The Royal College of Physicians and Oxford Brookes University
Medical Sciences Video Archive MSVA 128

Lord Phillips of Ellesmere, KBE FRS in interview with Dr Max Blythe
Oxford, 29 April 1996, Interview III

MB David, at the end of our last interview we got you to Canada. You'd arrived in Quebec, crossed the Atlantic, and you were about to take on a post, a fellowship, at the National Science Research Laboratories in Ottawa.

DP The National Research Laboratories in Ottawa, that's right, yes. So we landed in Quebec and looked at the sights and got on the train and went to Montreal and then up to Ottawa. When I say we I mean myself and the other two new postdoctoral fellows that had travelled across the Atlantic with me.

MB Oh, so you'd met up?

DP I'd met with two other people who were going to the same place.

MB Because they were recruiting quite a lot of postdoctoral fellows?

DP It was a rather ingenious scheme the Canadians invented. It was probably a chap named AWR Stacey, who was a well-known physical chemist at the time, who thought we must get a greater flow of young scientists into Canada than Canada itself can produce, so why don't we have an international postdoctoral fellowship scheme. And it was advertised and lots of people came from all over.

MB And initially you went for one year?

DP Initially I went for one year extensible for two as a postdoctoral fellow, and in the event I was taken on the staff and stayed another two years after that.

MB You settled into Ottawa quite well.

DP Yes. Ottawa is a rather different city nowadays. In those days, in the early fifties, it had the atmosphere of a smallish provincial town with a provincial repertory company and no residential orchestra. There were visiting orchestras, visiting chamber quartets from time to time. Lots of emphasis on doing things yourself, playing tennis in the summer and skiing in the winter. They were the fixed activities virtually every weekend.

MB An isolated small community and we'll put the time on it, I think that was about early October 1951.

DP That's right, yes. One of my earliest recollections – and it sets the tone of the place – was walking up Sussex Drive. The laboratory that I was working in was a basic research laboratory on Sussex Drive, which was something short of a mile from the centre of town. The corner of Sussex Drive has another of the large railway hotels, the Chateau Laurier was a large railway hotel, and you turned the corner there and walk along the road and there are the Parliament Buildings. So, I remember one day quite early on in my experience in Ottawa walking up to town for some purpose round the corner past the Chateau Laurier and up Wellington Street towards the Houses of Parliament. And a grey-haired gentleman was walking down the pavement towards me absolutely by himself. It was the prime minister St Laurent. No security, nothing.

MB Different times?

DP Just living in a...

MB A remote, small community. David, you start work there. Who did you work for?

DP I joined crystallography, a research group in the physics division which was headed by a chap named WH Barnes, whose principal claim to fame was that he'd worked at the Royal Institution in about 1928 and worked out the structure of the principal form of ice, working with WH Bragg, of whom more anon no doubt. So, he'd gone back to Canada and been a professor of McGill for a long time and then moved to the National Research Laboratories and was heading up a laboratory in the physics division on crystallography. There I found a couple of postdoctoral fellows senior to me who'd come from Glasgow and another one who'd come originally from Pakistan via Manchester Tech, so it was a rather UK-orientated but a slightly cosmolitan place. Later on, a New Zealander and an Australian and various others came. It was essentially Commonwealth, I suppose.

MB And was there an open chance for you to start something that you were interested in or take from Wales what you'd been working on?

DP I began by working on projects that I'd brought with me from Cardiff. And the fact is that I worked for all the four years I was there mostly on those projects, but also on getting familiar with more modern equipment than had been available in Cardiff and in particular, playing a part in introducing modern digital computing into the crystallography lab. It was a period when computers were becoming, but only becoming, fairly commonplace. They were still mainframe computers in central labs which required a lot of programming and to begin with we still did the calculations in Ottawa by hand. But I was well aware from the papers in the journals that people were now using digital computers, so I persuaded Barnes that he should recruit somebody who'd been involved in that sort of work as a postdoctoral fellow. So, they recruited at my suggestion a chap named Farid Ahmed who'd worked in Leeds, but was originally an Egyptian and he'd gone back to Egypt from Leeds and I knew he wasn't particularly happy there. Barnes recruited him and he came to Ottawa and set about programming... And this shows how distributed the whole thing was in those days; the Ferranti computer was actually in Toronto, so all our punched cards had to go off to Toronto for the computations, but it made a great deal of difference to the sort of things we could do.

MB How did you come across Ahmed?

DP Well, I only knew him as a name in the literature who'd worked with Durward Cruickshank who was...

MB Quite a seminal figure.

DP Yes, quite a seminal figure and actually my exact contemporary. That is to say, he was born on the same day as me in the same year as I discovered later. But he very quickly built up a reputation in the late forties and fifties which I followed through his papers in *Acta Crystallographica* on topics, in particular, like how do you decide how precise a crystal structure analysis is, how well you know the positions of the atoms, that sort of quantative addition to crystal structure analysis. So, he was...

MB He led the way in that.

DP Yes, he did and he led the way into computing and Ahmed had done lots of computing with him in Leeds and that's how I knew the name. So, I wrote to Cruickshank, I suppose, I don't actually remember in detail and said who would you recommend to come and introduce us to computing and what about Ahmed who's gone back to Egypt. He actually took a Leeds girl back to Cairo as his wife and she was one of the influences that led them to think that maybe life in Canada would be easier for everybody. So, he emigrated to Ottawa.

MB Came to work there. I'm getting an impression of a laboratory that really was waiting for somebody to lay a hand on it and actually take it somewhere and that perhaps equipment also needed to be built up at that time?

DP It was well set up with what was to me a new sort of x-ray camera, a so-called precession camera which had been invented at MIT by a man named Martin Buerger, again I suppose in the late forties. And I knew about that from the literature, but I'd never actually used one and that was the principal tool at that point. We were not yet into the diffractometer period, so I never had a diffractometer to use in Ottawa. That came later. But cameras, computing, low temperature methods, I experimented with all of these things. Barnes himself concentrated a little on powder photography, which is a method of identifying characterising materials. And one of his sidelines, which he conducted with the local jewellers, was to distinguish between real and cultured pearls by an x-ray method which brought him in a little money on the side I think. But it wasn't startlingly novel science in any way, it was essentially a radiology method rather than a diffraction method.

MB You were working on acridines?

DP Yes, that's right.

MB And getting three-dimensional patterns?

DP Yes, that's right. I moved the analysis into working in three dimensions rather than in two dimensional projections, that is what does it really look like rather than what does it look like built up from what does it look like in that direction and that direction. So, one could make much more detailed precise analyses and the only problem was it involved making a lot of measurements and doing a lot of calculations.

Well, the computer solved the calculation problem, but the measurements were still made by taking x-ray diffraction photographs. And they were made with a different sort of camera, a so-called Weissenberg camera, after the chap who invented it, which one took in a variety of different ways and I'll come back to that in a moment. But, having got these pictures which were roughly transparent film with lots of black spots on them, one then estimated by eye how black the spots were and that was the basis of the measurement. Terribly crude, really.

MB Did you have a shadow chart alongside you?

DP You had a row of spots which were produced by exposing a pin hole to x-rays for different lengths of time; one, two, four, eight, sixteen, thirty-two, sixty-four seconds or something of that sort. And you ran this little strip alongside the spots and said ah yes, that's a sixteen or that's a two or that's a sixty-four or that's too strong to measure or whatever. And then one took the photographs in stacks of four films at a time, so the spots on the second film were weaker than the spots on the top film and so on. So then one had to work out what the ratio was between the films and then put together all this information. I have notebooks and notebooks full of jotted down numbers and then average intensities coming out at the end which had to be corrected and turned into the numbers which were then used to produce the images of the structure. It seems incredible now that all of that, the whole of the work that I did in Ottawa, except the experience I gained, was all the scientific structure analysis of two different polymorphic variants of acridine worked out in three dimensions which was relatively new at the time. All of that would be commonplace now and a graduate student could do it all in a month, if he wasn't working very hard.

