careful, that when this had evaporated down to like 50 ml I was just to pull it out. The reason was that there was enough acetylene in the air in Berkeley that it concentrates in this stream and then you get an explosive mixture if you go too far down. So that was it. Now, if I had used liquid nitrogen under the system I was using, I would actually condense oxygen into it.

I worked by myself...very good for me in all sorts of ways...not what I had gone for. But in practice, I had already, from the last year with Hinshelwood, been an atypical student. With Hinshelwood I had an ICI fellowship even though I was working for a doctorate and that was pretty unique at the time. That, again, was one of the things, that if you worked with Hinshelwood you might get away with. I think that was the first time anyone had got away with it. But it was very nice for me financially.

**VM:** Sitting in Donner, you had some contact with ORL; you went in...?

**MF:** Only really for these weekly seminars. Gradually, as you got to know one or two people you would pop over and I got to know them that way. At that time, for example, Marilyn, his secretary, was actually in Donner, as was Norma.

**VM:** Were you conscious of any great difference between the Donner set-up and the ORL set-up?

**MF:** I think the Donner one was, in a sense, more physically instrumentally based and the other one was more the organic-bio; one certainly got that feeling. I was in the right place in that sense.

**VM:** OK. Then after four months, you...

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**MF:** Then Calvin came back.

**VM:** Can you remember your first contact?

**MF:** I'm not sure I can remember my first contact. I can remember...it's the trivia you remember...the trivia I remember is going to his house, the whole group, to see his movies — which were awful. They had all the errors...I was a photographer and I had a 16 mm Rolex and Leicas and the lot. Calvin had all the errors of people who pan. My wife said it made her quite seasick watching these things. So that's the first thing I can really remember. Then I guess we must have had a discussion. He said, "Look: we've got this NMR machine" (it was actually a big Varian and, I think, the third ever made, and the second ever made never worked — that went off to South America and so clearly we were using the second ever machine). And Calvin said "we've got this machine; so and so is working on it, he's a physicist, why don't you go over and see what you can do with it?" That was my introduction. I went over and subsequently worked on NMR.

**VM:** The physicist was presumably Power Sogo?

**MF:** The physicist was Power Sogo. We were actually learning how the machine worked and all sorts of lovely things. I made a major discovery which subsequently put me off NMR forever because it was too late; someone else had made it. I didn't know that at the time.

**VM:** What were you trying to do with NMR?

**MF:** I think we were just trying to see what it would do.

**VM:** You were playing.

**MF:** We were playing. As it turns out, very unfortunately, I discovered the thing which was known as "hindered internal rotation". It was frightfully exciting. I spent quite a lot of time on it. I think we understood. We did it in a way which was really, in retrospect, extremely clever with the instrumentation we had. No one would think of doing it in this awful way now. I thought I was going to be famous.

**VM:** Was it published?

**MF:** My stuff? No, no because I then found it in the *Journal of Chemical Physics* by Yankwich in Urbana who became famous for it and he was about six months ahead of me. But I had no idea that other people were working in the field or anything.

**VM:** During this period in Calvin's lab., what was your attitude continuing to be like with respect to the family business? Were you thinking this was your last year as a chemist, or were you thinking there were going to be many more years?

**MF:** No. It's extraordinary. I think one just puts off these sorts of things so you don't think about them at all. I can't remember thinking...all I can remember thinking of was



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what a lovely place this was and how nice the people were. I think I was thinking in all honesty that if I stayed in chemistry I was going to have to do something very different from what I was doing. I didn't see this as part of my career.

**VM:** Were you concerned about acquiring publications while you were there?

**MF:** Not at all.

**VM:** Because you didn't see them as necessary to take the next step?

**MF:** I think I was naive. I don't think I'd thought about things like that.

**VM:** My memory of Calvin was that — I accept that you didn't see him for four months — was that he was very anxious to publish and publish quickly, because he was aware of the competition.

