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Lord Phillips in interview with Dr Max Blythe
Oxford, 6 August 1996, Interview V

MB David, in our last interview we talked of the lysozyme story and that great presentation at the Royal Institution in '65, and the way in which the mechanism, the collaboration and the way that they came out and the international interest that immediately grew in that. I think you might want to tie up a few events. We looked at the central strands of that, and kind of events around that. It did become a big story with a lot of publications around it.

DP I suppose one of the regrets is that, curiously, there wasn't a major publication setting out in technical detail just how it had been done and I don't really know how that came about, writers block or something of that sort. Perhaps I had a touch of the sickness that affected John Kendrew when writing up myoglobin, but some papers came out starting with the *Nature* paper in '65. The big event at round about Easter-time '65 was a meeting of the Federation, meeting at Atlantic City in the United States where I'd undertaken to give a talk on lysozyme some six to nine months earlier, not really knowing what stage we would be at, at that point.

MB So, you planned to go there blind, as it were?

DP That's right, and I was able to write to American friends who'd organised it, Fred Richards particularly at Yale, whom I'd met a couple of years earlier, and John Edsel from Harvard who was also involved in organising this symposium, to say that I actually would have something to say. So they were all agog about it, and I flew off to Atlantic City and stood up and described the structure. The major slide was the slide of Bragg's drawing of the molecule. And I have to confess that, in describing it, I got lost halfway around which produced quite a laugh in the audience, but I recovered where I was and got started again and found my way to the end of the molecule. And it all went down, I have to say, rather well.

MB Was this an international meeting of biochemists, crystallographers?

DP It was the Federation of American Biological Societies so it was a very crowded symposium hall in Atlantic City and it was an annual event, I mean, a major event in the calendar. And there wasn't a paper, there wasn't a published paper from that. That was simply a lecture. Afterwards we gathered outside on the boardwalk at Atlantic City and people chatted excitedly about it and a couple of slightly nondescript people, whom I didn't know, came up to say 'Well, that was a very exciting talk. Tell me, do you think that after this it's likely that some of these other crystallographic studies that have been hanging fire for a year or two are likely to come to something?' And I said, not knowing who they were 'Oh yes, I think in the next year or two you can be fairly sure that studies at Buffalo and Newhaven and Harvard and California will produce similar sorts of results.' 'Oh' they said, 'well, we are very encouraged to hear that' and went off. And I later discovered that they were officials from the National Science Foundation and the National Institute of Health who'd been supporting this work for the previous five to seven years without anything very much coming out, and they'd been seriously considering cutting off support. And, as it turned out, in 1967, that's a couple of years after lysozyme, these structures that I'd talked about in Newhaven and Harvard and California started coming out. So, it had that effect. I mean, it created quite a stir. I went to the Gordon Conference in '67 and that was very nice, I must say. It was all very enjoyable and I went on a little tour of other labs in the States and talked about lysozyme. I was a popular lecturer that year, let's put it that way.

MB You'd not had a major career in lecturing, had you? This was quite a transition.

DP Well, I'd given a lot, quite a number of seminars by then, so it wasn't anything terribly intimidating, particularly since I had a good story to tell. I mean, the key thing about lecturing is to have something to say.

MB And you gave your Royal Institution discourse that year?

DP That was on the 5th November, in 1965.

MB And that did make an impact?

DP That, I think, was alright. My mother and a couple of friends from Ellesmere and an aunt and uncle came down and sat in the audience. Diana sat in the front with the vice-president for the occasion, that was Alexander Fleck, the chairman of ICI at the time. And there was quite a good audience from all over and they sat there and listened. That was before we knew about the activity, so that lecture ended with the very first difference electron density map showing that we knew where the substrate bound. So, that was a good, you know, this is where the future is leading us sort of conclusion to the lecture. So, that was a precursor to the events in 1966, the Royal Society discussion meeting in '66, at which we were able to describe the activity.

MB And which we did discuss last time, a great event.

DP Which we discussed last time, that's right.

MB And after that, I think, there was a paper in Tokyo that had profound effect?

DP Well, in '66, I again went to the Gordon Conference in New Hampshire and gave a talk which now included a discussion of the activity and in the summer of '66. I was asked by *Scientific American* to write a paper for that popular science journal on lysozyme and I said to them 'The problem is that it will require rather good illustrations.' So they sent along a man, a very characteristic New Yorker named Irving Geis who is an artist. And he took a multitude of photographs of the model and then started making pencil sketches with the aim of producing a painting of the molecule in which no atom obscured any other atom, which was an absolute *tour de force*. And this painting of the molecule itself showing the cleft and then the molecule showing the hexasaccharide substrate in the cleft, with the other subsidiary sketches showing what we thought the activity was, was the absolute foundation of this

Scientific American article which came out in the November of '66 and I believe is still the best-selling *Scientific American* reprint that has ever been. And it's really, aside from the Royal Society papers that came out of the discussion meeting, after rather a long delay in the spring of '67, that's really the only description, the earliest description, of the lysozyme mechanism.

MB That painting was quite remarkable. We're going to turn it up and put it into this video.

DP It is a remarkable picture and Geis went on to make a career… He'd actually begun by doing a similar painting of myoglobin so it wasn't his earliest *Scientific American* painting, but it established him as *the* person in the pre-computer graphics era who produced illustrations of protein molecules.

MB David, we've got you into 1966. I think you were being head-hunted, it probably is the wrong term, but Oxford University showed interest in you moving to Oxford. Can we begin to take that story in?

DP Well, that had begun relatively early on. I mean, as I've said, I'd known Dorothy Hodgkin vaguely from my days as a graduate student in Cardiff, but relatively well as a postdoc worker in Ottawa where she used to visit us, and then she became a regular visitor at the RI and she watched the myoglobin work that we did rather closely. And in '61, she made quite a serious attempt to get me to join her group in Oxford, but...

