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**Sir Stanley Peart FRS in interview with Dr Brigitte Askonas
Oxford, 2 December 1994, Interview Three**

BA Stan, it's a pleasure to see you again, in any case. Now last time, you spoke with Max Blythe, and you had produced a rather impure material at St Mary's.¹ And you made a decision to continue your research at Mill Hill [National Institute for Medical Research]. Why was that?

SP Well, the decision was, of course, partly made for me, because I'd said that I was struggling rather at that stage and I needed some expert advice as to how to continue. And George Pickering² suggested I should go to Mill Hill, because he was a friend of Harington's³ - remember, Harington had been at University College, and so had George Pickering, working with Tom Lewis,⁴ you see, and like all good networks, he knew and talked to Harington and it was arranged that I would go there.

BA And who were you to join? Was that pre-arranged?

SP Well, this was a decision made by Harington, actually, because he saw the sort of problem I was up against - and remember, he was a pretty eminent biochemist himself, having got thyroxin out - and he'd decided, much to my subsequent amazement, that I should join the Department of Physical Chemistry. If it had been the Department of Physicians' Chemistry, I might have understood it! But, in actual fact, it was Physical Chemistry. And this was the Department run by Archie Martin.⁵ And when I went there and talked to Harington in the first place, he said, 'Well, you should join this Department.' And so I did and went to see Archie Martin, who, initially, just said, 'Well, you should work in Porter's⁶ laboratory.' So that's how I was transferred next door, as it happens.

BA Next door, yes. On the third floor?

SP On the third floor, yes, that's right - how well I remember it - and to work in this laboratory. It had rather nice views out of the windows, of course, like all those laboratories.

BA Oh, very pleasant. Very pleasant. So what happened when you first arrived in Rodney Porter's lab, who was a particularly purist biochemist, shall we say?

¹ The partial purification of hypertensin, later named angiotensin, is discussed in Interview II, MSVA 107.

² Sir George White Pickering (1904-1980).

³ Sir Charles Robert Harington (1897-1972). Director of the National Institute for Clinical Research, 1942-62.

⁴ Sir Thomas Lewis (1881-1945).

⁵ Archer John Porter Martin (1910-).

⁶ Rodney Robert Porter (1917-1985).

SP Well, yes, he was and he wasn't, you know. He was, essentially, a pragmatist. And, of course, he was such a great human being. I mean, you know, he could even accommodate the thought of having a medical person in his laboratory, because his views on doctors were somewhat scathing, actually! And he didn't trust them an inch. Very wisely, I think! But he put up with it. And, of course, when I got to... if you remember, I used to use bioassays and I used to use the rat. Now, here was a laboratory equipped... if you remember, Rod was in the early stages of purifying gamma globulin.

BA Yes. This was 1952?

SP Yes, that's right. And he was also struggling like I was, actually. But he was rather more competent at struggling than I was. But he, in fact, was at a stage... there was adjoining, in that laboratory there was a separate area which was refrigerated, because you had to be able to hold the temperature low and consistent.

BA It wasn't the cold room?

SP No. No, he had... no, he had a separate room, actually, off there. And remember, it was the time when they were using partition chromatography of a particular sort; this was the tri-phase... Martin was very enthusiastic about using these tri-phase solvent-salt-water mixtures, and you got this tri-phase, and you ran it down a, what was, essentially, a kieselguhr column, and I saw Rodney trying hard to get any sort of separation of gamma globulin on these columns.

BA Into different fractions.

SP Yes. And what you got was a great broad spread of material, and at one end he thought he'd got a difference between the front end and the back end of the rather spread peaks, and it was obvious it wasn't going terribly well with those solvent mixtures.

BA He was looking for a reaginic antibody, I think.

SP Oh, it was, was it? Well, that's... I'd expect you to know better than I would! And the... but that was what he was doing. And then in I came, you see, and occupied the bench closest to the window and introduced a rat into that place, with rather nice gamma globulin on one bench as far away from me as possible, and there was the rat and there was I, busy putting up this rat for bioassay of the blood pressure raising properties of the material I was interested in. But he got used to it.

BA He got used to it. He was really quite kindly then, was he?

SP Yes. He was not only kind, but he was very helpful, you see, because I used to discuss these problems with him. And, of course... I emphasise he was a pragmatic biochemist, you know, he just knew, it seemed to me, what was the right way to go about separating things, and so that was a great experience.

BA He had that flair.

SP Yes, he really did.

BA So what did you actually do?

SP Well, I was set. I'd always been very impressed by the capacity of partition chromatography, one way and another, to separate materials. And, of course, Martin had made his early reputation in that sort of area, and I got familiar with the plate concept, of separation on columns, in partition columns. And, of course, then it was a case of... to talk of reversed phase chromatography was really quite something then, you see. And silanizing kieselguhr had only just come in, where, in fact, you made the surface of the kieselguhr able to accept, by making it glassy, really, effectively, with silane, and you then prepared your kieselguhr in that way, so that it would accept the organic phase of your biphasic mixture, of say butanol and acetic acid and water.

BA I know. It's amazing that anything survived in those solvents

SP Yes, that's right, isn't it. But they did. And so that I then set to to investigate the properties. What you did, on a small scale, you got the separation, you got the partition coefficient for the material. I could then dry down the phases and work out the partition coefficient, using bioassay to do it.

BA But did you then know it was a short peptide?

SP Not really. I didn't know what it was. I thought it was, because of the steps I'd used previously, I knew it had to be something like a peptide, and I thought it would be a peptide because renin acting on a substrate is likely to produce a peptide, because it was known that it was an enzyme, therefore it was likely to be that. But, of course, the separation processes, and how you could get out material at the end, with a good recovery and an improvement in the specific activity of the material off the bottom end of the column... again, one has to say, 'Well, you set up a column and you just collect the eluant.' Now, of course, these columns, particularly in the size you had to use them, quite large, and you then became very familiar with what were then the current problems of fraction collectors. Fraction collectors! Because they worked on a siphon principle, principally, then. Now, the one thing you could guarantee was that if you left them, the things would continue...

BA Overnight, you mean?

SP Overnight, in particular. If you had any precious material, you had to stay overnight. You could not... I stayed overnight many times, to make sure that I could watch that damned siphon, because otherwise, one drop on, one drop off, was the characteristic of a siphon design, and nobody ever seemed to improve that, until better fraction collectors came along. But that was some time ahead.

BA And they were homemade, of course, in those days, at Mill Hill.

SP Oh yes. There was the Mill Hill, there was the Mill Hill Fraction collector, but the siphon was still just as difficult for materials, because it was all to do with the surface tension and the design of that siphon. And many a time, precious materials running over the floor. Unfortunate! But then...

BA So how long did it actually take you to really get a much purer preparation?

SP Well, the first year was very hard work, because I investigated numerous steps. Now, in came Archie Martin at this stage. He'd become very interested in electrophoretic separation, which had just come in, in a way. Electrophoresis and its applications wasn't, at that time, very current in biochemistry, but...

BA Gels, or...