**MF:** I think I was sufficiently orthogonal to what they were all doing that it didn't matter very much.

**VM:** When you started doing NMR, was that not in ORL?

**MF:** Yes.

**VM:** Did that bring you closer to that group?

**MF:** No, because Power and I worked together on this machine in a room all by ourselves.

**VM:** So you still didn't terribly much mix with...

**MF:** Nobody else was interested in NMR at the time. This was thought to be a sort of typical Calvin...If they had realised how incredibly important it was going to be as a technique, they would have thought differently. But nobody did at that time and it needed a hell of a lot more development. We just didn't have a field that was very stable. Everything had to be scanned in seconds and all the rest. I did publish something on NMR in the end the *Journal of Chemical Physics* or something, something like that, but it was trivial compared with what I had really discovered, but too late.

**VM:** It wasn't a great publishing period.

**MF:** I think I was young and stupid in one sense. Because having been so disillusioned that I had missed the boat, we didn't publish a whole lot of stuff which was publishable in that area and was very, very good indeed and wasn't actually, particular things there, found for another four or five years. It's just that when you think you have actually made the major discovery, the things you think are less important are of no importance all of a sudden. That was very silly. I would never make that mistake 20 years later.

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**VM:** As a young married, going to this rather luxurious accommodation in a new and exciting country for you, how did you organise your social life?

**MF:** Some of it was organised for us because it was an incredibly friendly group. As I say, my interactions were mainly social rather than scientific in all sorts of ways. There was a young chap, who had just got married there, Irv Whittemore, who, because we were of an age, we made friends with and have been ever since (in fact, their daughter is named after my wife)...

**VM:** Do you know where he is?

**MF:** Exactly where he is: he is in El Cerrito and we saw him last year when he came over.

**VM:** I hadn't realised.

**MF:** Yes, it's a great pity, that. But he...I'm trying to think what happened in his...but he left the group fairly early on and went to work for an oil company in El Cerrito and was an analyst for them and did very well financially anyhow. So that was one. Norma Werdelin also was a secretary and thought we needed looking after to some extent and did. The Lemmons: we would go skiing with them; that sort of thing. They were a mad skiing lot, as I'm sure you know at that time and we got introduced to it in all its lunacies — leaving at four o'clock in the morning and all the rest.

**VM:** I have to tell you that those very same people have been lunatic skiers until the last couple of years until they started breaking things a lot and had to stop.

**MF:** I understand absolutely; I see it in other people here. We might have gone on forever except that one came back to England and it's not that easy to ski here and we started a family and, you know, that was the end...we never went back. So our social life was built around the lab. It was so exciting that you could do quite a lot that wasn't in a sense social life; I mean food...the existence of frozen food and pies and things, I remember, just the whole business of shopping was so exciting.

We were very well looked after. When we needed a car, which we did immediately, Bert Tolbert immediately took over and said there is this chap up on The Hill in the Radiation Lab. and he has a second-hand Mercury which has been looked after incredibly well — it had its entire life history — which served us very well for two years.

**VM:** Did you travel?

**MF:** We travelled a lot in California, every weekend basically, I think, we went away. If it wasn't skiing it was doing all the other things.

I don't think it was deliberately, as it were, putting things off, but I think I must have felt at some stage that the work that I was doing there, such as it was, was certainly not central to any scientific career I had, if I was going to have one. That's why I then decided to spend another nine months in the States in Harvard. I'm pretty sure that I

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went to work in Harvard with Kistiakowsky on the recommendation of Hinshelwood. Kistiakowsky probably took me on that and the Fellowship could be extended for nine months if you wanted. I spent nine months in California, three months travelling across the States, because that was part of the Fellowship, and then I spent nine months in Harvard.

**VM:** You didn't know Ken Sauer, did you?

**MF:** Yes, of course.

**VM:** He was with Kistiakowsky.

**MF:** Yes, we overlapped.

**VM:** He was there when you arrived?

**MF:** Oh yes. He was working for his PhD and I was a postdoc. I knew Ken well and he visited us here in England years and years ago.

**VM:** What did Calvin say? Did he say anything when you told him you were going to leave him and go and work with Kistiakowsky?

**MF:** No.

**VM:** Good luck?

**MF:** No. Oh yes. He was always...I mean, in retrospect it's kind of funny because my wife, and she only mentioned it the other day because of something else that had happened...my parents flew out to see us while we were there, to see that I wasn't beating my wife and things like that — they were very concerned. She had had this sheltered background, you know, and how were we getting on?

