

**Lord Phillips in interview with Dr Max Blythe
Oxford 18 November 1996, Interview VI**

MB The last time we got you into the setting up of an Oxford enzyme group and it went on at a pace, and we had a look at a whole range of research interests that grew as you grew into Oxford. But we didn't take the family side in, and I know that we have important things to recall of the late sixties and early seventies on the family side. I should also at this stage recall that in 1967 you were elected to the Royal Society.

DP That's right.

MB Because that's going to have implications as we go down this next part of our talk.

DP That was a good day. I was actually on a visit to Israel giving some lectures organised by the Weitzman Institute when the results of the election were announced. But people had kindly written to see you in the list' and so on, 'better in than out', you know, that sort of remark that people often produce. It was very nice that Charles Bunn was also elected in the same year, because he was an old figure in crystallography who'd written *Chemical Crystallography* which was one of the textbooks that I'd used as a young student...

MB Yet another selection that gave you satisfaction.

DP ...and Dorothy in her engaging way when she talked to me about it said 'Nice to see you elected although, of course, I was even more pleased about Charles Bunn!'

MB We shall take in your Royal Society associations and all the works of those Royal Society years in due course. But first of all let's take a more serious look at the tragedy that overtook your mother. It was about 1968?

DP Yes, in '67 I was elected to the Royal, and I was still very much on the lecture circuit and very much going to Gordon Conferences. And in '68, as was quite common, I went to a Gordon Conference in New Hampshire where by this time I had a lot of American friends whom I looked forward to seeing. I came back at the end of July '68 and drove back from London Airport one Saturday morning, and there was Sarah, 6 at this time I suppose, playing, waiting for me outside the garage. And I opened the car door and she said 'Mummy's got a migraine and Granny's had a stroke.' Well, Mummy having a migraine at this time wasn't terribly unusual, so I went inside to get from Diana what the story about the stroke was and how the migraine was, and they'd simply had a telephone call to say that my mother had had a stroke. She had a cousin staying with her at the time, so there was someone to pass the news on. She was paralysed in her right side, but still talking, so here was I, looking forward to a nice doze in the afternoon to get over the jet-lag and so on, and

there was Diana in no real state for travelling. Nevertheless we organised some lunch and in the afternoon, drove up to Ellesmere - Diana, Sarah and myself - and arrived in Wharf Road to find the house full of visitors clustered around my mother who was sleeping downstairs at this point, because she was already fairly frail and, as described, paralysed in her right side, but still talking. So, we had a bit of a chat and had some supper and Diana and Sarah went off to sleep at a friend's house and I went upstairs to bed. And the cousin who was staying was to sit up with my mother or be on call, anyway, because they recognised that I was a bit sleepy. So the cousin came up in the morning and said 'I'm afraid Edie has had another stroke during the night.' So I rushed downstairs, and at this point she was aphasic and was simply making meaningless noises and looking to say the least rather wan. So we called the doctor and an ambulance came and took her to the local cottage hospital and they got her into bed and began to give what attention they could under those circumstances. And I stayed with her most of the day I suppose, but she was a bit comatose, as you would expect. That was the Sunday. On the Monday she was more alert again, but she was still only saying 'coly, coly, coly, coly, coly' in a slightly different tones of voice. I tried quite hard to get her to see if she would write her name with her left hand, but although I tried to fool myself that she was trying to do something of the sort she clearly didn't, and early on Monday afternoon we drove off back to Oxford. The car was parked up by the door of the cottage hospital, and to get out of the drive we had to pass by the window of the ward my mother was in, and as the car drove past she waved from her bed at us - well, it must have been with her other hand of course - driving past. So, I thought, there must be quite a lot there in fact for her to work out that we would be going past the window in the car. And we went back to Oxford, and I rang up to find out how she was going on and there wasn't really any change. I talked to the gerontologist in Oxford and asked him what he thought was the thing to do and he said 'Well, bring her down here and I'll get her in the Radcliffe and have a look at her. It's important she should have some physiotherapy, even though you might think it brutal, because she may live for quite a long time and if she does it'll be important that you and others should be able to help her walk about a bit.' So after a week or two I drove up to Ellesmere to see her and the medics organised an ambulance to drive her down to Oxford. We brought her down to the Radcliffe still in much the same state. But by this time I was quite convinced that although she couldn't talk she could actually understand perfectly well, and we had telephone conversations in which I would tell her the news and she would respond with her 'coly, coly', but in different tones of voice, 'coly, coly.' So, as I think it probably was by then and is certainly now well-established, that one ought not to assume that someone asphasic from a stroke or any other condition doesn't actually understand because it's different parts of the brain that get involved. That was something that other people found very hard to take. They would talk across the top of her head as though she wasn't there and she would deeply resent this. Anyway, she was in the Radcliffe for a bit and the gerontologist who was very kind and helpful made her walk around and learn to swing her right leg. And then she was moved up to the Churchill, and they carried on with that sort of physiotherapy there. Until in November, by a stroke of extreme good fortune, some friends in Ellesmere discovered that there was a lady who had lived in Ellesmere for quite a long time throughout her married life whose husband had died young, who had then moved back to her native Lancashire, but wanted to come back to Ellesmere to live. She was a highly competent lady named Mary Jones. And she responded positively to the proposition that she should live with my mother and look after her and that I would go up every other weekend

and relieve her so that she could have some time off. Part of the picture with Mary was that her daughter was at this time a nurse in Oxford, and that gave Mary an Oxford connection as well. So one day, I went up to the Churchill and said to my mother 'Well, I'm taking you home today.' We'd already been taking her home for weekends just to see how she responded to that. And my mother reacted perfectly straightforwardly and insisted on being wheeled round the ward to say goodbye to all the other people in it, and I got her in the car and drove her home to begin with. She said goodbye for the time being to Diana and Sarah, and I drove off to Ellesmere with her stopping at a garage on the way for the obvious purpose. And we got her back to Ellesmere and she walked into the house with my help and sat down in her familiar armchair in front of the television set and in an that instant, looked perfectly at home and normal.

MB Just settled.

DP Mmm. And that worked extremely well. Sarah and I went up every other weekend from then until 1972. We started taking occasional holidays and I still travelled a bit. I took the family round the world I remember in 1972 because we had a lecture engagement in Sydney. So we flew to Sydney via Fiji and I gave some lectures for Harry Messel on television in Australia along with George Porter. And we came back through Bangkok and Delhi where Diana, through the haze of a heat-induced headache, looked at the Taj Mahal and other places that her forbears had told her about because they were great India hands, and then we came back. Later on that autumn, after four years and a bit of this condition, my mother died. The benefits of this if you can call them that were that Sarah became accustomed to going to Ellesmere, so she knows the place where I grew up extremely well. At least once she stayed there for a week while I went back to Oxford to do various jobs. She also got used to dealing with someone with a severe disability and treating them like normal human beings, which she's still very good at. On holiday once we encountered a terribly disabled girl of her age who was spastic and Sarah took to her like a duck to water and helped her to do all sorts of things. It's very nice to see that, and that was if you like a hidden benefit.

MB She has this enormous ability to give to people in these circumstances and accept them exactly for what they are and develop relationships.

DP That's exactly so. So some good came of it, but it was a fairly wearing time. At the same time Diana's mother who lived a little nearer to us, halfway between London and Oxford, also became rather frail and ill, and when we went off to Ellesmere Diana would usually drive off to see her mother. So it was a slightly disruptive family time.

MB These were long years of family stress and distress. But, as you say, there were rather curious benefits and Sarah came out of it remarkably.

DP That's absolutely right, I think.

MB She was a late light of your life.

DP Well it wasn't all that late. I suppose she's still a light of my life. We went to the theatre twice last week.

MB I mean you were very senior, you had a long career before she came into your life.

DP Oh yes, I was 38 when she was born, that's right.

MB And she's been this significant star ever since. And her career has had a fair element of caring involved in it.

DP Oh yes, she's a very caring young woman, now with two children, and that means I'm involved with the grandchildren, so yes.

MB Perhaps we should put these young children on the record at this particular point.

DP Oh well. Sammy is now aged five and a half and Isabella is three and a quarter, something like that, and we see them at least every weekend and often during the week as well and by now they're both by way of being friends. I had some little difficulty when Isabella came along. Sammy was two at this point and had been somewhat frail as a premature child and deeply resented the arrival of his sister. So, when Sarah came home with Isabella and I observed Sammy's resentment, I said to myself 'I shall continue to treat Sammy as though he was really the only star in the sky.' So I did that for I should think getting on for two years and it led to a slightly comic situation which I hope won't have any lasting ill-effects. One day when Isabella was two Sarah was going through a photograph album with her and they came across a picture of me and Sarah said to Isabella 'Well who's that then?' and Isabella said 'Sammy's grandpa.'

MB Telling comment!

DP Yes! But by now she's noticed that I'm quite interested in her as well and things are better.

MB Different relationships brewing. You're writing a letter, I think, to Sammy which I should put on the record here.

DP It struck me I suppose about two years or more ago when Sammy was already deeply into using computers and knew very well how to handle a mouse and what the 'Enter' key meant and could begin to spell his name on a computer and things like that, and was playing computer games and leading a very active life in Islington, that his life was really very radically different from the life that I remembered in Ellesmere. So, I thought I'd better write Sammy a letter to explain to him what life was like in Ellesmere. So, that's the origins of my *Letter to Sammy*. It's reached the stage of my leaving home to go to Cardiff, at this point I'm 18, so there's quite a long way to go.

MB Yes, seventy pages in. But there's a biography in gestation there, David.

DP Well I don't know about that. I hope that Sammy might read it one day and think that maybe Grandpa's life was quite interesting.

MB David, taking Grandpa's life back to the death of your mother. Was that a point of great sadness? Because you'd been close for many years. What kind of point did that mark?

DP Well, undoubtedly, it *was* a moment of sadness. But I suppose after that four and a half year period with my mother manifestly declining in the last year, I think - *I suspect* - that in the last year she decided herself that she'd had enough and decided that she didn't need to eat really all that much and things like that, so she got weaker over the last year. And I think by the time she died I was probably ready for it, so it was a parting, a grief...

MB But a reality that had been partly faced?

DP With an element of relief as well.

MB Because you had had quite busy years, a difficult time to go through with all that family stress on both sides of the family, illness...

DP I suppose I was brought up to think it was the kind of thing one has to do. I mean, various people said 'Well, you know, why don't you put her in a home?' and so on but...

MB But that wasn't the thing.

DP ...that wasn't the thing at all.

MB David, I'll move you back to that career development and look at the attachment you got to the faculty board in about 1972. I believe you became the chairman of the faculty board in Oxford at that time?

DP Yes.

MB I'll mark it as an important point because, from that point on, you were never going to be free of sitting in chairs of various kinds.

DP Well, that's the way it turned out and there were connections between these various chairmanships. I had been chairman of the x-ray analysis group at the Institute briefly, but that was a slightly different thing and I don't look back on that with any particular affection I suppose, or sense of achievement. The faculty board was entertainingly difficult to begin with. It was the point at which John Pringle, having achieved his new building at the end of South Parks Road and the corner of St Cross Road, thought he'd done his bit for zoology and didn't really want to be head of department any longer but rather concentrate on research. So, he thought to himself 'Why don't I get David Phillips to be made head of department and I can concentrate on research, and he can do some administrative work for a change?' Well, since I owed John quite a lot, much as I didn't like the prospect, I thought maybe I had to

accept this. So I told him that I'd do that. But it came up for decision at the faculty board not long after I'd become chairman.

MB This was a kind of rotational chairmanship, was it, you just had your turn, as it were?

DR It was a new idea at that point, as John had been appointed head of the zoology department for the life of his appointment as Linacre professor, I suppose. But there was on the faculty board a reader in the department who was a 'real zoologist', let's put it that way, and here was I in the chair on the faculty board and this issue as to whether or not I should become head of the department of zoology came up. So naturally I had to leave the room. It came up rather early in the meeting and I found myself..