MB You weren't having that.

DP ...I wasn't having that. I thought we still have some way to go with developments at the RI and it wasn't long before we started seriously - well, I think we had begun seriously - with lysozyme. And I thought well, no, Oxford sounds exciting in the future but for the moment the RI is the better place to be.

MB But there may have been many reasons for you not to have been in that kind of group.

DP Well, I was also somewhat involved in being newly married and having a child on the way and things like that, so...

MB It wasn't the time.

DP It wasn't the time. But, later on, Dorothy and Hans Krebs, joined by John Pringle who was newly at Oxford as professor of zoology, began to make a rather serious effort to get me to go to Oxford. And at the end of '65/'66, knowing that Bragg was on the point of retiring, and he did retire at the end of '65 though he was still about at the the RI quite a lot, they made a serious effort to get me to Oxford. And I made it perfectly clear from the beginning that simply going by myself was no good, that wouldn't do. It was a team that they needed in Oxford if they were going to compete with the developing reputation of Cambridge in molecular biology. We talked to the MRC, and Himsworth of the MRC was very keen on this and gave the team idea his full support, and he talked to the registrar in Oxford, Folleat Sandford, and said 'You really need a team and you'd better arrange that.' That led to all sorts of problems because the people in the RI team... Tony North in particular was already paid by the MRC rather more than an Oxford lecturer's salary, without a fellowship, and therefore there had to be something done about a fellowship. And you could imagine the sort of problems in Oxford that that led to, because colleges felt, as I understand now much more clearly, that unless there was a teaching job to be done a remunerated fellowship wasn't a sensible course for them to take. So, it needed separate money for fellowships. And then Florey got involved, who was a key figure in Oxford of course, a key figure in the Royal Society, a key figure in all sorts of ways. He was already a member of the House of Lords. And he came to the RI and we talked about what the long-term prospect in Oxford was and whether the RI was likely to maintain its support. I remember him sitting in one of the RI's - I can only describe it again as 'dusty rooms' - in an old armchair saying 'Well, the MRC had better live up to its obligations, I am a Peer of the Realm, you know!' So, he was strongly supportive and the MRC, I mean, I have to say was strongly supportive from then on. And the Oxford fellowship dilemma was eventually resolved by Bragg talking to two of his friends; Sir Kenneth Lee, who was one of the major figures in Tootal, Broadhurst & Lee, the textiles firm in Manchester, whom Bragg had got to know during his days as professor in Manchester University, and another friend, Harold Hemming, who'd been a fellow officer in the sound-ranging business in the First World War and was a reasonably well-to-do character. And these two people put up private money at Bragg's request to fund fellowships for these extra people that I needed to take to Oxford, for whom a simple lecturer's salary wouldn't be enough.

MB And these were at Wolfson, I think?

DP So eventually, it turned out. To begin with, the people whom I wanted to take were Tony North, Colin Blake from the RI group and a chap called Ken Holmes who was in a different line of business. He was working on muscle in the Cambridge lab and was by now a personal friend, and he agreed to come to Oxford. And the fellowship problem affected him very largely and to begin with he was gong to have a fellowship at St Peter's College, and then again there was a last-minute uproar because he was offered a post as a director of the Max Planck Institute in Heidelberg. And, very much at the last minute, he decided, probably for the same sort of reason that made me feel that I would rather go to Oxford as a professor than stay at the RI with George Porter, felt that going to Heidelberg as his own man was probably the best future for him, so he pulled out. There was then a last-minute readjustment, as a result of which I recruited Andrew Miller, who was also working on muscle and fibrous proteins in Cambridge, and also, as a bit of top-dressing, a protein chemist in Cambridge named Robin Offord. Because, at this stage, I'd formed the view that protein chemistry, amino acid sequence determination, that kind of thing, was going to be an essential part of the base if we were going to do a proper job. So, somehow, I managed to wangle Robin Offord into the team as well and he got initially, I suppose, a temporary post in Oxford and Andrew Miller and Colin Blake got fellowships at Wolfson College, which was then fairly new, using the Lee-Hemming money. So in the end we went to Oxford with Tony North, Colin Blake, Andrew Miller, Robin Offord as sort of pseudo-lecturers, Gareth Mair as a postdoc still supported by the MRC, Helen Scouloudi supported by the MRC, Win Brown supported by the MRC as a research assistant, and a graduate student John Moult, and probably one or two other people. So quite a team. And we moved into some premises known at the time as 'old physiology.'

MB Before we actually do make that move, David, one or two things that we haven't just crystallised out. That coming to Oxford was tricky at times. It may not even have gone ahead. You did get a request to go to Edinburgh we should put on the record, I think.

DP Yes, in January 1966, when things were looking tricky in Oxford, I don't know whether Michael Swann actually knew about this but I had a letter from him in Edinburgh - he was the Principal and Vice-Chancellor – saying 'We think it would be opportune to set up molecular biology of your sort in Edinburgh. Why don't you come and give us a lecture and let's talk about it?' At that point, Bill Cochrane was professor of physics at Edinburgh, and he'd been a major figure at the Cavendish Laboratory in Bragg's day and was a very eminent crystallographer whom I knew. So I went up to Edinburgh and gave a lecture and we had some talks, and indeed they were very keen to recruit me, and just for a week or two it hung in the balance.

MB They moved quite quickly on it.

DP Then things swung a little bit in Oxford's way. And I felt that I'd got rather a long-term semi-commitment to John Pringle and Hans Krebs and Dorothy Hodgkin, at least, and I ought not to change ships too abruptly. And I had a chat with the people at the RI and we agreed that if things could be managed at Oxford it would be Oxford.

MB You'd already been down and addressed the faculty board of biological sciences, I think?

DP Well, I forget exactly when that date was, but going back as far as 1994 Tony North and Ken Holmes and I gave a series of lectures in Oxford, just to talk about the kind of thing that we would bring to Oxford that they hadn't got.

