

The Royal College of Physicians and Oxford Brookes University

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**Sir Christopher Booth in interview with Sir Gordon Wolstenholme
Oxford, 10 February 1988**

GW Chris, perhaps we might start by talking a little bit about your own personal background. I know you were at school at Sedbergh from 1937 to 1942 or so, and then because of the war had to go - I suppose, in any case, you would have chosen to go - into the Navy, so that by the time you came to medicine, there was quite a gap. Had you always intended to go for medicine, when you were at school?

CB No, not at all. I did languages at school and had the good fortune to be taught by J H Bruce Lockhart, who was the brother of Sir Robert, and he spoke beautiful French, and he taught me to speak French. And I learnt German too, and a bit of history. But I was on the arts side, so I didn't know what valency was. I didn't have an idea about anything to do with medicine at all. And I went away to the war in 1942, and I survived it, mercifully, which a lot of my contemporaries didn't, and came back in 1946 and went up to university. But the reason I did that was because the doctor in my unit was a Scotsman, called David Robertson, and he was from Dundee, and he and I used to sail together on Sundays, in canoes, against Blondie (?) Hasler, amongst others. Blondie was our training commander. And it was he who said to me, 'Now that the war is over,' - we were sitting together on a tropical night on an island off the north coast of Sri Lanka - 'why don't you go in for medicine?' And I said, 'Don't be so ridiculous.' And we went down to Colombo and bought a book on biology and a book on physics and a book on chemistry, and I didn't get beyond the first page because I didn't understand them. So I had no original interest in it at all. And it was he actually who filled in the forms for the University of St Andrews, to his own university, sent them in and to my surprise, Willie John Tulloch, the then dean, accepted me as a mature student, post-war student who was taken on. I was very lucky indeed.

GW Of course in a Scottish university there would be the preliminary year for you to do the sort of subjects that you hadn't done.

CB Well, it meant you had to do zoology in one term to first MB standard. You had to do physics and chemistry in two terms, and you had to do botany in one term. I have never worked so hard in my life.

GW I had a somewhat similar experience, as I was going up to university also to do languages, and I did everything simultaneously. I know what it must have been for you. But, nevertheless, you of course came to this as a mature student. I know a lot of people, because of the war, were in the same position, but do you think that was a considerable advantage, compared with going straight from school?

CB I can only say, as you, I know, understand probably far better than I, that war in my experience was one of two things: either moments, or very rare moments, of high excitement, and an awful lot of dull miserable boredom. And coming out of the war and going up to a university like St. Andrews, and working as I did in Dundee, and being exposed to modern chemistry by the man who became Lord Wynne Jones, and zoology by a man called Peacock, who was a very good zoologist. This was the first time I'd ever heard of Darwin, the first time I had ever heard of Avogadro and things like this, and it was an absolute revelation. One's mind was expanded in a way that I still remember it. It was a dramatically exciting period of my life. I enjoyed it enormously and the work was not difficult. I got first classes in everything.

GW And carried off the gold medal or a gold medal.

CB I got a gold medal for my MD, not my qualification. I got a *cum laude*, I think.

GW Yes one of the highest.

CB Well it was alright, but I wasn't top of the year. I didn't work hard enough. It really was a wonderful intellectual experience to do that at that period of time.

GW There was no family background of medicine?

CB No medicine at all, no. My father was a mathematician who became an optical expert and designed the first telephoto lenses in this country. He then worked for the Aircraft Establishment in Farnborough. So he invented all those high altitude lenses which the RAF used to use for their bombing work. A very intelligent, but difficult man.

GW Just before we go any further with you career, were you born in North Yorkshire, or where?

CB No, we were all born in Farnham, in Surrey, because my father worked at the factory at Farnborough, the Royal Aircraft Establishment. He was of a Yorkshire family, from between Bradford and Leeds originally, a little place called Gildersome. His family were organ builders in Wakefield. We were born there [Farnham], but after the age of four or six, my mother bought a house in Wensleydale for a holiday cottage, and then when the war came we went to live there. And I was at school at Sedburgh, as you've said, so my involvement with the Dales goes back a very long time. I regard that as my roots.

GW Yes. You've certainly maintained them.

CB And I still possess that house.

GW And you are still with the Wensleydale Society and so on?

CB Yes, I'm a member of that. That's right.

GW With Pickles¹ and all that kind of stuff.

CB I knew Pickles very well. He was our local GP. He was a very interesting man and did all that wonderful work on epidemiology in the practice.

GW Coming back to your own career, very briefly after qualification you had junior jobs in Dundee. One I noticed was with Ian Hill. Then you went over to Hammersmith with McMichael². That must have been quite a baptism and then on to Addenbrookes before you really began almost to settle at Hammersmith for quite a long time.

CB Yes, that is true.

GW Have you any particular memories of the people you worked with in those early formative years?

