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

MB David, at the end of our last conversation, we just got you organising a European Science Exhibition, the UK contribution, and that had made its mark, I think, because you're earmarked to put on another display, about 1960. You could probably tell me about it.

DP Yes, 1960 was the tercentenary of the Royal Society, which was founded in 1660, or so it might be claimed. So the Royal Society asked its Soirée Committee, which was chaired at the time by Bragg, to organise a tercentenary exhibition. And instead of having the exhibition in the rooms of the Society which were still in Burlington House, they had the exhibition in the Diploma Galleries of the Royal Academy which were next door, so it was all very convenient, but a much bigger affair. With Bragg's agreement, we again roped in Gunther Hoffstead, the Brussels International Science Hall designer, to make it look more professional than it usually does. So, he draped the diploma galleries with muslin of appropriately neutral colours and tried to persuade the scientists who put on exhibitions to do rather better than their usual scientific posters. But, the Soirée Committee had to meet to discuss the general layout of the exhibition and the thing that I remember most about it was that Gunther and I attended this critical meeting of the Soirée Committee with Bragg in the chair, and people began competing for who should have the pride of place in the exhibition. I was quite clear in my own mind that with the myoglobin and haemoglobin work having just come to fruition, the centrepiece just had to be a myoglobin model. Christopher Hinton, on the other hand, the head of the Atomic Energy Authority and the person most involved in the extensive nuclear power station programme of the time, was equally clear that a model of a nuclear power station had to be at the centre of the exhibition. And Bragg, who was somewhat diffident when confronted with people like Hinton, had to hold the ring. Anyway, exactly how it turned out I don't really remember in detail and what the rest of the committee thought the compelling arguments were I don't remember, but I do remember very clearly that myoglobin was the centrepiece of the exhibition.

MB And you essentially fought off Hinton, I think, because Bragg didn't have much to do with it, he left you to it. So, you were spoiling in the ring with quite big fish then. I just wanted to draw this out, because you weren't a Fellow of the Royal Society, but you were pushing fairly hard by then.

DP No, I wasn't a Fellow of the Royal Society and I wasn't even pushing particularly hard at that point, I suppose.

MB I mean, not to be a Fellow, but actually to hold your weight in quite large groups.

DP It didn't worry me having to argue with people like Hinton, for some reason. And I suppose it goes back to my experience in the Navy and having to deal directly with the captain over radar issues, that sort of thing, I don't know, but maybe it's just my somewhat bolshy character. I wasn't intimidated by Hinton, or anybody else.

MB Can you spell this out, because you were going to have a long career in boardrooms and making decisions, and I just thought that was a point where you clearly arrived at quite hard decision-making and negotiating.

DP Yes, I quite enjoyed that, I remember.

MB And that was a very important display.

DP It was considered, I think, an important exhibition. It had all that was best of UK science at the time represented in it and was rather a good show.

MB David, I'm going to take you away from that centrepiece of myoglobin now because Kendrew's moved on, and you decide to go down a new avenue.

DP In about 1961, we recruited a new postdoctoral research worker at the Royal Institution, Roberto Poljak, who came from the Argentine originally. But when we recruited him he was a research worker at MIT, working with Martin Buerger, the chap who invented the precession camera, who was himself a mineralogist and Roberto was working on mineral structures. He fell in touch with Howard Dintzis, who'd moved back to the States from Kendrew's group, and was now setting up protein work in MIT. And I don't know how they came together, but Howard said to Roberto Poljak 'I don't know why you're wasting your time with minerals, proteins are much more interesting. And furthermore, I don't think it's as difficult as all that. I mean, you've just seen the myoglobin structure published, I think you could set about determining a protein structure. So, why don't you crystallise lysozyme and see whether you can do anything with that?' So, Roberto I suppose, rather in his odd moments, did indeed crystallise lysozyme - the recipe is very straightforward and very well-known, it can be crystallised directly from hen eggwhite, for example - and he took some x-ray photographs and tried to diffuse in some heavy atoms. And it looked as though he might have got some differences in the x-ray intensities, so he was rather encouraged by all that, and then came to the Royal Institution. He told this story to me and to Bragg, and Bragg and I talked about it, and we agreed that it would be very sensible if the RI research went its own way at this point essentially, and if I brought people together to make a concerted attack on the lysozyme structure, and Roberto was quite happy with that. Tony North, at the time, was doing his year in the United States, but he came back and he was happy to join in and Colin Blake, who'd been working with me on the high resolution myoglobin data collection, he was also quite happy to join in. So the four of us set about the determination of the lysozyme structure. We realised that computing was going to be an important part of this, and we would in part use the London University computer system, so that was arranged, but we felt we needed a small computer actually at the RI to do the data processing and some other things. And Tony North who'd done some computing over in the States, he was to be the ringleader on the computing side of the activity. Colin Blake, who was a chemist by origin - Tony and I were both physicists - he was going to think