MB You said '94, I think you meant '64...

DP No, I meant '64.

MB So, the link had been forged, and I think it was in '65 you came and talked to the faculty board...

DP It may have been in '66. It was a rather awkward meeting of the faculty board. I didn't know about faculty boards. I was brought in from the outside and sat there and they said 'Well, it would be nice to have you.'

MB 'But, why a team?'

DP And various luminaries said 'Well', you know, 'why not just you? I mean, why not just come and let it grow. There are lots of bright people in Oxford who will want to come and work with you.' And I said 'It isn't quite like that. One', you know, 'one has to start running.' And in the end they came round.

MB Yes. I just wanted to establish that there were quite a lot of areas of debate before you came.

DP Oh, absolutely.

MB With a lot to resolve. It wasn't an easy story.

DP That's right.

MB But, you came. You were appointed to arrive in October...

DP That's right..

MB ...'66?

DP '66, yes.

MB To the old physiology building?

DP That's right.

MB Where you established a presence quite quickly?

DP Yes.

MB But before then, even that summer you'd established a presence, I think, by running a short course in Oxford?

DP Yes, the British Biophysical Society decided that it should begin running summer schools and it asked me, I suppose, because I was the figure of the moment, to organise this summer school in Oxford in the September of '66. So I, together with an organising committee involving John Pringle and Tony North and Andrew Miller and various other people, put together a programme. And we ran a British Biophysical Society summer school, again, based in the zoology department, and numbers of people came from all over the country, and we gave lectures and had after-dinner speakers. The after-dinner speeches were an interesting idea because we had people like Krebs and Pringle talking about the future as they saw it. And I must say they weren't at the time terribly popular lectures, because a lot of the participants in the conference didn't at all understand, I mean, really what people like Krebs and Pringle, what their view of the future was because they were talking about, you know, slightly way-out topics. But the whole thing was a considerable success, I think, and it got us launched in Oxford really rather well.

MB Did the Pringle and Krebs' view of the future coincide with yours?

DP They were much less detailed molecular people than I was. Pringle it has to be said had a view of zoology which was way ahead of its time. And he saw zoology as a subject which ran all the way from detailed molecular structure up to populations, and that's what he tried to build up in Oxford. That was why he was keen to get me and my group established in Oxford and why he brought in other people as well. That was a rather far-sighted view.

MB Your ambitions coincided with that, surely?

DP Well I agreed with that. And if you take a look at the recent, relatively recent, technology foresight exercise, they talk about this as 'integrated biology', so it's become the fashionable view which was Pringle's. It was Pringle being quite significantly ahead of his time. And that's one of the big reasons why we came.

MB Because you had this view of creating a department that could really bring in a lot of disciplines together, looking at fine molecular development.

DP Yes, though as things turned out, of course, it's one thing to have a professor with a vision of that sort and Pringle certainly had that, but it's another thing to attract students to a zoology department who share that vision. And the zoology students continued to be, in a very large part, attracted by a much more natural history view of zoology. So they were people much more attracted by Timbergen and animal behaviour and that sort of end of biology. It extended through to genetics and physiology, but molecular structure never became, I'm afraid, a central feature of the zoology course that we... We worked very hard at introducing options on structural and molecular biology as we called it eventually, and I think it's fair to say we attracted only one graduate student who'd done honours biology into the molecular biophysics laboratory during the whole of my time in zoology. So, in terms of undergraduate teaching, it didn't really work out because that wasn't the image that students have of zoology at school. Still isn't, probably.

MB But you did attract, in due course, a lot of postgraduate students to Oxford.

DP But, they came. That was strongly my view that whereas the Cambridge lab had worked essentially with postdocs, many of whom came from the United States supported by Helen Hay Whitney fellowships and suchlike and then went back to the States, a multitude of them, the Cambridge lab had rather few graduate students. I think it's true to say that Max Perutz had only two graduate students in his career as director of the LNB, Cambridge, one of whom was David Blow. And John Kendrew, I think, had only one graduate student. But in my lab it was essentially based on graduate students. We had some postdocs, of course, and we had numbers of - some of them very distinguished - postdoctoral visitors who were attracted by things like All Souls' fellowships. That was one of the attractions of Oxford. And graduate students included Rhodes scholars; that was another of the attractions of Oxford. But we were essentially a graduate student based lab and these people came from chemistry, biochemistry and physics essentially, not really mainstream biology at all, because molecular biology ... well, let's be frank, what were molecular biologists? They were, as Chargaff elegantly put it in the heated days of the DNA debate, they were biochemists without a licence, and it took some time for that to break down. And that coloured, to some extent, our reception in Oxford in 1966.

MB Yes, I'll keep that for a moment, because that reception was an interesting one. But while you're talking about the difference between the Cambridge molecular biology laboratory and the one you were creating in Oxford, the one that you did create and take through in Oxford over 25 years, what were the relationships with Cambridge like? You talked about collecting people from there to be part of your original team. Were relationships good?

DP Oh, relationships remained very friendly. They were nothing like as close, of course, as they had been in the period up to '61/'62, when Colin Blake and I were still involved a bit with myoglobin and Tony North was still involved with haemoglobin. But we broke free a bit from the Cambridge connection in the latter part of the RI days. And although we remained on very friendly terms with the Cambridge people there was an element of rivalry, of course. I mean, we certainly saw it as 'What can we do to compete with this world-famous outfit in Cambridge?' setting up something new in Oxford. And I'm not aware that to begin with they felt that we were a threat at all, but I hope that towards the end they began to wonder whether they were quite matching some of the things that we were doing. But you'd have to ask somebody else about that.

MB You tended to keep what you were doing under wraps from Cambridge, a bit?

DP Well, no, no, not really.

MB No?

DP Not really.

MB So, you had an open relationship?