CB Well, intense memories, because the two men who influenced me most in my career were Hill and McMichael, who had of course been students together in Edinburgh. They had been in the same year. Hill had fallen behind a year because he'd had an illness. McMichael still doesn't like the story that Hill told me, because it's too favourable to him and he's embarrassed by it. But Hill fell behind a year and they were both battling for the medals, and Hill told me that after a week of his illness, one of his visitors was McMichael, who came in and said, 'I have been taking notes of the lectures. You'll be falling behind, here are the notes.' And that was for his competitor. And I think McMichael has shown that sort of quality of generosity through his whole life; it's part of his being, and I hope he won't be embarrassed by my saying that and putting it on record. But Hill of course came to Dundee from Edinburgh, where he'd been a very brilliant young cardiologist, and he was like a breath of fresh air. Dundee, until then, had never seen anybody of the really modern world of medicine. Hill just went in like a whirlwind. And to get his house job was a really fabulous experience. I'd done a surgical job with a remarkable surgeon called Frank Browne, who was one of the finest surgeons that I have ever seen. Then I went on with Hill. At the end of that time I said to him that I really thought it would be nice to go and do a couple of years in physiology with the professor there, a man called George Bell. And he said he'd fix that up, and then he came in the next day and looked at me - and he was a military style person - he sort of barked at me and said, 'You must go to the Hammersmith.' And I said, 'Really.' I was petrified. Hammersmith was the great Mecca of clinical research at that time. And he said, 'Yes go down and see McMichael.' So he sent me down to see McMichael. I remember it was a hot July day and I went into the old lower medical corridor, which was a temporary hut which was put up in 1935 - it's still there - and McMichael called in Sheila Sherlock and the two of them interviewed me. And I got the distinct feeling that they didn't really know too much about what to say to this curious fellow from the north, but very kindly McMichael offered me a house physician post with him. That was a splendid experience because it introduced me to Hammersmith medicine,

¹ William Norman Pickles (1885-1969).

² John McMichael (1902-1993) Knighted 1965.

to him and to a whole lot of other friends who I've kept ever since. And it was basically Hill who set that up, with his old friend McMichael.

GW That's interesting, a sort of Scottish Mafia, because Sheila Sherlock herself told us in the interview on video how James Learmonth....there was at that time no opening at all for a woman in Edinburgh and because he was born in the same village as McMichael they managed to fix things for her between them.

CB Did she tell you what the reference was that he wrote?

GW No.

CB Learmonth just wrote, 'Dear Jack, if you get Sheila Sherlock, you'll be darned lucky.'

GW Oh well, Ian Hill must have done something of the same kind. Anyhow, there was a trend for quite a time towards haematology, wasn't there?

CB Well, what happened was, I went to Cambridge for a year, and I can't say that was the happiest year of my life because I found the old Addenbrookes Hospital somewhat authoritarian compared to the very free thinking environment which I had been used to at Hammersmith and also, really, with Hill in Dundee. But I got my membership [of the Royal College of Physicians] and that decided me to go in for medicine and I went back to be the resident medical officer at the Hammersmith in 1954. And in the autumn of that year McMichael called me in and said the department of haematology was wanting to collaborate with the department of medicine and would like a young clinician to go and help David Mollin, who was working on vitamin B₁₂. This was a stage in the years after the discovery of B₁₂ when radioactive B₁₂ was being exploited for use, and I was being seconded to haematology, retaining my position as a clinical registrar in medicine, to do research with David Mollin in the department of haematology, then headed of course by John Dacie. John Dacie, of course, that was the great heyday of his building up of British haematology. It was a very exciting department to work in, and I was very lucky indeed to be given some vitamin B₁₂ labelled with Cobalt 56 prepared by Lester Smith at Glaxo. How grateful we are to Glaxo and our commercial links there, because Lester Smith was an outstanding scientist and a very fine man, and we received this material from him and it was the hottest that anybody had ever had, so we could make measurements that hadn't been made before. For example, you could give a shot of it by mouth and then measure the changes in the blood that followed. Now nobody had been able to do that. The fascination of that experiment was that, unlike any other substance... I mean, if you feed glucose to somebody for a glucose tolerance test, it's all over in two hours. It goes up in half an hour and then comes down. The thing about B₁₂ that was absolutely fascinating was that we found nothing in the blood for three hours. And when I did the first experiment I remember measuring it in the blood up to six hours and there was no radioactivity until six hours. This was quite extraordinary. I thought I had made a complete mistake. I remember doing those measurements in Pat Morrison's old MRC hut. He was head of the MRC transfusion unit and we borrowed his counter. We had no equipment to measure the stuff. And then Mollin said, 'Why shouldn't we do it at twenty four hours.' And we found that

nothing happened for three hours; it went up to reach a peak at about eight o'clock and came down. Now that, of course, raised a very interesting question: what was the delay due to? And the delay was obviously one of two things: either it was a delay in transit across the gut mucosa or else it could have been absorbed lower down. So what we did was to work out in rats, a similar animal model, and we were then able to show in the rat that it was the distal half of the intestine that picked up the B₁₂. And that was the first time anyone had shown the localisation of absorption in the gut. And it took me on from there to studying it in man. And so the whole of that period, which was my MD thesis for St Andrews, was related to working out where B₁₂ was absorbed and why. And this was fascinating, quite extraordinarily interesting. I remember some years later going to the States and a young surgeon had been trying to prove, using some experiments in man, firstly that intrinsic factor was not necessary for B₁₂ absorption, and secondly that B₁₂ absorption occurred in the ileum. By then I had become professor of medicine at Hammersmith, and I remember saying to this American audience, I said, 'If that man's work turns out to be true, it will shake the foundations of the Harvard Medical School, because of Bill Castle's relationship to the Thorndyke Laboratories there, and equally shake the department of medicine at Hammersmith, if my work was proved wrong.' In fact, it has been proved completely right, but it was a very interesting period because it took me into something that then became a major interest of my life. When I started, the intestine was just well - people looked at it as a dark area between the stomach and the colon, the small intestine. Absorption was known to happen there, but anything specific beyond that was not much known about. The concept of active transport had only just really started coming through following Fish and Parson's work. And then the development, in Sheffield, of the everted sac technique by [TH] Wilson and [G] Wiseman. And of course the idea of localisation at once proved that active transport must occur, because it meant there were localised receptors rather than a diffuse osmotic idea, which is what the pre-war physiologists had thought. I remember the physiologists. I remember demonstrating that work at a meeting of the Physiological Society and several distinguished physiologists came along and said, 'Oh but the cells in the jejunum and ileum look the same and their function must be the same.' And I said, 'Not true, actually.' But we had quite a job persuading them. But it's so obvious now that no one even remembers it had to be discovered.

