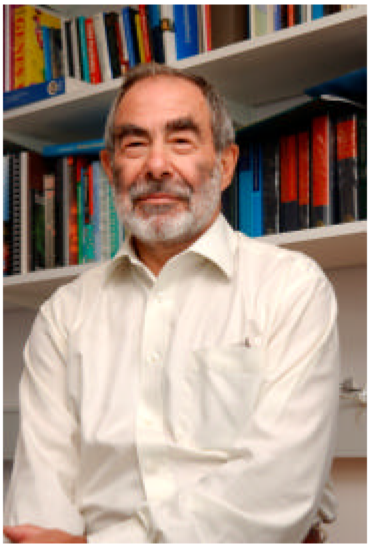
Martin Bobrow



Personal Details	
Name	Martin Bobrow
Dates	Born 06/02/1938
Place of Birth	South Africa
Main work places	Oxford, Amsterdam, London, Cambridge
Principal field of work	Cytogenetics, Clinical Genetics
Short biography	See below

Interview	
Recorded interview made	Yes
Interviewer	Peter Harper
Date of Interview	01/11/2004
Edited transcript available	See below

Personal Scientific Records	
Significant Record set exists	No
Records catalogued	
Permanent place of archive	
Summary of archive	

Biography

Martin Bobrow (born 6/2/38) studied medicine in South Africa, and joined the MRC Population Genetics Unit in Oxford. He was Professor of Medical Genetics at the University of Amsterdam, Guys and St Thomas' Hospitals, and the University of Cambridge; and Director of SE Thames and East Anglian Regional Genetics Services.

He has been: Deputy Chairman of the Wellcome Trust and the Nuffield Council on Bioethics; Chairman of Committee on Medical Aspects of Radiation in the Environment and Unrelated Living Transplant Regulating Authority; and member of the MRC Council; Human Genetics Advisory Commission; NHS Central R&D Committee; and is a Non-Executive Director of Cambridge University Hospitals NHS Foundation Trust.

INTERVIEW WITH PROFESSOR MARTIN BOBROW, 1ST NOVEMBER 2004

PSH. It's Monday 1 November 2004; I am talking with Professor Martin Bobrow at the Nuffield Council for Bioethics. Martin, can I start more or less at the beginning and ask the question I have been asking everybody. What interested you first in science and medicine to get you into it?

MB. Hard to answer that, isn't it. I drifted into medicine because it seemed less unattractive than other options at the time. I don't think I ever thought I was going to practice medicine. I was much more interested in research and in the theory and in understanding how things worked than I ever was in doctoring. This was in Johannesburg, so having started my medical course, I took a year out to do a BSc, then another year out to do what was then an Honours degree over and above a BSc in that system before going back to medicine. During those two years out, not only was I essentially doing research in a small way but I was very lucky to work in the department of anatomy, which was at that stage just handing over from Raymond Dart. I was the last year of medical students who were taught anatomy by Dart himself and it would be handed over to Philip Tobias, who of course has become a very distinguished physical anthropologist and palaeoanthropolgist and who was himself was recently back in South Africa, having been in the UK and done a research degree himself involving chromosomes, became interested in genetics and was thinking about the variation between the peoples, the living peoples of southern Africa. So on those beginnings I spent virtually all my long holidays during the rest of my medical course doing living physical anthropology, rushing around the sub-continent measuring peoples' noses and adipose tissue and so on and so forth. I became deeply impressed with the fact that was a really silly way to try and characterise human diversity, and during those student times actually ran a couple of studies on colour blindness testing and on phenylthiocarbamide taste testing on indigenous nomadic Bushmen in the Kalahari. So that was my introduction to genetics. I then finished medicine, did house jobs, travelled the world, sort of moved along the lines of a clinical career, but brought myself up short with the realisation that this was a poor idea and applied for a job in Alan Stevenson's Unit in Oxford.

PSH. Just sticking in South Africa for a little while, I mean I hadn't realised that that unit was already involved in what one might loosely call human genetics. Did it have a broad human evolutionary approach?