MB These were those meetings at the Indian Institute?

DP ...I found myself sitting in the hallway of the Indian Institute - I'm not even sure there was a decent chair to sit on - pacing around, walking up and down, while they went through what was obviously a very long debate about whether this was a practical proposition or not. I've never had a report of that discussion, but I can well imagine that a real zoologist with some backing from the department would think that this physicist turned molecular biologist/pseudo zoologist wasn't really quite appropriate as the head of a Zoology Department. Anyway, however the discussion went, that's how the decision turned out. It wasn't agreed.

MB So, you were called in and told this?

DP So I was called in, nobody said anything. I just went through the rest of the agenda but afterwards was told.

MB No go.

DP I've only regretted one thing about that. I didn't regret the decision at all. I've regretted that being an inexperienced chairman I didn't have the good sense to say 'We'll have this item as the last on the agenda so that I can go home!' But the rest of my period as chairman of the faculty board passed off reasonably uneventfully, to the extent that it was clear that some people thought that I had done it better than they had expected me to do, which one or two of them actually said. Rodney Porter, who of course was an official member of the faculty board at this point and a great friend, was talking to the secretary of the MRC one day, who was John Gray, and said to him 'This reorganisation of the MRC board structure that you're thinking about. If you're thinking of a chairman for the new boards, I think you should consider David Phillips, because I've just watched him in action on the faculty board in Oxford and think he would do it quite well.' This of course was reported to me months later. Now the MRC had supported me then and kept on supporting me until I left Oxford in 1990. John Gray rang me up and said 'I'm thinking of reorganising the board structure. We'll abandon the biological sciences board and the clinical sciences board', which were the two boards under the council that they'd had for a long time, 'and we'll have some boards which reflect two things. First of all, the increasing diversity of medical research and secondly the importance of bringing the biological and clinical science

aspects of this research together. So we'll have three boards, maybe four. And they will be the cell biology and disorders board which will look after molecular and cellular aspects and the clinical context, the systems board which will look after physiological systems and the medical aspects, a neurosciences board which will obviously look after neurosciences. And we'll probably have to keep up the tropical medicines board somehow, but the three main boards will be the cell board, the systems board and the neurosciences board, and I'd like you to chair the cell board. It will involve you becoming a member of the MRC council as well.' So naturally feeling under an obligation to the MRC I said 'Well, fine. What will this involve?' And John said 'Well, we have a meeting of the council roughly once a month, and we'll have a meeting of the cell board roughly once a month. We shall need some grant committees under the board, but you won't be expected to play a part in them, though we'll need your advice about setting them up. So apart from briefing meetings it will be two meetings a month and some paperwork.'

MB David, that change was quite a massive change for the MRC. What had actually pushed it? Was it molecular biology's advances in Britain at that time?

DP It was partly that. And it was partly the fact that in 1972, you will remember, there was the Rothschild Report which had recommended that some money be transferred from the Medical Research Council to the Department of Health because the Council was not really paying enough attention to the needs of the Department of Health. It was too much hooked on basic biomedical research and it needed to be more concerned with clinical research.

MB So, that was the other factor?

DP Similarly the Agricultural Research Council was not concerned enough with the needs of agriculture, so something like half of their budget was transferred to the Ministry of Agriculture, Fisheries & Food, and some money was transferred from the Natural Environment Research Council. Only I think the Social Science Research Council and the Science Research Council survived this Rothschild process unscathed. So it was a period of trying to bed down a closer relationship between the MRC and the Department of Health. It was those two things.

MB That was it.

DP The relationship with the Department of Health aspect quite soon became a rather difficult political issue when we got the cell board set up. Setting it up wasn't all that troublesome. Two senior officials from the MRC came down to Oxford and spent the afternoon talking to me. One of them was a doctor named Chris Norton who was on the staff and was going to be the official secretary of the cell board, and the other was Charles Kirkman who was finance officer, I think, of the council and wanted to talk about various aspects. And the three of us sat in our sitting-room in Fairlawn End and talked about the sort of people I thought would be good members of the cell board and what sort of grant committees we wanted. I said 'Although I appreciate that we need to bring biomedical science and clinical science together on the board, I think it would be too much to bring them together on the grant committees. So we need two grant committees...'

MB Right, so you split the funding arrangements.

DP '...one in biomedical science and the other in clinical science.' And we agreed on that soon enough and agreed on a bunch of members, people like Walter Bodmer and Anne McLaren and Sidney Brenner and Alastair Currey and so on as members of the cell board. Rather a prestigious bunch of people. And there I found myself, after a bit of briefing, sitting in the council room of the MRC chairing this rather high-level board. I must admit that the first meeting or so was a bit difficult because I didn't really know how these people thought and did things, but I got over that I think fairly quickly...

MB And you were soon into a problem at Cambridge?

DP ...and had a good time until the Cambridge problem.

MB Did that come up within that first year?

DP There were rumblings of it in the first year.

MB In 1974.

DP It was a two-year stint. It started in '74, and already in '74 I used to have chats with John Gray in which he would complain that there was a strong body of feeling that the MRC was putting too much effort into molecular biology. And this was clearly led by Ernst Chain of penicillin fame who it was said had the ear of the then prime minister, Edward Heath, and used to talk to him about the way the MRC was wasting money on a subject that was clearly never, ever going to come to anything of any medical relevance. Now it happened that towards the end of '74, I suppose, without looking up the dates, Max Perutz was reaching retiring age. And the tradition was in the MRC, that when a unit director came to retiring age there had to be a serious study into whether or not the unit should continue. So we had to set up a committee to discuss whether the famous molecular biology laboratory in Cambridge, because Max was retiring, should be closed down or not. And that led to a certain amount of...

MB You were presiding in a way over the fate of your opposition!

DP ...anxiety in Cambridge. So the cell board, with the, I suppose, approval of John Gray, set up a rather prestigious committee - it included people like Andrew Huxley, Walter Bodmer and Henry Harris - to consider whether or not molecular biology in Cambridge should continue. And I went off with this committee to Cambridge to have an open meeting with the members of the laboratory to tell them what was going on, and they sat there in the seminar room looking anxious.

MB People like Crick!

DP That's right. And I stood up and said 'Well relax, it's a routine thing that has to be done, and what we're going to do is to have a review of what's going on and to think about Max's successor and things of that sort. But I wouldn't honestly think

that you have a great deal to worry about.' Anyway, Francis thought that this was a fairly reassuring statement that I had made.

MB But before that it must have been quite difficult for them having an Oxford man coming out to look at their unit?

DP Well, I was reasonably well-known there, of course. I think that they saw me not quite as a built-in enemy. But anyway, we had great discussions. Tony Vickers, an official of the MRC, was the secretary of this unit, and he was somewhat imbued I think with the notions that, following Rothschild, there were dramatic changes in store for the Research Councils and who knows what might happen and so on. So it was a somewhat anxious time and we produced what became known, naturally, as...

MB The Phillips Report.

DP ...the Phillips Report, which in general endorsed what was going on at LMB and made some faintly critical noises about the fact that...

MB Yes, you tweaked them on a number of issues.

DP ...yes, that they'd used too many American postdocs and hadn't done enough about disseminating their findings and their philosophy throughout the UK laboratories. That they hadn't had enough graduate students. That maybe there was still a bit too much emphasis on structural studies and maybe some of their stars ought to go out and spread the gospel in UK universities, just as I had done I might say. So not all of that went down terribly well but by and large it was the status quo and the report went down quite well with the MRC. John Gray thought we'd done a good job and had disarmed Chain, and the people in the lab accepted it. David Blow for example left the LMB and took a job at Imperial College as professor of the physics department, in fact, where he found himself teaching quantum physics and doing research on protein structures as usual. That was the major event as a result of the Phillips Report that I remember, but otherwise it all passed off alright in the end. But it's a very curious reflection on the state of debate in biological sciences that Chain should have taken that view as late as that, because we were already into RNA sequences and things of that sort. And yet he didn't perceive it and I dare say a good many people didn't perceive that it was going to turn out the way it did.

MB Was he too entrenched in past concepts?

DP Oh, he strongly believed that the way to advance the discovery of drugs was to follow the penicillin model and to look at natural products and screen them for biological activity and things of that sort. A business that goes on still around the world and still produces results, but not the systematic way to go at the foundations of biology at the molecular level.

MB David, that pronouncing on the laboratory of molecular biology in Cambridge must have been the main event of your first two years at the MRC?

DP That was the main outcome at the MRC, that's right.

MB Did you go in for a second two years? Were you taken on for a second two years in the chair of that board?

DP No. They decided that the board should be chaired for two-year periods and that a clinical scientist and a biomedical scientist should alternate. So I was succeeded by one of the clinicians on the board, Alastair Currey, who was professor of pathology at the University of Edinburgh and another good friend.

MB And he was the man doing work that was later to bear fruit, but wasn't all that recognised?

DP He was the man doing work that wasn't recognised at the time, but now if you mention apoptosis to anybody those in the know will say 'Well yes, Alastair Currey was involved in the start of that wasn't he?' Programmed cell death, of course, and I'm afraid he died a year or so ago, a little before this had really hit the headlines in the way that it has now. Anyway, he succeeded me as chairman. And again after a bit of discussion on the council, while I was asked to wait outside for what seemed to be rather a long time, they decided to invite me to stay on the council for another two years, which was the full four-year term of a normal council member. So, I stayed on the MRC as a member until '78, but Todd intruded earlier than that.

MB Yes, Alex Todd.

DP That's right. In '76, when Todd was in his first year as president of the Royal Society, he found himself confronted by the situation that the biological secretary Bernard Katz, and the treasurer James Mentor, and the foreign secretary who was Kenneth Durham were all retiring, and he had to find three new officers. So he scratched his head and took advice, and among other things wrote a letter to me saying he had to find himself a new biological secretary and would I be interested? If so, would I go and have a chat with him?

MB He wasn't the easiest man in the world, I've got an impression?

DP Well, at this point my view of Todd was entirely coloured by the views of my various friends who'd had dealings with him, including Dorothy who had not been on the best of terms with Todd over B12, for example.

MB Didn't she feel that he'd tried to steal some of her thunder?

DP There was something of that.

MB Was there?

DP Yes, there was something of that. And there were other people in Oxford who thought not particularly well of Todd, I suppose. The general view was that he was a somewhat arrogant character.

MB I was trying to make the point that I think Dorothy had the view that there was an attempt to pull away some of the credit for B12?

DP Well, I think that's right, yes. Anyway, I went to see this chap as I say with a certain amount of prejudice lying in the back of my mind, and I found that I liked him well enough. He seemed down to earth and matter of fact. I said 'I hope you haven't taken on the presidency in order to keep things going exactly as they are, because I wouldn't want to be involved in that.' And he said 'Oh no, no, that isn't my view at all', and 'I'm trying to collect a bunch of new officers who will also be interested in changing things a bit.' So I said 'Well, I'd be interested to know who they are.' And he said 'Well, for treasurer I'm talking to John Mason, who's the director of the Met Office, who I hardly knew at all, Michael Stoker who at this point was a director of the Imperial Cancer Research Fund labs, who I had known because he had previously been a director of the MRC virology unit in Glasgow, as Foreign Secretary. So, I thought that sounded quite interesting. And Harry Massey, the professor of physics at University College London, was to go on as physical secretary. I thought that sounded quite interesting and I said 'Well, if the Council of the Royal agrees with this, I'd be prepared to accept your offer.' So, he said 'That's fine. I'll put it to them.' So, after the anniversary meeting in November '76, I found myself sitting at Todd's side as biological secretary of the Royal Society with a number of people on the council whom I knew. Hans Kornberg, I remember, in the middle of the council - I'd met him as a member of Tim Jones's committee on enzyme chemistry - sent round a note saying 'how nice to see you sitting there.' And that began a long association with the Royal Society from '76.

MB And the MRC continued?