GW It's interesting that your introduction to gastroenterology, which at that time was probably just in its toddling stage, almost came that way through a specific absorption and specific place of interest in B₁₂.

CB I have always said if you study B₁₂ absorption, it's so complicated everything else is simple.

GW Well, I may have known you earlier, but I certainly met you over the intestinal biopsy meeting we both shared, and remember then the excitement over those biopsies.

CB Well I think that's the first time I met you. It was in Madrid.

GW Coeliac disease. I think it was Madrid. I think it was the first time certainly that we got to know each other at all well.

CB In honour of your CIBA...

GW Yes.

CB Well that of course arose out of Margot Shiner's work. That wasn't my work. Margot Shiner was the first person really to put gut biopsy on the map and she did that in Sheila Sherlock's department at Hammersmith, and we used to send patients to her for biopsy - patients we were studying. And that of course was an interesting relationship because it was Hammersmith's relationship with the old Commonwealth that led to that discovery. It was basically in Australia that Doig and Wood developed a tube for biopsying the mucosa of the stomach. And Margot Shiner came to work as an unpaid assistant with Sheila Sherlock, and she started doing some gastric biopsies, taught by Selwyn Baker, who was then an Australian research fellow working in haematology with Mollin and Dacie. And he taught her to do it, and she then pushed on into the jejunum and that's how it developed. So our links with Melbourne were an important one at Hammersmith.

GW That's an interesting comment because when you became professor of medicine at Hammersmith you were very international in your attitude, a very rare attitude, if I may say so, in this country. Well, you had that Commonwealth, or at least Australian thing, but presumably because of your earlier French language inclination and so on, you did help to make Hammersmith a Mecca for, I think, people from other countries, which it wouldn't have been without your interests of that kind. On the whole British people are not averse to having people from these countries showing initiative and coming, far from it, but you did much more than that, didn't you? You really made people feel very welcome, and that coming to Hammersmith was the thing in their lives.

CB Well, it's nice of you to say that. It really began, that particular thing, after...the first thing that happened was that I became a gastroenterologist and I succeeded Sheila Sherlock as gastroenterologist. In fact, I'd better just tell you how that happened. Again it was McMichael, he was the chairman of the department of medicine and I met him walking down the corridor one day when Sherlock had just been appointed professor of medicine at the Royal Free and he said to me, 'Ah, Booth, we need a gastroenterologist, how about you?' And I think the best description of how one reacted to McMichael was that he was a man, who, if he said to you 'Jump,' you said 'How high?' That's essentially how you responded. So I saluted and said, 'Sir,' and he looked at me and said, 'Have you ever put that tube down into the stomach, done a gastroscopy?' I said, 'Never in my life.' He said when you come to the interview I want you to be able to say you've done one. I went across to Avery Jones³ at the Central Middlesex and he tried to teach me. It was a lost cause. I was never any good at that sort of thing but I was able to say I'd done one. Anyway, it was when I was gastroenterologist at the Hammersmith - I became professor of medicine in 1966 - but when I was head of gastroenterology we were visited by a Frenchman, a very remarkable man whom I'm sure you know called Portense, and Portense came as a sort of medical ambassador. And I met him at lunch and we were speaking French

³ Francis Avery Jones (1910-1998) Knighted 1970.

and I said, 'Look here, I am forty minutes from Paris, and the only gastroenterologist I know in Paris is Caroli,' - Jacques Caroli, the very famous hepatologist who I'd met through Sheila Sherlock, who knew him well. And he said, 'Well next time you're in Paris I'll introduce you to everybody you need to meet.' So I went across to Paris two months later and I met Jean Jacques Bernier at Saint-Lazare, Bonfils and (?)Boucher. I went to see Caroli at Saint-Antoine. And we already had a very close link with a very good bone histologist called Phillipe Bordier and he, in fact, in later years had a joint appointment in Paris and in London in my department. Now that all came from that gastroenterology set up. And then it was really after I became professor of medicine, we had to look around a bit and see which way our teaching should go. And you referred to the Commonwealth building and Commonwealth links and the School had been a Commonwealth School. It was founded in 1935 as an Imperial School. You know the King in his foundation speech had said, 'It's for people from all parts of my Empire.' By 1966 that had changed and the European scene was becoming very much more exciting. I think it must have been about then that Servan-Schreiber wrote his book, *Le Défi Americain*, which expressed a sort of French worry about American influence dominating everything. And I took the view that really we had to change that. So we examined our course structure and instead of doing three month courses for general medicine, we accepted that specialisation was in, long after it was in, and we said we'll run specialist courses. And we advertised them particularly in European schools, and at the same time we set up a scheme of inviting a European visiting professor each term, and specifically he was European. We had some Americans obviously as well, but I was trying to make it European, and through that scheme we invited some of the finest people in Europe. And it was very interesting that in later meetings that I attended, say at the Charlemagne Building in Brussels, you'd usually find the committee was usually made up of all your pals you'd had as friends, invited as visiting professors. And that scheme, I think, did something for us all. The other person who contributed to that was my old friend, Colin Dollery, who'd been president of the European Society of Clinical Investigation, which of course, you had played a major role in founding.