MB. Oh yes. Well the department was totally dominated by Dart and the history of australopithecines, and was absolutely concentrated on human evolution and the living descendants of the early hominids, so to speak. So there was a considerable amount of work being done on the variation between the current living races in southern Africa.

PSH. Was there an actual kind of genetics lab at that stage?

MB. No.

PSH. Doing things like blood groups and serum proteins?

MB. Not that I am aware of. I think it was a pretty traditional physical anthropologists' approach. There were blood group labs in the blood transfusion centre nearby and there was some work done on blood group distributions in different indigenous people, but I had no contact with that and it wasn't an explicitly genetic approach, but it was beginning to grope towards it as in the examples I have given you, which to me seem pretty natural, but I guess as far as the people setting up the programme are concerned that could have been innovative for its time.

PSH. Yes. I am interested in that Tobias learnt chromosome techniques. Do you know where he came. Did he come to Paul Polani or Charles Ford or .. .?

MB. I think he was in Oxford and it was fairly brief. I am trying to remember that but I can't. Do you know I think his own MD thesis was something vaguely to do with chromosomes in some species, not human.

PSH. Animal rather than plant?

MB. Animal rather than plant, yes.

PSH. What year was it then, that you did that major project in Johannesburg on the.

MB. '57/'58. Went on, I was involved in these Bushmen expeditions between about '57 and '61 at varying stages and I can't quite recall exactly what happened in which year.

PSH. So around the time of the correct human chromosome number?

MB. Yes. I learned that during my course as a medical student.

PSH. Do you mean you learnt the right number or the wrong number?

MB. Both, but in the right order.

PSH. Yes. And tell me, was Trefor Jenkins yet in Johannesburg?

MB. No he wasn't.

PSH. When did he come?

MB. After I'd left, as far as I know. I left South Africa in mid 1963 and I certainly was not aware of Trefor being there at that stage. I think he came sometime afterwards. I was taught chemistry as a second year student by Sydney Brenner, but that had no influence on either me or him as far as I am aware!

PSH. Growing up in South Africa, and being yourself part of a minority in South Africa, how much did that shape your outlook?

MB. Scientifically, not much I suspect. I think it shaped my political views very strongly. I was left with all the obvious aftermath of someone who just didn't like the society in which they were growing up, and from as far back as I can remember I was pretty plain that I needed to get out of it. I didn't see, I saw no way in which I believed I could influence what was going to happen. I saw a future which I thought was going to be cataclysmic; turns out fortunately that I was wrong, but I have to say looking back on it I still think the odds were on the way I was looking at it and I just didn't want to be there on either side.

PSH. How many generations had your family been in South Africa?

MB. My mother was born there, but her parents went out recently before her birth, and my father was actually born in some part of the Eastern Soviet Union and went out to South Africa as a babe in arms, so that was a pretty transient . . .

PSH. Between the wars?

MB. That was in, even earlier than that. It was before the First World War. Well before. It would have been 1905 or something like that.

PSH. One of the things that has always fascinated me is the huge contribution that South African Jewish diaspora has made, in this country especially. Something which I feel maybe hasn't been as explicitly recognised as it should, because it is very big.

MB. It is curious how many South Africans there are around, concentrated in academia, not exclusively. There are some very eminent jurists of course, Hoffman being an example. There are a few eminent people of great wealth but it has been predominantly in academia. I don't know what to say about that, you are right, the numbers seem disproportionate. I guess it's partly the fact that academia was seen as a very desirable career goal. My two comments, totally uninformed and untestable, firstly I think I had the benefit of a really lousy education. I went to school and did what people did at school, but school finished at lunchtime every day or 2 o'clock every day, and my recollection of my childhood, right though until senior school, was of vast amounts of time that I could go and do what I wanted to do, which ranged from the chess club to going very long distances across mad countryside on a bicycle and just exploring the world in a way my own children growing up here just didn't stand a chance. Their school life was so structured from so early a stage, there were so many hours and so much work after school that I think they got an enormous amount of information, and I think it killed their curiosity and their capacity to self explore in a way that I still believe is really just wrong. I think we have the proportion wrong and people get blocked from having enough time to themselves far too early on. The second thing is that, although less so today, Britain is a pretty class-ridden place. Coming from outside of the country enables one to by-pass many of the class distinctions that native-born people find themselves grouped by. Us colonials, ex-colonials, just are not expected to know how to behave properly, we don't behave properly and it doesn't seem to have formed as much of an inhibition as it might have done for other people.