DP ...and the MRC continued. And then, when my membership of the Medical Research Council itself came to an end in 1978, the Royal Society of course appointed an assessor on the Council of the Medical Research Council, and they thought well, what better than to continue me as assessor. So I stayed with the MRC as long as I stayed with the Royal Society, and then remained involved with the MRC but in a slightly different capacity.

MB Yes, in a slightly different capacity. We'll take that in, but let's stay with the Royal Society for a moment, David, because things did change in the Todd years while you were there. I think some new fellowships came out of your interests?

DP One of the things that we all agreed on and I think I probably unfairly get a bit of credit for was that we thought the Royal Society grant scheme, whereby they made rather small grants, it may have been as little as £5000 a time to people who applied for bits of apparatus and so on, was rather chicken-feed in the context of the grants that were then issued by other agencies, the Medical Research Council's in particular. And they weren't really helping with what we thought was the major problem, which was that there were by now rather few new posts coming along in universities for people who showed promise as research workers. And maybe it would be better to use the money for long-term research fellowships for particularly promising research workers who'd done perhaps one, or even two, three-year stints as postdoctoral research workers. So we invented something called the Royal Society University Research Fellowships, which were for five years in the first instance but in principle extendible to ten. And they were to be financed in such a way that the fellow was paid tolerably well by university standards...

MB A kind of junior professorial salary?

DP ...and get some research money, and they were to be planted in departments which made some sort of undertaking that they would at least try to take these people on the staff at the end of their period as research fellows. We started off with a relatively small number of these - I've forgotten how many - say fifty, something of that sort. And we set up a mechanism for selecting them, which was split between physical sciences and biological sciences, and there were enormous numbers of rather splendid applications from which we selected a lot of very bright young people. That scheme grew over the years so that by now upwards of one hundred are elected every year. There are a lot of them out there in the field, a lot by now, since the thing has run its course, more than run its course. Numbers of them have been taken on in university departments and it's a reservoir of talented people who have actually made their mark, a lot of them.

MB It is a reservoir, isn't it?

DP Many people think it one of the better things that happened in UK science in the 1970s, so I feel rather pleased about that, without claiming that I was absolutely the prime mover in it.

MB And the small grants weren't phased out altogether. In fact, they came back a bit later on in greater quantity.

DP Small grants were phased out at that point altogether, but some ten years later they were brought back.

MB In George Porter's time?

DP In George Porter's time they brought them back because at that time there was a need, I think, again for that sort of thing. So, that was I think maybe the major change which had an external effect during my period as an officer of the Royal. We did various things about the election procedure which involved the council members more in the processes, the rather arcane processes, of sectional committees which look at the candidates for election in various subject areas. And that I think led to perhaps an improvement in the decision-making process. The thing that I regret is that I was the titular editor-in-chief of the journals on the biological side, *Proceedings* and *Transactions*, and it often occurred to me that we ought to do something about the *Proceedings of the Royal* which was in a rather moribund state and nothing like as successful as the *Proceedings of the National Academy of Sciences* in the United States, for exampl. But I didn't for some reason take a grip on that and I think the chance was lost. Attempts have been made since to change the style of the *Proceedings* and the *Transactions*, but it still hasn't taken off as a journal of first choice by many people. Many people still think, I believe mistakenly, that if they publish something in the *Proceedings* or *Transactions* it's rather like burying it. So they go for *Nature* and *PANS* and *Science*, and even the EMBO journal which was invented after my time at the Royal. We could have stepped in and produced something that would have been perhaps rather like the EMBO journal and just as successful but, I didn't, and that's a regret.

MB David, looking at those years - and we're getting you through the seventies and towards the eighties now - those years working at the Royal Society, putting a lot of time in at committees and at the MRC. Were they years when you had a chance to put time in at the bench as well?

DP I suppose I would have to admit that as technology developed and as particularly computers got more powerful and more pervasive, as computer graphics overtook model building as the way of building structures in interpretation of electron density maps - one did the whole thing, plotted the map and built the model on a video display unit rather than physically with one's hands - I increasingly left off doing things with my hands and increasingly saw it as my business to try to promote the careers of the younger people coming along in the lab.

MB So you became a sponsor of a new generation?

DP I tell myself that that's the rationale of what I did, but that broadly is what happened to me, yes.

MB But you continued to lecture widely, you continued to have a very lively life, to travel a lot. And in 1980 you had the great accolade of giving the Royal Institution Christmas lectures, which I still remember.

DP Well, that's very kind of you still to remember those. I remember them with affection. It was the Christmas of Sarah's first year as a mathematics student at University College London, and she came along to help. The Porters, of course, were great friends and we stayed at the flat at the Royal Institution and...

MB And Bill Coates was there.

DP ...Bill Coates was still very much in action as a lecturer's assistant. And he and I together spent a good deal of 1980, on and off, having chats about what sort of lecture demonstrations we might be able to put on. The BBC anxiously clamoured for scripts, and I equally perversely refrained from producing any scripts. Anyway, it came to Christmas-time and we'd sorted out a lot of possible demonstrations to include. Peter Dunhill, who'd been a student of David Green's back in the RI days, I suppose in about 1962/3/4, that sort of period, was now a leading figure at University College London in biochemical engineering. He'd chipped in with numbers of suggestions for demonstrations and brought his young son along as a member of the audience, whom I was able to pick on during the lectures to do things. And with odd notes scribbled down in my filofax diary in my usual style I embarked on these lectures. I was going to say six lectures, but in fact I only gave five because Max Perutz, I thought, since I was going to talk about molecular mechanisms and things, really ought to give a lecture on haemoglobin. So, without much difficulty, I persuaded him to do that.

MB 'Haemoglobin: The Breathing Molecule.'

DP Exactly. So, he gave the fifth lecture, to which I'll come back in a moment. Anyway, some time before Christmas in 1980, with Sarah in the audience and a very

attractive young student from Oxford, a girl from the Philippines, Rissa de la Paz, also roped in as a lecturer's assistant, and Michael Sternberg, a graduate student from Oxford, roped in as a lecturer's assistant, and various help from the lab of course, I embarked on these lectures.

MB What was the theme, David? You should put that on the record here.

DP Well, it was 'The Chicken, the Egg and the Molecule', I think, which wasn't a terribly catchy title, but it was the best that Sarah and I could think of. I think I'd have to implicate her in that decision. Anyway, the lectures began as they always do at the RI with the left-hand doors opening and the director sitting in, having made sure that I was standing outside the other doors, heavily protected by the staff to stop me running away, and then the other doors opened and I walked in. I have to say that at that moment, although by then I'd talked a lot around the world and given lectures to very large audiences, for example in the Tokyo Opera House or somewhere, where I gave a plenary lecture at the International Union of Biochemistry Congress in '67, for the first time in my life my mouth went totally dry and I wondered whether I'd be able to get a single word out. But I opened my mouth, and something came out.

MB It happened.

DP It happened, that's right, and the first exhibit was a turkey. Since it was Christmas and everybody's fond of turkey muscle, we wheeled in a turkey. And I hadn't really got anything to say much about a turkey, but it relaxed the audience a little to see this turkey being carried in and we got started. I think it went off alright, but the BBC was anxious most of the time that I didn't have any notes and they didn't know what I was going to do next.

MB So, it was run on those kind of slender lines. I mean, you really did come up with it slowly?

DP Slightly, slightly. I mean, Bill Coates had a list of demonstrations and if I looked as though I was going to forget one he would signal to me desperately and I would see him out of the corner of my eye and wonder gosh, what have I left out? There was one bit that was rather well rehearsed. I thought I had to get into cells, so we devised a rather novel demonstration. We had a model of a human female figure of the kind where you can take off the skin and look at the organs inside. And I proposed that I should take this to pieces and say 'These are the lungs and this is the heart', and then we should show on a microscope some heart cells, and then I should go a bit further and dig out the liver and say 'And this is the liver and here are some liver cells and here are the kidneys and here are some kidney cells.' And the BBC thought this was alright but couldn't we make it a bit livelier. So they then told me that they had a trick whereby they could organise a camera so that it doesn't see anything with a blue background. What emerged from this was that they hired an actress who was to walk in and sit on a seat. And there would be one camera looking at her appropriately clad in a blue bodystocking, right, and this model of a female figure and the two images would be combined, so that the actress's face was the actress's face and the body was this model of a female. So that's what the viewers saw, I suppose, and I took off the actress's breasts and the actress looked down and looked puzzled and took out the heart and we went through this whole rigmarole.

Well, to begin with, the BBC went over the top slightly and when the actress walked in they started to play some background honky tonk music because it really was going to be a stripping scene. And Sarah, I remember, objected very strongly to that and overnight said to me 'You can't let them do that, Daddy, it's not right in a childrens' lecture.' So, we cut that out, but apart from that I think it was quite a success, that lecture.

MB I remember seeing the children enormously involved in it.

DP Well, that's right, the kids...

MB And in the end you enjoyed it, and no dry mouth.

DP Well, that's right, yes. We did things like mincing up celery in a blender and chopping up liver, and Sarah and Alice Bragg, the great Bragg's granddaughter, chopped up the liver, I remember. And we then poured these two extracts into separate flasks of hydrogen peroxide to see which one had the more peroxidase in them, and it began by looking as though the celery was going to win but in the end the liver won. But there you are, it was all very messy and in the end good fun, yes. There's a good story about Max of course who alarmed the BBC producer no end. Max, you know, suffers very badly from his back. Like many other people he has back troubles and finds it difficult sitting in a chair, in fact doing anything much other at times than standing up straight or lying down flat on his back. Well, another tradition at the RI is that before a lecture the lecturer has to go and sit in the lecturers room and be locked in there, so that he can collect his thoughts about the lecture and be collected...

MB The old Faraday room.

DP That's right, Faraday's study, and be collected by the producer, or the director of the RI if it's a Friday evening discourse. So, for the third of these lectures Max was incarcerated in this room and the BBC producer at the appropriate moment went in to collect him, and there was Max lying on the floor flat on his back with his eyes closed. And the director rushed out saying 'Dr Perutz has collapsed!' or something like that. Well, of course it was just Max resting his back before the lecture. And he came in and delivered an absolutely splendid lecture on, as you say, 'Haemoglobin: The Breathing Molecule.'

MB It was a great session. A great Christmas lecture series. As I say, I can still remember it.

DP Well, as it turned out, fun. But a bit agonising to start with.

MB And it was at the beginning of the eighties which we're going to be...

DP That was '80/'81.

MB Yes, a fascinating decade which I'm going to take a break before coming to, so that we can talk on the eighties pretty fresh.

DP Alright, let's do that.

MB We'll just wind down for a moment.

MR David, we talked of the 1970s and the fateful calls and letters from John Gray and Alec Todd, and when we come to the eighties there's a pretty fateful letter from Keith Joseph on the mat one morning in 1982.