SP The gels were, of course. And, of course, I did use starch gel subsequently. But this was, in fact, the continuous electrophoresis, where you ran down a sheet of paper, the fluid phase, watery phase, the electrolyte containing phase, and at one corner, you would put in your solution. You'd got your cathode and anode at either ends of the sheet, and as the material ran across the paper, with the down-flow of the electrolyte, it would separate out into different channels, and you would collect it, in theory, at the bottom. Now, he became very enamoured of that. And he said, 'You ought to give up using partition columns, because you're not doing well enough with them. And you ought to do this.' And I said, 'Yes,' without wanting to be deflected, because I knew I could easily handle the partition columns, relatively easily, anyway. But this was a new venture which I wasn't confident would actually work. And I did try it, half-heartedly.

BA And did it work?

SP Well, I couldn't make it work.

BA You couldn't? It was too complicated?

SP I couldn't make it work! And he got very irritated with me, actually, you know. He really did! He sort of came very close to losing his temper with me. And I said, 'Sorry, but I must just go down this line. I can make it work.' And it was getting better.

BA And then he left you alone, did he?

SP He left me alone. He just left me alone, you know. He gave me up as a bad job, really. And, fortunately, Rodney didn't, actually, because he could see the sort of... because he was having real difficulties with his own separation techniques! And so we had something in common at that stage. But I did persevere with it. I had a low patch after that first year, but it got better. And the turning point was, I'd got the next phase, reverse phase, the first lot, the appropriate methods of doing it on a large scale, so I ended up with a decent amount of material. And then I ended up with an adsorption step, that sort of thing.

BA I see. You were very good at adsorption steps?

SP I liked it, yes.

BA You liked it?

SP I liked it, and it worked. And then I got to the stage, in the second year, when things were really going very well, because I was on top of the problem and still making these very large bulk preparations, of course, still at Mill Hill, in just the same way, and carrying it through. And eventually, I ended up with a material which I thought was reasonably pure, actually, and I separated that on...

BA Why did you think that? Because of all the separating?

SP Because I... no, no, I did electrophoresis, paper electrophoresis. I did try starch gel electrophoresis. That's again, another thing, incidentally, you made your own starch, actually.

BA Oh absolutely, I remember that, too.

SP You could almost eat it, you know! But it was good. And I enjoyed that – in much the same way as you made your own DEAE [diethylaminoethyl], as you remember, you know, there was no commercial preparation of DEAE cellulose, at all. You just made it and separated the fines off in great decanting bowls and things. It was tremendous. But that electrophoretic separation enabled me to select out... I used a different pH ...

BA And how did you detect it on the paper, then?

SP Oh well, bioassay. Always bioassay.

BA Always bio... you had to elute every strip?

SP Well, what you did is, you just ran the strips...

BA You knew roughly where it was?

SP You used ninhydrin and you picked out, and you saw that there was only one spot. Now, on paper electrophoresis, that's not very high-grade purity, in theory, but, as it happened, there really was only one major spot there and that was where the activity was. And on hydrolysis of that – acid hydrolysis – the amino acid display came out. And, in fact, I used that...

BA Did you do that?

SP Yes, I did. Yes. And, of course...

BA How much material did you need, then?

SP Well, it... the total would only be about a milligram, you see, after all this...

BA And gallons and gallons of plasma?

SP Oh yes, that's all I would have, actually. Yes, that's right. That's all I did have, really. The first good preparation was only about that amount, and hydrolysing that with care. And then I got the amino acid array. Now, what I found, because until that

time... Moore and Stein,⁷ incidentally, had visited the Institute of ... you know, some time in that period, they came, basically, to see Albert Neuberger,⁸ actually, and Tom [Thomas Spence] Work.

BA I see. And Tommy Work.

SP And they were so enthused, of course, about ion exchange resins, which they'd, so to speak, invented, and at that stage they weren't, certainly, as reliable as they subsequently became. And while I did try them out, I could never make them work as well as my good old-fashioned methods of partition chromatography. But what, what we ended up with, I determined... well, we haven't got the thing in being to tell me the amino acid ratios in my peptide, my peptide, and...

BA Hypertensin?

SP Hypertensin, as it was. What I would do was to do the elution off the paper, do the ninhydrin, and run it in a Beckman spectrophotometer and get the absolute amounts, as ninhydrin conjugates. Now, a lot of people would throw up their hands at that, I'm afraid, and say, 'You can't do that properly!' But, strangely enough, when you ran your controls...

BA What are we talking about now?

SP About the controls, the amino acid controls.

BA Hydrolysed? Individual amino acids?

SP Yes, individual amino acids, and eluted those, and got the... what the closest you could get to the ratios of 1, 2 or 3, or whatever it had to be. You could actually get surprising – what was to me, very surprising – accuracy, about it.

BA In the molar ratios?

SP Yes, in the molar ratios. You see, the molar ratios came out of simple paper chromatography. And that, actually, I think, did please Martin, actually, after all this time!

BA So you became friends again?

SP So that he... he thought, well, perhaps it wasn't too bad after all! But... so that I then was able to say, 'Well, that is... those are the molar ratios.' And, interestingly enough, you see, I had tyrosine in there, and I was just hydrolysing these... the peptide, in the little glass tubes with hydrochloric acid, which often used to explode, if you're not careful! If you let any air in, then you were in trouble. So you had to learn how to fill them decently. And then I... at the same time, in Neuberger's Department, Jacobs

⁷ Stanford Moore (1913-1982) and William H Stein (1911-1980). American biochemists. They were awarded the Nobel Prize in chemistry for their contribution to the understanding of the connection between chemical structure and catalytic activity of the active centre of the ribonuclease molecule. The Prize was shared with Christian Anfinsen.

⁸ Albert Neuberger (1908-1996).

was busy with the first – the first in Mill Hill, certainly – of the ion exchange columns to separate amino acids and to quantitate them.

BA It was the Moore and Stein column.

SP Yes, that's right.

BA And it was very early, actually.

SP It was, yes. Well, it was good...

BA 1953, probably.

SP ...except for me. Because I gave him, you know, I'd done this on paper, and I thought, 'No, we must have the real official backing for this.' So I gave him all my precious material to put on - the hydrolysate. I hydrolysed it, and handed him the hydrolysate to put on the column. And they came down, and every amino acid came down in the same molar ratio as I'd obtained off paper, except the tyrosine had disappeared. And I was... I was really so downcast. I mean, I... you know, I almost accused him of stealing tyrosine. It was difficult. And to this day, I do not know where that went, except that I did not put any material down a Moore and Stein column in Mill Hill again, in actual fact. I stuck to what I'd found.

BA And you considered it pure?

SP Yes, I did.

BA Because of the molar ratio?

SP Because of the molar ratio.

BA And then what happened after that?

SP Well, at that stage there was another decision to be made, because, you know, I'd talked to Harington about this, and I'd been back, I used to go back to St Mary's every now and again to give, you know, to... often, one day a week, to talk to students, you see, to keep my clinical... well, it was hardly clinical, but, nevertheless, my teaching abilities in place, so that I didn't lose contact completely with the Medical Unit. Though, of course, being at Mill Hill, I was really out of commission. But then there was a decision to be made. Having got the molar ratios and the decapeptide, which we published, then I said, 'Well, now, I want to go out and get the sequence. I must get the sequence. And I want to do this,' you see.