hard with Roberto about the heavy atom derivatives. And who the acknowledged leader was was a little vague because it was clearly Roberto who'd brought the project to the RI and I had hitherto become the leading protein worker at the RI. But we didn't worry too much about that, we just got down to it and set about the job. In '62, we produced a low resolution structure of lysozyme. Now, it turned out that this was not anything like as interpretable as the low resolution structure of myoglobin which had been, to a considerable extent, interpretable at the low resolution stage, partly because there was a haem group that one could look for and find and secondly, because it was largely alpha-helical, which produced these links of sausages in the structure. Lysozyme, as it happened, had a somewhat more complicated structure, so the low resolution model was, well, interesting and promising, but not immediately identifiable. There was also an interesting complication that we were by no means the only group in the world that was interested in the structure of lysozyme and there was a degree of international competition about that. Larry Steinrauf, who'd worked with the Cambridge group at an earlier stage, and Dick Dickerson had teamed up and at that point gone back, I think, to the University of Illinois at Urbana and they were working on a different crystal form of lysozyme. Meanwhile, the famous Linus Pauling and Robert Corey at CalTech were also working on the lysozyme structure. And Pauling had an idea about using very large metal clusters as the heavy atoms to incorporate in proteins in order to produce the heavy atom derivatives with enough phase determining power to make the crystal structure solution easy. So, they were doing that at CalTech. And the three groups produced maps of a sort at roughly the same time or were on the brink of it. So, we arranged that we would publish letters in *Nature* together of which ours, I think, was the most complicated. Pauling and Corey had a fairly detailed map and Steinrauf and Dickerson published only a short note at this time.* Anyway, it all seemed very promising and, at this stage, we were absolutely convinced that we needed our own computer and we would have to press on with making the measurements. We made the measurements at that point on the laboratory prototype linear diffractometer. So, two things happened. We then approached the MRC and said 'We need a computer.' Now, Bragg - this was not quite his style, he was essentially, as people said, a paper and pencil, string and sealing-wax type; a scientist who'd always worked with the equipment that he'd made or designed himself - and he thought that buying an expensive computer was going it a bit. But nevertheless, he talked to the secretary of the Medical Research Council. And the secretary sent along one of his senior officials, a lady named Joan Faulkner who's actually Richard Doll's wife, who was working at that time in a senior role at the MRC and may I say in passing, a role that I think is very important and easy to undervalue. Because this lady came along and talked to us about why we needed a computer, was persuaded by our arguments, advised us and Bragg on how we would make an effective application for this quite considerable support, and generally nursed us along through choosing the right computer and so on. And in the end, we actually got the computer. I should have mentioned that from 1960 I had indeed been appointed a member of the MRC's external scientific staff, so my life was a bit more secure than it was and I did have a link with the MRC through that, as indeed so did Tony North. So, number one, we had the promise of the computer. Number two, I had an idea about further development of diffractometry, which was that if you have very large unit cell structures, as in proteins where the molecular weight of

* Lord Phillips has pointed out that this should have read: 'Pauling and Corey had a fairly detailed map, and Steinrauf and Dickerson published only a short note at this time.'