GW I inserted him as the one young man who could be president.

CB And he was jolly good. He brought us into that Society. So there was a strong European tradition. It wasn't me alone you know. It was a trend that was happening.

GW Yes, but I think without your leadership, it wouldn't have been what it was.

CB Well, it's kind of you to say that.

GW I'm not just trying to be flattering. The mere fact that you came with a knowledge of French, a really good knowledge of French, to English medicine has been an extraordinarily good catalyst.

CB I think it was useful. You remember Cuthbert Cope very well. A wonderful endocrinologist and a cantankerous old chap at times, but a fine man whom I greatly admired. I remember after I was made professor of medicine - and thereby hangs another tale - Cope was heard to comment one day, 'Very different from McMichael's time! You hear people talking French.'

GW I have a bitter memory of his failing to support an international endocrinology society, and doing it a lot of harm for a few months.

CB Nice man.

GW But a very fine man.

CB And a good scientist.

GW How did you become professor. What's the story there?

CB It was curious because I'd never expected to succeed the great McMichael. It was a thought that had never passed through my head. And I think when you interviewed Cyril Clarke - I remember seeing his tape - he explained that the appointment of a professor is never a simple question. And on this occasion what happened was that I was invited by the then dean, who was Selwyn Taylor, a surgeon, and Sir John Dacie, who was my old chief in haematology, to be the school's candidate for the chair. I was very flattered and said, yes, of course I would. And nothing happened for a long time and then about three months later Selwyn Taylor came to see me and said, 'There has been a bit of awkwardness and difficulty about your appointment. The committee wants it to be advertised and we're going to advertise it, but you just apply.' So that put me in rather a difficult position because the school had said be the candidate and so on, and now they were being pushed to advertise. I knew perfectly well I had a chair of gastroenterology in my pocket and that would have been scientifically the better way to go, because I could have stuck with my science more. But I went back to see him the following day and extracted the situation from him, which was that the six or seven people senior to me in the department who were very distinguished people had written a letter to the committee complaining about my appointment. And the one person, I may say, who had not joined that was the next in line, who was Russell Fraser, who interestingly refused to object. I said, 'Well, look you're in a difficult position as dean, but, if you as a school, despite opposition, put me in, I'm your man.' But I said, 'If on the other hand you're prepared to cow tow to opposition within the department from people who will, if I am to be the chairman, be people I've got to direct, then under those circumstances I'm not going to apply.' So, three days later they came through and appointed me, without having an interview. It obviously was a management situation that was difficult. As soon as they had appointed me of course, and given me the position and given me their backing then there was no problem at all, because the great thing about Hammersmith was the unutterable loyalty of everybody. And everybody came round and supported me, and I had nothing but loyal support from everybody in the department and we never had any bitterness about that at all. It was a very heartening experience, and chairing that department - I was chairman there for eleven years - it was an absolutely fascinating experience. As you said, very international and tremendous fun, but I increasingly left my own science; that was sad.

GW Yes, it's a price that has to be paid to some extent.

CB If you become a scientific manager you have to accept that.

GW And there have to be scientific managers who know what it is all about.

CB Well, I found it very rewarding, very rewarding.

GW We'll come back to this question of appointing professors in a more general sense later, but just going on ahead with your own career; when did you consider the possibility of going to Northwick Park?

CB Well, that was an interesting one because I was very happily ensconced at Hammersmith.

GW You might well have seen your time out there.

CB Yes, I think it would have been wrong to have stayed much longer. I think ten or eleven years is about right for that job because otherwise you tend to get a bit stale and out of date, I would have thought. I flatter myself that I hadn't reached that. I hope not. What happened was I was at that stage sitting on the advisory board for SERC [Science and Engineering Research Council], so I knew about the MRC. I'd never sat on the MRC or even been on a board of it. I'd never been on any of the MRC committees, and I think with Hammersmith as a major user of MRC resources that was no bad thing. But the job came up because Graham Bull was going to retire in about 1976. I think it was in the summer, July, and the Council looked around for people and invited a number of people to say what they would want to do with Northwick. And Colin Dollery came to see me and he was very keen at that stage to do the job. I said I would support him and I did. And he looked at the job and there were bits of it that he didn't like, and there were problems for him in terms of his relationship with industry through pharmacology and so on. And I think all in all after some months of negotiating he decided not to take the post. And the MRC took their own decision, and I don't know how exactly it happened, but at that stage of course the MRC had this big institution with a budget which came between ten and eleven million a year, which is a lot of money in clinical research. It had been in a state of limbo really for a year and a half whilst a successor was being sought. No institution can sit around like that: it is not a good way to handle things. And at that stage John Gray, at the end of an advisory board research council meeting in the Department of Education and Science, one day took me over to the Athenaeum and sat me down by the fire. I knew exactly what he wanted and he said, would I go and sort out Northwick Park, take the job up. I had no real intention of doing it. I went home and said... No, before I went I said, 'Look, you know, John, you are in difficulties. You have been hawing for six months. It is an important post, and has an important amount of your budget. What I will do is give you an answer, yes or no, in two weeks time from today.' I said, 'That is all I can tell you at present, but I will tell you yes or no.' So that was the Thursday and on the Monday I went to Northwick Park for my first official visit. It was snowing. I had been unwell over the weekend, I'd had the flu, and I was feeling dreadful. I went out to the car; the front wheel tyre was flat. And I drove up to Northwick Park dictating in my mind a letter to John Gray suggesting ten rather than eight, and I reached Northwick Park and I spent the day looking around and...The point about Northwick Park is, firstly, there were some remarkably fine people there, particularly men like David Tyrell, and Tim Cohen in