BA Yourself?

SP I wanted to do it myself, you see, you know, a cookbook biochemist! I wanted to go up the chef's ladder, you know, a bit. And so I thought, 'Well, I'll try and persuade Harington and Pickering that I can stay on and do this.' And I talked to Harington, and he said, 'You know, the chap you really ought to work with is Don Elliott.' And I said, 'Oh'. He said, 'Because, one, he's a very good chemist, and you'll

get there quicker than if you try and do it yourself.’ And Pickering said, ‘Well, perhaps it’s about time you also came back to do a bit more in the Medical Unit, you know, we haven’t seen you for a long time!’

BA Who paid you during this time?

SP The Medical Unit.

BA The Medical Unit?

SP Yes.

BA That’s already very progressive, isn’t it.

SP Yes. Absolutely. But then George Pickering was, you know, an unusual man, you know. He just did the things that he thought were appropriate, you know. After all, that’s how I ended up with [John] Gaddum⁹ in Edinburgh, you see, but that was an MRC Fellowship. But this time it was the Medical Unit.

BA So the Medical Unit paid for...

SP My one afternoon a week with the medical students was obviously rather well paid, I suppose! But the decision then was made. I reluctantly, I have to say, reluctantly, said ‘All right. I’ll talk to Don Elliott.’ Now, fortunately, Don Elliott was an absolutely charming person. He really was. And he, again, approached me a bit like... approaching like Rod Porter had approached me in the first place. But, nevertheless, we got on terribly well. And the fact that he was such a nice person just made it terribly easy.

BA And, of course, it was a very wise decision.

SP Oh, wasn’t it just, actually, yes. Because there were a lot of competitors around, you know, on the other side of the Atlantic, actually. Very good competitors. Yes, excellent. And they were going pretty fast. You know, that nasty sinking of the heart you get, when you go into the library and look at the latest edition. This was the... you know, *Journal of Biological Chemistry*, for example, you’d open that and your heart might sink if you saw something... you know, it looked even approaching anything you’d got.

BA Heart sinking has become more frequent in recent years, I would say.

SP Yes. A lot more competition.

BA Yes.

SP Yeah, that’s right. But, nevertheless, we... Don Elliott and I got on. I just made the stuff, and made it... and he was using the methods which were then current. You actually approached the peptide and... you cleaved it, using partial acid hydrolysis, and then you took the fragments, separated the fragments, and then looked

⁹ John Henry Gaddum (1900-1965).

at the N- [NH₂] terminal end, and you... of course, you ended up with derivatives which you could run separately on paper chromatography, identify them by their position. A very painstaking... when you compare what you can do now. In thirty seconds you could have had the whole lot out today. Well, it took rather longer.

BA How long did it take, actually?

SP It took another year, actually.

BA Another whole year, the sequence?

SP Yeah. Oh yes, it was... it was very exacting, because, you know, he was relying upon me to produce the pure stuff all the time, and I was... you know, having to make sure the... you know, because I was doing it single-handed, actually, so that it took some doing, and a lot of hard work, obviously. But his work was harder. And he separated those out. He allowed me to do the carboxypeptidase end, you see, to get at the C- [COOH] terminal end of the peptide, you see, so that I was going up that way, and so that we ended up, then, with all the overlaps that you get from that process, you ended up with a sequence. And we published that sequence.

BA Where was it published? In *Nature*?

SP That was published in *Nature*,¹⁰ yes.

BA A classical paper.

SP Yes. Very short. And then subsequently, the *Biochemical Journal*, really rather a fuller version. But that was, that was it. And we actually, you know... as if these things really matter, well, they do matter to those working on it, that, you know, you get first past the post. But, you know, as you look back, you recognise a lot of people are working along similar lines. But that was the...

BA It must have been exciting.

SP Oh, it was. It was tremendously exciting. One of the most pleasurable things about the excitement was presenting it at Mill Hill, actually, in a lecture... you know, one of those seminar presentations in the big hall at Mill Hill. You remember? That was really very exciting.

BA Colloquium.

SP Yes. That was right. It was very...

BA It must have been wonderful for you.

SP It was. It was. No, it's something you never forget, you know, because it's rather unexpected, you see, and...

BA Not really.

¹⁰ Elliott, D.F., and Peart, W.S., 1956. Amino-acid sequence in a hypertensin. *Nature*, 177:527-528.

SP Well, anyway, that was it, and that was good. And so that was... when I bade farewell to Mill Hill...

BA But before you bid farewell, perhaps we could talk a bit about some of the...

SP The people?

BA The distinguished and wonderful people that inhabited Mill Hill in the early fifties.

SP Oh yes. I'd love to do that.

BA I don't know, I mean, you must have collaborated with quite a number to start with.

SP Oh yes. Well, it was very difficult not to, you see. The... you know, if you want an excuse for an institution, about which there are large arguments these days, about institutes of this, that and the other. Mill Hill, at that time, was really a fermenting place, because there were so many brilliant people there, willing to exchange their views and information, particularly at lunchtime, and whoever you sat next to, practically, seemed to me, to be wanting to talk about what you were doing, about what they were doing and you exchanged ideas all the time. The only thing you had to be careful of, of course, was if Sir Chas sat next to you at lunch! Because one of the things that... Harington, underneath, who was a tremendously shy person...

BA But a very wonderful person.

SP He really was, you see. You had to... he was one of those people who, as they say, you have to know them to appreciate them, because he was so shy that he couldn't really easily enter into a conversation, particularly about trivia. So, now, if he sat next to you at lunch, you would start off by, you know, opening gambit of some conversational aside, you know, about what you were doing, and you wouldn't necessarily get an answer back, actually! So you tried again and then you tried again, and you eventually had to give up. And so your lunch would proceed... hopefully, there would be somebody on the other side that you could talk to, because Sir Chas just couldn't get out of what seemed that cage. He could, on occasions. Of course, the other thing too is, occasionally, that people had to be careful about, was being on... he drove, I think he drove a Jowett Javelin – I seem to remember that. That was a very racy sort of car. Jowett's was a Yorkshire firm that went out of production. But he had this car, which was really... seemed out of character with Sir Chas, but people were always terrified about being offered a lift with him because he wasn't one of the world's great drivers. But he was... he was a man... interestingly, you see, he couldn't understand Archie Martin either, and nor could Archie Martin understand Sir Chas, you see.

BA Yes. That was a pity.

SP It was a pity, because Archie Martin was, I would have to say, at that time, was the only person I've ever met, and still, as I reflect on it, that you could say, 'Well, that

man was a real genius,' because he was thinking of things which nobody had ever just thought of. He was thinking, at that time... just give you an example. He was trying to design a machine which could feel round the shape of a molecule and tell you precisely what its configuration was. Now, you know... I remember him discussing this, you see, and he would have thoughts like that. And, you know, you always sort of believed that he could bring it about, because it was another time there when he was at the start of high pressure liquid chromatography. And I used to see him; there was a workshop further down the corridor, on floor 3, and he used to be there with his oxy-acetylene torch and his helmet and mask, and he used to be there, welding the copper tubing together, which was going to provide the support for his high pressure liquid chromatography. And that, when you think about it, that was the start of... you know, when you think of transfer foresight, here was a man making the way ahead for oil companies all over the world, because without those processes which he worked at, with Tony James, you know, well... and he was quite unique at that time in the world. I mean, there was nobody else doing that particular sort of approach. He had thought of it and he just brought it about, like everything else he did. He was a man with a real magic touch, actually.