MB And the spots you were seeing were x-ray reflections?

DP No, they were not representing the image, there's a stage between. They were representing the intensities of the x-ray reflections from which one had to deduce where the atoms were. I may have said this to you before, but it's very like an ordinary microscope. If you have a source of light and an object, normally you then have an objective lens and an eye piece lens and you just look at the image of the object. But if the source is actually x-rays and the object is a crystal, then you've got no objective lens to focus the x-rays, so you put a film here and you record the diffraction pattern. Now, what the objective lens does is focus the diffraction pattern to form an image. But instead of that, you stop short at a film of the diffraction pattern and now, in recording the diffraction pattern, you record in effect the amplitudes of the diffracted x-rays, but you don't record their relative phases. You've lost that information and you have to recapture it somehow. So, the whole business of crystallography is concerned with how you fill the place occupied by the objective lens of an ordinary microscope really.

MB But, I was thinking that it's like a beam going through a woodland and being bounced off various trunks and you were making sense of that, in a way?

DP That's right, exactly, the various beams interfere to produce this diffraction pattern. It's become just a routine laboratory tool now.

MB Yes. But, at that time, you were building equipment and you were getting into diffractometers as well, I think?

DP Not quite then. I was interested in the beginnings of that and on my first visit to the States, I went to visit a laboratory at Brooklyn Polytechnic where one of the leaders in crystallography at the time had decided that he would go into studying proteins. He realised that this photographic method of measuring the intensities simply wouldn't do and that he'd have to go into a new method. So he developed a three circle, four circle x-ray diffractometer which was really harking back to one of the original machines which WH Bragg had developed in 1913 or something like that, from an ordinary optical spectrometer. So again, you had a table which had the crystal on it. In an ordinary optical spectrometer you would have had a light source and a telescope with which you looked at reflections from a crystal or light refracted through the crystal and so on. Bragg had an x-ray source and a crystal and over here an x-ray detector which, at that time, was a gold leaf electroscope or something terrible like that which was extremely difficult to work, but never mind, that's how they did the original x-ray studies. Well, the new diffractometer had the x-ray source – a circle that the crystal sat on which would have rotated this way, that on itself sat on a circle which would have rotated this way – and the x-ray detector which by now was a geiger counter or proportional counter which went round in the circle this way. So it was a three-circle diffractometer, and Harker was developing this for protein studies. We never got round to having one in Ottawa, but when I moved to London as we shall discover, I got into exactly that.

MB So, your mind was marking this development?

DP I'd been noting that development, yes.

MB David, you published in those years. You published some papers on acridines and also on technology advances?

DP Yes. There were technical problems obviously associated with this visual estimation business. One of them was that as you took different sorts of photographs with a Weissenberg camera, according to the geometry, some of the black spots got pulled out into long thin lines and others got compressed into smaller and smaller black spots until they turned in on themselves and started to expand again, so that was a well-known phenomenon and it had rather discouraged people from collecting, as we called it in those days, three dimensional data. So, I worked out geometrically what was going on in this elongation and compression of the spots and produced a formula for how you correct the intensities if you estimate them from Weissenberg photographs and this was published. It was quite quickly taken up in the field, except that the take-up of precession cameras and the beginnings of x-ray diffractometry rather quickly put Weissenberg cameras out of fashion. So, I doubt if there is anybody who uses that methodology any longer, but for the work that I was doing, it was a necessary development and for a year or two it was I suppose reasonably valuable in the field generally.

MB It was one of the early papers putting you on the map.

DP Well, yes, it...

MB At that time.

DP I suppose so, yes. I mean, I became known by people like Kathleen Lonsdale, for example, who noted this paper and when I got back to London, pointed out that I'd lost a factor of two in it somewhere which I found rather galling, and then she went on to develop the method a little and it was all re-published in the handbook for crystallographers, International Tables for X-Ray Structure Analysis in three volumes or something, which covered all the space group data and formulae of this kind and was the book that we all referred to if you wanted to know what was the appropriate method or how to understand space groups and things of that sort.

MB David, anything more about those years, because eventually you get called to come back to London, but I don't want to miss anything from that Canadian...

DP Well, the social life was really quite important. I suppose, I rather expanded in terms of my personality to some extent at least in those days. As I said there were two postdocs from Glasgow, one of them was a member of a Scottish/Polish family and she's now settled in Canada. The other one was a girl named Violet Shore. And she had been involved in Glasgow in working out the three dimensional structure of anthracene, along with a leading crystallographer named JM Robertson who set up a school in Glasgow which included important figures like Sandy Matheson and Sidney Abrahams and so on. Matheson eventually lived in Australia and Abrahams in the United States, but what I did with acridine was very much based on what they had done with anthracene which is a similar, as you know, molecule. So, I became very friendly with her. Originally, I found lodgings in a house in an area of Ottawa known as New Edinburgh which was on the other side of the Rideau River from Ottawa, and lodged with a family in Mackay Street which was a couple of doors up from where Violet Shore lived with her husband. Now her husband, Alf Shore, had read law in Glasgow and stayed behind to finish off his degree and professional training and then came out to Ottawa to marry Vi. He then found a job in Canadian Defence Research, I think it was. Anyway, it was to do with the Canadian Defence Department and they became very close friends. I actually lodged with another Scotsman named Jim Watson, who was a physical chemist who worked in Stacey's lab. So I quickly got involved in a group of these postdoctoral fellows and their consorts, and we were all members of the Rideau Tennis Club. And when the first snow came along at the beginning of December, Alf and Vi said to me 'Well, you obviously have to ski. Because if you don't, there's nothing else to do in the winter, except go to the cinema and twiddle your thumbs and you'll find it very boring.' So I said 'Alright,' with a little bit of nervousness 'I'll have to try to ski.' So, they said 'Well, all you do is go down to the shops which are now stocking skis and ski boots and buy an anorak and a woolly cap or something. That will do.' So, I went down and bought some skis and some ski boots and a woolly cap and a pair of gloves. And on the first Saturday when there was reasonable snow and I was kitted out, they took me up to Rockcliffe Park which is a rather upper-class, let's put it that way, upmarket residential area in Ottawa which has some quite nice hilly ground overlooking the Ottawa River. They took me to the top of a hill and said 'Well, what you do is just slide down.' So, I slid down and collapsed in a heap at the bottom and they all stood at the top and laughed like drains. And I picked myself up and thought well, this isn't much good is it, and clambered up to the top of the hill slowly because, you know, it was a case of two steps forward and three steps back most of the way. And they said 'Well, that was alright. Everybody does that. Now, you try it again.' So, I did it again a few times and they did it and made it look easy though they were not, at that point, terribly good skiers. And I graduated from that to going with them every weekend, sometimes Saturdays and Sundays but at least one day every weekend through the winter, out to the Gatineau Hills which were three or four miles to the north of Ottawa over in the province of Quebec in fact, to ski. And mostly we did cross-country skiing rather than downhill skiing. But, I became... I wouldn't say I ever became competent but I lost some of my terror at least. And we had quite a lot of good fun going along these trails and we did fit into that a little bit of coaching on parallel ski work on downhill slopes which sounds very advanced and, as I say, I never did it very well. But at least I enjoyed myself and it was socially, I mean, rather splendid. There's a story related to all that which brings out Barnes' attitude to the use of x-rays. I fell over on a trail in the Gatineau Hills one afternoon and there was nothing unusual about that at all, but as it happened, I landed awkwardly on my hand and this bone, this metacarpal, I thought was fairly obviously broken. Anyway, it was painful and it swelled up and things like that and I went into the lab on Monday morning and Barnes looked at it with great interest and rubbed his hands and said 'We'll x-ray it on my radiology machine.' This was the machine he used for his cultured pearl investigations. So, not knowing any better, I let him x-ray it and there, sure enough, was a splendid x-ray of my hand looking like the original Rontgen x-ray with a splendid crack diagonally across this metacarpal. So, he said 'Well, there you are, you see, you've cracked it.' And I said 'So, what I do now?' And he said 'Well, you go to see the doctor and get him to fix it for you.' Well, the Research Council had set us up with modest medical insurance, so I went off to see the doctor and said 'Well, I fell down skiing and I've broken a bone in my hand.' So, he looked at it and said 'Ah yes', in the way doctors have 'we shall have to x-ray that' and I said 'No need at all,' and produced the x-ray out of my pocket.