**SM:** Had you come out by boat or by air?

**MF:** Boat and train — lovely. My parents flew out and they flew out — God knows what it must have cost, because they flew out via Greenland in a plane that had beds. They went to sleep in beds, can you imagine? Anyhow, they arrived in California and I told Calvin that my parents were visiting. Of course, he invited us all for lunch and all the rest. It was very nice. I didn't think anything of it, which is sort of the free and easy way one has. But, you know, I was just a postdoc. starting and that's very nice, isn't it, a very distinguished chap, your parents are coming, "oh, you must all come to lunch". I'm trying to think whether I would have done it, you know, twenty years later!

**VM:** I think one of the factors is how young everybody was, including Calvin.

**MF:** That's right. Calvin was in his forties, that's true.

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**VM:** He was not a stuffy man — not stuffy and only in his forties.

**MF:** I can't honestly remember...if he'd had been upset that I was leaving him to go somewhere, I'm sure I would have remembered. So I don't there could ever have been anything like that.

**VM:** Was the nine months that you spent with Kistiakowsky very different in scientific terms?

**MF:** The nine months with Kistiakowsky determined, in a sense, the course of my scientific career. It was a carry-on with things I'd done with...no...it wasn't a carry on with anything that I had done with Hinshelwood but it could have been. I could have done things like that with Hinshelwood but I hadn't for various reasons. I had done another pathway. What I did with Kistiakowsky, then I built on.

**VM:** The time with Calvin, therefore, was completely an interlude...

**MF:** Yes.

**VM:** ...and no follow-on scientifically?

**MF:** Other than I knew something about NMR and nobody else did, that was quite a plus and interesting in a way. And I think just going to America, that was the first exposure, I think seeing group working was an eye-opener for me and very important. In a way, I think I didn't like the thought of being a cog in a team.

**VM:** Before you'd gone or after you'd seen it?

**MF:** After I'd seen it. Before I didn't know anything about it at all. Of course, it's all the rage now to be part of the multidisciplinary team. I saw a multidisciplinary team and I think I saw more of the snags than the gains for me at that stage. Probably I have changed now and see the necessity.

**VM:** Interesting because I came a year later and was in the other place and for me Donner was the distance place. I was part of that team and there was no sense of being a cog on a wheel. The sense was entirely that I owned that activity, as everybody else did, and it was my personal interest that it should go forward because it was mine, too.

**MF:** I wasn't part of the team and it seemed to me extraordinary in a way. It seemed to me that Calvin was the chap who integrated everything; he was the only one who had any conception of everything that was going on whereas the ORL people really didn't know a lot of what was going on in Donner and to some extent vice-versa. Calvin could stretch from the physicists to the pure bio people in a fantastic way...

**VM:** Knowledgeably in your experience?

**MF:** ...knowledgeably, and I thought that was very plus.

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**VM:** When he came back then, in the latter period when you were in his lab., did he visit you a lot in your...?

**MF:** He'd pop in rarely. He was very busy still with atomic energy stuff. We would see him, of course, at the weekly seminars.

**VM:** Did he want to go through in any detail what you'd done?

**MF:** No.

**VM:** Never?

**MF:** Only when we came to write up the paper.

**VM:** Was his name on it?

**MF:** No.

**VM:** So it was really an activity outside his main stream, as he saw it?

**MF:** I think at that stage, yes. Calvin had gotten an NMR machine with...I don't think with any real feeling of what he was going to use it for. He got it because it was a new technique and let's play with it and see what it will do. Maybe it will be something that will be important to use.

**VM:** Did he understand what it was or what it might be used for?

**MF:** I don't know. I am sure he understood what it was without any shadow of doubt. No, I don't think it was very clear. It's hard to remember...I mean, it's not hard for me but it's hard for people nowadays to remember how little was known about NMR. At that time I went over to Shell (*Development*) in Emeryville to hear a talk by a guy and they had been playing around with NMR for some time. They came to the conclusion that it was unlikely ever to be of any importance in the oil industry! That's absolutely wrong from beginning to end — it's one of their most powerful tools. But at that stage, you see, and I suspect with the sorts of resolutions and instabilities we had, they couldn't see all that was going to happen. All great people, and Calvin certainly was one, have these hunches, I suspect. I suspect he had a hunch that it could be important. Nobody at that time could have a hunch at how important it is now because none of the bits of NMR that we now have, two-dimensional pulse systems and all the rest that allow you to look at very big molecules, were even thought of.