DP It was a fairly open relationship. We had lots of seminar speakers from Cambridge. And we had a weekly seminar series in molecular biophysics every Friday during the whole period that I was there, as well as a Monday lunchtime series when people from the lab talked to one another, and anybody else who wanted to come, about what they were doing. That was an attempt to make sure that graduate students exposed their problems as well as their results, and sometimes there were people from Cambridge there listening to that. So, the relationship with Cambridge was perfectly amicable. There was no problem.

MB As good as could be expected?

DP Oh, absolutely, yes. And in fact relations internationally were very close. We had not only close relations with Cambridge, but as time went on there was a migration of some people from Cambridge to Bristol where another group was set up, led by Herman Watson and Hilary Muirhead who came from, went there from Cambridge. And we had close relations with them as well. And we had close relations with Fred Richards'' group at Yale and Lipson's group at Harvard. And it was a very friendly time in the area because there were relatively small numbers of workers in the field. I mean, one of the features in '65 that I should have mentioned, I think it must have been '65, in the winter of '65 I think, Perutz organised an international workshop at Hirschegg which is just in Austria, but you can only get into it from Germany. So we all flew to Munich and travelled by bus to this little valley in Austria, families included, in the winter of course because Perutz was a keen skiier. Then in the morning those of us who could ski at all did, and in the afternoon we talked, and in the evening we talked science, and lysozyme was a big feature because it was one of the only two structures known. I suppose there were about fifty people there, there may have been a few more not counting families, and they represented roughly all the people in the world who were really expert in this subject at that instant. We all knew one another and were, in a large part, personal friends and it was a very, very pleasant time. It has, of course, grown from that enormously. But there we were, all the children or grandchildren of Bragg or Pauling or Perutz or Kendrew or, rather few precursors, and it was a very good time to be in the subject.

MB You come to Oxford, David, and get a mixed reception I gather. Because there are people who welcomed you with open arms, but there were some quite suspicious biochemists, I think, because there were protein biochemists I think...

DP There were people of course doing protein biochemistry in the biochemistry department, and although eventually they became very good friends initially I think their reaction was 'Well, why do we need these people?' On the other hand, there were people junior to Pringle, Krebs and Dorothy who thought 'Well, very good.' And chief, perhaps, amongst these were a couple of young men in chemistry who wrote to me when I was still at the RI and said 'We've heard about lysozyme and we are very interested in moving chemistry into biological chemistry and we'd like to come and have a look.' So they came and had a look. And these two were Jeremy Knowles and Gordon Lowe and they came and had a look and they became friends, and when we got to Oxford they were extremely welcoming. I remember we - Diana and I - stayed a weekend with Jeremy Knowles and his wife during which, for some reason, we had to go back to London, and there was a car problem, so they lent us their Morris Mini Minor in which we drove recklessly back to London. That was all very welcoming and friendly. So there was a mixture, as you say, of suspicion and welcome.

MB Yes, Hans offered you a laboratory.

DP Hans Krebs said 'Well, we're very interested that you brought this chap Robin Offord with you, and he will make a very good link with the biochemistry department and I'd like to offer you a room.' And I said 'Well, that would be very useful because he will need a facility for high voltage electrophoresis which you haven't got, which will be useful to you too and we certainly need that, and he has demonstrated in his work in Cambridge how valuable it is.' And Krebs said 'Well, fine, I'll give you a room in the old part of biochemistry, but I'll have to put it under the new rules. I shall have to put it to the departmental committee.' Now, the departmental committee contained some of these slightly suspicious people and some who'd already been quite friendly, but when he put it to the vote at the departmental committee the committee decided unanimously that they were much too short of space to give any to this new molecular biophysics outfit. Nevertheless, Krebs gave us the room and it was set up with a splendid new high voltage electrophoresis system and before very long the biochemists were using it. Among the biochemists who, no doubt, took part in that vote, at least I suppose they did, but showing a different side of their welcome was a lecturer named Paul Kent who was also, in the splendid Christ Church terminology a 'student at Christ Church', that is to say he was a tutor at Christ Church. And he came along and said 'Well, I'm sure you need graduate students and I have a young man just graduated who would be quite interested. Why don't you take him on? And his name is Ian Swann.' So he was our first Oxford-recruited biochemist graduate student who joined the lab roughly when we arrived, and was very successful and went off eventually to become a lecturer in the University of Glasgow, to start the subject there, and then extremely sadly died young of some form of leukaemia. So that ended sadly, but it was a part of the very promising welcoming beginning of Oxford. So don't get too gloomy a view. I mean, all of this initial difficulty faded away quite quickly. And then of course there was the college side. There was a sort of competition. The general board said 'We've appointed a new *ad hominem* professor of molecular biophysics. Would any college like to take on this professor as a professorial fellow?' And I don't know how many colleges expressed interest, but Corpus Christi did, led I think largely by a historian there named Trevor Aston who'd heard of molecular biology and thought it was quite a good subject to bring in. So I went along and had dinner at Corpus, and they put in a formal bid which was accepted, and I was appointed a professorial fellow at Corpus. When we were moving to Oxford there was a slight hiatus because we had a little trouble finding a house and we had a little trouble selling the house in London. So, Diana and Sarah stayed behind in London during the autumn of 1966. But by that time we had actually settled on a house. Having looked at numbers of houses on the market, we eventually found a builder who was building a little block of houses in Upper Wolvercote in the garden of a house owned by a lady named Joan Scrutton, who was well-known in Oxford I think at the time. And we went to look at her garden and had a picnic under a chestnut tree, it was in full-bloom so it must have been in May of 1966, and we thought 'Well, what a very pleasant garden.' There was a line of willow trees and some fruit trees and this splendid chestnut tree and we thought 'This is rather nice. We'd better go and talk seriously to the builder.' So we did and we visited his house. He was a chap named Rendel who lived on Cumnor Hill. So, we went along to see him. And he lived in a house he'd built by himself, and we looked at his house and we looked at him and we asked what his plans were. And he said 'Well, I'm going to divide the garden into four plots, of which the top left one will be the largest. And then there will be three others, one next to it, and then two further down the road, and there'll be a new road joining onto the road to Upper Wolvercote.' And we said 'Well, we'd like this plot in the top left hand corner.' And he said 'Well, I've already got an offer for that.' And we said 'Is it serious? I mean, we really would like a house there.' So, he said 'Well, what sort of house would you like?' So, we said 'Well, how about a house like this one? It wouldn't fit very easily onto that plot because of the slope of the land, but the mirror image of this would fit quite well onto that. It would put the front door in the right place. And we'd want a few changes like an extra window instead of that fireplace and so on but nevertheless, essentially the mirror-image of this house on that plot.' So, realising that this was a rather larger venture than the offer he'd got, he closed with us there and then for his house on that plot. So that was settled in May and he started building, well not until September probably, but he built the whole thing in three months because we moved in in January, as it turned out.