MB The Barnes masterpiece.

DP The Barnes masterpiece, that's right. And he looked at this somewhat astonished and said 'Well, yes, alright', and proceeded to put a rolled-up bandage in the palm of my hand, closed my fingers over it, tied the whole thing up and said 'Keep it like that for about a fortnight and I expect it will be alright.' And I said 'Well, fine, think you very much. How much is that?' And he said 'Well, I didn't do anything, did I?' So, I said 'Well, no, not much. Thank you very much,' and went back. But, I claimed one hundred and twenty-five dollars from the insurance, so Barnes was pleased. At least it produced that for me and it healed up quite quickly, as it happened.

MB David, in those years, you had the pleasure of visits, contacts still from Dorothy Hodgkin who came out to Ottawa?

DP Well, Dorothy had a sister living in Ottawa and she used to come and see her from time to time. I'd seen Dorothy at meetings in the UK of the X-Ray Analysis Group, which was the professional organisation for crystallographers in those days. And so when she came out to Ottawa, she was far too professional a person not to look at the local x-ray crystallography group and she came in and we talked about what I was doing and that became quite important in the development of my career.

MB Which at that time was at an interesting phase because the whole field of crystallography was expanding dramatically in the fifties. I mean, you've got a new journal and you've got a whole range of groups being set up. It must have been a fascinating field to be in?

DP And people like Dorothy of course who led the way were much involved in large structures, proteins in particular. And of course the early fifties was also the period when all sorts of things were happening of which I have to confess that I was unaware. For example, the DNA structure of Crick and Watson came out in '53 when I was in Ottawa and I missed it and I was absolutely unaware of it or its significance, though I was following the protein work. I was aware of the '54 paper, it may have been '53 again, of Perrutz, Ingram and Green on the use of the heavy atom method in studies of haemoglobin, which was really the key breakthrough paper that led on to protein structure analysis again as a routine subject. So it's obvious from that that I followed some of the literature reasonably well, but some of it I didn't.

MB I think it's fascinating from what's to come later that that was a phase in which all of a sudden protein crystallography was becoming quite respectable, computing was coming in and new techniques were making it a plausible opportunity.

DP Well that is easy enough to say in retrospect, but Barnes for example, like a lot of crystallographers in the UK and in the United States, was extremely sceptical about whether protein crystallography would ever be possible. One of the things that happened in '52 which was I suppose important and certainly memorable, was my first visit to the United States. I went down to a meeting of the American Crystallographic Association which was probably in Pennsylvania, but you got to it from New York easily enough, and I went down with Qurashi, this Pakistani chap who'd been at Manchester Tech. And that was a fascinating meeting because it was a meeting at which Karle and Hauptmann described in some detail their direct method for phase determination, a method by which you could derive the missing phases from the amplitudes to some extent. And that's the method that has revolutionised small structure crystallography, which nowadays you simply plug the numbers into a computer and the answer comes out. But it's due to Karle and Hauptmann, I mean, helped by earlier work by Harker and Kasper and later work by Cochran and Wolfson and Zachariasen and various other people, of course. It's rather rarely one person that makes the big breakthrough. But thanks to these people small-scale crystallography is now a fairly routine technique and it was at that ACA meeting in 1952 that we had the first rather dramatic announcements. I remember that I gave a paper there on using the Harker and Kasper version of direct methods on getting into three dimensions in acridine, so I was following that sort of work fairly closely at the time.

MB It was a period in which your confidence was building and in which relationships across the field were building.

DP I think that's true. I mean, I saw it slightly then as a period, and this harks back to my biblical upbringing, in which prophets spend their time in the desert. Now, in many ways Ottawa was not a desert. I mean culturally, in terms of sport and in terms of companionship, it was anything but a desert, but it was a place for reflection and thinking about what might come next and things of that sort. It was a very formative period.

MB With some relationships that were important.

DP Yes. The summer as I said was filled up with tennis at the Rideau Tennis Club and occasional weekends at camps along the river. I mean, typical North American camping sites more or less in those days with chalets to stay in and bathing in the river and more tennis and all that. It was a very companiable friendship forming time and some of those people... Jim Watson is lamentably dead by now, but Alf and Vi Shore eventually came back to the UK, and they live in Edinburgh and I see them from time to time and they are still close friends. It was a good time.

MB And then it was all to stop?

DP Well, four years was quite a long time. During that period, when I'd been there two years and became a member of the staff, one of my friends from Cardiff by a slightly roundabout route also came out to a postdoctoral fellowship in Ottawa. So we spent roughly two years together and in that period we shared a flat together out in a different part of Ottawa. I'd gone through a stage for a year in which I shared a house in Rockcliffe Park, the upmarket district, with Alf and Vi Shore. And we had super parties there, and there were lots of other friends and Alf's stepmother came out to stay, and people from Ellesmere came out to visit Ottawa and I showed them round and things like that. The more I think about it the happier a time it gets to be. There were of course girlfriends. There was a fairly steady girlfriend for two years, but I still had a problem about making permanent relationships and that again didn't come to anything, but it was a very happy time while it lasted.

MB Then this letter came in '55.

DP Then in '55 I got a letter from the Royal Institution. And it said in effect, 'As you will have heard I have moved from the Cavendish professorship in Cambridge and I am trying to build up a research group at the Royal Institution to work on the structure of proteins. And I wonder whether you would be interested in coming to join me? If you would perhaps you would send a rough form of application and referees and things like that.' It wasn't signed, but it said at the top that it came from WL Bragg and I could have told that from the context anyway. The fact that it wasn't signed has of course become a family joke because the lady who was at the time WL Bragg's secretary is now my wife. And from time to time we bring this up, this curious omission, that Bragg didn't sign this critical letter. So I showed it to Barnes and he said 'Proteins. There's no possibility that anybody will ever work out the structure of proteins.' And I said 'Well, I don't really agree with you, and anyway I'm getting to the time when I ought to go back to the UK.' Because my parents, by now, were well into their seventies and I thought I had some responsibility to watch over them a little bit. I mean there were certain possibilities in Canada, but I didn't feel particularly like staying there, though I had become a landed immigrant and I did enjoy North America, as I still do. Anyway, I replied to Bragg and said...

MB You were coming.

DP ...I'd like to follow up your offer, and this has been my career so far. And [I] got a letter offering me a position in the Davy Faraday laboratory of the Royal Institution at a salary of six hundred pounds a year as I remember it, which was less than half what I was then being paid in Ottawa.

MB And that was an MRC-funded post, was it?

DP No, it wasn't. Bragg had raised some money from the Rockefeller Foundation, I discovered later. And it was Foundation money, not long-lasting, not secure, nothing long-term about it, just a temporary job. But nevertheless, I...

MB You came to the RI.