myoglobin, after all, is roughly 14,000 with 1,000 atoms in it, excluding hydrogen… So that was a large structure and the unit cell of the space lattice was very large. This means that in the unit cell of the reciprocal lattice that I talked about earlier the spacing between the spots gets to be rather small. So, it struck me that since these spots on a photograph would be rather small and close together, one could actually design a diffractometer to measure more than one at once. So, I devised a modification of how you used the linear diffractometer to measure three reflections at once. Now, that very much speeded up the data collection process and with these things together, between '62 and the end of '64, we collected all the data to two Angstrom resolution for the lysozyme crystals and three derivatives. We also collected not only enough data to make use of the isomorphous replacement differences in intensities, we also collected data to make use of the anomalous scattering of the heavy atoms, which is a slightly more complicated issue. It means that if you have a heavy atom in the structure, it will scatter the x-rays differently from one side of the reflecting planes than it does from the other side of the reflecting planes and you can make use of that difference also in the phase determination. And Tony North worked out a way of incorporating that in the Harker phase determining graphical procedure which, by this time, had been computerised by, well, a mixture of people… CrickAnd David Blow devised the methodology and Dickerson and Stranberg devised the first computer programme to do it, but Tony North elaborated that for our computer. So, all of these things came together rather quickly. Despite some setbacks; we hadn't noticed quickly enough that lysozyme crystallises in slightly different forms and initially we began collecting data from a mixture of these two forms which would have fouled up the results rather badly, but we saw that. Pauling and Corey, one of their contributions was they mentioned this and eventually we picked it up. So, we had to go back over our tracks a little bit but, nevertheless, early in 65 we actually produced a three-dimensional Fourier map, the final map being calculated on the new Atlas computer at the University of London computer centre. And we did numbers of the calculations there on the old Mercury computer as well, so there was a fair amount of going into the London University computer centre and working overnight on calculations. But, at this stage we recruited Gareth Mair, who did the Fourier programme for the Atlas computer. And on a visit to India in January 1963, which some people might remember as an exceptionally cold snowy winter… I'd been to India and while I was there, I encountered a young man who wanted to get into proteins and wanted to come to London, namely Raghupathy Sarma. So we recruited him and he turned up in the middle of '63, and he joined the team essentially not to do computer programming so much as to run the programmes with this vast amount of data that we had. So, it all came together much more quickly than Bragg had expected. Bragg said when the myoglobin structure came out, he said 'It will be ten years before we see anything like this again', and the fact is we did it in less than five. And he was by no means taken aback by that, but he was terribly pleased. We had a birthday party on the 30th March 1965 which was Bragg's 75th birthday at which Himsworth, the secretary of the Medical Research Council, was one of the guests. And the lysozyme team and the other people in the lab came in and we had the lysozyme model on display just completed and that, again, was a novel occasion.

MB So, you published in '65 on this structure.

DP We published the structure in '65. Two things about that. We built the model in one of the unused rooms in the Royal Institution's premises. There was a place used to stack infrequently consulted library books which was a dusty place full of book stacks as you might imagine, and we set up the frame constructed in the workshop on which we were going to build the model. There was a new principal technician by this time, Bruce Morris, and he made the frame and we bought brass models designed by Kendrew to construct the model from. And we built it all in this library stack space, and Bragg would pop in just about every day or twice a day, or three times a day, to see how it was going, comparing the look of the map which was plotted out on a succession of perspex sheets stacked one above the other with a light-box underneath and transferring that with a plum line and various other devices into a brass model. Eventually, Bragg sat on a stool in this library stack space and drew a freehand drawing of what the model looked like, and it was more complicated than myoglobin in that it did have some bits of alpha-helices in it which were easy to pick out. It also had a bit of the other standard structure that Pauling and Corey had predicted for fibrous proteins way back in 1951, the so-called beta-pleated sheet structure which is the structure of silk and is found in stretched keratin and all that sort of thing. So, there were some bits that we were half-expecting and recognised quickly when we saw it, and some other parts that were less expected, somewhat disorganised-looking bits of chain. But it all fitted very well with the chemical structure of the molecule which had fairly recently been determined, again, by two competitive groups; one group led by Pierre Jolles in France, in Paris, and the other one by Bob Canfield in New York. So there were two slightly different chemical structures to compare with our model and it all clicked into place really rather well. At the same time, roughly I suppose from '62 onwards, I'd also had a graduate student by the name of Louise Johnson. I'd done some reading around the lysozyme problem, of course, and knew that what lysozyme does is to open up bacterial cells, and it does that by breaking a connection in the structure of the cell wall. And the connection is a glycolytic linkage between sugar molecules and the sugars are, in fact, alternating polymers of two different sugars, N-acetyl-glucosamine and N-acetyl-muramic acid; they are both quite simple derivatives of glucose. So, I set Louise to work doing a straightforward simple crystal structure analysis of the strucure of N-acetyl-glucosomine, the component of the bacterial cell wall, and she did that perfectly well. Then, as a sort of top dressing we thought why don't we try, since heavy atoms will go into these protein crystals, why don't we try diffusing some of the sugar molecules into the protein crystal, because maybe they'll bind to the enzyme as inhibitors in the active site of the enzyme. So, we tried that, initially with N-acetyl-glucosomine itself, and then with the disaccharide and, eventually, with the trisaccharide and got some results at low resolutions.

MB So, these diffuse and bind?