BA A genius, as you said, because they were totally novel ideals, and you...

SP And people would go to him, wouldn't they, and discuss their problems.

BA Oh, many of us went to him. Absolutely.

SP And he'd come up with an idea which you just wouldn't have thought of, you know. I mean, he really was a genius.

BA And all this between reading detective stories in his lab.

SP Yes! That's right. But he used to sit out on the... there was a sort of terrace at the back.

BA Yes, because I inherited his lab...

SP And he used to sit there smoking. That was the thing that Charles Harington, you see, wouldn't care for too much, you see, because with Chas, you know, it was every minute of every day had to be occupied in a very firmly constructed way, whereas a real imaginative dreamer, as Martin was, worked in a completely different way. So it's not surprising they couldn't understand... But a pity that there wasn't enough give and take on either side.

BA A great pity because then eventually, Archie Martin left.

SP Yes.

BA After his Nobel Prize.

SP Yes. And he, he lived, obviously, a rather unhappy life after that, actually, I think, going from one unsatisfactory place...

BA Industry.

SP After he went to the States, and, again, they would never understand him in the States! Can you think of anywhere in the States that would understand somebody like Archie Martin! He might have worked together with Linus Pauling, you know, that would have been a good combination, I would have thought. But, other than that, no. And then you had, you see, you had got Rodney [Porter]. Again, the pragmatic scientist who knew... he had green fingers, as a scientist. He knew how to do things. He just sat and thought, but he was thinking all the time about, 'How can I do... how can I make this work?' And he always arrived at a solution which seemed to be the right one. And, of course, it's no surprise that he got gamma globulin out, finally, and got his Nobel Prize,¹¹ just as Archie, but in a different area. But a very, again, a pragmatic discovery with Syngge of paper chromatography, you see.¹² I mean, wonderful. What a good fortune it was for me, to even just brush shoulders with them, actually.

BA For two years, yes.

SP For two years. No, that was a tremendous experience.

BA Did you have any contact with John Humphrey?

SP Yes, I did. Again, in many ways, he was very like Archie Martin. He was, however, much more down to earth, and yet, he again, was an enormous source of ideas. As an immunologist, and as an everything else *ologist*, you see, because he, he really had a breadth of knowledge which was unsurpassed - tremendous. But he was... he'd sit there, I remember, puffing this pipe, you know, and he was a little hard of hearing even then, actually, and he became completely deaf ultimately. But he would puff at it for a while, and then give you a reflective answer to... and suggest, 'Had you thought of doing this?' He was always, 'Had you thought of doing it that way?'

BA In a more biological sense, of course?

SP Yes. That's right. He...

BA Very different from Archie!

SP His good medical background!

BA Always useful!

SP It's always useful! But he was... he, again, was an important mainspring in the Institute, I would have said. There were, of course, a lot of other figures there. My first meeting with Albert Neuberger, of course, was there. He subsequently joined me at St Mary's, because he became professor of chemical pathology. He was running the Biochemistry Department at Mill Hill, and his department, of course, was very helpful.

¹¹ Rodney Porter shared the Nobel Prize in 1972 for physiology or medicine with Gerald M Edelman for their work in determining the structure of an antibody.

¹² Archer Martin shared the Nobel Prize for chemistry in 1952 with Richard Syngge for their invention of partition chromatography.

Helen Muir was there, of course, who subsequently went to the Kennedy Institute. She actually worked in my Department at St Mary's for two or three years.

BA That's right. In between, before the Kennedy, I remember.

SP Yes, you know, in between, because they didn't have enough space in Albert Neuberger's Department and so she came to work with me, and she was working on things which interested me a lot, actually, like the aortic wall, the blood vessel, the main blood vessel wall, when she was doing her early work on proteoglycans, so that there were those in that department. Feldberg¹³ was in Pharmacology, and ... I was lucky enough to know Feldberg rather better, subsequently, and what a tremendous man. All those that had been touched by Henry Dale,¹⁴ were all out of the top drawer scientists. They really were.

BA I mean, in that era, everybody was doing their own work. We didn't have these large groups. And I think that had a great influence on people's interests and interactions.

SP Oh yes. The...

BA At lunchtime, and at other times.

SP Yes, I think you're right, actually. What I have come to term the *grande armée* approach to research, which has become rather more prevalent here, as in the States, it just wasn't in existence. I mean, if there were... yes, when you think about it, two or three people working on one problem was about it, at the most.

BA That's right. And I think that made a big difference, so people knew each other very well in those days.

SP Of course, they were all... they were all practically first-class, you know! That does make a big difference too, actually, you know! And they were so good. I mean, there was Parkes¹⁵ there, you see, who was a pioneer endocrinologist. And ...

BA And Audrey Smith.

SP Audrey Smith. Yeah, I must tell you that story which I think I may have told you before. But Audrey Smith, he... there were two comments, and I hope this tape is destroyed some time before it becomes a matter for the law courts. One of the stories – Audrey Smith was interested in cryobiology and was... she was on an expedition to the Antarctic, and she was the only female on this expedition. And somebody said to Parkes, 'Oh, that's a bit unusual, isn't it, Audrey going with all these men to the Antarctic.' And he, being a sort of rather taciturn Northerner, actually, said, 'Well, at least she'll have scarcity value!' And the other one is – you see, because in some ways, dear Audrey was a bit naïve, actually – she was heard at lunchtime, to lean over the table and say, 'I say, Joe, do you think you could let me have some of your sperm after lunch?' And I mean, it's ridiculous, but she did!

¹³ Wilhelm Siegmund Feldberg (1900-1993).

¹⁴ Sir Henry Hallett Dale (1875-1968).

¹⁵ Sir Alan Sterling Parkes (1900-1990)

BA She did, in a loud voice!

SP Yes, in a loud voice, actually, you know!

BA And, of course, she was the first to freeze sperm, with Lovelock,¹⁶ at Mill Hill.

SP Yes, that's right. Because that was, that was an accidental discovery, wasn't it?

BA Yes.

SP Wasn't that the glycerine in the...

BA Yeah, well, then Cornforth¹⁷ came in.

SP He did. Because that was the glycerine left in with the cells, and, much to their surprise, they could freeze them and thaw them, and lo and behold...

BA But they tried to freeze cells.

SP They did?

BA But they, they tried many bottles. But then eventually, one worked. But, unfortunately, the label had dropped off, they didn't know what it was. And so that's when Cornforth came in. They took it to Cornforth, the top chemist, asked him to define the substance. And...

SP What did he say about that?

BA Instead of trying to get it on the carbons and everything, he tasted a bit, put it on his finger, tasted it, and said, 'It's glycerol!'