DP Yes, that's right. I suppose it was in the autumn of '82 I had a letter from Keith Joseph asking whether I would go along to the Department of Education and Science and meet... I don't really remember whether it was to meet him directly to begin with or the junior minister, the parliamentary under-secretary of state, whose name was William Shelton. He didn't say what it was about. Now, I'd met Keith Joseph and William Shelton a bit earlier in the year because John Kingman, who at this time was chairman of the Science and Engineering Research Council, had invited these two ministers to come down to the Rutherford Appleton lab to see what was going on and to have some presentations from a particle physicist, and as it turned out from me, on two widely different aspects of science. Atomic theory and particle physics on the one hand, and molecular biology protein structure on the other. So, I had accepted that invitation and gone along and there were these two people. Keith Joseph of course I knew by sight because his face was really rather familiar. He was a noteworthy political figure at that point, and had been for many years. William Shelton I didn't know. But the two of them were there together with an under-secretary from the department by name of David Tanner. And it started off with somebody, I've forgotten who it was, giving a talk about particle physics and atomic structure. Now Keith Joseph, it soon emerged in this presentation, knew something about science. And what he knew was mostly culled from assiduously reading the science and technology article in the back of the *Economist* every week, so since particle physics had featured he knew something about that and could ask the occasional question. And Mr Shelton, feeling that he had to keep up, occasionally butted in with a question, and it all went really quite well. Then it got round to me, and making the transition from this model they'd been building up of what atoms were like to a protein molecule - I'd taken a wire model of lysozyme with me, about so big, on a scale of an inch to the angstrom unit. That was a bit difficult for them. I explained to them that proteins were long polypeptide chains and they folded up in this rather complicated way, and they did this because in an aqueous environment they tended to fold up so that the side-chains that liked being in contact with water were on the surface and those that don't like being in contact were on the inside. And I went into that in a bit of detail and looked at the active site and told them about how there was this great cleft in the surface of the molecule where the substrate fitted and so on, and they listened attentively. Then we got into some general conversation about the nature of these two ends of science which in terms of the sizes involved were not all that different. Except of course that particle physics leads on into cosmology and astronomy and all sorts of curious phenomena which were then being talked about, and it was at this point that they began to get a bit confused. So, here they were, with their minds full of protein structure and the importance of the interaction with water which in general was on the outside, and on the other hand particle physics, the constituent atoms of this molecule, which they hadn't quite

registered I don't think. Somebody brought up the question of neutron stars, at which point the other lecturer I suppose in this says 'Well, the thing about neutron stars is you have to imagine the atoms being sort of totally condensed down so that you've only got nuclei and no surrounding electrons. And these nuclei are somehow, under pressure, squeezed into close contact, so you have a solid mass of neutrons and it weighs goodness knows how many tons per pinhead.' And Shelton said 'Ah yes, I see, all the water's been squeezed out.' Which suggested that somehow he'd got, which wasn't surprising, a little confused between these two areas of science. But we got over that reasonably satisfactorily, and the two ministers went off apparently feeling that they'd had quite a reasonable exposure to scientific thinking at these two ends of the spectrum. Well, that I think - and John Kingman would be quite clear on this - was the background to the letter I had from Keith Joseph. Because when I turned up at the Department of Education and Science to see Shelton, the proposition on offer was that maybe I would like to be considered for the post of chairman of the advisory board for the Research Councils. Shelton was accompanied by the deputy secretary in the Department who was in charge of higher education and science at the time, a man named Richard Bird, who was a linguist, a classical scholar really, and who became a good friend. And I talked to these two people about the job and they said 'Well, fine, we'll write to you.' And I suppose they were considering somebody else, I never knew who. Though there were it's fair to recollect two vacant jobs at the time, the chairmanship of the Advisory Board for the Research Councils, ABRC as it's familiarly known, and the University Grants Committee which was coming up in about a year's time. So, they were no doubt wondering about both jobs.

MB And which we should put on the map, you had been associated with.

DP I'd been a member of the biological sciences subcommittee, but I'd never been a member of the UGC itself. Well, a week or two later I got a letter or a telephone call asking if I would go and talk to Keith Joseph himself. And I went along to see him and he said 'Well, we've thought about it and I'd like to offer you the post of chairman of the ABRC. Alec Merrison the present chairman wants to give up as soon as possible, so I'd like you to take on the job as soon as possible. And if you're prepared to do it I suggest you should go and talk to our officials' - Richard Bird again, the deputy Secretary, and David Tanner who had been at the event at the Rutherford Appleton lab, who was the under-secretary in charge of science. So, I said 'Alright. I'll be glad to do that. I am still involved with the Royal Society and it's a critical time of year because the elections are coming up in January to March and I can't easily walk away without completing that, because the process has already started. But I expect they would let me give up when all that's been settled, and I expect I could chair a meeting or so of the ABRC before I finally give up with the Royal Society.' So, I went back and talked to Andrew Huxley, the president of the Royal by then - he'd succeeded Alex Todd. And he, who I suspect had been asked about it anyway, said 'Well yes, that's a good idea. You'd better do that, and you'd better stay on as biological secretary until April, but we'll find somebody else to take over from you then', which would have been six months earlier because the natural changeover time would have been in November. So, I went back and spoke to Richard Bird and David Tanner about what was involved, and came to the conclusion with them that I should take over as chairman for the January meeting of the ABRC.

MB Right at the start of the year, 1983.

DP So that was quite brisk, January '83. So I had some briefing. I should have mentioned that I was already involved a little bit with the ABRC. Because the ABRC had set up a subcommittee under the chairmanship of Dick Morris who was an industrialist, the chairman of Brown & Root UK Limited, a subsidiary of a US oil company, who became, again, a close friend as I shall go on to say. This committee was concerned with the balance of support between universities and research council institutes. And as an MRC person I was a little partial to institutes, and there were other university people there who were rather thinking that universities were getting a short deal and institutes too much. So at the next meeting of that committee, Dick Morris said 'Well, if you're going to be the chairman of the ABRC, maybe you'd like to chair this committee?' and I said 'Oh no, no, no, Dick. You're doing that, so you carry on.' So, that was produced as the first of the Morris Reports, which of course was a very well-balanced report saying on the one hand this and on the other hand that, but not as I remember it producing any very radical proposals for change.

MB So, the research councils were going to go along pretty well as they had been.

DP They were going to go on supporting the work in institutes, but with institutes more closely associated with universities wherever that was possible. That, I think, was the meat of the report and that's a debate that continues to this minute. I mean, the stage we are now at is following the Prior Options Review of 1996 there's even a move that universities should become the owners of a lot of research council institutes. But that's looking into the future and some distance ahead.

MB Yes, we'll get there in the end, but...

DP But generally, in '83 I found myself sitting at a long table with goodness knows how many people around it, because the ABRC at the time was made up of, first of all, the heads of the five research councils... And these heads were slightly different. Some research councils had non-executive chairmen and executive secretaries, the MRC and the ARC for example. The other research councils had executive chairmen – the SRC, the NERC and the Social Science Research Council, as it still was. But it was the executive head, so it was either the executive chairmen or the executive secretaries that were members of the ABRC, five of them – John Kingman from the SRC, Jim Gowans from the MRC, Ralph Riley from the ARC, Herman Bondi from the NERC and I'm now going to have a block about who was the chairman of the Social Science Research Council...

MB You got four out of five, David.

DP ...Michael Posner, five out of five.

MB That's impressive!

DP Then there was a group of lay members of the ABRC, Dick Morris was one of those, a mixture of academics and industrialists. But to fill up every conceivable vacant space around the table, there were all the chief scientists of all the government departments that had chief scientists. The ABRC of course was set up post the Rothschild Report in 1972. And it's first chairman was Fred Dainton, who'd

previously been the chairman of something called the Council for Science Policy. And one of the outcomes of the Rothschild Report was the transfer of money to government departments. It was also the creation of chief scientist groups within government departments. And all these people were made members of the ABRC, so it was a very, very large committee.

MB Sounds unwieldy.

DP I had taken the trouble before agreeing to go and talk to Fred Dainton, who by this time lived in Oxford, about the ABRC and his view of it, and had a very helpful conversation with him. And I'll come back to that later on. So, there I am at the first meeting. The dust that was all in the air at that first meeting had been kicked up by two debates. At their debate in the previous year under Alec Merrison, who'd, thankfully for him or in *his* mind, gone back to be full-time vice-chancellor of Bristol University and had given up on the ABRC, his last responsibility at the November meeting had been to make proposals about the allocation of the money that the Government had voted to the science budget in the annual public expenditure round in October/November. That was the second part of the ABRC's central function. And in that round the ABRC in its wisdom had said that the universities were in some financial difficulty and their budgets had left off rising. They'd actually left off rising in 1972 in any dramatic way.

MB Yes, the golden period was over then.

DP But there'd been enough slack in the system for nobody to notice that particularly. It hadn't loomed large in ABRC discussions.

MB But as you arrived, it was beginning to bite.

DP That's right. I'd noticed that, in reading back in the papers, they hadn't noticed much about it. But as you say when I arrived it was beginning to bite. And in the previous autumn's debate they'd said that 'Universities are suffering. Nearly all the research councils support universities, but the fact is that the ARC supports mainly its own institutes. So the research councils that support universities should be protected and this year we should recommend that the ARC should get only a modest increase', or maybe it was even a slight decrease.

MB And the others have a preferential slice.

DP And the others got preferential treatment. The secretary of the ARC, Ralph Riley, objected to this strongly. So the advice that went to the secretary of state, Keith Joseph, wasn't unanimous for once. The ABRC had a dissenting minority view, voiced by Ralph Riley, that the ARC was being unfairly treated. So that was something on the agenda for my first meeting. And the other dust in the air had been kicked up by Keith Joseph himself, who took the view that there was no such thing as social science and therefore a Social Science Research Council was an anachronism that ought not to exist. And he didn't want money put into subjects like sociology, which were not science anyway, and if he had his way he would abolish the Research Council. Well, at this stage – and it was probably after the first meeting, but having started that story, I'll carry on with it – there was a new parliamentary under-secretary

of state in the Department, none other than William Waldegrave. And he had worked in Rothschild's think tank in Heath's government and knew something about the social sciences. So, when Keith Joseph put it to him that the Social Science Research Council ought not to exist, the story goes - and I don't know quite how I picked all this up, but it wasn't directly from either of the principals as far as I remember it - Waldegrave said to Keith Joseph 'Well, you can't just abolish them. It's a Royal chartered body, you know, it has a Royal charter from 1965. You'd have to go through a due process. What you need to do is get some respected figure to produce a report on the Social Science Research Council and whether there is a national need for such a body.' 'Good idea', said Keith Joseph. 'Who shall we get?' 'How about Lord Rothschild?' said Waldegrave. 'Splendid thought' said Keith Joseph, 'I'll write and ask him to do it.' So Rothschild was commissioned to write a report on whether or not the Social Science Research Council should exist, in effect. And he wrote a report relatively quickly, being a brisk operator who of course was extremely well-known and well-respected in government. He wrote a report which began - as I remember it, though it's no doubt a paraphrase - 'Any civilised country would support the social sciences.'

MB So they were going to stay?

DP So Keith Joseph had been frustrated in this, and the only options that he had left were that he would, anyway, reduce the budget of the Social Science Research Council. And he couldn't tolerate the name, so they had to come up with a new name that didn't suggest that it was a science. So, the Social Science Research Council went into agonies and eventually proposed that it should be called the Economic and Social Research Council, which is a nonsense of a name. But nevertheless Keith Joseph accepted that and it continued to operate. Michael Posner, who'd seen them through this terrible phase, not long afterwards retired and was replaced by a man named Douglas Hague who'd been the professor in the business school in Manchester and was - how shall I describe him? - perhaps somewhat more in sympathy with some government views than some other scientists were at that time in the early eighties, and quickly began making public statements about what a curious institution the ABRC was. But I soon got onto reasonably friendly terms with him. The other commotion about the ABRC was I suppose, fortunately for me, though other people might not remember it all that well, a debate of the ABRC, I think at the first meeting, about just what was the brief of the ABRC anyway? Was it concerned with whether or not people supported research that was relevant to industry or was it really a body that supported fundamental basic research? I said, either then or at the next meeting, having taken the trouble to look back through the papers to Fred Dainton's day, that as early as 1972 the ABRC under Dainton's chairmanship had produced the criteria that ought to govern its consideration. Some of them were internal criteria about science; namely was it excellent, was it front-running in the world, was it an important part of training? Others were external criteria; was it relevant to industry, was it relevant to the economy and all that sort of thing. So 'To my mind', I said, 'undoubtedly the relevance to industry is a part of the picture that the ABRC has to consider. And had you known the back history, and I'm alarmed to see that you don't seem to know it, you'd have known perfectly well.'

MB You shot the rug from under them!

DP And one way and another, that got me off to a tolerably good start, after a couple of meetings.

MB I take it that Fred Dainton had talked about that?

DP Well, I'd talked to him about that and I had actually read the papers. The ABRC throughout that period had produced annual reports. And if you look back in the reports of '72, '73 – that sort of period – you'll find these embryonic common criteria on the reasons for government support.