**VM:** It was an extraordinary situation that he had that hunch. The hunch was enough to mobilise the resources to get the machine.

**MF:** Exactly, and they were extremely expensive machines.

**VM:** So he was a very powerful person to have acquired such a device on a hunch.

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**MF:** Yes. I mean I think he was already someone who could get money without the sort of difficulties that most of us experience .

**VM:** Before we close, I would like to explore briefly what happened to the rest of your career. But are there more things that you would like to tell us that I haven't touched on...

**SM:** ...anecdotes, happenings, and things.

**MF:** They will come to me, of course, on the train going back home, won't they? If they do, I'll make a note.

**VM:** Or influences that the experience might have had on you.

**MF:** In the sense that I did stay in science, it clearly couldn't have been negative in any way. If it had been really negative, I'd have come home and gone into the family business, which my younger brother then did rather than me.

**VM:** Poor chap!

**MF:** He's retired relatively wealthy! There are anecdotes that were really to do with the skiing. The first time we went, you know, they all said "don't worry, we'll look after you" and this, that and the other. We got out of the car in wherever it was (Tahoe or wherever) and there were a lot of people, several carloads, and within seconds they had all disappeared except one who was delegated to look after us, get us into our skis, one discovers all these terrible things like there's no flat ground anywhere so as soon as you are standing on the skis you start moving, these sort of things. The thing that struck us as unbelievable was, first of all, originally that they got up at four o'clock in the morning or maybe earlier, to go and the second, within seconds of arrival, they were off! There was no sort of hanging around for a hot drink or anything like that. The enthusiasm was incredible. That really did strike us as quite strange. The other thing with the group was how sociable they were in that at the drop of a hat, they'd have a party, where we'd all go out for lunch together and this, that and the other. I thought that was very different to Oxford.

**VM:** And to Harvard or not to Harvard?

**MF:** I think Harvard was...yeah...I was with...I think the difference is kind of hard to say because, in Harvard, I was an "older" member of the group whereas in Berkeley I was a very, very young one. In Harvard most of the people in Kistiakowsky's group were research students working for their doctorates and I was a postdoc. and there weren't too many of us. So it was different.

**VM:** OK; let's expand a bit. Do you think the social atmosphere was unique to the (*Calvin*) lab. or was it a Californian phenomenon inasmuch as you could see, or was it an American phenomenon? In your later experience, can you narrow it to any extent ?

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**MF:** No, I think it was a mixture. I couldn't see it happening...well, I think it required Calvin for it to happen. He was an integrating factor. But, of course, California was California and that was helpful, and America is America and that was helpful. In the absence of Calvin, there would still have been something; I doubt whether such a diverse group would have acted quite so much as a group without someone like Calvin. As I said before, he was the guy who actually knew everything that was going on. He didn't have...In a sense, of course, there was a hierarchy, but the hierarchy split very early on so it wasn't linear. Therefore, you did need someone like him. If it's linear, he doesn't have to know necessarily, someone else does. But he was the only one. I'm sure that whoever led the ORL people and it would be Lemmon...

**VM:** No, the ORL people was Benson and then Bassham.

**MF:** Sorry, yes; OK. I don't think they knew a great deal of what Bert was up to. I'm not sure that Bert knew a great deal (*about ORL*). But Calvin knew what everybody was doing.

**VM:** Several times you have said, and clearly we all recognise this, that Calvin was a great integrator. Was he also an originator?

**MF:** Oh, yes, I think so. In the sense, as we had said about NMR, why on earth he should have gotten...it can't just be because there's a new technique.

**VM:** No. There's a difference between appreciating a possibility maybe he heard from somebody else versus the sort of concept that he presumably had at the beginning of the photosynthesis thing, where he saw an opportunity and he saw how you could take this opportunity forward with  $C^{14}$ . That clearly is an original concept, the leap forward.