MB I think that's January...

DP January '67.

MB So, I wonder whether you first met him in '66?

DP Yes, that's right. It was May '66 that we sat under the chestnut tree.

MB We may have said '65.

DP Well, I'm sorry.

MB No, no, no. We've got it on the record now, but I was just thinking of that particular timescale.

DP Yes.

MB That was a fine move, but you'd had to stay back home in Hampstead Garden.

DP We were living in Hampstead Garden suburb at that time. We had been there for three years and liked it very much. In fact Sarah, my daughter, never forgave us for moving and when it came to her going to university fifteen years later when she was eighteen, she went back to London University and lived in Hampstead and wanted to go back.

MB Roots, yes.

DP Roots she had. It's funny how quickly they develop because she was only four when we left.

MB In that house you had an offer to become secretary of a research body?

DP Yes.

MB Before coming to Oxford, attempting Oxford?

DP That's right. Gordon Cox - another crystallographer, you'll find them everywhere, he'd been professor in Leeds, and then became secretary of what was then the Agricultural Research Council - lived in Hampstead Garden suburb and rang me up one day. Now, that would have been in the middle of all this commotion about going to Oxford, just about when it was settled. And he came along to tea one day for the first time, although we only lived half a mile apart. And it turned out that what he wanted to say to me was 'I shall be retiring from the ARC because I shall run out of time in a year or so, next year probably, and I think I would rather like you to be my successor.' This took me back a great deal because...

MB He'd never mentioned it?

DP And I had no experience of that sort of public service at all, and where his view of my propensities as a public servant of that sort had come from I've got no idea, but he said to me 'Can I put your name forward?' And I said 'Well, that's very kind of you, but I'm just about signed up for Oxford and I think at this point I'd better go there.' So, he said 'Well, I'm very sorry about that', and we ended the conversation, still friends. He'd actually been the external examiner in my PhD, but I can hardly believe that that experience which had taken place in '51, that's to say fifteen years earlier, had remained with him and left him the impression that I'd be a good secretary of the ARC.

MB But, somewhere he'd got this insight into you.

DP Somewhere he'd got that. I mean, he remained a good friend and I'm afraid he died just a month or two ago, and in the period leading up to his death I went to see him three or four times when he was still in very good form, though suffering from a terminal illness. He was still in Hampstead Garden suburb, and we had some of the most entertaining and happy conversations of my life, recollecting his career, my career, talking a bit about his illness, what treatment he was getting, but not dwelling on that, talking about the people we knew, recollecting this event and so on. It was one of the pleasant professional connections that one comes across in life.

MB And there were many. David, I've got you virtually to Oxford. We've gone through a whole range of foothills of getting you there; trials, tribulations, all kinds of accolades, people wanting you to come, a whole range of responses to your arrival. And at that point I wanted to just wind down, take a coffee, have a break at this point and then come back and have a look at how that Oxford story unfolded, but before we go just to mention we've done well to cope with quite a bit of building work going on behind here. I don't know how much has gone on the record as well, but I think we've coped very well. Thanks for staying with that.

DP I find it very interesting just to talk about it.

MB So do I. We'll wind down for the moment.

DP Alright.

MB David, we're winding back up. We can now talk about the Oxford years. We've got you established in a home there where you were very happy, but what was the home like they found for you in old physiology?

DP Rather an old-fashioned lab, as the name implies, divided up, no doubt, to suit somebody's earlier purposes with various wooden partition walls. Numbers of rather small rooms which were easily enough allocated to various individuals. But one episode remains very clear in my memory. There were a couple of rooms separated by a wooden partition which were not very much use as they were and would make a perfectly good lab if we could get rid of this wooden partition. Now, this illustrates perhaps what I hoped was the team spirit in those early days. We didn't write to the university surveyor and say 'We'd like this partition taken out,' or speak to John Pringle and say 'What would you do about this?' We went out and bought a chisel and a saw, and Robin Offord and Tony North and Colin Blake and I one afternoon removed the partition. So it was that sort of space, but it was perfectly adequate to get us launched in Oxford.

MB And you brought equipment down from the RI?

DP Yes, but not all that much.

MB And the MRC continued their support?