SP Did he really?

BA Yes, that's true.

SP Well, of course, very few people, of course, know that the Cornforths – well, biochemists know – but Cornforth was congenitally deaf, and his wife always used to interpret for him, actually. But they made a wonderful team. But there they were, I mean, and he was a tremendous chemist, actually.

BA Tremendous. Outstanding. Also a Nobel Prizewinner.¹⁸

SP And also another Nobel Prizewinner, you see. But to do it that way, that's a mark of genius, isn't it.

¹⁶ James Ephraim Lovelock (1919-)

¹⁷ Sir John Warcup Cornforth (1917-) Australian chemist.

¹⁸ Cornforth shared the Nobel Prize for chemistry in 1975 with Vladimir Prelog for his work on the stereochemistry of enzyme-catalysed reactions.

BA You did enjoy Mill Hill?

SP Oh, Mill Hill was... oh, it was obviously one of the real highlights of my life, actually. But it couldn't fail to be, because there was more to Mill Hill just than the absolute pleasure of working with first-class scientists, because they also could play quite well. There were tennis courts at the back of Mill Hill and I always remember, Rod and I used to pair off with one another to play, every now and again, at lunchtime. It was rather nice, actually. He was a bit ungainly, actually.

BA A bit like Jacques Tati?

SP Yes, a bit like that, actually! But he... but then again, he was a great mountaineer, of course, too, you know. He really could climb, actually. One of the... biochemists, actually, do tend to mountaineering quite a lot, don't they, when you think about it. There have been quite a lot of good mountaineering, good scientist mountaineers. Science and mountaineering, seem to me, to have gone together quite a lot. Perhaps not too many geologists, actually, as mountaineers, but other scientific disciplines well represented, actually.

BA Yes. Yes. And artistic ones.

SP Yes. But Rod was a very good climber. He used to tire Julia, his wife, out, a great deal, on walking expeditions. Yes, that's right.

BA So then what happened? You left Mill Hill?

SP Yes. Well, yes. What a wrench. I came back to what you'd think was the real life of a busy medical school and hospital. And it was, really, quite a... quite a shock, actually. I came back and took up my position again, and I'd been promoted, I think, about that time, to be a senior lecturer in the Medical Unit and...

BA How old were you then?

SP I was about thirty-two, actually, yes.

BA And it was 1954 you went back?

SP Yes, that's right. And so that... I got back into the clinical swing of things, because, you know, I was very rusty. I mean, I was a dangerous doctor, I should have said was about the right description at that time. But I applied myself, because I like clinical medicine, you see. That's another great love, of course.

BA I mean, you could have also remained in research, so...

SP Yes, I could have done. But of course

BA So what prompted your decision?

SP The pull... the pull... when it got down to it, you know, I'd had to make that decision before, you know, because I nearly stayed in pharmacology, but the pull of clinical medicine brought me back into it. And the same thing...

BA For what reason?

SP I suppose it's like, well, you know, it's very difficult to do this self-analysis, you know. All I know is that when I woke up in the morning, so to speak, and said, 'Well, shall I stay just doing research of this sort, or shall I go back?' I thought, you see, in my ignorance, that I could do both, you know. Those were times, you've got to recognise, when you say you're singleton workers, you see... the idea then, you could do research, and combine it with a clinical career, was not all that strange, you see. It was a sort of way of life that a lot of... I mean, Pickering had tried very hard, and succeeded to do that, you see. [Thomas] Lewis, before, his mentor, had done it. So there was a long tradition going back all the time, you see, that you could do it, you see. After all, Lewis's predecessor, T R Elliott, at University College, he'd, in 1902 it was, that suggested that adrenaline was released from sympathetic nerve endings, you see - 1902. Now, it was after the First World War, you see, in 1920, that he became professor of medicine at University College. So the tradition that you could combine somehow research and clinical practice was still around, you know. And that was the way you felt you could go. That was what academic medicine meant, you see, at that time. So that I thought I could do it. I felt, yes. And I liked dealing with patients, I really did, you know, that was what I'd, you know... in a way, that's what I'd come into medicine for, initially, only I'd been attracted by research, you see, and, therefore ... you know, I didn't want to let either of them go, you see. So that was what happened. So I came back and did that. And I was then, you see, at that time, George Pickering had been approached to go to Oxford, and he said, well, would I go with him? And I said, 'Yes'. I didn't actually want to go to Oxford, you know, particularly. I was set on wanting, you know... I really ...

BA To be at St Mary's?

SP Maybe I wanted to get out from under George, I suppose, is the real truth of the matter. And I had thought, 'Well, yeah, all right, I'll go though.' And I set to. I designed the laboratories I was going to take part in, I'd even, you know, arranged for the... the supports for the columns that I was going to run in the laboratory, on the walls, you know, they were there. All the fraction collectors, you know, which had improved by this time, quite a lot, and everything was set for me to go ahead and explore renin and angiotensin, as it then became ... fully in Oxford. But a little while later, when George had... his job had been advertised, then... I wasn't thinking, you know, about staying, in those terms, at all. It wouldn't have crossed my mind, you know, seriously. I just felt, 'Well, I'm just a young chap.' And then the people at Mary's approached me and said, 'Had you thought of staying, and taking over?'

BA Really? You were very young, presumably?

SP Very, yes. I was thirty-three then. So, you know, I wouldn't necessarily recommend it. But these days, of course, fortunately... fortunately, because one of my subsequent crusades is to support the young at an even earlier age, I mean, they've got the energy to do things.

BA Earlier than thirty-three?

SP Well, I believe, you know, most... I think, what they need is the chance of doing things in research, between the ages of twenty and thirty-five, because those are really the time when you've got the physical and the mental energy to do all these things. You haven't got the experience, but you've got the new ideas there, and you've got the strength to keep going twenty-six hours a day if necessary! And that's, you know, when you look at... when you look at what young people can achieve, I'm...

BA Some people seem to carry on a bit longer, shall we say?

SP Oh no! It doesn't mean you can't carry on, actually! But what it does mean is, you must, I'm sure you had an opportunity when you were young to just do it.

BA Yes. Independence, really, is the thing.

SP Absolutely.

BA To work independently, I agree.

SP And to actually prove yourself to yourself, because, you know, out of all this, the message that comes is, if you can overcome the difficulties, and however down you feel about, you know, the way your work's going, if you can survive that and come out and win, so to speak, then you've learned a lot about yourself, and you know...

BA Yes, I fully agree.

SP And that's what's absolutely essential for people to undertake at an early age.

BA For young people, yes.

SP Because you can't leave that until you're thirty-five, forty-five, to find out. I mean, it's too late.

BA And yet the tendency goes the other way now, doesn't it, for independence.

SP Particularly in medicine. I mean, you know, because you've got to go over all these clinical hurdles, you know, of qualifying to do this, that, and the other. I mean, a different route is necessary for people, in my opinion. But, you know, the professional bodies don't want to recognise that too readily. But, there it is. At any rate, I went, I went back and, you know, to cut a long story short, really, when the interviews came up for this, because I did throw my hat in the ring and...