MB But, I get the impression David from what we've said before in conversation that Margaret Thatcher was against money going out under the industry wing at that stage?

DP Yes. She had a view which originated I suppose very much from her days in Oxford when she was briefly a Part Two student. Chemistry in Oxford has a fourth year which is entirely research and Margaret Thatcher, as one of Dorothy's students, had elected to do that Part Two year with Dorothy. And she'd tried to work on the structure of the antibiotic, gramicidin, and it's difficult. I suspect it wasn't actually solved until thirty or forty years later. But she'd worked on that enough to know something about bench research and to remember, as it turned out later, quite a bit about the technologies involved in x-ray crystallography. And she took the view that it was Dorothy's sort of science... After all, she could point to penicillin and Vitamin B12. It was that sort of science that in the end was important to drug companies, but you ought not to be doing work that was directly relevant to industry. And she took the view really quite strongly. Fred Dainton and others had, as I say, produced slightly different criteria and they'd invented a category of research between basic research and applied research which is clearly the business of industry, which they called 'strategic research', and which was research that any sensible person would recognise as going to lead to results of industrial consequence.

MB And national interest?

DP In the medium to long-term, but for which you couldn't quite foresee what the applications would be. And to any right-minded person, of course, most of molecular biology, in fact all of it, and a great deal of cellular biology was in that category, and a great deal of chemistry too. Not much of particle physics perhaps, and not much of astronomy, but even there there was a bit of technical fall-out that was important in the development of other sciences. So, it wasn't an absolutely straightforward situation.

MB So, you really hit this chairmanship of the ABRC at a time when science funding was really coming under the microscope in a very tough way?

DP Yes. One of the more amusing episodes in the early days was that Keith Joseph called me in to his office one day and said 'The prime minister has had a letter from Lord Rothschild.' This was on a Monday 'He is not content with the way in which the Agricultural Research Council and the Medical Research Council have implemented his proposals for the transfer of funds to the government departments, and they're not paying proper attention to departmental advice. He wants to talk to

the prime minister about this, and she's going to see him on Wednesday.' So, that gives you some sort of view of the sort of *entre* that Lord Rothschild had to higher government circles, even at that time. So on the Wednesday I went into Number 10. Not quite my first exposure to Number 10, I could back-track and tell you about another one, but my second exposure to Number 10. And there was the prime minister sitting in the drawing room upstairs with Lord Rothschild and the minister of agriculture, the minister of defence, Keith Joseph and me, numbers of other ministers, listening attentively to what Keith Joseph and the prime minister and Lord Rothschild had to say. I intervened a little in the conversation, not I suspect to very much purpose, and eventually after what seemed to me to be a not terribly meaningful discussion the prime minister looked at the clock and said 'Well, Gentlemen, it's time we all went off to...' - some meeting or other. I suggest that Professor Phillips stays and talks with Lord Rothschild to see what needs to be done next. So off they trooped.

MB And you were left?

DP I was left with Lord Rothschild and Robin Butler, who at that point - he's now the secretary of the cabinet - was one of the prime minister's office, probably her personal private secretary, hovering around to make sure that everything went alright. And Rothschild and I had a perfectly friendly, but not terribly pointed, conversation in which I said things like 'It's interesting that the prime minister was willing to have this interlude with you at rather short notice.' And he said 'Yes, you know, I think she rather enjoys a little relief sometimes.' So again it wasn't a terribly meaningful conversation, but it was a little of an insight into how some people are in a position to pull strings and make bells ring. It was interesting. The previous visit to Number 10 had been in a very early period, shortly after I was elected a Fellow of the Royal Society. For some reason they decided they should make me a member of the council, and it was in the last year of Blackett's presidency of the Royal Society. And he of course, Lord Blackett, was on close terms with Harold Wilson. Wilson invited the council of the Royal Society along to Number 10 to sit in the cabinet room around the cabinet table and talk about what the council of the Royal was worried about. Tony Benn, I remember, came in as well and intervened at various stages. To my shame I didn't intervene at all seriously in the discussion, but I had a bit of a private conversation with Wilson and it was mostly about Huddersfield Town football team. But that's neither here nor there! I had been inside Number 10 before this Rothschild encounter. So yes, going back to '83...

MB Yes, you're really into the story of science funding.

DP Well we set about setting up the first part of the ABRC's annual tour of duty, which was to produce by April the so-called 'Forward Look' which was a document which went to the secretary of state saying what we thought were the budgetary needs of the research council system, and at that time the Natural History Museum which was included in our remit for the coming year, how much extra money we needed and the reasons for it. A closely argued case. The chairman of the University Grants Committee was an *ad hominem* member of the ABRC, so he sat on that debate and went back to his own committee to put in their bid as well. So there was a little bit of co-ordination, but at that point there wasn't a reciprocal relationship. I was not invited to be a member of the UGC. The chairman of the UGC was Ted Parkes who

was the vice-chancellor of the University of Leeds, and he fitted in quite well in the debate. The chief scientists of the government departments in the main sat silently, unless they felt that for some reason their departmental interests were threatened and then they would intervene in the discussion and say what was worrying them. But they were not terribly useful and constructive members of the ABRC and that was evident from the beginning. Nevertheless, they were there and from time to time some of them intervened quite usefully.

MB David, I just want to ask a question at this point about the time you spent there, because this was a demanding job producing budgets of a complex type. Was it two or three days a week at that stage?

DP It was formally two days a week and the university took the view that they would expect any senior professor to spend at least one day a week on public service, but if it was going to be two days a week they would expect to be recompensed for it. So the deal was that DES paid them my salary for the extra day a week and they paid me a retainer for the two days a week.

MB And you got some support in Oxford to cover that time, did you?

DP Yes, I got supported. I mean, my salary as a result increased a little, not all that much but it increased a little.

MB David, before we wind down this session, because we're going to take lunch in a moment, I would like to ask about the relationship between the SERC and the MRC which quite quickly you discerned to have tensions in it.

DP I think that wasn't too evident to begin with. Jim Gowans and John Kingman got on quite well, but there were always inherent problems within the research councils. The SERC saw itself as responsible for the wellbeing of science as a whole and the council of last resort. If the other councils with specialist interests like the MRC, NERC, ARC and even, I suppose, in principle what was now the Economic and Social Research Council, for some reason turned down applications for support, people were entitled to go to the SERC - the Science and Engineering Research Council as it became under Geof Allen's chairmanship, I think, a little before my time - as the council of last resort, and people could go and ask them. The outcome was that the SERC actually supported biology and they were deeply conscious that the cream of biology was being supported by the MRC and, to a lesser extent, the ARC. And eventually that led to a certain amount of rivalry and discord because the SERC started approaching clients of the MRC, including I may say, me, and saying 'A lot of what you're doing is becoming extremely technical, I mean, computers and diffractometer and things that we're really rather expert in in the SERC. We would be glad to consider applications for support from you.' To which I said initially 'Well, the MRC has provided computers and diffractometer and so on for me hitherto.' And they said 'Well never mind, next time you need a new computer why don't you think about us as well?' So for whatever reason, and any academic might surmise what the reason might have been, that's to say multiple sources of funds are always better than only one source of funds, I did in fact begin to put in applications to the SERC as well. Now, that developed a bit. Successive chairmen of the science board of the SERC began to be a bit more ambitious about biology. And the sort of debate that

had gone on between Tim Jones and the chemists began to go on between biologists and the physicists and the chemists at the SERC; 'Why aren't you supporting more biology?' And that in the end became uncomfortable between the MRC and the SERC. It wasn't too evident in the first year or two, but it became evident later on.

MB Is that about 1985 or 1986?

DP Yes, that's right.

MB And the SERC and MRC had different bureaucratic approaches and everything. There was a lot of difference between them.

DP The SERC had its origins in '65 when the old Department of Scientific and Industrial Research, which had been invented in 1915 in response to war-time pressures and was a government department, was broken up. And some of it went into, probably, MINTECH or something, and some of it went into the SRC and the NERC. But the SERC retained a lot of the flavour of an ex-government department and the civil service.

MB Yes, very bureaucratic.

DP The MRC had its origins in 1913 or something of that sort in the Medical Research Committee, and all grew out of Lloyd George's pre-war budgets - 'a penny out of every National Insurance contribution should go to medical research.' Very far-sighted that, the ethos of the Medical Research Council, as it became post-First World War. It was very different, and they for example had senior medical scientists on the staff of the MRC who played a part in helping rising scientists, including me, to frame their grant applications and tell them how they fitted in with the general MRC programme and how biomedical science was developing and so on. So it was a more personal body than the SRC which at that stage was rather impersonal.

MB Yes, they were streets apart.

DP And that persisted really.

MB And did that result in tensions as well?

DP Well, that contributed to the tensions, yes. I mean, looking forward to the end of my story, it undoubtedly played some part in changes that were brought in in 1993.

MB Yes. But I'm not going to go into that now, before lunch. But just winding up, I think it was quite clear by about 1986 that you were going to need some committee or some means of resolving the difficulties between these two councils?

DP Yes. It must have been in 1986, I suppose, that we thought that through the eighties we've actually been up against it, we have succeeded in getting modest increases to the science budget year on year, but as compared to the increases in the sixties, they were very modest increases indeed. You will remember that in '79 Shirley Williams had written an article in *The Times*, I think, which included as a first sentence - 'For the Scientists, the party is over.' And that really set the tone for the

eighties. These large increases were over, and we had to think much more seriously than people had thought in earlier days about the priorities that we should be setting. And the 'Forward Look reports' from quite early on in 1983/'84/'85, began to talk about 'are there areas of science that we could afford not to be in at all?' We'd set up a committee at some stage, chaired by John Kendrew, on the future of the UK's participation in CERN, which John Kingman and I largely engineered between us as a joint SERC, ABRC study. And it came up with the proposition that yes we should remain in CERN, but it was becoming a burden and we should seek ways of reducing the cost and find ways, if we could, of evening out the costs that result from exchange rate fluctuations and things like that. So, that had been a big issue. And in about 1986, by which time there was a new chairman of the SERC - Bill Mitchell, a professor of physics in Oxford - we decided that we ought to do a serious study on the future of the science base. So we went into great debates, in which the then secretary of the ABRC - we'd started off with a man named Ian Tom as the secretary and he lasted a couple of years and was succeeded by an excellent lady named Helen Williams - helped me recruit some external people as a secretariat at the ABRC. People from, as it turned out, scientific journalism and universities and research councils, not civil servants. And they were a great help. We thought we should produce a strategy for the science base and this was actually published in 1987 and created what I can only describe as an uproar, because it suggested that it wasn't going to be possible to support all universities at the same level to engage in scientific research. Universities would have to be differentiated. Some would continue to do research on all fronts as they were doing at the moment, and we would call those 'research universities'. Some universities which were not first-class in research in any subject at all, but were doing a good teaching job, would concentrate on making their teaching even better and would be known as 't-category universities'. Some universities were quite good at researching one or two topics, but not across the board, and they would be called 'x-type universities.' They ought to have been called 's' for 'selective', but 'x' stuck. So, one proposition was that universities should be differentiated as r, x and t universities. And the other proposition was that some of the money which still went to universities as part of the dual support system from the University Grants Committee should be transferred to the research council system, and the research councils should then pay the full costs of the research, except for academic salaries and the cost of the premises. This was because research councils were paying marginal costs and they were never really sure that the full costs were available to the research workers and it was a muddle, at least in our view. So those were the two propositions. And they both, as I say, raised an uproar.

MB Yes, you dropped the bombshell of the eighties on science funding?

DP That's right.

MB David, at this point, with that bombshell there and all its explosive force about to rock things, we're going to take a break and have some lunch.

MB David, how was news of the r, x and t philosophy actually launched on the scientific world?

DP It was published by the secretary of state as a blue booklet entitled *A Strategy for the Science Base. Advice to the Secretary of State by the Advisory Board for the Research Councils*, in 1987, and it hit the academic scientific community, and they were appalled.

MB You weren't popular.

DP No

MB You had a lot of press stick at that time?