DP Well, we had an equipment grant which illustrates really rather well the difference between life nowadays and life in those days. The equipment grant really had to do not with old physiology, but with the prospective new laboratory in the physiology building that John Pringle was already planning on the corner of South Parks Road and St Cross Road that later became known to some people as 'Pringle's Cave', you may remember. Well, John had secured money from the Nuffield Foundation to build the top floor on this building for molecular biophysics. And the rules in those days were that if a university was setting up a new department or laboratory in a new building the UGC automatically provided an equipment grant to set it up. And all that one had to do was to write out a list of the equipment one needed, and the UGC then submitted this list to a couple of referees; they chose the referees, it's fair to say. So, the referees they chose were John Randall, who was professor of biophysics at King's College, London, an obvious choice, and Albert Neuberger who was a very well-known professor of biochemistry. And they looked at this list and reported on it, and their comments were 'Well, it's a perfectly reasonable list. The trouble is that Phillips has somewhat underestimated the amount of biochemical equipment that he'll really need if he wants a proper and a disciplined laboratory, and he's somewhat underestimated the amount of workshop equipment he'll need. He'll need another precision lathe, for example, because he'll still be involved in making equipment and that will bring the total up to...', whatever it was. It was somewhere between £250,000 and £300,000, which was money in 1966. Now, that money I suppose was paid over when we actually moved into the new building, which was rather later, but that was where the equipment money came from. Imagine that happening now? That's part of the background I think to the problems that universities are experiencing with funding. They certainly were treated very generously in the fifties and sixties up to, I would say, about 1972.

MB And you feel there's now a disaster afoot, I think?

DP Well, things are certainly very different, and it's partly based on memories of that period. Running ahead a little, it was in 1979 I think that Shirley Williams, who was then the secretary of state for education and science in the Callaghan government, said 'For the scientists, the party is over.' That's what she was referring to.

MB And when we look at your interest in research and funding that went on and on in an administrative way of some prominence, we can talk about the transitions and summarise where things have got to. Right now, we'll just stay with that early laboratory. How did you settle when things got going? Did you begin to draw people in right away in a collaborative way?

DP Yes, I think the answer to that has to be, and it was partly the Jeremy Knowles/Gordon Lowe connection, it was partly ERH Jones, the professor of chemistry whom I'd been involved with in another activity with Bragg, which was the International Science Hall in Brussels somewhat earlier. So he was already a friend, and there were various other people about who made life not only easy but pleasant. There were scientific developments too which made it even easier to bring people in.