DP Yes, and we got some results which were slightly puzzling here and there at low resolution but were, nevertheless, very promising. So, when we published in 1965, we were able to publish one main paper in *Nature*, on the three-dimensional structure of lysozyme and another subsidiary note by Louise and myself on the binding of these sugars as seen at low resolution which illustrated how these sugars fit into the groove in the enzyme structure which we, at that point, supposed was probably the active site of the enzyme.

MB Beginning to get towards an enzyme mechanism?

DP Absolutely so, yes. Well, this was all extremely exciting, of course. So there we were in '65, 'What do we do next?' and the answer to that was absolutely clear. What we do next is push on this 'Can we get at the activity of the enzyme?' So, we went on to study the binding of these sugars at high resolution, which meant making all the measurements at high resolution. But, by this time, we were extremely well tooled up, with the diffractometer and the computer programmes and so on, to do that. And by the beginning of 1966 we had some electron density maps which showed the sugar molecules, of which the longest was a trisaccharide, bound to lysozyme and we were able to begin building models of how these sugars fit onto the enzyme.

MB Aligning them with residues?

DP Absolutely, yes. The lysozyme molecule has a splendid groove down it and it's quite long and with the trisaccharide, the three sugars quite clearly fitted into the top of the groove, just up there. So Louise, who had been playing quite a main part in this new data collection and high resolution... But all the others, of course, were still working together as a team, except that I ought to mention the thing that I've left out and that is that at the end of the low resolution stage, Roberto Poljak, for a reason which I don't understand and I doubt whether he understands, decided that he wanted to do some real molecular biology and he ought to go to Cambridge to the Molecular Biology Lab and learn about molecular genetics. So, at the low resolution stage he actually left the team and went off to Cambridge to learn new techniques, leaving the rest of us to carry on with the high resolution structure. So, at this stage, lamentably, he wasn't involved. I said to him years afterwards 'Don't you ever regret going to Cambridge and leaving us to carry on with lysozyme?' And he said 'I sometimes wake up in the morning not thinking about it.' Anyway, there we were with this model of the sugars.

MB Getting towards chemistry though.

DP Of course, and you will remember, maybe, that as a schoolboy, chemistry was the subject I liked, really. Bragg and other people, of course, had been very interested in the lysozyme structure and had organised that in the spring of 1966 there should be a Royal Society discussion meeting on lysozyme. And Max Perutz acted as the organiser and, in consultation with us, he invited people from all over; Jolles, Canfield, John Rupley who'd done lots of kinetic measurements with lysozyme which I'll come back to in a moment and various other people were invited, and among those was a chemist from University College. At the RI, we'd had, should I say, an eventful relationship with Linus Pauling's son, Peter who'd been a graduate student with John Kendrew at Cambridge and then left Cambridge and been a graduate student with Jack Dunitz at the RI, associated with University College, and then had gone off to a job at University College. I saw him quite often and I said 'We're getting into needing to know about mechanisms of glycolysis.' And he said 'Well, of course, this is one of the strong points of the University College chemistry department and you should talk to one of the people there, and the chap I think you should talk to is Charles Vernon.' So, I said 'Well, that's fine', and either wrote or rang up Charles Vernon and said 'We've reached the point where we are beginning to wonder about the mechanism and action of lysozyme. How would you like to come over and talk