BA Who interviewed you, I wonder?

SP Well, I'll tell you. What was interesting was, before the interview, because... the senior lecturer in the department was also a very nice man called Peter Sanderson, he was in the competition, but there were three others who were very interesting to me.

There was Malcolm Milne,¹⁹ who became the professor of medicine at the Westminster [Hospital Medical School]. Douglas Black, who is an absolutely charming man, actually; he became the professor of medicine in Manchester, and also the President of the College of Physicians. And Hugh de Wardener who went to Thomas' [Hospital Medical School] as a professor of medicine. Now, there they were, they'd served in the war, and here I was... there they were, chatting gaily to one another, because they knew each other terribly well, and they were far more experienced than I was. I mean, you know, they... and they'd got good research behind them, you see, all of them. And, you know, because... Douglas Black worked with [Robert] McCance and [Elsie] Widdowson, you know, on sodium deprivation, and he was... And so I thought, 'Well, dear me! They're bound to appoint one of these,' you know. And so I went to the interview, and I can hardly remember who interviewed me, actually. I can't even remember who the vice-chancellor was, who was part of the interviewing panel at that time, you know. And I, I can hardly, you know, remember the answers I gave to questions. It just was a blur. So I went back to Mary's, and did a ward round. And in the middle of the ward round, in came the professor of surgery, Charles Rob, who was, who subsequently went to the States, to Buffalo, as head of the Department of Surgery there. And usually, I disagreed with his... he was always one of these 110 per cent sorts of chaps, actually! But he'd served in the Parachute Regiment in the war, you know, he was a tremendous lean chap, and a very affable, enthusiastic chap. He came up to me, and put his hand out and said, 'You're now my colleague!' And that was it! And I was... so then I had to... thinking, 'Well, what am I going to do?'

BA Wonderful.

SP So that was it.

BA You were appointed professor of medicine at Mary's in 1956. And so your duties increased enormously, I should think. You were also in charge of the Medical Unit, and I wonder whether we could talk about the Medical Unit, which Pickering...

SP Created.

BA Created, actually.

SP Yes, he did. Yes, it's... at the same time, it might be worth a diversion to the history of – briefly – of Medical Units in Britain, because they were a concept which had been slow to catch on, because they were the... meant to be the start of academic medicine, in a university sense. Just to remind you, it was in 1913 that there was a Haldane Commission that had suggested that the correct way for medicine to go was to go partly down the university line and to try and create a milieu in which academic pursuits in medicine, involving science and its application in medicine, should really be pursued.

BA In 1913?

SP 1913. And, of course, after that, the war got in the way. But it's interesting that, in 1916, in the United States, that was before they entered the First World War, [Abraham] Flexner had produced a report which has got more prominence in the world

¹⁹ Malcolm Milne (1915-1991).

really, because it seemed to have a bigger influence than the Haldane Commission, though they arrive at much the same conclusions, saying that, for American medicine, and perhaps for all medicine, that the influence of science was to be encouraged and should be really brought in. Now, that message did, actually, strike a chord in Britain. And the University of London, for example, because there were very few other... there were the regius chairs at Cambridge and in Oxford, but very little else. I mean, Edinburgh, also, of course, had its chair. But very little else. And so in London, the University of London started on this trail and decided it would start off three units, of which the first, and, perhaps, from our point of view, the most important, was University College.

BA When was that?

SP That was in 1921. And the first holder of that chair at University College was T R Elliott.²⁰ Just to remind you, T R Elliott was the same T R Elliott who was working in Langley's²¹ Department of Physiology in Cambridge, and who had made the tremendous suggestion that adrenaline might be liberated from sympathetic nerve endings. And, of course, he wasn't believed for years after that. Even Henry Dale wasn't quite sure. But, of course, it took another twenty or thirty years before people did actually believe it. And, of course, that was where some of my research ended up, anyway, when I was with Gaddum, so that I'd got a keen interest in T R Elliott. But then, and, of course, Tom Lewis subsequently came to University College, and established real clinical science, much more than Elliott had, incidentally. He was the foundation of clinical science and the Medical Research Society, which became a very important meeting ground for clinical scientists, and it was started, actually. And, of course, it's not surprising that Pickering had joined Lewis, and that was the start of that particular line of clinical scientist.

BA Funded by the University?

SP Yes. That's right. And with, with a certain number of staff. But, if you remember, in those days, what is now called 'soft money' for support was almost non-existent, I mean, so people had to really work very hard. Now, the Medical Unit I joined, came into being because of Moran. Now, he started off as Dr Charles Wilson.²²

BA How do you mean?

SP He started off as a neurologist, Dr Charles Wilson, who became dean of St Mary's Hospital after the First World War. He rose through Sir Charles Wilson to Lord Moran. And he, of course, took the medical profession, with the aid of Aneurin Bevan²³ – putting it that way round – into the National Health Service in 1948. He coined these phrases like, you know: 'You fall off the ladder if you go into general practice,' you know, that sort of... that did not endear him to a lot of doctors! Nevertheless, he was the dean, and he, in actual fact, contrived to save St Mary's on many occasions. Being a friend of Beaverbrook,²⁴ the present library at St Mary's is

²⁰ Thomas Renton Elliott (1877-1961).

²¹ John Newport Langley (1852-1925).

²² Charles McMoran Wilson, Lord Moran (1882-1977).

²³ Aneurin Bevan (1897-1960) Minister in 1945-1951 Labour Government.

²⁴ William Maxwell Aitken, Lord Beaverbrook (1879-1964).

due to the generosity of Beaverbrook, so is the swimming pool underneath it, actually. So that he had a lot of friends. And he also led St Mary's into a profitable way of business, by encouraging sport. He really...

BA Oh, he was the one, then.

SP Oh, very much.

BA Is that the reason that you were permitted to study at St Mary's?

SP Oh well, yes, I mean it... at the interview I had, from my school, of course, he did, he actually did ask me how fast I ran the hundred yards, actually! Because I was playing wing three-quarter at rugby, you see, and it was rather important to run it pretty fast, which I, of course, did, by taking a few fractions of a second off my real time! But when you get to 1938, when Pickering came, that was because Moran, who'd got this background of knowing that it was good to have academic medicine - from his background, you wouldn't have thought he would have done, but he did. And he insisted that Mary's did have the start of a Medical Unit after the war. And with these three university chairs, he then added directors, because they wouldn't give him a chair.

BA Oh, really?

SP No, no, they wouldn't let him have chairs at that time. So he said, 'I'm going to go it alone.' So he created these directors of the unit, and then the University relented, and he could actually then appoint professors. So Mary's was early in the game of having a professor of medicine, and of surgery.

BA I see. There were no professors before then?

SP No. Not in...

BA In the Medical School ...