PSH. I think you are right.

MB. Probably not actually. That's real beer talk and you can edit it.

PSH. So am I right then, you came to this country and in which year now are we thinking, 1960 . . .?

MB. I came to the UK in late '64, early '65. I found that I was not registerable with the GMC because I had not done enough of the clinical practice that they wanted and because I had not done house jobs in South Africa. I had started it but had a disagreement with the hospital shortly after starting and packed my bags and left. I mean it wasn't a police type drama but I was primed and ready to go and I lost my cool over an attempt to redistribute housemen between several hospitals on the grounds of their skin colour, and I told them to get knotted and left. So Lyn and I . . .

PSH. So did you then go straight to Stevenson's unit?

MB. We had been in Israel for a year, because when we left we just didn't have jobs to go to. We got married and left within 3 weeks and I had two relatives abroad, two medical relatives, one in Haifa and one in Edinburgh, and I wrote to both of them and said, can you find me two house jobs at very short notice. Haifa responded positively sooner and so we went to Haifa and spent a year and a half in Israel. We then went to Edinburgh where relative two had come good and re-did my house jobs because the Israeli clinical experience wasn't recognisable, and that was fine actually. We had a very good time. I was working at the Western General in gastroenterology at a time when Court-Brown was doing some of his major early studies.

PSH. So did you have contact with Court-Brown and Pat Jacobs?

MB. No, not at all.

PSH. Strong and people?

MB. Well, I knew Strong because he was in . . .

PSH. Endocrinology

MB. That's right and I worked specifically for one of Strong's colleagues called Bill Sircus who became a well-known gastroenterologist. I did two six-month house jobs,   got registered and had actually registered to take an SHO position in general surgery with a view to doing an FRCS when I took a long cold look at this and decided that was an entirely dumb idea. Someone, a friend of mine, drew my attention to an advertisement for a post in Stevenson's Unit and in mid '65 I applied for it. As far as I know I was the only applicant and we went to Oxford.

PSH. Now Stevenson's Unit has come up with a whole series of people I have spoken to, including John Edwards, but quite a few others as well and, can I just ask, were you already thinking of genetics as a career when you applied for that or was it just a way back into research in general, or wasn't it even as clear-cut as that?

MB. It was that. It was a way to get back into a research career and it looked interesting and entertaining. I'm not sure I really cerebrated about it as a career. I knew what I didn't want to do. I did not want to get up at 3 am and take people's lives into my hands so to speak. I suspect I might have been a better surgeon than I have been a geneticist. I am actually rather better with my hands than my brain, but in any event I didn't want it. I thought it just seemed interesting. The time I got there, Edwards, Renwick, who had been associated with the place, but wasn't actually ever there.

PSH. He'd gone to Glasgow?

MB. Yes, he'd gone to Glasgow, Edwards had gone to Birmingham, Fraccaro had gone back to Italy, and the people in the unit, Peter Pearson who was a very big influence, arrived a couple of months before I did and the rest of the unit apart from Stevenson himself consisted of Clare Davison, Dennis Bartlett . . .

PSH. Now Dennis Bartlett was doing cytogenetics?

MB. Correct, he was running, he was analysing the chromosomes and there were a group of other, some absolutely excellent technical and general scientific people in the cytogenetics lab, but in terms of independent operators, really Clare Davison and Stevenson and Peter and I were the core of the unit at that stage and we were jolly new.