about it?' And I'd mentioned him to Max Perutz, so he was also invited to give a paper on chemical glycolysis in this Royal Society discussion meeting.* So, he wanted also to talk to me about what was going to come next, so that he could tailor his talk to focusing on the things that were proving to be important. So, one weekend when Louise was away, so she couldn't be involved, and the others were busy, I went into the lab where at this point I'd built this model which extended ... building these three sugar residues, just by guesswork and stereo-chemisty, how another three sugar residues might fit into this groove in the enzyme structure. When you did that, you found that the linkage between the fourth and the fifth sugar residues in the groove fell between an aspartic acid residue on one side of this enzyme cleft and a glutamic acid residue over on the other side of the cleft and the glutamic acid over on my side so to speak, the right-hand side as you look at it in the conventional way up. This was in a rather hydrophobic environment and in a naive way - this was a physicist thinking, you see, really, but longing to be a chemist - I said to myself 'Well, the chances are in that environment this glutamic acid residue would not be charged. It would still have its proton stuck on it. Whereas the aspartic acid residue over on the other side is in a polar environment and with a lot of hydrogen bonds and polar groups around it, the chances are that it would have lost its proton and be carrying a negative charge. So these two acid groups, one with a proton, the other without a proton, on either side of the sensitive glycolytic linkage' I said to myself 'must be involved in the process.' Now, there was one other feature. When I'd been adding on these residues down below the three that we could see, the fourth residue didn't fit in very comfortably because it got into too close contact with some of the atoms in the protein molecule. So I pushed it about a bit to see if I could get into a different shape that would fit. Now, you will know that glucose can take up a boat form or a chair form and then there are various intermediate forms called sofas or whatever. And I pushed this N-acetyl-glucosamine residue into a sofa confirmation, a slightly distorted shape, which had a planar group around the C^1 carbon atom. So, there's this model sitting, when Charles Vernon comes in to tell me 'What are the characteristics of glycolysis that I should be looking for?' So, he starts by saying 'Well, of course, you have to consider that the reaction would go through a transition state and the transition state would probably, is usually chemically in a carbonium ion where the C^1 carbon atom is carrying a positive charge. And that would lead to a change in the conformation, the shape of the glucose residue, so that there was a planar group around the C^1 carbon; that's because the C^1 oxygen bond has partial double-bond character. 'Oh yes?' I said. And then he said 'It usually goes by acid-base catalysis, so you'd need to have a group that can donate a proton and then it will be nice if there was something that would stabilise the positively charged carbonium iron.' So, I said 'Well, have a look at the model.' And he said 'Well, yes, I see, there's a proton donor, this glutamic acid over here, and there's a negatively charged group to stabilise the carbonium iron. Aand let's look at this sugar residue, you already pushed it into the conformation of the transition state. There it all is,' he said.* So, when he gave his talk at the discussion meeting, he explained chemical glycolysis in the way that I've just described to you stressing, as is quite proper, the transition state and acid-base catalysis and so on. And later on after Tony North had described the structure itself, I

* Lord Phillips adds: This is inaccurate. Consultation with Vernon's widow and others shows that Perutz asked the head of the chemistry department at UCL, Professor Ingold, to speak at the meeting and he passed the commission on to Charles Vernon.

* Lord Phillips adds: This account probably telescopes more than one meeting with Charles Vernon. But it is essentially correct.

stood up and described our model of the enzyme substrate complex, with this mechanism just crying out to be recognised. Marvellous.

MB The first time someone had proposed an enzyme mechanism?

DP Yes. But in retrospect I would say it was, not to me but to a lot of people, particularly physicists, it was a disappointment because it was straightforward chemistry. Nothing magical, nothing vital about it; it was just well-known chemical mechanisms. And it was all there, for the first time ever.

MB But, nobody had ever been there before?

DP Nobody had ever been there before, no.

MB Amazing.

DP And we were very fortunate in a sense in that although there were lots of other enzymes being worked on around the world where biochemists had studied the mechanism and identified the potential catalytic groups on the enzyme and had hypotheses about how the enzyme might work - ribonuclease was such an example or chymotrypsin another one - nobody had ever done any work of that kind on lysozyme. So nobody had seen this glumatic acid residue or the aspartic acid residue chemically. Nobody had any idea about the... The only bit of evidence that had come through which really was relevent and which really did help was that John Rupley had found that lysozyme catalysed - yes, he confirmed - catalysed the hydrolysis of the bacterial cell wall, but you could have smaller substrates. The trisaccharide wasn't a very good substrate, the tetrasaccharide was a rather better substrate, the pentasaccharide was better again and the hexasaccharide was the optimum substrate in small molecules. But adding on other sugars didn't make any difference. So, the hexasaccharide was the optimum thing to think about and that fitted exactly with the amount of space in this active site groove on the enzyme. So, that really did help, and of course knowing from the chemisty that we were confident that these two residues were glutamic acid 35 and aspartic acid 52, that was important too. So, you know, these things involved a lot of things coming together. Multidisciplinary work they call it, I think, but it all clicked that Sunday afternoon around the model at the Royal Institution.

MB That was the peak of it all, it must have been. Your whole career, I heard you say on one occasion, it was always downhill after that.

DP That's right, yes. And I was fortunate, and Louise and the others who'd been deeply involved in the work ... because they missed out on the Sunday.

MB There at the right time. But, you'd put a lot of time into that.

DP So, we had stereo slides of all this at the, which were assembled by Win Browne, the research assistant that I mentioned earlier on from the myoglobin days. And the lecture theatre at the Royal Institution was packed and people sat there, well...

MB Another milestone.

DP Yes. Very pleased.

MB So, from 1961 to 1965, you cracked lysozyme and put it into an exciting structure and also shown how it acts.