DP There were a lot of press comments and there were lots of letters to journals like *The Times Higher Education Supplement*. Questions were asked. A special meeting of the parliamentary and scientific committee took place in the grand committee room off Westminster Hall, which I had to address and explain all this to the assembled company. The parliamentary and scientific committee is made up partly of parliamentarians, members of the House of Commons or the House of Lords with an interest in science, and partly by representatives of outside bodies. And by this time I'd arranged that the ABRC was one of the outside bodies represented. I was a member of the parliamentary and scientific committee, accustomed to going to meetings, so I knew the form more or less. But I was a bit taken aback because I'd no sooner given my account of the strategy advice than people bobbed up in the audience being severely critical, not to say antagonistic, about the r, x and t proposal in particular, led by someone who's become a friend since then – Tam Dalyell, the rather well-known Labour MP, who I can only say attacked me most vigorously. I met him some time afterwards and he said 'I'm sorry that I set about you so vigorously at the parliamentary and scientific committee about r, x and t. But, you know, my father-in-law had a great deal to do with the foundation of Stirling University and I was afraid that your proposals were going to damage Stirling and other small new universities, and now I see it slightly differently', or words to that effect.

MB David, the proposals that you put forward were very much in keeping with your thoughts about science funding. You were one hundred per cent with that?

DP I was quite convinced at this point that choices had to be made and selectivity and priorities were two of the key words, and that we couldn't adequately sustain first-class science in this country unless we went down that path. That is still my view and I am still not sure that we've adequately grasped that nettle.

MB You wouldn't change a word of it now?

DP No, and indeed it seemed to me at the time and it seems now to be much more a prediction of the way things will have to go, rather than a recommendation to the secretary of state that he should put a brass plate on the doors of universities saying 'r-type' or 'x-type' or 't-type'. And indeed, Kenneth Baker, who was the secretary of state by this time, shrank from any such suggestion. That wasn't for him to do. He wasn't going to categorise universities in this way. And in a sense the tumult died down, but the realities didn't change and if you look around now at the way in which university funding has gone, we have a situation which is not a hundred miles away from r, x and t.

MB Yes, it's very much gone in that direction. Although you stayed with that philosophy and you've never been shaken, despite some of the rough words that were offered in response, I've got an impression that one or two of your colleagues would have back-tracked. I don't want to name anybody, but I've got an impression that not everybody who served in your team in preparing that document stayed comfortable with it.

DP Well, there were some members of the ABRC at the time whom I thought were a little less than one hundred per cent supportive in public of our proposals. I mean, it was told to me that one of them had said 'It must be a printing error.'

MB Yes. I just wanted to get that. I guessed that there wasn't total support at a time when you deeply needed it.

DP Well, I felt confident enough in the message that we had to put out not to feel all that vulnerable. No doubt other people would have thought 'It's alright for him, he's an Oxford person and whatever happens Oxford will be alright.' So there would have been some feeling of that kind, but honestly that sort of consideration didn't come into my mind at all.

MB David, soon after that you also had this relationship between MRC and SERC boiling up and the need for a further committee. This was in about 1988?

DP Yes, things became a little awkward, and again rather on the lines of selectivity and choice. There was something of a feeling that the research council system that had been established in 1965 with a minor, as it turned out, reform under Keith Joseph connected with the social sciences was no longer really appropriate. And as a result of that feeling we set up an ABRC committee, again under the chairmanship of Dick Morris who felt quite strongly about this, to consider the funding of biological sciences by the research council system. That was really their brief.

MB And they took it rather seriously and went over the top, I think.

DP And they went away and had a series of meetings. They were a high-powered, influential group. There was Dick Morris, but I don't suppose I can recall off the top of my head all the other members. They certainly included Jack Lewis, the inorganic chemist from Cambridge, and numbers of other people who'd been associated with the ABRC. And they went away and came back in the spring of '89 with the Morris Report, which indeed went some way outside their terms of reference. What they recommended was that simply tinkering with the responsibilities of the research councils wouldn't be enough, the whole system really needed to be changed. And what should be put in place, said Morris and his friends, was a single research council with operating divisions based upon, but not precisely the same, as the existing research councils. The single research council would have a non-executive chairman, an executive director, and the heads of the operating divisions would be executive members of the council, and there would be non-executive members of the council as well. And that actually chimed in with a study which had been going on, certainly with my knowledge and partly with my encouragement, within the Department of

Education and Science on whether or not a commercial board structure would be a good model for the ABRC. The operating divisions were to be in medical research and that was the MRC largely unchanged; engineering which would have been taken out of the SERC; physical sciences which would be part of the SERC; biological sciences which would have been the non-medical part - that is to say the ARC and the biology supported by the NERC probably; and the SERC, environmental sciences and social sciences. I don't recollect whether Morris actually went back to calling it social sciences, it was a rather taboo title, but they were roughly the areas. And this report came to the ABRC at a meeting in the spring of '89 and we discussed it and...

MB How would you have found it, David, just reading it before that meeting?

DP I wasn't particularly surprised that they'd rather generalised their terms of reference, because I knew Dick and his colleagues quite well and that's what they would have done, I thought.

MB But what they proposed...

DP What they proposed I had some sympathy with. But let me put it this way, when it came to the ABRC meeting I was concerned first to find out what the board thought about it. And I didn't give them a terribly strong lead because, to my mind, it could have gone in a couple of different ways. Initially the board was really quite sympathetic, including the chief scientists from the government departments who of course were all still sitting there and saw themselves somehow marginalised by this proposal because there was no room for them, or at least not for all of them on the proposed single research council. So, the board left it that we would come back to it again when people had had time to have a considered look at the report and the heads of research councils in particular had had a chance to consult their councils about it. So a couple of months later we came back to it, and by that time the board was overall a little less sympathetic to accepting the report as it stood, but still accepted that something had to be done. So in good committee fashion we decided that what we needed to do was to set up a small working party to recommend what the board's advice to the secretary of state should be on the basis of the Morris Report. The secretary of state by this stage was John McGregor. So, a meeting was arranged which involved me as chairman, the heads of the research councils, the secretary of the board who by this time was a young man named Peter Thorpe, and three non-executive members of the board. We met in Studley Priory, a hotel just down the road from Oxford, because that was a quite convenient place for a weekend meeting. It was joined by those people and John Vereker, who at this stage was the deputy secretary of the DES responsible for higher education and science. And we had a rather good debate and discussion and out of it came a proposal that we shouldn't set up a single research council, but what was needed was a reform of the ABRC, and the ABRC should be re-constituted rather like the board of a company, that it should have a full-time chairman. It wasn't made absolutely clear to what extent this board was going to be executive and to what extent it was going to be advisory. It was in the main still advisory, but with hints of executive about it - so a non-executive full-time chairman, me in the first place the recommendation was going to be, five executive directors, the heads of the research councils, and then a bit of debate. One of the heads of the research councils wanted there to be only the same number of non-executive directors as there were executive directors. Others thought there should be

a bigger non-executive element. And in the end it came down to six non-executive directors in addition to the chairman. The chief scientists from the government departments were to be excluded, but there was to be an arrangement that they would receive the agenda of the meetings and if they saw items which they thought were of particular concern to them they would have a right to ask to come for those particular items. They would receive the minutes, and annually the new ABRC would have a meeting with the chief scientists at which there would be a frank exchange of views and the way things were going would be discussed and whether or not departmental views were properly represented and so on would be aired, and so on. So, this came back to the ABRC as the advice of the Studley Priory group in the autumn of '89. And that advice was endorsed by the ABRC and sent forward to John McGregor the secretary of state. And he accepted that in the December of '89 to be implemented at the beginning of the financial year in 1990, because the ABRC had no more status than that of a committee appointed by the secretary of state for education and science. It wasn't a government quango with any more official status than that. So the secretary of state could in effect decide, after of course discussing it with his colleagues, whether this was the way forward or not. So that was what was decided, and I was asked from April 1990 to become, for a three year term, full-time chairman of the new ABRC.

MB David, two points from me at this stage. Before that Studley Priory weekend, were you aware that McGregor would not have bought a 'one organisation in control' situation?

DP No

MB Was that his position?

DP No, he'd not made that position clear.

MB Right. But it would have been his position, do you think?

DP At least I don't recollect that he'd made that position clear. If he'd made it clear, it would have been as much to John Vereker as to me, and John Vereker would have conveyed that message to the Studley Priory group had it proved necessary. But I don't think anybody at Studley Priory was sold at that point on the single research council idea.

MB But I've got an impression that McGregor probably wouldn't have gone that way.

DP I think that's quite likely.

MB The second point of inquiry is that by 1990 you are coming to the end of what is your Oxford career.

DP That's right. I would have retired under normal circumstances from Oxford at the age of 67 at the end of the 1990/91 academic year. Taking advantage of the sabbatical leave system, I'd actually arranged to have sabbatical leave for the 1989/90 academic year. So when all this was going on, I was on sabbatical leave and able to

devote a fair amount of undivided attention to it. Though I should also recollect that it was quite an exciting time in the lab. Rather good work was going on on a number of different fronts. Louise Johnson for example had produced not all that long before the structure of phosphorylase, which had created a stir. Work had begun on using Laue photographs to record x-ray defraction intensities extremely quickly using synchrotron radiation. And we were almost in the business of watching enzymatic reactions as they took place, almost but not quite. And Dave Stewart, a university lecturer in the lab, had worked out the structure of the foot and mouth disease virus which was a major *tour de force*. A piece of work of course that was made possible by the virologists at Wellcome led by Fred Brown, who were able to crystallise the stuff and were allowed by the chief veterinary officer to take these crystals to the Daresbury synchrotron, so long as there was somebody there all the time who knew how to inactivate the virus if for some reason one of these crystals got out into the atmosphere. So that was a tremendous piece of work, I mean, compared with lysozyme. I suppose that's more than a hundred times bigger, and a tremendous structure which revealed the way in which antibodies recognise foot and mouth disease virus or don't, as the case may be, and the way in which the virus recognises the cell receptor with which it interacts. It wasn't the first virus structure ever. There had been studies of polio virus and the common cold virus in the United States by then, but it was the first viral structure in this country and a tremendous piece of work.

MB Monumental. So what we're saying is that when you went you were really quite fulfilled because what you'd brought on-stream was actually paying off rather large returns?

DP Tremendously.

MB That must have been remarkable. It was a time when you could go and you weren't backing out on anything you'd not fulfilled.

DP So I wrote to the vice-chancellor, who happened to be Dick Southwood at the time, and said 'I've been asked to do this job and that means maybe I should retire early and what do you think about that?' And he said 'Yes', he felt sure the university would be agreeable to that and they would appoint a successor. The arrangement didn't affect my pension at all, it actually meant I would be paid for another two years. Not that that was a major consideration, but it was a consideration. And they set about going through the formal processes of deciding whether the chair of molecular biophysics, which you will remember was an *ad hominem* chair, should be continued or not. And they started a discussion of that, and then Edward Abraham came into the picture and said 'Well, I think it is important that this work that's been begun should be continued. And we do need a statutory professorship and I'm prepared to endow it.' So, he put up a sum of money which provided an endowment for the chair of molecular biophysics, and he went a step beyond that. He rang me up one day and said 'Would you mind if we called this chair of molecular biophysics the David Phillips chair of molecular biophysics.' And I said 'Well Edward, I should be flattered beyond words.' And he said 'Well, alright, I'll suggest that.' So, that is how it turned out, and an appointments committee was set up and they went to work. And there were various good candidates, at least three of whom were either in the lab or ex-members of the lab, and the first David Phillips professor of molecular biophysics was the graduate student who looked at lysozyme inhibitor complexes at the RI way

back in 1966, namely Louise Johnson. And all I can say is that I keep in touch with the lab and it goes from strength to strength. It's a very pleasant thought.

MB It must be a pleasant thought. Before we actually go on, and we haven't a long time for this because I don't want to break too far from your advisory work, but the eighties must have been a time when that unit that you set up in Oxford did move house. There were new premises, and that was quite a big development.