SP Well, what happened... no, not at all. It wasn't... the tradition of the London teaching hospital was run by consultants in the National Health Service. They were called voluntary. Why voluntary hospitals? Because they relied upon public subscription. There was no money coming in from government to support them, you see, so that the services of the consultants were given for free, you see, before the National Health Service in 1948, you see. So that was how it was. So he was far-sighted. So he created that whole atmosphere. And so it's no surprise, in a way, that he picked on George Pickering as the man to really start academic medicine. The first professor, who was extant until '38, was Langmead.²⁵ In fact, I've got his Windsor chair! I don't know how I came about to get it, actually, because I was in the ante-room of the gents' loo at Mary's and I noticed this very nice Windsor chair sitting there. And I thought, 'Well, what's that doing there?' So I looked at it and I turned it over and saw the name Langmead written on the underside. So I thought, 'Well, I'm having that!' And I've still got it. And I took it home with me, ultimately. But I had it in my office at Mary's for a long time. But that was Langmead. But he wasn't the person... he was a good physician, a good paediatrician, interested in diabetes. In fact,

²⁵ Frederick Samuel Langmead (1878-1969).

the laboratory I went into, which is in the first floor of the Hospital, not in any Medical School building, but in the Hospital, as a Medical Unit, was really devoted to measuring blood sugars. And there was even an old water-driven, a water-driven centrifuge in the corner. It was...

BA (Inaudible).

SP Yes, but water-driven. It was remarkable. And it was really a matter of some consequence when we got a really up-to-date centrifuge, you know, which worked by electricity! I'm not joking either, you know. That was how it was. But the old benches were there, just as they were... always been, and that's what we used, actually, and that's where I came to rest, initially, at St Mary's, in that Medical Unit. But the... when I was appointed in 1956, only half the medical schools had a professor of medicine. I mean, all the other disciplines were... oh, much less well represented. Professors of surgery were a tremendous rarity, and professors of obstetrics and gynaecology were... oh, hardly one about! But that was 1956, you see, so you know, the time... my lifetime in research and clinical work, you see, you've seen rather a transformation. But, then again, when you had a Medical Unit, there it was set, alongside the consultants who had taught me, actually, and who were there, and they set the standard. They ran the Hospital and the Medical School. The Medical School was there, much more like an appendage, you see, and it wasn't... it wasn't a collaborative venture.

BA It wasn't integrated?

SP Oh no. The deans, actually, practically speaking, were always chosen from the consultant ranks. And so you had a part-time dean; there was the secretary to the Medical School and the treasurer, and that was about it. But academic pursuits, it was, again, surprising that there were a number of extremely able scientists among that group.

BA So, when Pickering started it, presumably he picked people that...

SP He did. But, of course...

BA ...that he thought might do some academic work too?

SP Yes, that's right. In fact, the senior lecturer who was there then, Peter Sanderson, was... he was an excellent clinical scientist, and, in fact, [John Fletcher] Ackroyd, who was also there, had come from Bristol. He'd started life as a pathologist and become interested in immunology. It perhaps will interest you, he first described his basis of Sedormid purpura, which was the linking... it was a bit reminiscent of Alec(?), but the linking of the chemical to an antibody, which then linked with the platelet, to destroy it. And he did some very elegant experiments, actually, which were important, actually, at that time. But I had to set to in this sort of background, where you really had to say, 'Well, what am I going to do with a Medical Unit? One, I've got to show that, clinically, we're better than the rest, if we possibly can be; that we've got... you know, we have to try and set the standards in every way,' and all the people there had to do it. So you had to do the clinical at a high level, you see.

BA And choose the outstanding people who would be willing to do both.

SP That's right. And I was very lucky in the fact that I was able to attract two people, initially: Bob [James] Robertson and Jehoiada Brown. Jehoiada, there's a reason I pause, because the name, it's a Welsh name, became corrupted to Joyda, but it's Jehoida Brown. And those two were absolutely superb people.

BA What did they... they worked on your interests?

SP They worked on my interests at that time.

BA So what was...

SP But they were working on high blood pressure, and we were...

BA So that interest remained with you.

SP Yes. And we were, of course, at that time, working, by this time not on hypertensin, angiotensin, the peptide. We were, of course, working on it in the sense we were finding out how it worked, by infusing it, because Ciba, of course, had come into the picture, because they were the first people that produced on a large scale the peptide.

BA Oh, they did. They synthesised it.

SP They'd synthesised it. Yeah, they'd synthesised it.

BA It must have been one of the first synthetic peptides.

SP Yes. Indeed. And they supplied it to us, and we...

BA You put it into people?

SP We put it into real people! Yes.

BA Into real people?

SP Yeah, that's right.

BA And what happened?

SP Well, it pushed up their blood pressure! Had an effect on their kidney performance, and we measured its effect on cardiac performance and respiratory... everywhere you could look, we looked to see what it was doing, you see. At that time, you see, we thought, quite simply, this stuff comes out, renin comes out of the kidney, as an enzyme, works on its substrate, produces this peptide, the decapeptide, which is converted to the active octapeptide. Skeggs and his colleagues in Cleveland had discovered the converting enzyme, which subsequently, you know, became the target for one of the more important hypertensive drugs, you see... the ACE converting inhibitors, the angiotensin-converting enzyme, ACE inhibitors, which, of course, are

tremendously important nowadays, therapeutically. So, at that time, we were working down those lines. I was still with my old love. I got a liking, as I say, for this cookbook chemistry, and I was pursuing the isolation of renin, because I thought, 'Well, yes, I must try and get this enzyme out, because if I could get that out, I'd have a tool which I could use rather better.

BA To produce the hypertensin?

SP Well, no ... to see what it did, how it did it, because it wasn't known how it did it.

BA What was the source material, actually ...

SP Pig.

BA ...for hypertensin?

SP Well, I used to go up with George Pickering, at one time, to Northumberland, actually, to get renin, to get rabbit kidneys. You see, at that time, you must remember, the rabbit hadn't been assaulted by that nasty virus, you know.

BA Right. So they were plentiful.

SP There's a tradition, there was a tradition in the Department that we used rabbit renin. In retrospect, I should have used pig, actually, it would have been easier to come by.

BA From the slaughterhouse.

SP But, as it was, it enabled me to go up to Northumberland. But you must remember...

BA But after the war, a lot of rabbit was eaten too.

SP Pickering came from Northumberland, you see. He went to Newcastle Grammar School initially. I was born in the north part of Durham, in South Shields, so to go back to the North was a sort of slightly natural pull! But, at any rate, Northumberland's a wonderful county, and it was wonderful for rabbits too, because they used to trap a thousand a night. A thousand rabbits a night! And gut them, send them on the train down to London and all points south, you know, where they were consumed, actually.

BA And sell the meat?

SP Oh yes. But, of course, we then did a deal whereby we would take the kidneys out of these rabbits. So we used to go there and spend a few days collecting all these kidneys, put them into Thermos flasks, big Thermos flasks, large Thermos flasks, pack them all in dry ice, and then set off by train for the south again! And, of course, I remember, one day, we went to the local pub for lunch, and, of course, guess what they served up! It was kidney pie, of course!

BA Your favourite.

SP Favourite! But, nevertheless, we did that. And I used to extract the enzyme from the kidneys, so that we were pushing down that line. I had an extremely nice Australian girl working with me for some time. I practically always, on the Unit, had Australians. Australians, Italians and Greeks have been my main overseas people. A sprinkling of people from the States.

BA And a sprinkling of Brits?