**MF:** I think that's right. I am too far away to see most of the things that he was very good at. But when he was young, he had some very nice work on triplet states which is totally different and, for me, a very exciting thing when I discovered it some time later, his name on a paper that I had looked up for a very different reason. I would never have known that. Of course, it became very important in all photosynthesis and in all photochemistry. Yes, I think he deserved his reputation.

**VM:** We're almost at the end of the...well, we can go on a little bit longer; there's still a few minutes. Having spent your nine months with Kistiakowsky, as you say, you then had determined that chemistry was going to be it.

**MF:** I don't know. When I was with Kistiakowsky I very much enjoyed what I was doing. Again, I was very largely master of my own thing. That's much easier in a university where the groups tended to be much smaller and nobody else was doing work on methylene which is the work I was doing. Ken Sauer was doing work that involved gas chromatography and I took that technique over from him and developed it in a rather different way and applied it to methylene. Now, the previous work on methylene had been done by a chap called Bruce Mahan who actually went as professor to Berkeley and died quite young, but a very distinguished chemist. Again,

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unfortunately, I had the horrid job of showing that some of his stuff was misinterpreted. But it was great stuff that I did and the two papers I published — I don't think Kistiakowsky's name was on either of them; it has thanks to him — were certainly among my best papers.

*Tape turned over*

**VM:** Yes; you said Kistiakowsky's names were not on the papers.

**MF:** I don't think they were. I would have to look back: it might have been on one of them and not on the other. And then I gave a seminar on the work in my last few days in Harvard and I remember Bob Woodward came and said to me afterwards that it was the best stuff he'd heard from a one-year postdoc. in a very long time. In fact, it was quite extraordinary in a way because I had applied...

You see, by then I had applied for posts in England. I was still talking in terms of "let's try it for a couple of years and see what it's like."

**VM:** Where?

**MF:** Well, I was saying that to my wife about coming back to England. "Let's try a post in university". At that time they weren't that hard to get, I suspect. I applied to four universities. One turned me down flat, that was Reading so I was quite pleased to go back there as a professor in the end; but, anyhow, that was the only one that turned me down. The other three places were Southampton, Liverpool and Newcastle.

Southampton offered me a lectureship sight unseen by telegram. That was really terribly awkward because I wanted to see what the other places were like, etc. So I said to Kistiakowsky, "couldn't we just pretend that this telegram hasn't come?" Kistiakowsky said "yeah, no problem, I will just put it in my drawer and send it to you in England". One mustn't do things like that because I got into a slightly tricky situation. As I say, Bob Woodward had come to my thing (*seminar*) and wrote off to Cookson, who was the new professor at Southampton (who was a previous postdoc. of his), saying "take this man, Frey; he's good etc. I've just come from a seminar." I was due to say that the telegram, which had come two days before the seminar and Kistiakowsky was to write and say that I had left for New York where we did spend a week. So there was a minor frisson there about timing. But anyhow, it didn't turn out to be bad in the end.

When I came back I then was due to be interviewed in Newcastle where they had kept the job open for about six weeks — they had interviewed everyone else — and then the Liverpool job, by the time... what Liverpool wrote and said was "terribly sorry, but by the time your application came, we had already offered the job". By the time I got to London they phoned me to say that Barry Tracknell (*spelling?*) said he is leaving and you can have the job. AND Then I went up to Newcastle, and I thought of Newcastle as being in the Arctic Circle or something like that; it turned out to be really a very nice town, I was quite surprised. They subsequently offered me a job. so



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I really did, in the end, have a choice of three places. I chose Southampton because it was the closest to London, to be perfectly honest.

**VM:** Are you a Londoner?

**MF:** Yes. Basically, we said we have come back to England because our families are here. It would be very strange if we then go and live a long way away. So I took the Southampton job, which was the worst paid of the three, I remember. The work I then went on was a continuation of what I had started with Kistiakowsky and it was fairly important.

**VM:** How long were you there?

**MF:** In Southampton? I was eight or nine years: '55...'57 I got there: nine years.

**VM:** And did you go to Reading from there?

**MF:** Yes.

**VM:** Where you've been ever since?

**MF:** Mm. Again you see...and gradually it became clear that I was going to stay in academia and not go into the family business; it became clear to my father, anyhow. He noticed.