DP Oh goodness, yes. We moved from the new Pringle building to yet another new building which was built on the lawn. And you can imagine the debates that went on in the university about whether this bit of lawn should be built on or not. It actually obstructed the view of the professor of physical chemistry, who previously had been able to see the cricket pitch in the park from his office window. So it was built at the end of the physical chemistry building - the professor of physical chemistry, John Rowlinson, was very supportive I have to say and didn't stand in the way of this - in the angle between physical chemistry and biochemistry, actually connected with a covered way into physical chemistry. And that's actually ... if you want to choose an ideal location for molecular biophysics that's not bad - between physical chemistry and biochemistry. The ground floor was a part of biochemistry, and it also had Oxford enzyme group connections because it had in the basement the NMR machines, and the money for it was raised by Rodney Porter and Rex Richards who went round to charitable trusts like Wolfson and the Wellcome, no doubt.

MB They raised a lot of money.

DP A sizeable amount of money. And Edward Abraham put money into it too.

MB Am I right in thinking it was going to be, initially, a two-storey building and then it was four?

DP That's right, it's a basement and one, two, three, four storeys, something of that sort. So it had a basement with NMR, and a ground floor with the NMR people, Raymond Dwek and Iain Campbell, and some other things. And then the first floor was Rodney Porter's MRC unit of immunochemistry, which Rodney was going to keep on running after he'd retired formally. And then the top two floors were molecular biophysics, again with a bit of Oxford enzyme group lab for Bob Williams thrown into that. So that was a kind of almost physical realisation of the goals of the enzyme group, bringing it as a sort of central focus for the enzyme group, although the individual members mostly stayed in their own departments still. And that of course was called the Rex Richards Building.

MB It's nice to have those recollections of the eighties completed. Now we can move into the nineties and talk about the move ahead, you in a full-time post with ABRC. How did that work out? Was that a more comfortable setting than part-time? You obviously had a much bigger job by then.

DP Yes. That ABRC was I think an improvement, the smaller number around the table, and we changed all sorts of things. How we did the forward look exercise was done collectively rather than the independent members interviewing the heads of

research councils one at a time, that sort of thing. It became, well I suppose much more corporate if that's an acceptable word these days.

MB I think we said in conversation before that John McGregor had good dialogue. He was an easy man to talk with and it was a very comfortable time?

DP That's right. John McGregor was one of the better secretaries of state in terms of going along and chatting to him in his room about the problems of science as we saw them and what were the best ways to approach it and so on. And he was generally very supportive over the reform of the ABRC, and in other ways. The sad thing was that at the end of the year 1990, in the annual government announcement on public expenditure, the science budget was I thought rather poorly treated.

MB You said it was the worst settlement you had.

DP Yes. The news of it reached me in a slightly peculiar way. John McGregor negotiated the settlement, but then he was moved from the Department of Education and Science after rather a short stint there. My suspicion is that he wasn't seen as the person to push reforms in education quickly enough after the rather different style of Kenneth Baker, and some people in the government thought he was dragging his feet perhaps. He wanted a period of consolidation. So, he was moved and Kenneth Clarke was appointed. And that happened over the period of the public expenditure settlement. So, when I first met Kenneth Clarke he said to me 'We must now open the sealed envelope that John McGregor has left us with the public expenditure amount in it.'

MB Does that happen?

DP He of course knew very well what the settlement was, but I didn't. And that's the way he chose to put it, no doubt realising that I wasn't going to be terribly happy about it. So I looked at it and said 'Well, I think this is creative accounting.' In brief, what they'd done was to say that a certain proportion of the science budget had been provided for capital expenditure. This had now been spent on capital expenditure and it wasn't in the base-line of the budget, whereas it was on the ABRC's advice that that proportion of the budget had actually been used for a capital development. And I thought that was cheating, and I said so.

MB And you said so publicly.

DP And I said so publicly. Upon which ... since I've already said that the deputy secretary in charge was John Vereker, I might as well say that John Vereker came down to see me and said 'Since the ABRC was reformed you are no longer an independent, you know. You are a civil servant and you are not allowed to make public statements like that.' To which I said 'Well, that isn't in the letter of appointment that I've received on the reform of the ABRC. I was just given a letter that, you know, you'll please act full-time and there'll be a negotiation about your salary. There was nothing about these conditions in it, and I don't accept that. I don't think I would have taken the job on those terms. So, if I see a need to say that this settlement is very bad, I shall say so.' So, he went off and a week later he came back and said the same thing again. Probably I'd said something in public again in the

interval, and I said the same thing again. And he said 'Well, ministers will have to speak to you about it.' So I said 'Alright.' I then went along a day or so afterwards for one of my reasonably frequent conversations with the then junior minister, who was Alan Howarth, and we had our usual friendly chat about science and the science budget and how things were going. And he never said anything about me not being able to make statements in public. He said something on the lines of 'We're lucky to get a person like you in this job.' And that was the end of it. I didn't take that as a reproof particularly, but of course I had to see Kenneth Clarke afterwards, and we trooped along to his room in Westminster. John Vereker was there and the secretary of the board was there and various other secretaries, Kenneth Clarke's private secretary and so on. And I thought 'Well, this is where I get carpeted.' And we had a conversation about the settlement and the budget and Clarke said 'Well, I understand that you're unhappy about it and I understand that you've made public statements about that. And all I would say to you about that is that I wouldn't want to say you mustn't make public statements; we wouldn't get a person like you to do the job if we did make that a rule. But you will of course understand that the kind of public statements that you make will affect your ability to influence me to do the things you want me to do.' And I said 'Well, I've always understood that perfectly well. I think secretary of state and I am perfectly happy with that as a statement.' And he said 'Well, fine then.' And off I went.

MB End of story, for then?

DP End of story, for then. Now, that year we went through the usual cycle and produced a 'Forward Look' paper for Kenneth Clarke on the needs of the science budget, coloured by what had happened the previous year. And also pointing out, which we'd often pointed out to no effect, that it wasn't any use producing any extra money for year one of the coming three-year period if one didn't also produce a good profile for years two and three of the period, because you'd have to spend the first year's money on capital investment or whatever, and you wouldn't be able to launch continuing programmes. Well, Clarke obviously took that message and come November - I'd seen him in the interval - I was summoned to his office for the usual statement to me of what was in the public expenditure settlement. And he began by saying 'You were quite right in what you said last year, it was a terrible settlement.' So, I said 'Well, thank you very much, secretary of state, and I hope you've done better,' or words to that effect! And he then produced his settlement, which was in my view the best settlement we got because it did indeed have a rising profile over the first three years.

MB That was a major advance.

DP It wasn't the largest increase in the first year, that credit goes to Kenneth Baker, but he didn't produce a steady profile after that. Kenneth Clarke produced a steady rising profile over the three-year period, and that was splendid. So, that was how that rumpus died down eventually.

MB So you went into 1992 feeling a lot more comfortable?

DP Yes.

MB I mean, you'd offered the science community a much better deal.

DP That's right, a programme for three years. And then of course came the election.

MB And you were going to get a bombshell thrown into your arena?

DP Well, there was the election, of course. And we'd already been engaged in conversations with treasury ministers and officials about the 'Forward Look' process and the prospects for public expenditure and things like that. And then comes the election and I go away on the Thursday evening, voting day as it happens, walking to the station with the very treasury official that was responsible for the science budget. We had a chat about how it was going to turn out and we agreed we couldn't really call it but on balance we thought Labour might conceivably win, and then we went home and no doubt watched the television and lo and behold the government was returned.

MB March 1992.

DP So I go back into the office on Monday morning, and I'd been there an hour or so when the permanent secretary of the department, John Cains, suddenly appears in my office looking, I thought, rather shocked. And he sat down and said 'I've got some news for you. I haven't given it to anybody else yet, and I don't mind telling you that it comes as a surprise to me, but the prime minister has decided that he wants to reorganise the support of science. Science is going to be transferred from the Department of Education and Science which will, therefore, become the Department for Education, to a new Office of Science and Technology which will be under the aegis of the chancellor of the Duchy of Lancaster in the cabinet office. And the head of the Office of Science and Technology under the chancellor of the Duchy will be the chief scientific adviser, Professor William Stewart,' who was of course an ex-officio member of the ABRC and a friend, but that's neither here nor there. The nub of the news was we were suddenly to be taken away from the DES and put into a new office. In the short-term of course nothing could be done about the office location. We had to stay in the DES building until premises could be found for the Office of Science and Technology. But I was no longer in a position of advising the secretary of state for education and science, but rather the chancellor of the Duchy of Lancaster. And when the government appointments were announced, the secretary of state for education became John Patten and the chancellor of the Duchy of Lancaster was William Waldegrave, and his junior minister was Robert Jackson whom I had already known as a junior minister in the DES. So they'd both been junior ministers in the DES in my day. So that part of it at least seemed familiar and promising enough. And I assumed - nobody ever told me for some time - that the role of the ABRC was just the same, except that we advised the chancellor of the Duchy of Lancaster instead of the secretary of state for education and science.

MB In terms of Waldegrave you found that very comfortable, you said?

DP Yes, sure. He announced quite quickly that it was his intention to produce for the first time in many years a white paper on the government's support for science

and technology, and that in the meantime he would just continue looking after the research councils as they'd been looked after before, with the ABRC's advice.

MB So, immediately you were set to contribute to a white paper?

DP So, immediately a call went out for advice to be sent in to the chancellor of the Duchy on what should be in this white paper. And the ABRC, drawing on its previous experience with the debate about Morris and its previous experience in framing the strategy for the science base in '87, set about a long debate which ran through the summer and early autumn on how would we really like to see a new structure of the research council system and how the research councils should be shaped and funded. We recommended numbers of things, going a bit closer to Morris in some ways, recommending for example that the terms of reference of the individual research councils should be changed so that they looked rather more like the Morris operating divisions of a single research council, but not going all the way to a single research council. Though there was again on the ABRC serious debate about whether we should be recommending a single research council. But on balance it turned out that in order to get a pretty unanimous bit of advice, that the others didn't want to go all the way to a single research council. I think there would probably have been a majority for a single research council, but there would have been a minority view and things like that. It wasn't entirely straightforward. Indeed, I have a feeling that there's a note in the front of our advice saying 'all members of the ABRC don't necessarily agree with all of this advice' or some such statement of that sort.

MB You feel there was a meaningful reshaping of the boundaries of the subdivisions?

DP Yes, that's right. People did go along with that, with various degrees of enthusiasm probably, but they went along with that.

MB David, before I come to asking about the implementation of what you recommended and what came out in the white paper, I just have a feeling that what was done at the time of 1992, transferring you to cabinet office control, something similar had been proposed by Labour in earlier proposals of the Labour Party.

DP That's right. What actually happened in view of the election result came much as a surprise to me as it came to John Cains. But looking back the Labour Party election manifesto included some reform of the support of science roughly along those lines. And it was known that in advance of the election, in case Labour should be returned, Robin Butler and Bill Stewart had had conversations with representatives of the Labour Party about the sort of things that they would want to do. Because it's perfectly normal that the civil service has to be alerted to the sort of things it might suddenly find itself needing to do. And it's hard to escape the impression that Bill Stewart and Robin Butler, of course with the prime minister deeply involved, thought 'well, that sounds quite a reasonable proposition which I think we might very well implement.' So that's the background to it.

MB What I don't understand is why they grabbed that ball so quickly and ran with it straight after the election.

DP I've already explained that I wasn't party to any of that discussion, and if you can find Bill Stewart in an indiscreet mood he could probably tell you more about it than I can.

MB It just seems amazing though, doesn't it? You were really caught.

DP Yes, and it certainly came as a shock to the permanent secretary in the DES, which in itself is interesting.

MB David, we have that white paper and you getting involved in implementing its recommendations.

