Chapter 2

GRANT BUCHANAN

Bath

March 27th, 1996

VM = Professor Vivian Moses; GB = Grant Buchanan; SM = Sheila Moses

VM: This is a conversation with Grant Buchanan in Bath on March 27th, 1996.

Can I start by asking you how you came ever to be in Calvin's lab., what your background was and what was the chain of events which brought you there?

GB: In early 1951 I was a research student in Cambridge, England, working on vitamin B_{12} and I was starting to write my thesis and so on and I heard from A. R. Todd, who was my supervisor, that he'd had a letter from Calvin asking if he had somebody he could recommend to go to work with him and he asked if I would like to go. So I said I would and I applied for some money (I think it was through the US Public Health Service or some such organisation) which was unsuccessful, in fact. But then Calvin that said he would organise something and Todd said that he could come up with my fare and so I was going to work with Calvin. But also in that lab was Charles Dekker who was working in the area of nucleotides.

VM: In Todd's lab.?

GB: In Todd's lab. And he had an offer of a job at the University of California in Berkeley. He was married, he had two small children and he had to get over there. He also had a car, an English Austin A40, and he aimed to drive across from New York to San Francisco.

VM: He was going to take his car from England?

GB: Yes. So to cut a very long story short, that was how I actually got to California. We dropped his wife and two children off halfway over, and Chuck and I carried on and we arrived in Berkeley in mid-October in the lab. Calvin obviously felt I ought to have arrived on the first of October and he was a bit, I felt, upset but he was very friendly. I was going to stay in International House, which I had already organised,

and he came up with an instant loan, as I recall of $100, to get me through until I got everything organised.

I would like to return to Cambridge. In the work I had been carrying out I had carried out a lot of paper chromatography. In fact, in the Cambridge lab. I was the first in the organic chemistry lab. to actually carry out paper chromatography which I had learned from Sanger and from Partridge. The other aspect of that work was I had really hardly any experience of making compounds and handling crystalline compounds except as an undergraduate. So I was really highly competent in a certain area and rather incompetent in other areas. When I'd got settled into the lab. (into ORL) I was given the problem of carrying out some degradations on sedoheptulose.

VM: Can I ask a question — when you say when you got settled in, what was it like arriving, when you came in through the front door? Had you already spent the night in Berkeley? Had you gone first to International House?

GB: No, we actually drove straight on to campus and I just went into the lab.

VM: Whom did you see, do you remember?

GB: Calvin, himself, was there and there was a sort of friendly atmosphere but I honestly can't remember who else I saw at that point.

VM: Do you remember what impression that building made on you when you first saw it?

GB: Well, it was light and airy, and there was possibly slightly an element of pre-fab about it. It was all wooden but I wouldn't like to...that's the overall impression I had of it.

VM: When you arrived and you walked into the front door and you saw Calvin very quickly, did he immediately start talking about what you were going to do?

GB: (Laughter) I honestly can't remember that. We had come from the north, from Sacramento, and we'd had a very heavy rain coming over the Sierra and we had travelled a long way, as you will gather and I can recall him almost instantly writing his cheque for $100, which was very useful.

VM: And significant in those days, wasn't it?

GB: Oh, it was, yes.

VM: This was 1950, was it?

GB: No, this was October '51.

VM: You then, presumably, settled into your room at International House?

GB: Yes.

VM: And then you came back to the lab., perhaps the next day was it?

GB: I honestly can't remember that. I had a nice open bench and the recollection I have is that in Cambridge the benches were all of wooden tops. These were black, shiny, plastic ones, obviously aimed to clear up radioactive stuff and so on.

VM: When you first began to talk to people about what you might do, did the inspiration come primarily from Calvin or did he send you around to talk to everybody else?

GB: No. It actually came from him. What happened is that they were interested in finding the order of the labelling in sedoheptulose and relatively recently Richtmeyer had come up with a proper structure for the anhydride of sedoheptulose which is a highly crystalline compound. It was a question of converting sedoheptulose phosphate into the free sugar and then acid on it would form the anhydride, and carrying out some periodate cleavage on the anhydride in order to get out each radioactive carbon atom. I realised quite early on that I would have to learn quite a lot in order to achieve this. It was really not the kind of chemistry I had ever done and I would have to go through a learning process in order to do this.