**VM:** ...who offered the succession to your brother at some stage.

**MF:** Yes. I can remember after I had been at Southampton six or seven years, at that time there was none of this — “you're good, we'll make you a personal professor”. So I remember putting a compass on London and drawing a circle around it and then looking at *Who's Who* at the ages of professors of physical chemistry and discovering there were only two places I could go to outside London and I knew I couldn't go to London because we could never afford to live in London. Just because having got used to living on the salary we had outside London, the prices here were just so much higher that we thought we couldn't possibly survive. The two places were actually Sussex and Reading.

**VM:** You said...you told us a little while ago that when you left Kistiakowsky's lab. you were coming back to England for a couple of years to see what it was like and you never went back to America. Why not?

**MF:** Oh, I think...first of all, we came back for family reasons. Then we started our own family. I think it was the thought...I think it was probably combinations, partly the education system — we wanted them educated here — and things grow on that and I think you make excuses. So probably we really didn't want to go back even though we thought it was fantastic. I think we must have made excuses because I did have offers.

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**VM:** Have you been back to America?

**MF:** Many times.

**VM:** Now looking at it from the viewpoint of later in your career, forty years later, forty years after that, did you make the right decision? What does America look like to you now?

**MF:** Yes, that's interesting because, you see, even though I wanted to go to America, I was very anti-American before I went. I'm not sure that all young people at that stage weren't.

**SM:** You read the *New Statesman* faithfully?

**MF:** Well, I certainly was fairly left wing then and I gradually got more and more right wing, which is kind of funny, because when Kistiakowsky came over some years ago and I introduced him to someone, they said he was the only man they had ever met of distinction who had moved steadily left in his lifetime. I think the nice thing was we went pretty anti-American and we came back incredibly pro-American and spent a lot of time being very irritated with people (a) who didn't want to know about America. That's another thing about this country. You come back full of enthusiasm and you are welcomed back and great, and now can you please forget the last two years. We all go through that. I think that was very important for me that I actually became that because a lot of the science was there and one recognised that very early on. The number of times I went back to conferences in America was more than the number of conferences I went to in England and Europe, I suspect.

**VM:** Did you never go back for a sabbatical or an extended visit?

**MF:** No. I think if I had to run my career again that's the big change I would make. I think I suffered in all sorts of ways through not doing that, not developing the new techniques, which I should have done and all the rest. That was an error.

**VM:** Very briefly, what sort of chemistry have you been doing these last forty years?

**MF:** I went on, and on and off I have done methylene chemistry which I started with Kistiakowsky even to about five years ago. But then I went...Interestingly enough, Hinshelwood was famous for unimolecular reactions and I did nothing like that with him at all. But I then switched to unimolecular reactions after I came back, because of all sorts of reasons. That's really what I have done for an enormous amount of time. And then I did photochemistry. Quite interesting in a way, that I did do photochemistry because it was the early work that Calvin did and a lot of his photosynthesis derived from that, yet I never did any of that with Calvin. So, you know, whether there was some subtle effect, I don't know because there was nothing anywhere else for me to do some photochemistry.

**VM:** At the time you were with Hinshelwood had he by then acquired his biological interests?

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**MF:** Yes.

**VM:** But you were not part of them?

**MF:** No. Hinshelwood had two groups and they never discussed anything. We discussed things very rarely, I should say, as a group in the gas kinetics group. I can only remember twice ever in the whole two years having a group seminar with Hinshelwood. Only twice, in his room; he gave us sherry at the end.

**VM:** Was Alistair Dean there at the time?

**MF:** Yes.

**VM:** You knew him personally?

**MF:** Yes. I knew him very well but we never talked about his bugs work at all. But, of course, that was before I went to America as well. No, we never discussed the bugs work. It was a smaller part of the group; there was Alistair and two or three people who did bugs.

**VM:** It's interesting to think that if you had discussed the bug stuff and you'd been more familiar with it, you as a physical chemist, the different attitude you might have had to some of the work going on in Berkeley.

**MF:** Absolutely, it's quite extraordinary to come from that group and not carried any of it over. One was very isolated in that sense. As I say, you couldn't imagine Calvin running a group, even the size of Hinshelwood's, which, of course, was much smaller, and not having seminars.