In about '68, I suppose, it may have been as late as that, it might have been a bit earlier than that, a couple of biochemists in Leeds, Peter Campbell and Keith Brew, were working on the chemical structure of a milk protein, alpha-lactalbumin. And on, I must admit, rather sketchy evidence, they suggested that it may be related to lysozyme. Now, the evidence was indeed rather sketchy, but they went on and determined the complete amino acid sequence of alpha-lactalbumin, which was roughly the same length of polypeptide chain as lysozyme and there were a lot of clear relationships between the two sequences. So, in about '69 I suppose, though I'm a bit shaky on the dates here, we built a model of lysozyme, yet another model, and by we I mean Tony North and Win Brown essentially. And then we took off the side chains that were different in the two molecules and replaced the lysozyme side chains by the alpha-lactalbumin side chains and did a bit of twiddling about here and there in only three or four places and lo and behold the sequence fitted perfectly well into what was essentially the alpha-lactalbumin in the lysozyme model. So, we wrote a paper about this which was published in the journal *Molecular Biology*, and it was the beginnings of an interest in evolution at the molecular level. How did all these different protein molecules evolve? Are they all all that different? What are the relationships between sequences? One can imagine that protein molecules with the same sort of function that kinase has, for example, have perhaps the same sort of structures, or dehydrogenators have the same sort of structures. But lysozyme and alpha-lactalbumin which have no known relationship in function, how do they have the same structure? Is it right that they have the same structure? So, we set about trying to crystallise alpha-lactalbumin to try and work out the three dimensional structure and make sure that we were right. And that I have to say took a very long time. It wasn't until I was nearly leaving Oxford in the late 1980s that we actually got round to working out the three dimensional structure of alpha-lactalbumin. But, indeed, it does have essentially the same structure as lysozyme. And since then there's been a great deal of interest in evolution at the molecular level of course, promoted a great deal by developments since then in the sequencing of nucleic acids and the whole human genome effort and all of that, which is a totally different story, and somewhat in the future. However, the alpha-lactalbumin story played some little part I think in what was quite a major development in Oxford. We began to say to ourselves 'It is all very well to study an individual enzyme, lysozyme. That's exciting, and we think we know how it works. But when you get into biochemistry…' And, of course, we were all trying very hard to learn a bit more about biochemistry at this point, those of us unlike Robin Offord, who were not professional biochemists What about all these pathways in biochemistry, how did they evolve? For example, there's a famous pathway in biochemistry which is involved in, effectively, to put it in a simple physicist's terms, the controlled combustion of sugar. If you need some energy, one way of doing it would simply be to put some sugar on the fire and it would all blaze up really rather merrily. And that's what happens in your body but it doesn't blaze up, it has to be done in a controlled way, step-by-step. And it happens in the glycolytic pathway, so called, which was well worked out by this time and involves a series of say eleven separate enzymes which carefully stick on a phosphate group, move a phosphate group, take off a hydrogen atom, cut a hexamer into two trimers, isomerise one trimer into the other trimer, manipulate the other trimer and in the process generate ATP molecules, which are the molecular storehouses of energy. So, instead of flames, you get a store of ATP. Well that in simple terms is glycolysis, but how on earth did that evolve? What are the relationships between the different enzymes in this pathway? The produce of one enzyme is the substrate of the next enzyme. What is the relationship between the two? Does the binding site of one enzyme look like the binding site of the next enzyme, and so on? So, why don't we start looking at enzyme pathway instead of picking on individual enzymes? Well, it happened that some biochemists, protein chemists at Cambridge, particularly a friend from Cardiff days, Ieuan Harris, who'd been an exact contemporary of mine in the physics class in Cardiff though in fact he became a chemist, biochemist, protein chemist, protegé of Sanger and was now in molecular biology in Cambridge, was working on these glycolytic enzymes. Glyceraldehyde-3-phosphate dehydrogenase I think was one of his targets, and he was one of the people who said 'One should look at pathways, let's not look at individual enzymes.' And he also persuaded Herman Watson, who was the Cambridge crystallographer who came rather late into the myoglobin story and then went off to Bristol to start a school there with Hilary Muirhead. He was persuaded of this as well. So a little, to begin with, UK project began on 'let's do glycolysis.' Now, there was in Oxford a chap, Stephen Whaley, working at the time in ophthalmology of all places, who was working on one of these enzymes, triose phosphate isomerase. And he thought wouldn't it be a good idea to crystallise it, and he did that, and since this was shortly before we got to Oxford he went to tell Dorothy Hodgkin about it and she thought that sounded a very interesting project. And when we got there she, I think, mentioned it to us and about 1970 maybe, and again you know dates are difficult to remember precisely, Louise Johnson and a visitor from the States, Richard Wolfenden, did some preliminary crystallographic work on triose phosphate isomerase. And I persuaded Robin Offord that he should join with Stephen Whaley, who by this time had moved into the pathology laboratory where he was working with Edward Abraham of penicillin fame to work on the amino acid sequence of triose phosphate isomerase. And so a development began there. Now that was all tied up with the beginnings of perhaps the major early development in Oxford, which was the origins and development of something that became known as the Oxford enzyme group. And that was an important part of my life in Oxford for the whole time I was here and deserves a little bit of discussion. Tim Jones... And incidentally everybody else in the world called triose phosphate isomerase TPI, but we called it 'Tim', but never mind about that. The reason was Tim Jones at the time was a member of the council of the Science Research Council and was involved in a very heated I think you'd have to call it debate with the Science Research Council about why the physicists, especially the particle physicists and astronomers, got in his terms vast amounts of research money whereas the chemists got really rather little. And he went on about this, supported by some chemists and also some biologists, because the SRC supported the whole of science at this point and a lot of people were uptight about the relative proportions of funding that went to the different disciplines. So eventually the chairman challenged Tim Jones and said 'Well, suppose we gave chemists more money. What on earth would you spend it on that would compare with the money that particle physicists need?' So, Tim Jones said 'Well, for example, look at recent developments in enzyme chemistry. We now begin to know the structures of enzymes. Think of the potential of that in chemistry and biology. Think of the effort that's needed in that, in equipment and everything else. What you need is a special initiative in, let's call it biology or chemistry or something of that sort.' 'Alright', they said 'We'll set up a subcommittee to look into the sort of work that we might support in that area which would need extra funding. And since you've made such a fuss you can chair it and you can assemble a committee and get on with it.' So Tim Jones recruited Jeremy Knowles as the scientific secretary of this committee. He got me to be a member of it, and since it couldn't obviously be an entirely Oxford effort he collected various members from around the country. And we had this committee operating for a few months which produced a report which said precisely what Tim Jones had said, that here was a fantastic opportunity for the future of chemistry and biology and biomedicine and everything else, and it would require a very considerable investment and why doesn't the SRC do something about it? So they considered the matter and agreed that they would set up, for the short term at least, a grant committee of which Tim Jones would be the chairman and I and others could be members. And we would ask for grant applications from people in this area to see if there was any real interest on the ground in this field. Well of course there was interest on the ground. And in Oxford we'd already begun to generate that interest in quite an elaborate way, starting with, say, Jeremy Knowles and Gordon Lowe, but very early on involving Rex Richards, the then professor of physical chemistry, who was the NMR expert and was wanting to get into biological studies with NMR. And there was a chap named Arthur Peacock beginning to do such studies, and Bob Williams, RJP Williams, the inorganic chemist, who'd been led into the role of inorganic materials in biology, partly through a collaboration with a biochemist in Harvard Medical School, Bert Valley. So Bob was extremely keen on developing the subject of inorganic biochemistry. Stephen Whaley too. People scattered around, you will notice, in a number of different departments decided that maybe an inter-departmental group should somehow be brought together to initiate research in this new area, and how we were going to make this gel was a problem. Now, at some point before this, sitting innocently in my office one morning, my secretary announced that a gentleman whose name I have now forgotten but who came from the famous multinational chemical firm Du Pont would like to see me. So, unannounced, this chap came in and he was a friendly little man and we had an amiable conversation, at the end of which he said 'We've been very interested in the work you've done with lysozyme and we see a great future for this work. And it's part of our policy to try to encourage innovations of this sort by providing limited but absolutely free resources to people that we think are likely to advance subjects of this kind. So, what I'm here to say to you is Du Pont would like to give you a grant of $3,000 (three thousand dollars) a year for three years to spend however you like. If you want to buy a new car, alright; if you want modern paintings to hang on your rather shabby walls, well', - this was still in old physiology - 'alright; spend the money however you like.' So, I said 'Well, thank you very much', and off he went. And sure enough, a cheque for $3,000 (three thousand dollars) arrived which I paid into a special account, and later on that was increased to $4,000 (four thousand dollars) a year for the second two years. So, here I was with this little sum of money, and I didn't want simply to spend it on a little bit of apparatus here or a trip to Utrecht there or whatever. I wondered how I might usefully lay out at least some part of this money. So I said to myself why don't I get together the people who were interested in enzymes in Oxford, they're quite keen on coming together for fortnightly seminars, so why don't we make that more attractive by having a fortnightly dinner? And we'll take advantage of the college system and have dinners in the different colleges that these people belong to and we'll talk about their interests in enzymes and what they would like to do and how we think this work might develop. So, that was really the practical start of the Oxford enzyme group. We set up this series of fortnightly dinners on Monday evenings and we migrated from one college to another, which introduced us to lots of colleges. It brought some more people in, we had occasional outside speakers to talk about what they were doing with enzymes. And by the time the SRC had said 'Well, we'll have a grant committee' we were a fairly well set group of people prepared to say 'Well, now just what is it we need?' So, what we needed was some rather expensive NMR gear, that was Rex (Richards) and Arthur Peacock initially, and other people like Bob Williams beginning to feel that this was a side of the story that they could get into technically. We needed some more work for kinetics. Eric Newsom in biochemistry needed some more gear. But most of all what we needed was some postdocs, not just particular postdocs for the SRC to say 'Yes, you can have a postdoc to work with Bob Williams', but for the group to decide for itself. You can have a postdoc who would work between, say, Robin Offord and Jeremy Knowles, or between Robin Offord and Stephen Whaley. So, we put in a sizeable bid for this apparatus and a pool of postdocs to be allocated to groups within the enzyme group by the group itself. We would be a self-managed group under this committee and, for some reason, the SRC bought that and produced a sizeable grant and we recruited postdocs and we allocated them in this collective way, and the whole thing started to run really rather successfully. We recruited people, we bought apparatus, we continued our fortnightly meetings with dinner as long as my money continued, and without dinner when it ran out. But for the whole twenty-five years of this enterprise when I was in Oxford we had meetings every other Monday in term, with a seminar on what was going on in the evening after dinner at 8 o'clock, on what was going on within the enzyme group. And at intervals of three years or so we put in new applications to the SRC which abandoned this committee after a time, but it continued under other committees to consider these grants. And we built up the NMR from 90 megahertz to 250 megahertz to 360 megahertz to 500 megahertz and now it's 750 megahertz NMR machines. And the whole thing was a tremendous success. If you want some sort of measure of how successful it was... And it involved as I've said people from more departments than I can remember off the top of my head - I mean, molecular biophysics, physical chemistry, the Dyson Perrins, inorganic chemistry, pathology, clinical biochemistry, physics for a short time and so on, all these different departments. It has one way or another affected, I think, the election of getting on for a dozen people to the Fellowship of the Royal Society over that twenty-five year period. So on any sort of count you'd have to regard that as some sort of success. And it began with Tim Jones, and Rex Richards, who was the first chairman of the enzyme group. He stood down when he became vice-chancellor and for a shortish sort of time I was the chairman, but the thing was up and running, with Bob Williams probably the most active and creative member of the enterprise by that stage.