VM: Presumably, when you first got there you were not very familiar with the whole photosynthesis project as an organic chemist?

GB: Oh, I was, yes. I had read into it quite a lot. As an undergraduate in Cambridge I had done Part I Biochemistry and I actually knew quite a lot on the glycolytic cycle, and so on. I was actually quite well into that. Also, in the lab. in Cambridge I was on a floor where Charles Dekker and also, importantly, Khorana and Dan Brand, and I was into the chemistry of phosphates and sugars and I was actually quite into that. In fact, when had I applied for a fellowship to go to Calvin to raise some funds I had had some ideas on how to get hold of some of the reactive compounds. At the time I went they were talking of a two-carbon fragment which was going to react with carbon dioxide to form the PGA and I had some ideas of how to get hold of the reactive fragment. I was actually reasonably into the enzymes which were involved in the various transformations.

VM: As you can remember it, in the lab. in which you worked, who were on the neighbouring benches?

GB: Opposite, through the reagents, was Al Bassham. I'm not sure who was directly behind me, I think it was empty, and on the other side was Vicky Lynch.

VM: At the time when you came there was the big white table in use already for looking at radioactive films?

GB: Yes, I think so, yes.

VM: As a social centre did people congregate around this thing, drink their coffee and discuss the latest film, as it were?

GB: Yes, that's right, I have a recollection of that.

VM: How about the seminars?

GB: Ah!

VM: Yes, that rings a bell. What was it like when you were there?

GB: These were held in Donner at eight o'clock in the morning.

VM: On Fridays, was it?

GB: I can't remember the day of the week. All I know is that the people, in giving them, were really given a rough ride by Calvin. He was highly critical and wanted to know the exact evidence and details. He was obviously quite wide awake at that time in the morning.

VM: How much notice, at the time when you were there, did people have that they were going to give a seminar at eight o'clock in the morning?

GB: Well, I can't recall that, actually. And oddly enough I can't recall if I ever gave one or not; which seems slightly odd. I gave a seminar in chemistry in the Chemistry Colloquia on the chemistry of vitamin B_{12}. I have a recollection of that but I can't instantly remember having given a seminar in Calvin's group.

VM: In the seminar group at that time can you hazard a guess as to roughly how many people participated?

GB: Well, 12-15, but I haven't an impression of a crowded room or anything of the sort.

VM: How did people sit, do you remember?

GB: No, no.

VM: Not the individuals but generally speaking, what was the layout—did you all sit around a table or in a circle — what sort of set-up was it?

GB: It was a flat room, and the recollection, and I haven't any strong recollection of this, I have to say, is of us in chairs facing forward rather than round in a circle. But I honestly can't remember.

VM: Please, carry on with what you then did when you got stuck in.

GB: In general chat around the lab. I found that there was a drawer, or drawers, containing old chromatograms and these were complete with x-ray films. And also I heard that there were some unknown spots on these chromatograms. This intrigued me. These were experiments, I have to say, carried out by other people and I should say, now,

that I haven't ever carried out an experiment with radioactive carbon dioxide and a plant and a light source. I didn't ever use the lollipop set-up.

VM: But you saw it, presumably.

GB: Oh, yes. And I knew what happened. Anyway, there were these old chromatograms, and one of the unknown areas was in an area corresponding to phosphates. Also, I heard from Vicky Lynch that they had an enzyme which they used for stripping phosphates off sugars called "polidase-S".

VM: Yes, it was still in use many years later.

GB: She said that it contained invertase so that any sucrose phosphate there would have got converted into fructose and glucose. Anyway, I got hold of these old chromatograms which I assumed (I was not acting under cover or anything of the sort) were common property. Now, quite a number of them, I think, were actually done by Andy Benson or somebody, and obviously if he had been around I would have sorted it out with him. But these were in an open drawer and so, with Calvin's approval, I thought I would find out what was an unknown spot.

VM: Andy was away for the whole year that you were there, was he?

GB: I see in your list that there was somebody called Nordal. He was in Norway and Andy, I think, was spending a year with Nordal.

VM: And you were in the lab just a year, were you?

GB: Under a year.

VM: So you never saw Andy there at all?

GB: No.

VM: Have you met him since?