DP ...I blinked and came back across the Atlantic in early December of 1955 in fairly rough weather, again on one of the Empress Line vessels that came into Liverpool. My parents had hired a car to come and collect me and they picked me up at the Princes Dock or whatever it was and we drove back to Ellesmere and there I was back home again. And at the end of December, after spending Christmas at home, I went down to London. My old school friend Richard Williams was at this point living in lodgings in Tavistock Square and he said over Christmas 'Well yes, I can get you a room in my lodgings.' So I said 'Well, fine' and he fixed that up. And I found myself in a garret in Tavistock Square with a lumpy bed and a miserable shared bathroom and a totally grotty environment and I seriously wondered how I could have possibly given up the relative luxury of Canada for this. But on New Years Day, I remember, I went up to the West End and walked along Piccadilly and walked into Albemarle Street and looked at the outside of the Royal Institution and thought well, that's a fairly grandiose looking place, I wonder what it's like inside and prepared myself for starting there the following day, which I did. It turned out that Bragg, and I think I knew this from correspondence beforehand, had inherited from the previous research staff at the Royal Institution a chap named UW Arndt and he was actively engaged in developing diffractometers. He was and is an instrument developing type scientist and very good at it indeed. So it was agreed fairly quickly that I would initially work with him in developing the diffractometer, but I would also collaborate with the Cambridge people in the studies of proteins, which they'd agreed to share with Bragg in order to get his group at the Royal Institution.

MB David, it would be very useful to have from you, at this stage, just a picture of Bragg coming to the RI, what he'd left behind, how those relationships were forged and what was on the ground when you arrived.

DP Well, Bragg left Cambridge I suppose at the end of '53. It was the 1953/54 sort of period when he was sixty-three, so a little bit short of retiring. But the prospect of coming to the RI gave him the chance of at least an extra five years activity in a place with which of course there was a strong tradition, because his father had been the resident professor at the Royal Institution from about 1919 to 1940-something and had done a lot of the original x-ray work there, and WL had been closely in touch with that obviously. But Bragg was extremely interested in the protein research going on in Cambridge which had actually been initiated by Bernal and it was one of the things Bragg found in Cambridge when he went there in 1938. Perutz has a story that it took him a little while to build up the courage to go and see the new professor, because he'd been a protégé of Bernal's and Bragg moved in from the National Physical Laboratory and was obviously very taken up with all the business of coming to grips with what work was going on in the Cavendish laboratory. Anyway, one day Perutz summoned up his courage and went along to see Bragg carrying with him some x-ray diffraction photographs of haemoglobin. And it was also of course at a time when, '38 now, a few visionary people – in particular Bernal himself and JM Robertson in Glasgow and Dorothy - all firmly believed that it would be possible to work out the structure of proteins and they even had a perception of the methods that would have to be used to do it.

MB That early?

DP So Bragg looked at these, Perutz's pictures, and was immediately hooked. So he became an instant supporter of Perutz and tried his best to promote Perutz's work. The war of course intervened and Perutz was shipped off to Canada as a refugee who couldn't be trusted, and he lived in a detention camp on the Heights of Abraham above Quebec City for a time as he's described. But then he came back, because he was not only a developing expert in protein crystallography, he was an expert on glaciology and ice. And there was a hair-brained scheme developed under Mountbatten in combined operations during the war for creating a floating ice field in the North Atlantic for use as an air-strip on which aeroplanes could land as a part of the campaign against the submarine menace. Perutz was extracted from this detention camp and brought back to the UK and made a UK citizen and all that kind of thing to work for the Ministry of Defence on Mountbatten's staff to create these ice flows – I think ice reinforced with straw or something peculiar like that. Anyway, it all came to nothing but it brought Perutz back and towards the end of the war. And very soon after the war be began again, still by photographic methods, recording the intensities of reflections from his haemoglobin crystals and actually calculating, again by very laborious methods, a so-called Patterson synthesis of haemoglobin which was one of the wonders of the world at the time. Though what it actually meant nobody could quite tell and there were numbers of slightly far-fetched interpretations of it. But throughout all of this Bragg was a staunch supporter and he promoted all of this. Meanwhile, at the end of the war John Kendrew had met Bernal, I think in Ceylon. They were both involved in intelligence somehow and Mountbatten, of course, was in south-east Asia by then with Bernal still on his staff. Mountbatten was one of the few people who actually trusted Bernal who of course had a pre-war reputation as being a very left wing character, but never mind about that. Kendrew talked to Bernal about well, what shall I do after the war? And Bernal said 'Well, study proteins, study macromolecules. Biology, that's where the future is.' So John Kendrew came back to Cambridge and contacted Perutz and signed himself up as a graduate student to do work on proteins. He didn't work for very long on haemoglobin because he wanted a separate project, and one way or another he landed on myoglobin which is a related protein. It's myoglobin that stores oxygen in the muscle tissue and it's haemoglobin that carries oxygen to the muscle tissue from the lungs, so they are related. And it turned out that they were really rather closely related, but the myoglobin molecule is a quarter of the size of the haemoglobin molecule. And John Kendrew set about building up a group to study that. He must have started in 1947 or '48 or something, so he'd been at that for about eight years when I turned up at the RI. And the heavy atom approach which Perutz, Ingram and Green had shown would work in haemoglobin was of course the talk of the day, and everybody was trying to produce heavy atom derivities for myoglobin. John had a team in the Cavendish working on that. People like Bob Parrish and RG Hart and so on and an important character whose name momentarily escapes me – Dintzis, Howard Dintzis - another American. Well, here was Bragg in this ferment. DNA had just come out, Watson and Crick; proteins were on the brink of something. And Bragg is attracted to the RI, so he says to Perutz and Kendrew 'Well, why don't you come with me to the RI?' And they say what you would have said. They say 'Well, things are going rather well here and we are quite well set up, you know. And you've persuaded the MRC to support us here with a laboratory of...' whatever it was called, macromolecular structure analysis or something like that I think at the time. 'We think we're going to stay here, but we'll help you get started at the RI.' And they lived up to that. Perutz came in from time to time and one of the people that Bragg had recruited from King's College in London, Tony North, he worked with Perutz on the haemoglobin structure. And I when I arrived worked with Kendrew on the myoglobin structure.

MB With help from one of Perutz's students, I think?

DP Well, Perutz also had a student, David Green of Green, Ingram and Perutz, and he was recruited. But he had identified a protein that he wanted to work on himself, beta-lacto-globulin, and he was rather a loner and he worked on that more or less alone. But Helen Scouloudi, a Greek lady whom Bragg had recruited from Bernal's lab, she also worked on myoglobin. But she was also rather a loner and rather than joining in the collective effort on myoglobin, she worked on a different species form. When I began John had decided that sperm whale myoglobin was the species to concentrate on, but he also had crystals of seal myoglobin and Helen Scouloudi worked on that, and that came to something as I might have time to mention.

MB David, why I mentioned David Green is that I thought he showed you some of the basic techniques needed in that kind of protein crystallography field and got you started?

DP Well, David Green, yes, was the very first person who showed me how to mount a protein crystal. A key discovery, one of the many key discoveries, was the Bernal and Hodgkin – Dorothy Crowfoot Hodgkin, Crowfoot as she was at the time – discovery in 1934 that protein crystals diffract x-rays very well indeed, but they only retain their integrity as single crystals with highly ordered molecules arranged in them if they are kept hydrated. So if you take them out of the liquid of crystallisation and mount them on the end of a glass rod, the diffraction pattern doesn't entirely disappear but becomes useless.

MB You've got to keep them in the mother liquid.

DP So, what you have to do is draw them into a glass tube, suck out most of the liquid, leave a little drop of liquid at either end of the tube, seal the two ends with wax and mount the whole thing on a goniometer head on your camera. So, David Green went through all of this and handed the specimen to me and I took hold of it between the thumb and that finger in the middle of this very fragile glass tube and squashed it, and he said 'Well, there you are, these tubes are very fragile. You'd better try it yourself.' So that's how you mount protein crystals. And he showed me how. And he'd been involved as I said in the Perutz, Ingram and Green publication, so he knew about the isomorphous replacement method, but then I'd actually used the method in my PhD work in Cardiff so I knew about that anyway. I had the feeling that the protein crystallographers felt that they'd had to invent it for themselves so they used it, I thought, in a rather complicated way whereas I just used it in what I thought was a straightforward way.