psychiatry, John Nunn in anaesthesia - very fine and devoted people, and I was impressed by them. And the second thing was that the facilities there on the research side were absolutely superb. The hospital, of course, was totally different to Hammersmith. It was the district general hospital with men in consultant positions doing private practice as, of course, they had a perfect right to do. But one or two of them equally on the hospital side were very good, and the gastroenterologist, whom I had known well through my speciality interest there, Michael John Levi, is a really outstanding man. And I was impressed by them, and impressed by the facilities. I then went again, about three days later, and I finished up with a long talk with David Tyrell, who was then deputy director, fellow of the Royal Society, a most distinguished man. And I said, 'Look, David I really feel somewhat diffident about coming to direct you,' but I said, 'what do you think?' And he said, 'Well I think that you should come.' And so I wrote to John Gray and I said, 'I want simply to make two conditions. And they are: firstly, that you tell the trade union side and the staff side before you announce it in the press, because I think that will get us on the right foot; and secondly, that you have a heads of division meeting at Northwick Park, and obviously on an issue like an appointing a new director you are not going to consult, but you might pretend to, or something along those lines.'

GW Yes.

CB And he did. They announced it within the fortnight. And I said OK, and so I then had to announce it to my colleagues at Hammersmith and I think it was the right thing to do, because it allowed them to put in a very bright, younger man than myself, who is now regius professor at Cambridge, Steve Peters, a very fine man who did a great job in building on what Francis Fraser, McMichael and others had done there. And I think it was the right move to make, and all I can say is that I have enjoyed it.

GW Oh, I am sure you have. You have just till the end of the year before you....certainly not very long.

CB Well, we haven't discussed that in detail. I mean my official retirement is 1989. So it's sometime between now and then, with the changes that are now taking place in the MRC.

GW And in relationship to Hammersmith and Northwick Park and so on.

CB Yes, exactly. And obviously it would be right for them to put in somebody for the next phase, and I think that is right.

GW Well, I think that is absolutely fine. I would like to go back, rather now on a less personal note in a way, to the whole problems of academic medicine. I mean, when I was a student there was, of course, no professor in my medical school. I may say on a personal side, that when my medically qualified wife came from Yugoslavia to this country, it was inconceivable to her that any man of any note in medicine was not a professor. And so when she first met the first British one, who I think was Bill Kekwick, she thought that how could the other people not be under him. The fact that he was the most junior and on the whole it had been rather sort of palmed off him was something that she has never quite got over. It is so different from the continent of

Europe isn't it, in every way? And it took some thirty years, I think, for the London teaching hospitals each to have a professor of medicine, quite apart from other professorships.

CB Well, that is absolutely right. I mean it all began with the Haldane Commission which reported in 1913. And they were the commission on education in London that recommended professorial units. But those units didn't get started until after the first war; they had to get that out of the way first. The first appointee was Archibald Garrod at Bart's in 1919, and of course he left to take up Osler's post in Oxford as regius before he became professor at Bart's. And Francis Fraser was the first medical professor to be appointed. But until 1939, well until after the end of the Second World War, there were only five professors of medicine in the twelve medical schools in London. And it wasn't really until the expansion that took place with the implementation of the Goodenough Report, which was 1944, on medical education, that the London Schools were forced really by the UGC [the University Grants Committee] and others to put in professorial departments. But it is interesting that King's, for example, didn't have a professor of medicine until 1965. It was a long business. And, as you rightly say, the point was that the professor was just one firm, and each of the other chaps was a big man in his own right and the professors therefore didn't control things. Now, this was the great virtue of Hammersmith; at Hammersmith the professor is the big man. Well, I don't mean the big man, you know what I mean; he is the continental style professor. And it has all the deficiencies of authoritarianism that can creep in. And my predecessor, John McMichael, could be authoritarian, but the great thing about McMichael was he was a benevolent despot who encouraged the young in a way that virtually nobody ever did. And I perhaps should perhaps quote one marvellous thing that he said when he was president of the Association of Physicians, and it was a very difficult time for him personally because his wife had just died, his then wife. And I remember he insisted on giving the speech at the dinner, despite his grief - it was that same week. And he said something which I have always been impressed by, he said, 'We must have the young standing upon our shoulders, not trample them under our feet,' you know. And that was his whole viewpoint. He was so encouraging to the young. He was magnificent in that sense, wonderful.

GW It's interesting that Francis Fraser and John McMichael were both very exceptional men.

CB Absolutely.

GW And yet with those models it was still so difficult to get through with the idea of professorial units.

CB Absolutely. It took a long time. And we still have the problem because the professors still are just single firms. And I think that it's been a disadvantage to science in medicine because one of the difficulties is that once you have a professor in post with a certain speciality, he trains the next generation, and that means that the next generation follows the same speciality interest as the predecessor. And I think that is why so many of our professors of medicine followed the clinical physiology of Tom Lewis for too long, when the new science of molecular and cell biology and

biochemistry and so on had come in, and they hadn't taken into account of that. And this has been a defect in our system too.

GW And of course a defect which is in fact becoming of greater importance with every passing month.

CB Absolutely.