MB It was a massive surge in Oxford.

DP It was an enormous surge and it spread out into other parts of the country as well. I mean, you mustn't think for example that this new SRC committee was simply something set up to benefit Oxford. One of the early grant applications that we considered on the committee was one from a couple of young people at University College London, one of whom, Peter Dunhill, had been a research student at the Royal Institution in my day, though he was working with David Green on beta-lactoglobulin rather than on any of the other proteins. By this time he had moved into biochemical engineering and he put in an application along with a colleague which said 'one of the big problems in this area is going to be preparing the material. We really have to go into biochemical engineering on a big scale.' So they put in a sizeable grant application and the committee in its wisdom... And I say that with a slight feeling of guilt because I can't honestly say that I was utterly convinced by the proposal at the time, but nevertheless, the committee overall was, and it went for it and produced a biggish grant for University College, London to support these two rather young people. And they set up what is now the department of biochemical engineering at University College London, headed by these two chaps. Peter Dunhill is a Fellow of the Royal Academy of Engineering and his colleague is still there and a Fellow of the Royal Society. And it has been the training ground for a good many of the people who are now involved in the bio-technology companies around the country, who depend greatly on being able to prepare proteins in large quantities. So that and other places such as Bristol, benefitted from this initiative and, I think, helped to keep the UK running for quite a long time ahead of the rest of the world. By that I mean, at least keeping pace with the UK.

MB USA?

DP The USA, yes. Tim Jones, it has to be said, did...

MB Set something really off.

DP ...did quite a good job.

MB An enormous contribution. David, just before we wind this interview down, because we're coming to the end of this particular tape and our time here, it would be good just to get you to the point where I think you've become a member, as an *ad hominum* professor, a member of the faculty board of biological sciences. And you chair that board in the early seventies, '72 to '74-ish?

DP Yes, those were the years. People took turns more or less in being chairman of the faculty board, alternating between so-called 'official members' - and they were elected from the professoriate... I was an *ad hominem* professor so I was an ordinary member of the faculty board, and I was elected from that group by the board as chairman and I did two years as chairman. It was my first experience as chairman of the faculty. Of any committee. No not quite, I had been, or was roughly at the same time chairman of the Institute of Physics X-Ray Analysis Group, which was the UK's professional organisation for crystallographers. But that was a different sort of committee and a different sort of experience.

MB The reason I put that benchmark in there is because you were going to chair so many things after that. It really is the beginning of a lot of chairmanship.

DP Well, the faculty board committee was a baptism of fire in some ways.

MB Well, I'm going to come to that next time because of our time limitations now. I just wanted to get you to that particular appointment as chairman. I wanted also to say that we've got you into Wolvercote and I shall come back to the home of twenty-five years in Wolvercote. We've got you in Pringle's establishment, but launching out with a whole range of relationships with the enzyme group. I wanted to ask finally did you get accommodation before our story ticks over beyond 1972 to 1974, in Pringle's new palace? I'm just trying to work out when you moved there.

DP I think it was in about '72 but it may have been as early as '70. And before then we also got some extra accommodation in a vacant house on the south side of South Parks Road, and one rather interesting experience perhaps I could recollect there. It was during the period when Mrs Thatcher, as she then was, was secretary of state for education in the Heath government. And she had been of course a student at Somerville and Dorothy had been her tutor, but what really brought her in was Sir Harold Thomson, Tommy Thomson as everybody knew him, had also tutored her in physical chemistry. And she came to Oxford on a semi-official visit, I suppose, and he got her to visit various things in South Parks Road. And one of the places he brought her to was this house that we'd acquired where we were now building models of lysozyme and alpha-lactalbumin and things of this sort. That was my first encounter with that redoubtable lady. She was very interested and talked really quite knowledgeably about chemistry. If you talked to her about what was a carbonium ion, at that point she actually knew what you were talking about. So that also had some effect on future developments.

MB Plus many encounters with that lady.

DP Yes.

MB David, we shall go on to such stories, but for now, for today, thank you.

DP Alright.