DP We've left out of the conversation another government committee of which I was an ex-officio member from 1982. It actually went through two incarnations. When I became chairman of the ABRC in 1983 there was already a government committee called the Advisory Council on Applied Research and Development, which had been set up partly at the suggestion of the House of Lords select committee on science and technology. And that was chaired by Henry Chilver - the vice-chancellor of Cranfield and chairman of the Milton Keynes New Town Trust or whatever it was called, anyway the organising committee for that - and a stalwart committee man, let's put it that way. It was changed after a time, and I think the House of Lords select committee probably played an even greater part in this, to the Advisory Council on Science and Technology with a somewhat different remit. And that was chaired by Francis Tombs, who was also the chairman of Rolls Royce and various other companies at that stage. By the time of 1992 that committee was chaired by Robin Nicholson who, early on in my time as chairman of the ABRC, had been the government's chief scientific adviser and of course an ex-officio member of the ABRC. I was a member of this committee. And it too was charged with giving the chancellor advice about the shape of the white paper. So I found myself in the funny position of chairing the ABRC producing its advice, and being a member of ACOST, playing a part in framing the ACOST advice, which it's fair to say was radically different. ACOST was not at all keen on the existence of the ABRC and was all for changing that radically and changing the research council system radically, so that there would have been a basic science research council and an applied science research council. I was adamantly opposed to that, but nevertheless the ACOST advice was along those lines. Adamantly opposed because I thought that that would create an artificial division in what the ABRC had always seen as a continuum from basic science through to applied science, with exploitation being something that we had to be concerned about. Well in the event the white paper, which was drafted through the late autumn of 1992 and the spring of 1993, the lead being taken by an under-secretary in the OST and a good friend of mine, with Bill Stewart putting a major oar in from time to time and me playing a part from time to time, and eventually the chancellor deciding on the line he wanted to take, came out in May or June 1993.

MB Was this under-secretary Wilkinson?

DP Yes. David Wilkinson. You will recognise that by this time I should have retired because I was due to retire in April 1993. But William Waldegrave said to me at some time in that spring 'We're obviously not going to get the white paper out in

time to implement it before you retire. So would you be prepared to go on until the beginning of September, by which time we might be able to implement things and you could look after the changeover?’

MB That was being a little optimistic.

DP And then a little later, again when I was talking to him, he said ‘Well, we’re not going to get it out in time to implement by September, so I think our reasonable target is that we will implement the changes on the 1st January 1994. And would you be prepared to go on with the ABRC through the normal ‘Forward Look’ and allocation of resources exercises to the end of December 1993?’ So I said ‘Yes.’ Then he said ‘Well, since we’re actually looking to abolishing the ABRC,’ something that I tried to resist but unsuccessfully, ‘and we shall then have a director general of research councils come the autumn of ’93, I’d like you to act as though you were the director general of research councils, though of course keeping the Advisory Board for the Research Councils in the picture.’ Well, I had argued against the abolition of the ABRC. The reason for it was that a tradition had grown up that although the ABRC’s advice on how much money was needed for the support of science was almost invariably not accepted, the advice on how the money that was allocated should be allocated almost always was accepted. And William Waldegrave took the view that that was really a ministerial decision and it had begun to look like an ABRC decision, and that wasn’t right, the process ought to be internalised. He would get his advice in effect from a civil servant, the director general of research councils, and then the decision on the allocation would clearly be as it should be, a ministerial responsibility. I always argued that it always had been a ministerial responsibility and that if appearance made it look otherwise then he could make it clearer that this was his decision and I would be quite happy with that. But I much preferred the ABRC. And I tried very hard to make sure that the director general of research councils was actually helped by a group of advisers, five in number, which some people involved in the debate thought was me trying to reinvent the ABRC, and I suppose to a degree they were right. But they didn’t like it. And what has happened subsequently is that those advisers have never been appointed and the DGRC acts on his own and gets his advice from wherever he thinks appropriate, which I think is less than satisfactory. I also had a discussion with Waldegrave about ‘wouldn’t it be better to have a single research council?’ and he thought a single research council would be much too powerful an external body and it would erode even further ministerial responsibility. So his changes to the white paper - at least that part of it - were based entirely on the notion, a perfectly proper notion, that the responsibility for the allocation of public funds rests with ministers who are answerable to Parliament. And that philosophy is hard to quarrel with, but how it is implemented in detail I admit I’m not particularly happy with.

MB It wouldn’t have been your way?

DP No

MB But you did end up being the first un-official DG?

DP That’s right. The part of the white paper that I much agreed with was a reconstruction of the research council system. So in July William Waldegrave said

'Well, we need to go ahead with this because the new research councils will have to be in place in time for April 1994, as we need to decide in detail the boundaries of the new research councils. Will you please, as embryonic director general of research councils, set about doing that. Now, maybe I set the wrong model at that point, it suddenly occurs to me, because in conversation with David Wilkinson who was the under-secretary still in charge of science, we agreed that there was really no need to appoint a committee to do this with me. That would just prolong the debate, because what had to be done was really all set out in the white paper. Whatever the new boundaries of the research councils were, it really didn't need a committee. It needed me, supported by David Wilkinson and other officials, to walk round and talk to the research councils about how they saw it and to come up with a plan which we could put to the chancellor of the Duchy, and which he could then accept or not. So in the event that's what we did. And over a couple of months or so, no more than that, we came out with a blueprint with what each research council should be responsible for. SERC biology in the main went to the Agricultural & Food Research Council, which was to be renamed the Biotechnology & Biological Sciences Research Council. The MRC possibly got a bit, I remember that, but I don't remember it in detail. The NERC also profited a little from some work in environmental science that the SERC had been doing, the Economic & Social Research Council was left more or less as it was. And a new research council was set up, the Particle Physics & Astronomy Research Council, to take these highly expensive international activities out of the old Science & Engineering Research Council, which itself became the Engineering & Physical Sciences Research Council. So these boundaries were reasonably straightforward to define. There was one slightly tricky bit. The SERC latterly had put some effort into bringing the chemical community into rather closer contact with molecular biologists. And there was a special initiative on biological chemistry and they didn't want to lose that entirely because it was by now, as they saw it, reasonably as much a part of chemistry as it was of biology. So eventually, it was proposed that the committee which looked after that interface should now be a joint committee between the Engineering & Physical Sciences Research Council and the Biotechnology & Biological Sciences Research Council. And that's the way it stayed, I think, reasonably happily. The scientific community that got most uptight about all of this were the chemists because the name... For some reason 'Engineering & Physical Sciences' to them - it surprised me since I'd always thought of chemistry as a physical science - sounded like two research councils for physics and none for chemistry. And lots of us took a lot of trouble explaining to them that now chemists would be able to go to the EPSRC, the Medical Research Council, the BBSRC, the NERC...

MB But they weren't happy.

DP But they weren't happy. They really wanted a research council of their own. The physicists had got two, why shouldn't they have one? But that seems to have settled down more or less, and that research council system is the way it stands.

MB And that's how you left it...

DP And that's how I left it.

MB ...when you retired?

DP That's right. At the end of 1993 I cleared my desk, had conversations with the incoming director general of research councils, John Cadogan, and went home. Home by now of course was in London, because in 1990 Diana and I had moved back to London, the twenty-five year interlude in Oxford put behind us so to speak, and on to a new life in London which has happy aspects because Sarah lives down the road. She got married in 1988 and was now living in Islington, so the family were reunited in London. And a couple of years later Sammy came along.

MB David, coming to 1994. You had retired, and all of a sudden life was going to open out on an entirely new front.

DP Well, I was retired, but as many people say retired life means different things to different people. I was still, as I had been since '82, a non-executive director of the biotechnology company Celltec. I think I was by then a governor of De Montfort University, I was a trustee of the Wolfson Foundation, and I had various odd jobs to do of that sort. And quite early on in 1994 I had a telephone call from Michael Peckham, the research director under the new system for the Department of Health, who said 'We're having trouble reorganising the hospital system in West London under the new trust system and the new proposals for closer connections with the University of London. And I wonder whether you could look into the position of, in particular, the Hammersmith, the Royal Post Graduate Medical School in Hammersmith and the Charing Cross Hospital?' So I went along and had a talk with him and said 'Well, yes, that sounds an interesting retirement job. I think it would be quite sensible if you invited my old friend Rex Richards to play a part in this because he has been involved with medical research in London over the years and either was or is the secretary of the Leverhulme Trust and so on, and knows a lot about this kind of thing.' And Michael Peckham, who knew Rex very well - they have a joint interest in modern art, said 'Yes, what a good idea.' So he appointed the two of us to do this study of Hammersmith Hospital, Charing Cross Hospital, the Imperial College connection and all that kind of thing. And we had an office somewhere in Bloomsbury and set about producing a report helped by an ex-secretary of Imperial College. The report said 'Yes, it would be possible to consolidate Hammersmith and Charing Cross on the Charing Cross site, provided the government is able to put up money to do a fair amount of rebuilding and reconstruction with a new building on the Charing Cross site. And unless you can absolutely guarantee that you'd better not go ahead with it.' We put this to the secretary of state for health, Virginia Bottomley, whose junior minister at that time is the present chairman of the Conservative Party. And they said 'Well, thank you very much for this sterling piece of work.' And that, as far as I can see, was roughly where it rested because the recommendations were not entirely taken up, let's put it that way. Even so, things have evolved. And Imperial College is now the focal point of the medical schools - St, Mary's, Charing Cross and so on in West London, and Hammersmith is seen as the postgraduate research element of this rather large new medical school. And they've got a new director of the medical school who is a splendid chap and they're going to have a new building at Imperial. That part at least was pursued with Higher Education Funding Council backing, so that was fine. And then before that was finally put away I get a letter from the prime minister that says 'Her majesty the queen has it in mind to appoint you to a life peerage. I would be grateful if you would let me know whether you are disposed to accept this offer?' or words to that effect, which I showed to Diana who

burst into tears and which I told Sarah about over the telephone and at which she whooped with pleasure. And I wrote back and said 'Well yes, alright, I would be happy to do that.' And it appeared in the birthday honours list of 1994. I then went into a negotiation with garter king of Arms about what I might call myself. He said 'Well, you mustn't tell anybody about it,' and I said 'Well, that's very difficult,' and he said 'Nevertheless, you can't tell them what your title's going to be until it's been gazetted and the queen has approved.' So, I said 'Well, I propose to stick to my surname since otherwise one disappears behind a different name and nobody quite knows who you are, but since there's been a Phillips before, then it will have to be 'Phillips of somewhere'. And I'd like that to be Ellesmere in Shropshire.' And he said 'Well, that sounds fine.' And I said 'Well, it would at least be courteous to ask the chairman of the local council or the mayor or whoever he is, whether they think back in Ellesmere that that would be a good idea.' So, he said 'So long as you don't tell them what it's about, that will be alright.' So, I wrote a letter to the mayor whom I'd known since I was sort of so big and said 'In absolute confidence, if such a thing should conceivably happen, what would you say to Lord Phillips of Ellesmere?' And of course people in Ellesmere were, or so they told me, very pleased about this. So it shortly appeared that I was to be the Lord Phillips of Ellesmere and I was introduced into the House that October, and made my maiden speech in the debate on the queen's speech on the subject of agriculture as I remembered it in Shopshire in my childhood, foot and mouth disease, of which there had been an outbreak in Shropshire in my middle years and of course, of which the structure had been determined in my Oxford lab in recent years, and that sort of thing. It wasn't controversial, but then it's not supposed to be. And since then I've settled down rather happily in what is a very pleasant institution to be associated with as a member of the select committee on science & technology. And we've recently completed a report under my chairmanship on the 'Information Society: Agenda for Action in the UK,' which is to be debated in the House a week from today.

MB David, which benches did you go to in the House of Lords?

DP The cross benches.

MB I just thought I'd put that on the record.

MB David, we've come to the end of walking many a mile together from the early days when a boy struck out on the Mere at Ellesmere to fight the algal blooms and prove himself, to a title that involves Ellesmere. And we shall talk again about the work you are doing under that name in due course.

DP Well, I shall look forward to it.