GW But an interesting point, because of course there was no co-ordination of this in anyway. Because although they all got professors of London University, those who had a speciality and therefore trained in clinical science, their own people - if you like, sons and grandsons - within that speciality, there was nothing in London University that said that there shall only be one professor of gastroenterology or...

CB No way. No, there was no organisation for that at all it was entirely...

GW So it was a pure fluke really, what speciality became the professor.

CB Absolutely, yes. They tended to be very often to be what they considered was the speciality. So if there was already an endocrinologist, even though you had a world prize winning endocrine man in sight, you might not be able to appoint him because the hospital side would say no. And that was equally difficult. And they did say no on occasion.

GW If I remember rightly at the Middlesex, Hugh Marriott was offered the first chair. He was the least academic of all the people I have ever come up against. He was a good clinician in many in a very business like way. And it passed from him, in almost as a sort of nepotism, to Bill [Alan?] Kekwick, in a sense that they were close associates.

CB Well, I knew Kekquick quite well because his wife was a Shackleton from Wensleydale; she was a Wensleydale girl. I knew her father. In fact, the first time I met Kekwick was at a country house party in the bottom end of Wensleydale.

GW Interesting. Frequently you come back to that. I have ignored, and it might not be the right moment, but Wensleydale reminds me again of your, not only your interest in Pickles and people like that, but what gave birth to, really, to your major interest in medical history. I know that you did, of course, that book with Betsy Corner - *Chain of Friendship* was it called⁴? Anyhow, the Letters of Dr John Fothergill, which was a very masterly historical work and I think properly recognised as such. But you have never not been, for a long time, involved in medical history, quite seriously.

CB Well, that is quite true. It really started when I did that year in Cambridge, because I had to do my MRCP. And in those days your membership of the College of Physicians implied firstly, some written papers with translations from the French and

⁴ Corner, B.C., and Booth, C.C., 1971. *Chain of friendship: selected letters of Dr John Fothergill of London, 1735-1780*. Cambridge: Belknap Press of Harvard University Press.

German, which was useful, and secondly, a clinical exam. And then you had to wait and see if you got through that bit and then there was a frightening pathology *viva* with...Really, I mean the examiners in those days were terrifying, and then if you got through that there was a final *viva* with the censors and the president himself in his own office, in the censors office in the College. It was a dreadful sort of steeplechase you had to go through. And to my surprise I got through the first bit and so there was a pathology *viva*. There was an examiner called Donald Hunter, whom you remember well, and he was very interested in the past and history and would always ask historical questions. So between the two exams, I went to the library and I got hold of Donald Hunter's - not Donald Hunter, the Edinburgh historian, Douglas Guthrie - Douglas Guthrie's *A History of Medicine*. And I read it and it said in it that John Fothergill, who was a distinguished Quaker physician of the 18th century, who had been born at Carr End in Wensleydale, Yorkshire. He was also educated at Sedburgh, which was my school. So at once I was interested because I used to make hay at that farm as a boy. When I was a lad I used to go and help with the haymaking. I knew his farm very well. So I started reading about him and I discovered that a biography had been written in 1919, I think it was, by Hingston Fox⁵ - very interesting work. And Hingston Fox's daughter was my Quaker Aunt Barbara, Barbara Duncan Harries, was her GP in Croydon, a lady called Eleanor Sawdon. And she had Fox's own copy of the book with the annotated notes after it had been published, a subsequent thing, which she gave me. So I started collecting his letters and I went up to Edinburgh in '54, I remember, with my brother and spent a whole week just copying out in longhand letters of Dr Fothergill in Edinburgh University Library. I mean, a ridiculous way to do it now, but that is how... I didn't know anything about it. And I just started collecting the letters. And then I found a reprint in the *Royal Society Medicine* of a paper written by Betsy Corner. I didn't know who Betsy Corner was, neither did I have any idea that her husband would turn out to be the very distinguished discoverer of progesterone, George W Corner. But I wrote to her for a reprint and she said, 'Oh we are going to be staying in the Ciba House next week.' It was a meeting you were running at Ciba on gerontology.

GW Yes, we did some in the middle fifties.

CW Yes, that's right, about '55, something like that. And George and Betsy were staying in your house. So she said come and get your reprint. So I went along and by then I had found from Patrick Mounsey's aunt who lived in...Patrick Mounsey was a physician who became Provost in Cardiff [the Welsh National School of Medicine], a cardiologist. And his aunt lived in Darlington and she had a collection of thirty-five of the doctor's letters, which had never been seen before or published. I found thirteen letters to John Ellis in the Linnean Society. John Ellis is the man who discovered the animal nature of coral in this country, and a Frenchman did too. I went along with all this and showed all these things to Betsy and her husband at Ciba. And he describes the event quite amusingly in his autobiography, because he said, you know, I was obviously totally green. I had no idea that like a journalist all historians protect their sources, you know. I just handed over copies of everything. And I think both of them were somewhat startled to find anybody quite so naïve. But he describes

⁵ Hingston Fox, R., 1919. *Dr John Fothergill and his friends: chapters in eighteenth century life*. London: Macmillan.