GB: Oh, I have, yes. So, I had a look at these unknown spots of which there were two of interest. I haven't got my lab. records here and I can't tell you the order in which it happened but the first one, I think, was a rather slow-moving area in the phosphate region. In order to find out if it had altered, if it had decomposed, I re-ran it on the usual system of two-dimensional chromatography and found it had, in part, decomposed.

Now, here is where an interesting thing happens. Around that time I got from the bookshop, I think on — what's the name of the main street in Berkeley?

VM: Shattuck Avenue? University Avenue? Telegraph Avenue?

GB: Shattuck, I think, the road to Oakland. And I got a book which was an account of a meeting held in early '51, in Baltimore I think it was. It was published by Johns Hopkins. It was called Volume I of "Phosphorus Metabolism." I have it upstairs, actually. I was reading the article by Leloir in this book and he was describing the chemistry of UDPG and he said that in the presence of ammonia it decomposed into a cyclic phosphate of glucose, and when I looked at what I had of the re-running of this unknown spot there was something carbon radioactive at a higher R_f which actually wasn't as high an R_f as a sugar and yet it was a higher R_f compared to a sugar phosphate. And I thought, no, I wonder if it is a cyclic phosphate. Again, I come to Cambridge in that I had actually isolated a phosphate component from B_{12} and I was well into R_f values and what happened when you got OHs and phosphates and influence on R_f value and I thought, now I wonder if it is the cyclic phosphate of glucose. And it was. So I went right into this area and got out from it a radioactive uridine. Oddly enough, in Cambridge I had had a spray reagent which was good for sugars, glycosides, and I was able to show that I'd got a radioactive uridine. Also, coming out of it, there was a radioactive inosine in that the polidase also has a deaminase in it — and all and all this was obviously a rather interesting area. From the sugars in the area after complete hydrolysis there was glucose, galactose, mannose and xylose. Obviously I was interested in the glucose/galactose thing in connection with Leloir's work but in the case of mannose it ought to have had a guanosine component in it which I actually didn't nail down. But it was very interesting in the light of Hassid's later work to have got xylose from there. So, this was, as far as I was concerned, far more interesting than trying to find the labelling in sedoheptulose.

VM: Can I interrupt you at that point to find out with whom you collaborated, if anybody, both inside the Calvin lab. and outside. You knew Hassid, presumably?

GB: No.

VM: Did you not meet him while you were there?

GB: Well, I saw him at seminars and things. But if I can continue on the thread of this: in order to sort out the structures, I was in correspondence with Leloir and he sent me a sample of UDPG. Unfortunately, while I had this correspondence with him, in Edinburgh I moved labs. in the early '70s and I had some very interesting letters with him and with Khorana and all sorts of interesting stuff and they are all lost. They got lost in a move and I've tried to find them, I just cannot.

VM: Leloir was in Argentina at that time, was he not?

GB: That's right. Anyway, the point was that he and his collaborators had discovered UDPG. That was his compound and so on. He actually found it because he was looking at a yeast which grew on galactose. So he found the co-enzyme for the interconversion of galactose and glucose, and this was a rather special yeast at that time. We had found it in an alga(e) and obviously, to me, and to Calvin when I told him, it had some rather more widespread use. I said that compounds of the kind of UDPG and also guanosine diphosphate mannose were concerned in the sugars

themselves undergoing changes and then transfer in an active form into polysaccharides.

VM: This was the point that Harry Beavers liked a lot, didn't he, in his comments in the annual reviews where he compliments you on the imagination...

GB: No, this is Axelrod...

VM: Axelrod and Beavers. And there is this comment in that paper on the "imaginative use..." He puts it something like that.

GB: Actually, there is something by Leloir himself. Here is an account of a meeting held in Argentina on the biochemistry of the glycolytic linkage.

VM: That's 1972, is it?

GB: It was actually '71, published in '72. This was held, I think, in connection with Leloir's Nobel Prize. He has a preface to it and in it he says that the first experimental information on the donor role of the nucleotide sugars came from the work of Dutton and Storey who, in 1953, reported that the glucuronide donor was UDP-glucuronic acid. As an aside, I would say that I was actually in London and I was working in London at the Lister Institute and working with Jim Baddiley, and he and I in '53 had the idea of this glucuronic compound and we called round at the Chester Beatty Institute and tried to sell it to them as an idea. Dutton and Storey's paper came out shortly after that. So, anyway, to return to Leloir's preface: it was also suggested in the Calvin group that UDPG served as a glucose donor for sucrose phosphate at synthesis and he carries on and Cannon and co-workers also mention that "compounds of the UDPG type could be concerned in the transformation of sugars and their subsequent incorporation into polysaccharides." So he realised it at an early date.