DP That's right. How it acts came at the end of '65, that's right...

MB And no-one's shaken that since?

DP ...early '66. Well, of course, you wouldn't expect other scientists everywhere, it wouldn't be proper for them simply to say 'Well yes, alright, that's how it works' and go away and not bother with it.' Lots of people thought well, it may not be like that and we don't really like this distortion of the sugar ring in position four and we must do some, we'll use some calculations. People started doing energetic calculations on protein molecules and it didn't work out in their calculations that you had to distort this sugar, the enzyme got distorted instead, and then people did molecular dynamics calculations and came up with alternative mechanisms. Then just three years ... no, 1991, a Canadian crystallographer in the University of Alberta in Edmonton, Mike James, and one of his graduate students actually studied the binding to lysozyme of a tetrasaccharide. Louise and I and others later on had tried that, but we didn't get quite such a good high resolution result. But Mike James and his graduate student produced an absolutely beautiful structure of the tetrasaccharide in which you could see the sugar in the fourth site, in this fourth site, actually in this sofa conformation making exactly the interactions that you would expect with the glutamic acid and the aspartic acid. So, yes, even the sceptics - there were some sceptics we encountered later in Oxford who tried hard to introduce their own ideas into it... But at this instant, I think the mechanism that arose between Charles Vernon and me on that Sunday afternoon back in - it was probably February 1966 - that stands. That's the way lysozyme works broadly.

MB David, it would be an anti-climax to really go on to much more after that, but there are one or two points that aren't quite to be missed at this stage. In that time, 1960 to 1965, I just want to come back to personal issues. You've established yourself in a strong research programme, you were married by then.

DP Married at the end of '60.

MB Yes, to a wife who probably didn't see a lot of you because you were tight into work, often at computer work into the nights, you were saying. But you did have a daughter at this time and you did lose a father, and I wanted to mark these events on the map before we close for today.

DP Well, my daughter was born in June 1962, a great event.

MB A great event.

DP That's right. She was born in St Thomas' Hospital. And that was just at the low resolution stage, you'll understand.

MB Came together!

DP A small baby and a low resolution lysozyme, that's right. And we were living in a flat in North London off the Finchley Road and my mother came to stay. My father, at that point, didn't come to stay, but he came to stay in December when after a certain amount of family disputation, it was agreed that Sarah should be baptised. So, my father who was somewhat dubious about this to put it mildly, he actually came up for the christening with my mother, and they stayed with us.
But he was by this time 85 years old, very bronchitic, and he came along to the church, and Sarah cried through the whole performance and my father said 'Well, I agreed with Sarah!' And we went home, and he was overtaken by bronchitis. It was the last of the London smogs. There was a very thick fog, and it brought on his bronchitis, and he struggled hard. The doctor produced successive doses of antibiotics which seemed to clear it up for a bit, but it would soon come back again. He couldn't throw off the infection and the doctor did his best to tell me that there was not much hope and so on. Anyway, come about December 28th, something like that, we arranged that an ambulance would come and take him to Paddington. And we'd reserved accommodation on the train and we'd take him to Gobowen and people would collect us at Gobowen station and take him home in Ellesmere and I would, of course, go with them. So, we woke up on this morning and there'd been a blizzard, but nevertheless the ambulance arrived and we got him downstairs into the ambulance and the ambulance got to Paddington station. No trains. So, we waited there for several hours in the first aid section at Paddington Station and it was clear that there were not going to be any trains, and the ambulance came back and we took him back home to the flat in Broadhurst Gardens where Diana had been re-making the beds, her mother was with her, and they hurriedly got things ready again for my father and mother. And we came back in and on the 1st January, New Years Day, he died.

MB Having, in a way, regained in the granddaughter, the daughter he'd lost.

DP That's right. He was sleeping in a twin-bedded room and we would bring Sarah in. She was then 6 months old, and she would lie on the bed next to him kicking and trying to roll about; that's the age at which they begin to turn over just about, but not quite in her case. And he would look at her with, I used to think, wondering eyes. Not saying much, but obviously thinking a lot. He was cremated at Golders Green and I went back with my mother to take her back to Ellesmere where we had a memorial service in the local chapel. And I came back to London, snow still everywhere, and two days later went off on a trip to India.

MB You'd said your goodbyes. David, events at that time begin to overtake you and a slight turn in your career is round the corner, but for today, I'm going to call it a day and we'll take up that next story on our next meeting here.

DP Thank you very much.