MB A standard way.

DP So we just used it in a straightforward, standard way.

MB There were two kinds of stables, weren't there, people who thought protein crystallography was an entirely different kind of world...

DP That's right. The Perutz and Kendrew group really, despite the Bragg influence, because Bragg wasn't like that.

MB He was a generalist.

DP They felt that protein crystallography was in some way different and they would have to invent it for themselves. People like Bragg and Dorothy Hodgkin and Harker came to it from old-fashioned crystallography and to them it was a part of crystallography, it just needed an extension in order to get into proteins.

MB And that's how you felt.

DP But, there remained a lot of others... Henry Lipson, Fankuchen who worked in Brooklyn, but not with Harker ... all these people thought it was simply a lost cause and it was a pure waste of time.

MB So, there was still a lot of scepticism?

DP A lot of scepticism still in 1956.

MB David, at this point I've got you a confident young researcher, well established, coming from Canada to the RI to take up work effectively with Kendrew on myoglobin.

DP Well, Kendrew on myoglobin and Arndt on diffractometry with Bragg stamping across to see us. The labs we worked in were in the house next door to the main Royal Institution building and my office was on the top floor and the x-ray labs were in the basement. They were part of the labs which Faraday had worked in in the 1800s, and Bragg's residential flat [was] on the top floor of the Royal Institution proper, connected by a rickety wooden corridor through to this office, these office floors at the top of the Davy Faraday laboratory. So from time to time, we would hear his feet very characteristically stamping along the corridor and then he would stop outside the office door and always went stamp-stamp, so we knew it was him. That was the notice, it was Bragg arriving and he was going to come in. So, he came in and chatted to the people in this room. I actually shared this room with one other scientist, Jack Dunitz, who'd also been attracted back to the UK to the RI from North America on the advice of Dorothy Hodgkin.

MB Another recommendation.

DP And it turned out, of course, that it was on Dorothy's recommendation that Bragg had written this famous letter to me. So, Jack and I shared this office and we also had a work bench in it at which we mounted protein crystals and the diffractometry went on in neighbouring rooms up on the top floors of the Davy Faraday lab.

MB David, you've set me that scene very well. We haven't got into the diffractometry and we haven't got into the myoglobin work yet, but at this point I'm just going to take a short break.

DP Alright, fine.

MB David, we're going to move now to look at the myoglobin story, your work with Kendrew and the group he built up, but before moving into that, you were getting into work on diffractometry?

DP Before I got to the RI, Uli Arndt had already begun to assemble equipment both for the mechanical parts and the electronics for a diffractometer with a view to this being used, as Harker indeed was using it, in protein studies, so I joined in with him and played some modest part in the development of the very first diffractometer which was, as I've described for Harker's machine, an x-ray source, a circle that the crystal moved round on, a circle that that moved round on and a circle that the x-ray detector moved round on. Uli had done a lot of work in the development of proportional counters, a somewhat improved version of what's generally known as a geiger counter I suppose, and it enabled some degree of wavelength discrimination in the measurements which was a useful feature. So this was reasonably well ready when I got there and I was involved in putting some of the finishing touches to the measuring equipment and so on, but it still all had to be done manually. One had to calculate the angular settings on these three circles and then set them and then go through a measuring routine and record the measurements manually and then move on to the next one, so it was by no means automated. Lipson, who as I have said was not a great supporter of the idea that one would be able to work on proteins – we did have contact with him at various meetings – he said that he too had used a digital diffractometer. We were slightly surprised at this and he said 'Yes, it was one where we moved the circles around by hand,' and ours was indeed rather like that. We employed a young lady to help make the measurements, so they were automated enough for semi-skilled people to be able to operate the thing alright.

MB But, it was rather laborious and very quickly you were thinking about...