VM: In the lab, back in the early '50s, how much were you talking to the other people about what you were doing, other people in the lab.? Calvin and the others?

GB: Well, a bit. Apart from putting chromatograms onto x-ray films I was really not learning any new techniques. I was actually using what I knew to solve problems. Consequently, I happened to be asking: "how do your carry out so and so and so and so". I was really getting on with it. I wouldn't say I was anti-social but I worked flexitime. I wasn't in early in the morning apart from eight o'clock seminars. I was in late at night and I was quite friendly with the night watchman.

VM: Were you the only one in late at night or were there others?

GB: I was largely on my own. But I wasn't, I think, anti-social. It was that I rather liked the peace and quiet at night.

VM: I am sure you weren't anti-social. When it came to things like having lunch, what did you do for lunch? Did you bring sandwiches...?

GB: No, no. It varied. Early on I went to the Faculty Club on campus. They had some rather nice lunches there.

VM: As part of a crowd — or by yourself?

GB: Do you know it is terrible, I should remember these things. But early on I went over there and I met Rapoport and Cason but...what did I do for lunch...? I think I had it in I-House, actually. It was quite a short stroll up to International House. I am certainly not a sandwich person.

VM: I wonder what sort of social life you do remember about the place? Did they have parties? Did you go out on weekend trips with people?

GB: No. I hadn't a car. The social life I had was largely in I-House. We used to go across to San Francisco in the evening. Also they had an annual I-House festival of dance and I was keen on Scottish country dancing. In fact, I actually learned how to do it properly. There was a Yorkshireman who was called John Bull, of all names (he worked with H.A. Barker when he wasn't repairing old cars), and I actually learned how to do it from him and actually some English country dancing as well for the I-House festival. But I didn't go off skiing or into the mountains.

VM: Not at all?

GB: No. Through friends in I-House I had an Easter trip to Yosemite, which was very enjoyable, and Christmas '51 I was in Los Angeles. I had a relation who I was going to see and unfortunately she was unwell and I couldn't actually see her. So far as socialising in the lab. was concerned, I recall going to Calvin's house on an occasion, an evening or whatever, but I wasn't heavily socialising in the lab.

Something I recall in the lab. as a focus of attention was the coke machine in that we had quite regularly an afternoon coke which cost 10 cents at that time — I don't know what it is now — and of course it was all in bottles. On the underside of it. it had the name of a particular plant at which the bottling was done and there was a game which I can't instantly recall: you paid an extra 10 cents into a kitty, as I recall, and the person who had the place which was furthest away got the kitty.

VM: Yes, there were a variety of names at the bottom of coke bottles.

GB: Yes, that's right. The other recollection I have of the coke machine was a scare one day where there was some escape of radioactivity. There was a lab. quite close to ORL which had things going on and somebody had come through from it and there were radioactive prints all up to the coke machine and then away from it again. And they had to find out what had happened and so on.

VM: The lab. bench you had, was it one of those with a writing desk at the end of it?

GB: Yes.

VM: So you didn't have an office somewhere else.

GB: No, I hadn't an office.

VM: Did you have people visiting you at your lab. bench and sitting down at your desk and peering at the results and things of this sort?

GB: Only Calvin and he wasn't in every day but he was interested.

Running in parallel with this unknown area which I mentioned, and this was really working on a hunch, and having heard that polidase-S has an invertase in it, I cut out the hexose phosphate area and had a closer look at that. There was a solvent which I had encountered in Cambridge. The early papers on paper chromatography of phosphates came from C.S. Hanes and Isherwood who were in the Department of Botany in Cambridge and they published, in 1949-50, around then, a paper in *Nature* on paper chromatography of phosphates. They had a solvent for the phosphates which included picric acid in it, of all things, and I tried this solvent on this hexose phosphate area. But this solvent had been slightly altered by somebody else to contain half the quantity of picric acid and I got a new band which I cut out and freed from picric acid, treated it with the phosphatase, a real phosphatase, and got sucrose from it.

VM: Yes, you report that and perhaps it was in this *Phosphorus Symposium*.