in his autobiography how in the aeroplane on the way back to the States, he decided that if you couldn't beat them you had better join them. And so he had some difficulty with Betsy because she wanted to do some work on Fothergill and he had not felt sure that she would have the stamina to carry it through. She, of course, was completely convinced that she could. And what he basically didn't want to happen was to be lumbered with pulling it out of the fire, which he might have been. I think that was one of his points. But he then, as he recalls in his autobiography, was delighted to see me in the offing. And through him we got a grant from the American Philosophical Society, which he was then executive officer of. And it took us fifteen years to edit an edition of about two hundred, two hundred and fifty of the letters, which was an enormously inspiring experience, because for me it was a friendship that crossed both a nation and a generation. I mean, they belonged to a different generation than me. They introduced me to a world of American intellectual life that I would have no entry to just through medicine. And they introduced me to 18th century history and through that, those letters gave me a lot of interest in other things. I did it all in spare time, odd weekends and holidays and things, odd visits to America. Betsy did most of the American side and I did most of the British side. I think the other thing that was interesting was that it brought me into contact, fairly intimately, with some very senior and distinguished members of the Society of Friends, like Amy Wallace and others, who were tremendously helpful. And I learnt how incredibly generous spirited the members of that Society are, and they were all so helpful to me. Ted Milligan, who was the librarian at Friends House, he has retired now. But they were a wonderful group of people and they taught me a lot.

GW Just leaping a little because your interest in history is still very strong, but in recent times it's become to some extent not 18th century but the development of clinical science in this country, and what is happening to it and so on. I think, this is becoming a greater part of your life, this kind of study of development of clinical science rather than that kind of long ago history and so on, fascinating though much of it is. Is this something that for the future you will look to be doing?

CB Well, I got involved in it I suppose. The first thing was that my ruffianly friends at Hammersmith, when they had their fiftieth anniversary, invited me to do the oration on that occasion in May 1985, two and half years ago. So I had to work on the history of Hammersmith and the history of the medical school. I already had done some work on the development of clinical research and on just precisely the point you raised a moment ago about the development of the professorships in the university schools in London. And I then did the Hammersmith story, and at the same time felt it my duty, because we were by now well into discussions about the future of Hammersmith and Northwick Park, to contribute to the debate, a historical analysis of the MRC's contribution to clinical research. So I then did, not only the Hammersmith story as a historical study, which of course was not difficult since I had lived through an awful lot of it, but I think many professional historians would regard it as an amateur effort because I was perhaps too much involved myself. But turning to the MRC, one was interested to see how the MRC had approached clinical research, and I published a paper on that in the same year or the year after, in the *Quarterly Journal of Medicine*⁶. And more recently the Oxford historians invited me to contribute to the

⁶ Booth, C.C., 1986. Clinical Research and the MRC. *Quarterly Journal of Medicine*, 59, 435-447.

book that they are doing for the seventy fifth anniversary of the MRC, and this is seventy five years of the MRC. Through that I became interested in Tom Lewis's work particularly and the way in which the MRC got involved, because it's intriguing the MRC always regarded Tom Lewis as the clinical wing of the National Institute of Medical Research. They said they wanted a clinical bit. And, of course, out of Tom Lewis's ideas and the idea of a clinical wing to NIMR, out of that, the idea of a clinical research centre developed. And through that I became involved with, for example: Charles Fletcher and Richard Schilling and others; and Archie Cochrane over the new pneumoconiosis unit; people like Nicolas Myant over the development of lipid research, and George Popják; Bill Poachin⁷ and the development of the original work in this country and the use of the isotopes, which took place in his unit at University College. And then there is the very interesting correspondence between [Sir Walter] Fletcher and Lord Dawson of Penn and [Lord] Moynihan, but particularly with Lord Dawson. When Fletcher in 1932 was secretary of the Council, he was viciously attacked by Moynihan publicly, and by Dawson privately; the implication being that the Council was much too interested in science and not enough in clinical practice. And those letters, under the fifty-year rule, have now been released. They really burn with passion on Fletcher's side.

GW Yes, of course, that is all part of the same picture, that is what medicine is about. Extraordinary that we had people of such remarkable competence in medicine, who didn't see the scientific...

CB Well, it was partly power. Fletcher was a very successful trader of the corridors of power. He understood government departments and he, through sheer determination from 1913 till 1933 when he died, just that twenty years, sheer determination, forged the Council into being the national organisation for medical research. And he was responsible for that, and I think it was a very important thing to do. He created close links with the Royal Society and valued those greatly. The Royal Society in those days nominated the members of Council. And it was the right way to go in a historical sense in my view. I think there was no other way he could have done. But of course he did offend the clinicians who were sitting in Harley Street using ultraviolet light treatment, opsonic indices and fatuous idiocies like this, which were revealed by the MRC to be quite worthless. Of course, there always is this battle between those who believe they have got a cure and the scientists, who are contentious enough to come along and test it out. I mean Fletcher [Charles M Fletcher] had that problem with observer error in x-rays. I mean he made himself very unpopular. Yes, I have become very interested in that and I think my future historical work...I have got several things that I want to do. I think I would very much like to be involved in an analysis of the development of clinical science in Britain, and I

GW We both greatly admire Harry Himsworth. It is interesting that in '49, that he should be made the secretary of the MRC, the clinician of the whole period so far of the MRC.

CB The only practising clinician to have been a secretary out of six.

⁷ Sir Edward Eric Pochin (1909-1990). Director of MRC Department of Clinical Research, University College Hospital Medical School.

GW Do you know anything about how he came to be appointed, or is that still not available?

CB I'm afraid I don't. I mean I know very well that Wilson Jameson⁸, who was the CMO [Chief Medical Officer]... Yes, he was still CMO.

GW Yes he must have been.

CB Himsworth had been involved with him during the war, so knew him well. And I think you know more about that than I do. They knew each other well, and I think the other thing was he knew Mellanby⁹ well. And the other person who must have been influential, certainly not at that time, not in '49, but who had a high regard for Himsworth, I think was Lewis. And Lewis equally was much admired by Himsworth. I mean Himsworth was a tremendous admirer of all that had Lewis built up with the MRC.