DP It was very laborious. Nevertheless at about this time John Kendrew and his people in Cambridge had produced some heavy atom derivatives of sperm whale myoglobin and they were busy measuring the x-ray diffraction intensities on precession photographs. Now, the key thing about precession photographs which I'd encountered in Ottawa of course is that the spots don't appear apparently at random across the film, they appear in straight lines. They are, as people often used to say, a direct representation of the reciprocal lattice. And without wishing to go into that in great detail the molecules in a crystal are assembled on a lattice in real space, and the diffraction pattern to some extent resembles that in that the reflections can be thought of as being arranged in a space, the dimensions of which are reciprocal to those of the crystal lattice. So, the reciprocal lattice is an important construct in crystallography, and since the spots on the photographs were in straight lines, one could use a straightforward optical densitometer to run along the spots and produce a row of peaks of different heights to learn what the intensities were. So that got away from visual estimation of the blackness of the spots. There was now a reasonably objective optical measurement of how black the spots were, and that was the method used to make most of the measurements for the first low-resolution study of sperm whale myoglobin. But at this stage the diffractometer was just about usable and I used it first in an experiment with Helen Scouloudi which very much appealed to Bragg because it went way back to his interests in the early twenties. There's a problem known as the 'absolute intensity' problem. In the end, the numbers that you need to calculate the map are related to the number of electrons in the structure that are scattering the x-rays. So those in a sense are the units that you are using, but in order to get on to that scale you have to have some measure of the absolute intensity of the x-rays that you are measuring, that is what's the relationship between the incident intensity and the measurement that you are making. One way of approaching that is to make some measurements from a known crystal structure and why not anthracene and then compare those with the measurements from the crystal that you are using. You need to know the volume of the crystal and so on, but supposing you can get over that you can get an estimate of the absolute intensity of the x-ray reflection. So Helen and I used the diffractomer to work out the absolute intensities of the reflections from seal myoglobin and that turned out rather well. And Bragg I think possibly thought at that point for the first time that here was somebody with a certain amount of experimental competence, maybe. He set great store by experimental competence. One of the family jokes was that the Braggs were good at experiments but that the Thompsons – JJ Thompson and GP Thompson – were not good at experiments. They always broke the glassware, like me the first time round. So having done that experiment, I said to John Kendrew 'Why don't we measure the intensities for one of these heavy atom derivatives using the diffractometer?' So this derivative was mercury iodide, and we measured the intensities and these measurements were incorporated with all the rest of the measurements which, in the main, were made in Cambridge. And John Kendrick would shut himself away in his room, and using a geometrical construction which had been devised much to our chagrin by David Harker in Brooklyn, he worked out what the phases were of the myoglobin reflections out to six Angstrom resolution. He used a computer in Cambridge which was just becoming available for this kind of Fourier synthesis. John Kendrew had interested himself very much in both the development of the optical densitometry and in the development of digital computing, so that the tools were there to accomplish what he needed to do. He used the EDSAC computer to calculate the map and this showed a molecular structure rather like a bunch of sausages, except that everybody was immediately convinced that each sausage represented one of the structures which had been described in 1951 by Pauling and Corey, the alpha-helical structure of the polypeptide chain. So, myoglobin became a structure immediately made up of alpha-helices which had a helix running this way and a helix running back and a loop over the top and a helix running that way and a helix coming back and then a helix going down and a helix coming up at the back. All very simply described in terms of eight alpha-helices with a haem group in the middle, which had an iron atom in the middle of that to which the oxygen atom attached. One couldn't see any of that detail, but one could see a disc for the heam group surrounded by all these helices. And that came out in 1958. [It] was published in *Nature* in a paper of which I was a joint author on the strength of having made these measurements on a diffractometer, and was subsequently described in a paper in the Royal Society *Proceedings*, of which I wasn't an author because I think I thought at the time that merely making some measurements from one derivative wasn't much of a contribution. Subsequently I rather regretted that, but never mind, that was me being idealistic at the time. So, that created a stir. A protein stucture. Maybe it was possible to do protein structures. Perutz of course was at roughly the same stage with haeomoglobin. He'd been earlier in getting a potential heavy atom derivative. He tried hard to get more derivatives and a curious phenomenon got in the way. Every time he tried to produce derivative crystals, the cell dimensions of the space lattice of the crystal changed in some mysterious way so that the derivative wasn't compatible with the native structure. And he got into a terrible state and couldn't get over this problem, and it was one of these frustrating episodes in science that one can only sympathise with. There was no clear-cut way of finding out what the problem was. He simply had to keep on trying different things to get these derivatives. Meanwhile, with myoglobin, John Kendrew assembled a slightly new team. People like Parrish and Dintzis and Hart went off, and a new American Dick Dickerson appeared and a Swede named Bror Strandberg. And I remained a member of the team. And at that moment Alf and Vi Shore came to London because Alf had been appointed science representative in the Canadian High Commission in London representing the Canadian Defence Research Organisation. And Vi was looking for a job. So I said to John Kendrew 'Here's a crystallographer just arrived in London, actually began as a chemist, would be very good for preparing heavy atom derivatives and helping with the photography. Why don't you recruit her to the myoglobin team?' So he said 'What a good idea' and Bragg saw her, and he saw her and they liked her and she was taken on at the RI to work on myoglobin. So the bulk of the work was split between Cambridge and London. Kendrew, Dickerson and Strandberg and a small army of helpers in Cambridge and Vi Shore and myself in London. Also with a small army of helpers, because we'd decided that we'd carry on making the measurements from precession photographs rather than by diffractometry which wasn't yet ready for it, it was still too manual. So, we needed helpers to run the optical densitometer and to measure the height of these peaks and so on. Somebody pointed out that there was an agency in London called the Married Women Graduates Employment Association and we got in touch with this agency and through them recruited I suppose at the peak, three people, maybe four people, who worked on this intensity measurement aspect. One of them was a lady, Winifred Browne, who was originally an American, had married an Englishman at the very beginning of the war, stayed in London while he was off in the Army, had three children, and then quite soon after the war, he'd died. And she'd been left with these three children to bring up and she'd got them to the point of being about to go to school and wanted a part-time job. She remained a close friend and colleague for many years as I shall have occasion to remark. Then there were a couple of others; a Dutch girl married to an Englishman and some others. So we had this troupe of people. We partitioned the work, so that we grew the crystals and made the heavy atom derivatives in London and that was Vi's job particularly. We took a proportion of the photographs... One had to take upwards of twenty photographs for each derivative and the native structure. And each one was a stack of photographs and they all had to go through the densitometry process and the measuring of the peak heights, and then it all had to be co-ordinated. When we got to the numbers, the measurements, the ones that we were measuring went off to Cambridge and they were all collected together there and part of John's local army of research assistants had to punch all this information into the computer and it went in. There's a picture of Dick Dickerson and Bror Strandberg carrying a pole from which are suspended innumerable punched paper tapes which have all these data punched on them ready to go into the next version of the EDSAC computer to calculate the map. So, that's how it was partitioned between the two and in the autumn of 1959 – so it went quite quickly – it was all put together and Vi and I rushed off to Cambridge to sit in the computer room while the map was calculated. Now, this was EDSAC 2 and it had to be programmed, of course. And the programme for the calculation of the Fourier map had been written by another postdoc in the Cambridge lab who was actually part of Perutz's haeomoglobin team named Michael Rossmann. He also had been a graduate student in Glasgow. So, he'd written the programme and we were all quite confident it would work. It was all fed in on paper tape, the data was all fed in to the computer, and then the paper tape output began to chutter and out came a piece of paper tape and we rushed with it to the tape reader. There were no visual graphics, it all went in on paper tape and came out on paper tape, and one had to devise a method of translating what came out on the paper tape into the maps, and I'll come back to that. But to begin with some paper tape came out and we rushed over to the printer and printed out what it said, and it said 'Michael Rossman, Michael Rossman, Michael Rossman.' And this was just the sort of identification labels of the programme and nothing else came out. And we carried on there until one o'clock in the morning, two o'clock in the morning, three o'clock in the morning; Michael wrestling with the programme, people making sure the tape had gone in alright, everybody else rewinding paper tapes, taking care not to tear them and all the rest of it. And eventually, at some ridiculous hour in the middle of the night, the map started to come out. Now, printing out the map was a complex business, so Michael had devised a system whereby it came out on two grids. It came out, if you like, in an alpha-numeric form. The first grid was all letters of the alphabet and that said whether the number was something between nought and twenty-five. And the other one came in ordinary digits and that told you whether the number was between nought and twenty-five or ten times that or ten times that and so on. I think it was that way round, it may have been the other way round. So this army of research assistants and all the rest of us had to get down hurriedly to the problem of writing out in a grid of plain language numbers what this peculiar output actually meant. And when it came out you had a figure field at the right scale about so big covered with numbers on which one drew contour lines around numbers of an equal number, that is the number ten contour line, the number twenty contour line and so on. Now it was a 2 Angstrom resolution map which isn't quite enough to show individual atoms, but it ought to show groups of atoms. So what we saw, to our delight, was something that looked a little like ridges of hills and these ridges of hills were the course of the polypeptide chain. In the middle of it one blob that was bigger than the rest, and that was the iron atom, and around that a rather plainer group of structures and that was the haem group around the iron atom. So, that was the first thing that I concentrated on. And Vi and I produced a contour map showing the haem group and the iron in the middle of it. And we knew we were there at that point, though the map wasn't tremendously good. However, for it's time and given the ways in which it was done it was most remarkably good and it revealed the whole structure in three dimensions.

MB That must have been a terrific moment?

DP That was an extraordinary moment, that's right.

MB Quite dramatic. You've given me an impression that the link with Cambridge was very good. Was it a close link?

DP We used to travel to and fro quite often. Kendrew came in a great deal.

MB He came over to see you every week?

DP He came up to London every week to talk about things.

MB He was an honorary reader, I think.

DP That's right, he was a reader.

MB Like Perutz?

DP That's right, they were both readers.

MB But, Perutz didn't come very often?

DP Perutz didn't come as often. He worked with Tony North. He was as I've said locked in this problem with haemoglobin which happily with his team of workers – Tony North, Ann Cullis, Michael Rossmann and others – they actually worked through it in 1959/60. And at the same time that the myoglobin map was published in *Nature* in the spring of 1960 - it came out in the autumn of '59 and then was published in the spring of '60 – there was a low resolution map of haemoglobin. Now, that was more exciting than low resolution maps often are because it showed that essentially haemoglobin was four myoglobin molecules arranged in a tetrahedral arrangement. So you could make a lot more out of haemoglobin given myoglobin than you could have done had it just been haemoglobin by itself. So it's fair to say that when those two structures were published and revealed to the world in the spring of 1960, the sceptics wree confounded.

MB That was by then the high resolution form though that you'd got to by 1960?

DP Of myoglobin, that's right.

MB Yes. That must have been an extra plod to the summit though because that first look at a low resolution...

DP 1958 to the autumn of '59 was pretty quick going.

MB Yes, it was.

DP And that was John Kendrew's perception that if you had an optical densitometer and you used precession photographs, you could take a good deal of the labour out of intensity measurement. And if in the background you'd done something about the development of digital computers you could do that last step much, much more quickly than was previously possible, even taking into account this curious alpha-numeric output. So that last stage really went very quickly.

MB So for that last stage you did use a stage two diffractometer?

DP No, we didn't use the diffractometer at all in the myoglobin 2 Angstrom work. Not at all, because it wasn't ready for it.

MB Really? It was before it came out. How did you get on with Kendrew himself?