GB: Yes, it is actually in *Path XIX*.

VM: Yes, that's right, that is the one I was thinking of. There was in one of these other parts that I noticed..., the one with the kinetics in that you did (*Path XVII*), where you commented in your letter to me that Calvin had... Were you party to these discussions about the kinetics and the early attempts at formulating the cycle?

GB: I wasn't, no. In fact, what I remember was that Calvin and Al (*Bassham*) — not, let me say Calvin himself, was very much into kinetics and pool sizes and such ideas and he felt if he could feed in the numbers he could come out with what actually happened. And I am quite sure if he had a computer he would have done just that. These schemes with how the different carbons get labelling and so on and so forth, I was actually not involved in. In fact, this, I think, was what Alex Wilson and also, I think, Peter Massini, were into — carrying out actual photosynthetic experiments and the rates of incorporation of radioactivity into the various compounds.

VM: So in a sense this paper was really written in two bits: you did the sucrose one, the sugar phosphate bits, and other people did the kinetics.

GB: That's right, I would think by Calvin, himself. I honestly don't recall exactly. All I know is...[*looking at the papers to refresh memory*]...that I wrote up to about...

VM: Most of it, by the looks.

GB: Well, it was. I mean, the only way kinetics came into it so far as I was concerned was Calvin pointed out, with hindsight, that the getting of the right degree of label into the sucrose phosphate actually agreed with it coming ahead of sucrose. But I am really inherently an organic chemist and I was interested in compounds and how the compounds got there and I was actually not part of that ...

VM: ..."that" is the early formulation of the cycle?

GB: One of them, yes.

VM: Just so I don't get confused, that is *Path XVII*.

GB: Yes. If I could return to the sucrose phosphate. It had an unhappy ending so far as I was concerned. I returned from an Easter holiday in '52 in Yosemite and found a letter from the Draft Board saying that I had to report and have a physical examination.

VM: What was your status with the draft?

GB: Well, I was on an Immigration Visa through bad advice and for no other reason. When I'd been contemplating going to the States, when I was in Cambridge, I hadn't got the Fellowship I had applied for and Calvin had offered something and I hadn't any idea how much it would be. I thought I might have to take some teaching when I was there in order to make ends meet. A friend, I won't say who it was, who had spent some time at Sloan-Kettering in New York and was recently back from there said, "If you want to do that you have to have an Immigration Visa."

VM: You would have been in your mid-20's or so at that time?

GB: Yes, I was born in 1926. So as far as I was concerned, it was only a kind of visa, without realising any of its implications at all. So, anyway, I had a trip to the US Embassy in London and a medical and fingerprints and heaven only knows what, and I got this visa. I arrived in New York and as I went in I was told that I had to register at the Draft Board within 6 months, which was the first slightly ominous thing to hear, which I did on the stroke of 6 months. Shortly after Easter '52, I was informed. So I had this medical and an intelligence test and I was graded A-1 and was going to be called up within 3 weeks.

VM: Within 3 weeks!

GB: Yes. The Korean war was on. But it was a rather shattering blow. I was still, of course, liable for a call-up in the UK. I wasn't running away from anything. So, at this rather fraught time Calvin was very helpful. Actually I recall also going to the British Consulate in San Francisco who were very unhelpful and who felt it was all my fault and they were very unhelpful. Calvin wrote a letter to the Draft Board giving all the circumstances and as a consequence I was given leave to go on holiday to England (*looking at the* letter: I see the Calvin letter is the 7th of May 1952). I left the

lab. in June with a couple of English friends and we drove eastwards — we drove south and then we went up into Canada and then we ended up in New York and I sailed home, having sorted out the income tax, which is the final thing you have to do when leaving the States, on the Queen Mary early in August '52.

VM: When you say the Draft Board gave you permission to go on holiday in England: was it a qualified permission on the condition you came back?

GB: No, it was unqualified. I got the impression they had relented and this was as far as they could actually have it in print. I returned to the States for some job interviews and things in 1959 and I was slightly apprehensive that I was on some black list when I was going in but I haven't had any further problems so far as that is concerned. What annoyed me slightly was that some time afterwards I was in Cambridge and I saw Todd and he obviously knew about it and he thought I had been silly or messed it up —anyway, he was slightly grumpy about the whole episode. At any rate, that was what happened.