**VM:** That's one of the things I found, and who came from England, so extraordinary about Calvin's group — maybe every other group in America but that was the one we saw. The breadth of interest and the interest that individuals showed in other people's activities. Something I had never known before and other people also hadn't known it. In a sense you hadn't experienced it either in Oxford.

**MF:** The two seminars we had in (*Hinshelwood's group*) in two years, one a year (not excessive), didn't involve the bugs people at all. I think we only had them because someone had run into real difficulties and Hinshelwood thought it might help if we all discussed it. It was so rare that I can remember one of them and what the problems were and the discussion and how intolerant I was.

**VM:** There's only one other thing, really, I'd like to put to you and I'm not quite sure whether you'll have very much to say. Many people that we have talked to have commented about the influence of that building (ORL) on the way the Calvin group operated. Did you observe enough of it to be able to get a sense?

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**MF:** Not really. I have to...the only thing I can remember, and this is, as it were not answering your question but maybe is relevant, is that when I went to the opening of the new lab., where the whole thing was built on a shape, etc. for interactions, I think I said to someone there “well, it didn’t seem to be necessary in ORL to design a building in this way to get people to interact”. It may have had an influence on me indirectly; I don’t know. It’s hard to bring all these things together. But when I first went to Reading, there was a sort of student magazine which was in its second year and as the new professor I had to do an article. They had just moved into a brand new building. I think I was not scathing but they were all head over heels about this new building, and I can remember very clearly saying that people were infinitely more important than the building. The people we appointed and how we then did is what would be remembered and not this smashing new building. You could do good work in a rather poor building and rotten work in a good building; it wasn’t a reason to have poor buildings! But I’m not sure that this didn’t have something to do with ORL because it wasn’t a great building physically.

**VM:** No, it was a building, however, which was moulded to fit the Calvin group’s mentality.

**MF:** I guess it might well have been. I think in that sense we thought Donner was very different. It seemed to me the “temple of togetherness” that was then built afterwards was a spanking building and it was based on a philosophy, but it never worked, I think, quite as well.

**VM:** Well, You are right. It was an interesting dilemma. The old building had been pulled down. Calvin succeeded in getting money to have a new one which he built. After all, there was this glowing, glorious memory of the old building which grew with each succeeding month after its demise. Here we were struggling to design something; we didn’t wasn’t to design a box. And so there was a distillation, we thought, and that was the best we could do in an attempt to recreate something on a different scale, in a different style but we weren’t going to rebuild the old building.

**MF:** And you see, there are some things that come into mind. You talk about effects on people, how has this influenced you and how’s that. There are also other things that are almost trivial but affect people’s lives enormously. Ken Sauer went there. When Ken was at Harvard, one of his pals was...I mean, one of the other people doing a PhD was Fred Tabbutt who subsequently actually came and did a couple of sabbaticals with me and that sort of thing. He had a daughter (now she wasn’t born in England. One of their children was born here while they were with us but she was the eldest) and she went and did a PhD with Ken Sauer.

**VM:** In that Round Building?

**MF:** Yes. Now that’s affected lots of people’s lives in these extraordinary ways. That happens a lot. There’s an enormous amount of scientific families, as it were, that people sometimes forget about. But they are enormously influential. So, that I have all sorts of other connections like one of Kistiakowsky’s students, long before I was there, was Sid Benson who went to UCLA, no University of Southern California and

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then SRI, and of the students from Cambridge who worked with Sid and then came and postdoced with me and is now a professor at Reading. Now, that has only come through because I did this and this, that they have come. They haven't said "Oh, you know; Frey is a distinguished chap, go to him". What they have said is "I remember Frey, he worked with so-and-so; I was with so-and-so; he's going to be all right; why don't you go to him?" There were a lot of Calvin people like that.

**VM:** That is interesting. I think...have we finished? I think we've probably more-or-less exhausted the subject, unless there's something you can think of.

**MF:** No, as I say, if I can think of any anecdotes, I'll let you know. The snag, of course, is that it's a hell of a long time ago now.

**VM:** Well, I'll turn it off now.