DP Well, very well. He, pre-war, was a physical chemist in Cambridge who'd taken his degree at Trinity, joined the Services, was in intelligence, and as I've said met Bernal who steered him towards protein work. He then was Perutz's closest colleague in the original unit supported by the Medical Research Council back in 1947 probably. Bragg, encouraged by Rideal and others, went to the Medical Research Council and said 'You may think it's a long shot, but I have some people in Cambridge who think that they know how to determine the structures of proteins. Now lots of other people think that that isn't possible, but I think it will be in the end. It'll be just like my development of the analysis of silicate structures. So you should get in there and support these people, because when the structure of proteins is determined all sorts of things at the molecular level in medicine and biology will become clear that are not now clear.' So the MRC to various peoples' surprise actually produced a grant to support Perutz and Kendrew and two research assistants to see what they could do with this problem. And it hiccupped along in various ways. I think the original grant was something enormous like three thousand five hundred pounds a year. That was four salaries in those days. But as we've said in '53 out came the DNA structure from this organisation and the heavy atom method and it grew eventually into the Laboratory of Molecular Biology in Cambridge.

MB Yes, this was the pioneering of molecular biology?

DP Yes, so that was the Perutz/Kendrew beginning. No doubt everybody is complicated, but John is complicated I think, doesn't perhaps find personal relationships all that easy as indeed many of us don't, was marvellously perceptive about what was needed to produce this first protein structure and tremendously well organised in getting it there. His organisation of all the calculations of the numbers where it would have been easy to get things mixed up, because you have to associate each black spot with a particular triplet of numbers. This is the twenty-six, fifteen, nine reflection from myoglobin where each of those numbers can range over values from say minus fifty to plus fifty, give or take a plus sign in one direction. So it was a feat of perception and organisation and scientific leadership.

MB And he was deeply committed to getting there before anybody else?

DP Oh, absolutely that, yes. He and I used to joke together. We used to say the first protein structure will be a Nobel Prize, the second protein structure will be an FRS, the third protein structure will be a PhD and the fourth protein structure would probably be a graduate student project, and it turned out something like that.

MB Not far out!

DP Yes. No, I got on very well with John. I've seen too little of him in later years, partly because having worked out myoglobin he would seem to have said to himself 'Well, what next?' He decided to put his efforts into the organisation of European molecular biology, and he went off from Cambridge to become the first director of the European Molecular Biology Laboratory in Heidelberg, and gave up bench work himself in favour of organising research for other people.

MB Virtually straight after myoglobin was out?

DP That's right.

MB David, looking at that period, we've not taken all the achievements of that period into view. The diffractometer work went on all the time?

DP Yes, the diffractometer development went in eventually two directions. The first part happened because the work that I'd done using Weissenberg photographs and using precession photographs had made me feel very much at home with the reciprocal lattice that I've mentioned. Now, there's a way of forecasting how a crystal will reflect or diffract x-rays by doing a geometrical construction which was first invented by a German named Peter Paul Ewald way back, which involves drawing out the regular grid which is the reciprocal lattice and rotating that inside a sphere which is the sphere of reflection. And when one of the points in the lattice intersects this mythical sphere of reflection, the line from the centre of the sphere through the point that's just intersected the surface of the sphere tells you the direction of the diffracted x-ray.

MB Neat.

DP Very neat – the Ewald construction. Well, I was very familiar with this. I mean, thinking in reciprocal lattice terms was easier in many ways than thinking about space groups and real crystal structures. So, it occurred to me one day, why doesn't one make a mechanical model of the reciprocal lattice? What you'd need would be three slides, and for some crystals they could be mutually at right angles, three orthogonal slides, and the top one has to move to and fro on the bottom two and they are pivotted down here. And that would define a point in space. You then have to constrain that point in space to be at a fixed point which is actually the crystal, the centre of the sphere of reflection. And if you put the detector on the line which connects the crystal and this fixed point, then this lattice, this slide system, would automatically put the detector in the right place to measure the reflection. If you coupled the rotation of the crystal with the rotation of this mechanical reciprocal lattice, it will automatically put the crystal in the right place as well. So, I went into the lab one day and said to Uli Arndt 'Why don't we construct a diffractometer based on a mechanical model of the reciprocal lattice on this basis?' and he said 'What a good idea. I think that might work,' because he was the instrumentalist, you will understand. And we brooded over it with bits of paper for a little bit and then he went home and he came in the next morning with a slightly primitive, but nevertheless, recognisable Meccano model of what such a thing might look like and we said 'Yes, that's worth a try.' And we went to Bragg and said 'We've had an idea for a new type of diffractometer which incorporates a reciprocal lattice' and, in effect, the reciprocal lattice model and all the slides and pulleys and gear wheels and so on that went with it was an analogue computer. So, Bragg said 'Yes, that looks very promising. What does Mr Faulkner think about it?' He was always extremely polite about people. And Mr Faulkner was the head instrument maker in the Royal Institution workshop, which had a very good workshop, largely because it had a great tradition of making equipment for the Royal Institution Lectures. But Tom Faulkner was a first-class instrument maker. And we told him about it and he said 'Well, if this coupling is really going to work, the tolerances will have to be very fine, otherwise it will be too sloppy and it won't orient the crystal or the detector and that will be tricky. It will be particularly tricky when the slide system is near the centre of rotation because then it'll slop rather a lot, but it might be alright when you get further out. That's an interesting Meccano model, but it doesn't really tell us anything, so why don't I make up a prototype?' So, he set to work. And, at this point or roughly this point, Uli Arndt went off on a postdoctoral fellowship to Madison, Wisconsin because everybody needed to go to North America at some stage and he was feeling a little bit out of it because I'd been to North America and he'd never been and it was a necessary career development step. And anyway he was leaving behind this new development which was mainly in the workshop's hands. But we also got in touch with the National Research Development Corporation with a view to patenting this device, and the patent agent came along and listened to the story and said 'Well, I will need a drawing of this for the patent.' So I went and talked to Faulkner who by this point had produced his first prototype. And he was quite right. It was altogether too sloppy and it needed to have extra bars built into it to add constraint somehow to improve the tightness of the whole device. And we talked about what he would actually make as the first working model, and when we talked about it, I tried to draw it. Well at that point I'd been interested in graphical work because it came into this reciprocal lattice thing and so on, but I'd never done any engineering drawing. So I went along to a bookshop and bought the Home University Library *Teach Yourself Engineering Drawing* book and I bought myself some extra kit like a set square and so on. And [I] thought well, the best thing to draw rather than various projections showing what it looked like that way and what it looked like that way, would be an isometric projection which showed the thing in three dimensions. So why don't I learn about isometric projection? So I read the book avidly and got a piece of paper set up on my drawing board and set about producing an isometric projection drawing of this new diffractometer with an analogue computer in it, which we called the linear diffractometer for no very good reason, except that the slides were straight lines and one simply moved along them. You see, you could scan the whole pattern by setting this one at one particular level, setting that one at one particular level, and simply scanning to and fro on this one and then moving out a step and scanning to and fro on that one. It was all rather nice, and Tom Faulkner produced a working prototype that actually worked. And Uli Arndt came back and looked at it and made a remark – we'd been in touch by correspondence of course and the patent had gone through alright with my drawing on it – that I've always treasured ever since. He said 'It looks like your drawing!'

MB Does that drawing still exist, David?