As it affected sucrose phosphate, as you can realise, I was sort of under some pressure at that time. The problem was, of course, that sucrose has got eight hydroxyl groups and the question is which one has the phosphate on it. This would only come from an acid hydrolysis of the phosphate to see which half the phosphate stayed with. And I carried out the acid hydrolysis and had in it a carrier of fructose-6-phosphate. I think I already had evidence that it was on the fructose end and I wanted to clinch that it was on the fructose-*6*-phosphate. So I ran the chromatogram and left it on x-ray film and off I went (home). As you recall, it requires a few weeks in order to get an x-ray spot so Al actually did the spraying after 3 weeks or however long it was. It looked as if it wasn't the fructose-6-phosphate. It wasn't a terribly clean picture but it really looked as if it wasn't the 6-phosphate so I thought, well, perhaps it was the 1-phosphate, which was the other known one. When I wrote it up after I got home I opted for that but it was a rather messy picture and I think, if I had actually not had to run away and scamper home, I would have got something slightly more clear. Anyway, it turns out, of course, that it *is* the 6-phosphate of fructose and later on I actually synthesised that compound when I was in Newcastle and it is actually in the Sigma catalogue from our route.

VM: And that is this paper here in *Carbohydrate Research* '72.

GB: That's right. But what I feel, well: this *Path XVII* came out as Volume 2 of the *Phosphorus Metabolism* which again took place, I think, in Baltimore, or wherever it was, through the Johns Hopkins Press. Now, Calvin went there and he would actually have been putting forward our paper. I have no idea what he actually said at that meeting, I really haven't. Here I would like to say of this whole work I carried out in Berkeley: I recall in the lab. one day a chap, Russ Bean [??] from Hassid's group coming in, and he had one of the Calvin-type chromatograms and he was having a look at the biosynthesis of a compound called fluoridicide, which is a galactoside of glycerol. It is from a seaweed and his chromatogram obviously had a whole heap of salt in it which was altering the R_fs in the phosphate region. And I said to him: "If

you look in there you will find fluoridine diphosphate galactose which is going to end up as fluoridicide."

Now, I think, after I left, Calvin was inherently not interested in this thing in that nobody took it up after I left. Andy had returned; I had an impression from Alice (*Holtham*) who I saws in London, that Andy had been slightly put out that I had done all this. I'm really not sure. But the upshot is that neither Andy nor anybody else actually followed up this work either on the sucrose phosphate or the nucleotides, and the people who really got into it was Hassid's group. And they came into it as a direct consequence of me, I would say.

VM: I guess Hassid's group was actually a dedicated carbohydrate group, wasn't it?

GB: Yes. But you see also, Hassid had got off on a wrong track on the biosynthesis of sucrose. He reckoned it came from glucose-1-phosphate. He had an active group. There are several names, Neifeld {??} and a whole crowd of them, some of whom are at NIH now, and I think, as I saw it, they found a rich source of these compounds from mung bean and they really did it properly. Now, as I left them, I do think that Calvin, if he had felt like it...he had this link to Leloir (through me) and as you probably realise it is a huge area and all polysaccharides come through this route.

VM: I think it was just a little bit too early in the life of the lab. in the sense they were still very dedicated to unravelling the primary thing of photosynthesis and it wasn't until 2-3-4 years later that they really began to take a greater and greater interest in the spin-off activities.

GB: Yes. I would go along with that.

VM: (We have probably ten minutes left on this one.) When you left there and came back to Britain, briefly what happened to you in the next 40 years?

GB: Oh!

VM: Well, in a few minutes. And if you could also tell me what contact you subsequently had with people from there and, if you like, what affect it had on your life.

GB: Well. After coming home, I was in London and I had some regular correspondence with Calvin over the writing up of the papers: *Path XVII*, of course, had already happened. That was still in '52 and he was very keen to write up everything. *Paths XVIII* and *XIX* originally came out as these reports for the Atomic Energy Commission or whatever it was.

VM: They were called "UCRL Reports".

GB: That's right. And in fact, I have some of them, I hope, still in Edinburgh. So I had some active correspondence over those. Originally, *Path XIX* had got several authors on it. In particular, I felt that Vicky Lynch had been extremely helpful over the actions of polidase-S. I ended up as sole author. Either he felt it was well done or else

he felt he would rather not have his name attached to it, I'm really not quite sure. Anyway, I was rather pleased about it.