GW I remember there being a good deal of suggestion that Harry was crazy to move to the MRC, and go to from clinical medicine into the MRC was an extraordinary choice.

CB Well, I am delighted he did because there is no question that in those years he did great things for clinical research. One of the patterns that developed was that a unit would be set up with a university professor as an honorary director. A good example is Dugald Baird in Aberdeen, George Pickering at St Mary's – the body temperature unit, Hermann Lehmann at Bart's. And now we have got very few of these. I mean David Weatherall in Oxford is one of the few people who is a university professor with a clinical unit. That is a rare thing now. But I went over the numbers of units founded by the MRC between 1950 and 1970. I mean Harry Himsworth, I think, retired in '68.

GW Yes, this was just about this period.

CB Roughly his period. Eighty-two units were founded, forty-six of them in the clinical field, so he made a big impact. And for those of us who lived through that era, by the time 1970 came round, there were by then, you know, ? university departments in all the schools, particularly in the great civic universities like Newcastle, Liverpool, Manchester and Birmingham. Good full time departments have developed in London. The professors that have come in are running good departments. Hammersmith has become Royal. The CRC had been founded. And, you know, we were full of optimism at that stage, tremendous air of optimism.

GW Yes. I certainly never thought that we would get into this period of regression.

CB I don't think we did. No, no. I don't know how we could have foreseen it, but...

⁸ Sir William Wilson Jameson (1885-1962).

⁹ Sir Edward Mellanby (1884-1955).

GW I would like to tempt you to speculate a little on two things really. One is this question of the academic and continuing necessity for clinical research. But in the light of the developments of molecular biology, which really is in a sense a unifying factor - I mean nearly all the specialities are beginning to look less and less suitable ways of tackling clinical practice. What do you think is likely to be the course of events, in so far as clinical research can be maintained at all in this country, in the next say twenty years?

CB Well, I can only really guess what the future is. And Peter Medawar always used to say that scientists and soothsayers should not mix. I can't foretell the future and wouldn't try. But perhaps one could just analyse what has been happening at Northwick Park, because when I went to Northwick Park from Hammersmith ten years ago, I knew no molecular biology other than odd little bits that I had read. I knew the genetic code, of course, but nothing else much, and I wasn't working in that field at all. When I arrived at Northwick Park there was no real molecular biology; there was some cell biology, but no molecular biology at all. And we had to change course, obviously, and what I did was to consult with a very good friend Bob Williamson at St Mary's, who has been a tower of strength and a magnificent cauterising critic, if I may call him that. And he has been a great strength to me, because I asked him to come and advise me and advise our heads of divisions, and we made a policy decision about eight years ago that we would selectively fund those people going in the direction of molecular and cell biology, at the expense of others. And that meant some people had to leave and they did. It released resources for us to redeploy, because I have never had any extra resources at Northwick Park. I always did everything we have done on the existing resources. And we went in that direction and I think it's had a spin-off, because what we have learnt from it is that the people working in that sort of science don't want to be standard gastroenterologists, cardiologists, dermatologists. I mean our dermatologist Mike Pope, who is a card carrying dermatologist, works on the molecular biology of collagen gene, came to see me last year and said, 'Look, I'm no longer a dermatologist, I'm, a clinical geneticist. Can you alter my consultant contract.' There were all sorts of problems but we have done. He is now a general physician with an interest in clinical genetics. He sees skin patients, of course, because he has still got a trade there. But this is happening all the time...

GW I think you still have to have- excuse me for interrupting - but you still have to have a head of department of dermatology? Or don't you bother?

CB Well we don't. We have a very good clinical dermatologist on the NHS side who runs that. But I think you do need clinical dermatologists, there is no question.

GW Yes quite.

CB But the question is how you do the science side? And I think the way science is going, is that what you need are either research divisions or university departments headed by people who are molecular scientists, cell biologists, immunologists, clinical pharmacologists and what have you, who cover a wide range speciality interests. By all means have your speciality people as well, but without these other people you

won't have it. And the Institute of Molecular Medicine, that David Weatherall is now building up in Oxford, follows precisely that. And what he is saying is just that; he is saying we must cover a wide range of areas. Now it is interesting that that view which was expressed in a chapter in a book I recently wrote, reviewed by a distinguished surgeon who attacked me for wanting to go back to the generalism of the 18th century. And that is not really the point: the point is that you have got to be taking on board modern science. And I have always quoted Arthur Kornberg on this because he made this splendid comment in which he said: 'In contrast to medical practice, which is fragmenting and differentiating by centrifugal dispersion into more and more parts all the time, what is happening with science is that it is converging. So that now, pharmacology, immunology, genetics – they are all being commanded by a common language.' And that is being reflected by the new breed of clinical scientist, such as David Weatherall has in Oxford, such as we have in our department of molecular medicine under that very brilliant young man James Scott, who is doing brilliant work on the apo-protein gene.

GW But I must say that the time that this will take to feed back into our organisation of clinical practice in this country is anyone's guess, isn't it?

CB Well, it is too slow. I mean it is too damn slow. We are not moving fast enough.

GW I think on that note I am going to have to say that we have finished this conversation, at least on camera. But I am immensely grateful. It has been very stimulating and interesting. It is very good of you to undertake to come here this morning.

CB Gordon thank you, and thank you for being so kind in your questioning

GW Thank you.