DP Oh yes. Well I'm not sure about the original; there are lots of copies of it that still exist, yes. So it worked. And to try it out I thought we'd try measuring the myoglobin diffraction pattern to a higher resolution. So we recruited yet another person – Colin Blake who'd been a graduate at Birmingham University where he'd done crystallography, and he then moved on to work in industry which he hadn't enjoyed very much and was looking for another job. So we advertised for another research assistant and he replied and got the job. And he and I, using this laboratory prototype linear diffractometer, measured the myoglobin data to 1.5 Angstrom resolution or a little bit better in some directions. And the data was used in Cambridge to re-calculate the Fourier map at 1.5 Angstrom resolution, using phases that had been calculated from the existing 2 Angstrom model of myoglobin. That was a much better-looking map and it enabled our new colleague in Cambridge, Herman Watson and I think Carl Branden - another Swede was involved at that point – to go through some part of the process of Fourier refinement of the myoglobin structure at 1.5 Angstrom resolution. But I slowly dropped out at that stage because other things had begun to develop. But to complete the diffractometer story, Hilger and Watts were approached by NRDC to produce a commercial version of the linear diffractometer. And they did that and we had one of the early models and it sold well enough to earn me as my share of royalties several hundred pounds a year for some years. But then it was overtaken by another diffractometer development which Uli Arndt had a considerable part in, and that went back to the multiple circle diffractometer but now computer controlled. It had got to the point where one could get a small digital computer – it was I think a PDP 8 in the first place – which would control the settings of the circles on a four circle diffractometer. And that had much more potential than an analogue machine and it's the basis of modern diffractometry really.

MB I think you mentioned on one occasion David that Bill Coates was an assistant in that development?

DP In the early days at the RI when we were developing the very first manual diffractometer and beginning to make measurements at low resolution for myoglobin. And when we had the first idea about the linear diffractometer, Bill was essentially Uli's research technician. And he, as everybody in the country knows really if they remember the Royal Institution's Christmas Lectures, he could put his hand to anything and make it work really. He was a tremendous chap. He'd been an officer in the Parachute Regiment during the war of course and had landed the night before D-Day and had felt that coming back into research associated with medicine was a very good way to follow up that war-time experience.

MB Just put him in context here David, because he's mentioned in several interviews that we've conducted.

DP Well, Bill was a major friend. When Uli was away in America the then research assistant in the Royal Institution, who'd worked at the RI before Bragg came… He was a man named Walden and he'd worked with Bragg's predecessor Andrade, who left the RI in rather a commotion before Bragg came there, but Walden stayed on for a time then followed Andrade to Imperial College and there was a great anxiety about how would he be replaced. I think a chap named Ronald King, who'd also been attracted to the RI by Andrade and was a research professor in metal physics, that was Andrade's line, said to Bragg 'Well, why don't we think about making Bill Coates' – or Coates as Bragg would have known him mostly – 'the lecture assistant?' So Bragg said 'Well, why not?' So in Uli's absence Coates was whisked out of the lab and whisked into the lecture demonstration preparation room and began doing lecture demonstrations with visiting lecturers and particularly with Bragg and King and he very quickly showed that he was just the chap for the job. So when Uli came back expecting to find this skilled research technician waiting for him, he found that he'd lost him, which was a sad blow for Uli but it was a tremendous career move for Bill Coates who as you know became a national figure really.

MB Great hero of mine.

DP Tremendous man.

MB David, we've got a difficult job here. We've got about five minutes left and I'm trying to get one more item in, so we'll watch the clock. In about 1957/58 you got involved in a bit of exhibition work that was to have repercussions?

DP Well, that's true.

MB Can we get that in, that short story?

DP Well, Bragg of course was a very senior international scientist and a natural figure to get involved in UK international activities. Well, 1958 was the projected year of the Brussels International Exhibition. I think it was one of the early major international exhibitions after the war and the Belgians had decided that they would build a major feature as the centrepiece of the exhibition known as the atomium, which is the biggest crystal structure model in the world. It is in fact the structure of a body centred cubic model. So it has four large balls at the corners of a cube and another one in the middle and these are all connected by tunnels along which there are moving staircases and so on, and the top ball has a restaurant in it. So that was a splendid attracter for Bragg. The British Council came along to him and said 'They are also going to have an International Science Hall showing the latest in international science. And they would like you to be the international president of the science hall and I expect they will be writing to you about that, but we have to make a UK contribution to it and we'd like you to organise it.' 'Alright' said Bragg in a slightly offhand way 'I'll organise the UK contribution.' And he did indeed take on being the president of the International Science Hall, which gave him something of an obligation to do a good job on the UK contribution. But the days went by and nothing much happened, and his secretary Diana Hutchinson was rather well aware that he had this obligation and he ought to do something about it. And she may have mentioned it to King and King mentioned it to Bragg and said 'This is really a major commitment and you'll need some help. Why don't you get one of the people in the lab to help you with it?' And Bragg who, when he realised the position he was in, began to get a bit alarmed said 'Well yes, but who?' And King said 'Well, why not David Phillips?' 'Good idea,' said Bragg and sent for me. So I find myself in Bragg's famous study at the RI, Faraday's old room – it wasn't the first time I'd been there, of course – and Bragg saying 'I have this small problem, Phillips.' It was Phillips you'll understand until much later. 'There's this International Science Hall at the Brussels Exhibition and we've got to get together a British contribution to it. I wonder if you'd mind being my assistant in getting that together?' 'Oh alright,' I said 'what will be involved?' And he said 'Well I think it's divided into physics, chemistry, biology, so we'll need some senior scientists to help us. Perhaps you could identify who they might be and then they could advise you what the exhibits might be and then you could go round and organise with the exhibitors.' And it began to seem a bigger and bigger job and I said 'Well what sort of help will I have?' And he said 'Oh Miss Hutchinson, my secretary, will help you with the letters and so on.' So I said 'Well fine,' and agreed with him then or soon afterwards that the key figures maybe would be ERH Jones the Oxford chemist who would advise on what the chemistry exhibits might be, and Alexander Haddow of the Chester Beatty Research Institute who might advise on what the biology exhibits would be. And to be honest I've rather forgotten who the physicist was, it was probably somebody in Cambridge but I've forgotten at this point. Anyway, I went to see these people and they were all very helpful and gave me lists of people that they thought might give exhibits and topics that ought to be shown. And of course it was clear to me that the Perutz/Kendrew work was going to have to figure in this in a large way, and that the DNA structure would have to be involved in it, and there'd been existing work on tobacco mosaic virus at that time. So I wasn't without ideas about what might be done. So I set about drafting letters to people and all of that, and in this found myself spending quite a lot of time in Miss Hutchinson's office. I had of course known her to some extent earlier on, she used to come to tea. We had a sort of communal tea party at the RI every day for the research staff and administrative staff and everything. And the relationship developed, let's say, and I married her, but some time later.

MB Some time later. It was turbulent at times but it eventually came right.

DP Well, it all came through in the November of 1960, but there were a few rocky patches on the way to that I have to say.

MB Just in the last two minutes, for that exhibition you'd contacted many, many people and you took it there in a highly professional way.

DP Well, the British Council had a chap named Sir Kenneth Loch, he'd been a major general in his day I think, and he said 'We shall need an architect/designer at least to do this professionally.' So we recruited a chap named Gunther Hoffstead who's a designer who has designed houses, furniture and all sorts of things, a marvellous chap. And he and I went round to these people and he devised a fairly simple framework within which most of the exhibits would fit. In the end he and Tony North and I went over to Brussels – we went over to Brussels many times – and eventually went over with a furniture van organised by the Pall Mall Deposit and Forwarding Company which Loch knew because he'd worked abroad quite often, and we arrived in Brussels with this heap of stuff and literally, Gunther Hoffstead, Tony North and I set up a great deal of it ourselves. Gunther, who was a professional designer, thought we were behaving like boy scouts. But never mind, we got it all set up and Bragg came to look at it. I must tell you the Bragg story. On our multiple visits to Brussels we'd fallen into the habit of going to eat at a fish restaurant and we nearly always had mussels. Diana was quite convinced that I always came home with a bilious attack because I'd been eating mussels, so we were close enough for her to know that already. Bragg came and we took him to the same restaurant. So since it was a special occasion I had moules speciale and Tony North being daring had moules au vin blanc, and Gunther Hoffstead, who had more exotic tastes, had moules à l'escargot. And Bragg had steak, egg and chips!

MB David, at that point we really have come to the end of the tape. Thank you.