VM: Well, it is a good paper. He did see the manuscript before it went out?

GB: Oh, yes.

VM: I notice you thank Al Bassham and him as well as Vicky Lynch. And then you went to Lister?

GB: Yes. I was with Jim Baddiley. I was hired to work on the synthesis of coenzyme A with Malcolm Thain. This is how I met Malcolm Thain. It was really through my connection that he and, I think, also Rod Quayle, went to Calvin. I had known Rod in Cambridge. He was doing his second Ph.D. in Cambridge.

VM: His second? Does he really have two Ph.D.s?

GB: I think he does.

VM: I'll ask him when I see him.

GB: I'm really not absolutely sure about that. He had started off with Hughes of Hughes and Ingold in North Wales and he has gradually gone from a physical organic to organic to...how he ended up.

So, I was in London for 2-1/2 years. I actually met Sheila in London — we were in the same tennis club and we met up there. Jim (*Baddiley*) got the Chair in Newcastle and I went up with him and worked on nucleotides with him and we sorted out the structures of cytidine diphosphate, ribitol and glycerol and got into dichoic acids and antigens and that kind of chemistry, and I also got into some sugar chemistry. I got a Readership in 1965...

VM: In Newcastle, was that?

GB: In Newcastle, and went to Heriot Watt as Professor of Organic Chemistry in '69 and was eventually Head of Department in Chemistry in the last four years I was there. I was largely involved in synthetic organic chemistry of a class of compounds, C-nucleosides, and I also worked on their biosynthesis.

VM: What are "C-nucleosides"?

GB: In which the ribose is linked to carbon rather than to nitrogen. These are actually naturally-occurring compounds. Some of them are antibiotics and antiviral agents.

VM: Nothing to do with nucleic acid?

GB: Well, in a way, they have that kind of look about them. But also I got into some carbon NMR in relation to biosynthesis in that, of course, you don't have to have it radioactive now which is rather easier in chemical time, anyway.

VM: (We've got probably just a few minutes left.) Do you feel you can look back and say what this nine months actually did for you?

GB: Well, yes. It was a very enjoyable experience. Calvin, himself, was an extraordinary man. As I said in the write-up I gave to Calvin's volume, I appreciate it that he gave me my own head and didn't say I ought to have been continuing to carve out sedoheptulose. I always have liked solving unknowns and problems and things rather than hacking my way through something.

VM: So you had plenty of opportunity of doing that.

GB: And I had that, yes.

VM: And what sort of contact have you kept with the people.

GB: Well, I saw Andy — he was in the UK in the '60's and he came to see us in Newcastle and he actually stayed with us in Newcastle. I felt if I had seen him at the time I was in Berkeley I would have enjoyed it.

VM Have you seen Calvin at all, ever?

GB: No, I haven't.

VM: Why not?

GB: That's wrong. He was in England for the Centenary Lecture of the Chemical Society in 1956 and he came to Newcastle and he gave his lecture, actually, at Newcastle.

VM: Yes, I remember that.

GB: And I saw him then.

VM: But that's been a long time, that's 40 years ago

GB: Yes. I wrote to him. He was in Oxford in the '60s. I wrote to him and asked him if he could come up to Newcastle and he wrote and said he couldn't, unfortunately. But at the time we were in Berkeley in '84 Al, I think, was on holiday and I didn't see him. I would have liked to have seen him. But I haven't seen Calvin himself since '56.

But turning again to this phosphate thing, as I pointed out in the letter I wrote you, even in his Nobel lecture he had the wrong structure for sucrose phosphate and it was quite clear it was wrong, you know.

VM: That Nobel lecture was put together very fast, actually, and one of the main difficulties was that apparently protocol demanded that it absolutely must not over-run time and Calvin was awful when it came to over-running. I was one of the ones who helped organise him and make sure he didn't and his wife was sitting up in the projection box almost conducting what he was saying. But anyhow, there really wasn't a lot of time, I think, the time he would have liked for his lecture.

I think we have just about got to the end so I would very much like to thank you for all your reminiscences. One thing I forgot to do is to bring a camera so either I'll come back and take a picture of you some time or if you happen to have one or can find one...

GB: Actually, I have had a recent ...

VM: I think we might as well turn it off now.