SP And Brits. But Australians have loomed large in my life, actually, in one way and another. I sort of got on very well with them, and they're very good research workers, they apply themselves very well indeed. And...

BA And was this on grant money you had to get, or was it...?

SP Oh well, they would always come...

BA With their own funding.

SP With their own funding, actually, you know. That was axiomatic, because, you know, one of the things was... while things were better at the start of the Medical Unit, from our point of view, because, you know, the old University Grants Committee consisted of a few people going around the country, of whom George Pickering was one. He'd go round the country, and they'd stop off at various universities, listen to what they were talking about, and then decide, 'Well, they'll get a cut of this university grants money of such and such.' It was really done like that.

BA Yes, that's how it was done.

SP It's quite remarkable, when you see the complexity of the formulae at which people arrive nowadays – however. So that the cut within the Medical School was determined by, really, largely by what the dean thought you were doing. And it was pretty arbitrary, but, you know, you just sort of made your play for the money, actually, and if you were lucky you got it and that enabled you to have to the lecturers. And there was no problem, you see, about the lecturers. You see, I could work on the principle, 'I want as many people in this Department as I possibly can attract from overseas, from local sources, fill it with as many eager people as possible, so that you can rotate them to the clinical duties.' You know, the same principle applies to research then as now. In other words, continuity of time is vital to research. I mean, I was lucky enough to get continuity of research, because you cannot do it on an in and out basis. You've got to be there all the time and you've got to be hands on all the time.

BA And you could still have hands on yourself?

SP Oh yes. Yes, yes. Oh, absolutely, yes.

BA Despite your big job?

SP I was doing... well, I was doing... yeah, as I say, when you're young, you've got a lot of energy. And the people that suffer... hence all these broken marriages, you know, in science and research, which people have been commenting on once more lately, haven't they, actually, that you sacrifice your family, you sacrifice your children, you sacrifice your wife or... well, whether wives sacrifice their husbands, probably so. And it's, it's one of the things that people don't... until recently, haven't talked about too much. But it's very striking as you look round your own circle of friends.

BA Because you did have to work...

SP Oh, worked pretty hard, actually.

BA Twelve hours a day.

SP Yeah. And you had to be, if you couldn't get into the lab in the day, you had to be there at night, and you had to be talking to the people doing the work and seeing what they were doing. One of the problems, as you know, with research, often is that people are running a bigger and bigger department, they get removed and the people underneath, if they're not engaged in conversation morning and night, to say what's going on, they go away. This is why some cases of deception have arisen in scientific quarters, isn't it? It's for that reason. You can see it.

BA Yes, not being close to the lab any more, and not actually knowing the methodologies of everything, because that gives you...

SP Absolutely. Yes, and if you don't see the lab notebooks, or the tracings, or whatever, you're lost. And so that that principle has followed. And I was lucky in the people I had. I really was, you know. And they worked and worked. And we produced lots of interesting things, in various directions, because my interests sort of widened rather, at that stage.

BA In which direction?

SP Well, one of the things which had intrigued us, you know, Roy Calne²⁶ was a registrar on the Surgical Unit at that time, and this was just at the time when he was interested in renal transplantation at the beginning, and it was at that time... he was being very discouraged, I must say, by most around him. But, on the Medical Unit, in the Department of Pathology, we had Ken Porter, who became, subsequently, the world's greatest authority on renal histopathology, and James Mowbray who was there, and he was one of my lecturers in medicine, you see, and we... I remember, one day we sat down and Ken Porter and James said to me, 'Why don't we get into this business of renal transplantation? Because Roy Calne has been over in the States, and he's discovered that using azathioprine and prednisone you can get the dog's kidney transplanted to survive reasonably.'

BA This was when, about 19...?

²⁶ Sir Roy Calne. Interviewed by Dr Max Blythe for the Medical Sciences Video Archive of the Royal College of Physicians and Oxford Brookes University, 12-13 December, 1996. MSVA 152, 153 & 154.

SP That would be about 1959/60. That's about it.

BA Very early.

SP Yeah, as early as that, yeah, that's right. So I said... you know, because I was being confronted all the time by people in terminal renal failure, and at that time, you see, dialysis had just come in, but there wasn't a lot you could do about it, unless you went into it on a big scale and set up to do it well. And I'd been enamoured of a simpler approach, which was peritoneal dialysis, and I'd started in on trying to get people to survive on peritoneal dialysis. This was good for acute renal failure where you, you know, you've got a chance of recovery. And there's a lot of acute renal failure around. So if you can get them through that, you don't need the big machinery, you can get them through on peritoneal dialysis. Of course, nowadays, I mean, people are maintained for years on peritoneal dialysis, you see. But at that time, this was novel. But we set to on that and got through it and managed to make it work. And that was the key... strangely enough, we carried out all the early renal transplant work on peritoneal dialysis, actually. But it was hard work. So that we set to, and I got our surgical colleagues to do it. I also did something which is quite unthinkable these days, I persuaded my other colleagues in the hospital to let me convert two beds in... I got two wards, two beds, take away two beds, put a little ramshackle container at either end – glass and wood construction, rather like a greenhouse – and we had positive pressure ventilation. We put a fan in! And we fed the patients on tinned food, you see, thinking that was sterile, because at that time, you see... right at the start, you know, you wouldn't know, because we thought that maybe exogenous infection was the major problem, whereas endogenous was quite good enough! But we set to, and we made it work.

BA So how many of you were there, then?

SP Well, there were three of us principally, the ones I've mentioned.

BA And the young students?

SP There were the surgeons, the young surgeons, you see, and Ken Owen and [John Richard] Ian Kenyon were the first of the surgeons to take part, and that continued until the time I left, in that way. But we set to, and we published the first, really, I think, good series of cadaveric renal transplants there.

BA Yes, I knew it was at St Mary's.

SP And it was... I tell you it was another aspect of clinical life, you know, which unless you described it, you can't understand why the appeal of clinical medicine is in that sense, you know, because you're having to persuade relatives and it's the same now, exactly the same, that they should contemplate giving... giving the kidneys for somebody else. And now, of course, it's a whole range of organs that have to be given. It's very hard, but it brings you face to face with realities, and they've not changed today, either, because the same, the same barriers exist, and they always will, of course, very properly, actually. But that was a... that was one direction.

BA That was one direction? Plus all your research?

SP Yes.

BA With the young people.

SP That's right.

BA And the students, presumably?

SP Yes. Students to be taught.

BA How did you do it?

SP Well, as I say, when you're young, you have a lot of energy. And also, if you can recruit people who are dedicated...

BA Who are good.

SP ...and they were dedicated, just dedicated. They worked, you know, they were always there, working harder than I was. They were just terrific.

BA Do you think it's still possible to do all these angles, research, frontline research?

SP No. No, no. Research has changed. In the... just the years before I retired in '87, I'd started to advise people: 'You can't do both. You have to make a choice.' And now, if you want to do research and be competitive on the international scale, you must devote yourself completely to it. You may do some specialised clinical sessions, but you can't do more than that, even with a big team.

BA Well, maybe this is the point where we stop this particular interview. Thank you.