PSH. One thing that I have been trying to understand is, that it seems that the Stevenson unit was founded with all the right principles in mind and it should have been a most superb unit in terms of human genetics and equivalent to the Galton or Guys, but yet there seemed to be problems with it, and certainly people like John Edwards and I think Fraccaro and others didn't hit it off, and I have never quite worked out so far why that was, or probably the reasons were multiple.

MB. Well of course they had all gone by the time I had got there, so I am not well placed. Stevenson was enormously kind to me and very supportive, and as far as I am concerned he gave Peter and I pretty much what you could ask. He supported us when we asked for it and let us get on with it when we didn't for really quite a long time; he was quite a withdrawn sort of person and could be a bit crusty, but he didn't until much much later on. I never had any difficulty with him at all. I guess the problem was that there wasn't a very strong unifying core of activity. He himself came from a public health background and had made his name doing surveys of disease frequencies in Northern Ireland and the best stuff that he did, which actually stands to this day, was on prevalence of genetic disease, prevalence of congenital malformations, things of that nature, but at the time that the unit was getting established, which was of course in the late '50s, was when chromosomes suddenly sprang into life and so it acquired the cytogenetics activity both as a diagnostic and as an academic activity, which he supported but which was somewhere parallel to his own interests. It got a bit spread between clinical and academic as well, in a way that probably wouldn't happen today. It was a fully funded MRC unit providing all the clinical services that there were and I suppose there just was never quite enough planning, quite enough resource, quite enough strategic thought as to how it would all come together, but it served both Pearson and me very well indeed because we were just able to cream off and do whatever seemed entertaining at the time.

PSH. And I think your first paper was on Down's, am I right? I found one on Down's syndrome in families; with Clare Davison and Stevenson.

MB. Yes, but I think even before that I wrote with Stevenson a large review on influences on human sex ratios, in the Journal of Medical Genetics if I remember, very long ago. So it was pretty wide ranging but

PSH. That is quite interesting that at an early stage, were you then interested in sex chromosomes?

MB. Oh yes, but what was really interesting at that stage, I think the interaction there from my point of view was with Peter, because Peter came in as a botany cytogeneticist, botanically trained cytogeneticist with a general interest, well he'd got a job in a human genetics lab and knew nothing about medicine or people or diseases and I was terribly interested in genetics but knew nothing about that really, and so we sort of leaned against one another and he certainly taught me an awful lot about chromosomes and I guess that I might have let a bit of medicine and disease and phenotyping rub off on him. So I got into the cytogenetics lab, started working with microscopes, started doing my own preparations and my own technical work pretty much under his guidance and under the excellent guidance of a chap called George Clarke, who was probably classed as a senior technician but was an absolutely talented technical handler of these materials, and I think was on John's Trisomy 18 paper if I remember correctly. So those two taught me an awful lot about cell culture.

PSH. So would it be fair to say it was a really well-functioning cytogenetics lab?

MB. For that time and place, yes.

PSH. Did you have much contact then with, for instance, Charles Ford up at Harwell or the folk at Guy's like Paul Polani?

MB. Remarkably little in the early years. A few years later of course I did know all of them because, as you know yourself, a sort of genetics community was forming in the UK and I'd got to know all of those people. I suspect initially it was pretty inward looking and that was also partly the history of Stevenson and Oxford, with whom he had not got on, and so there wasn't any, he didn't make introductions and he himself had very little contact with any of those other groups.

PSH. Because naturally one might have supposed that both being MRC units, you know the Harwell Unit and the Oxford Unit, both being early in cytogenetics might have collaborated a lot, but I guess that's often not the way?

MB. Well it certainly wasn't. There wasn't any formal contact at all that I was aware of. Informal contacts came later.

PSH. So am I right that you were in that unit up to the early seventies, and that then covered the area where fluorescence and banding and the Y chromosome being detectable, all of that happened then?

MB. Absolutely.

PSH. Tell me a bit about that, because that really must have been a kind of exciting phase and I haven't yet seen Peter Pearson to talk about it, but it would be good to hear about it from your angle.

MB. Peter has got to a slightly inaccessible point.

PSH. He's somewhere in Brazil isn't he?

MB. Correct, yes he is.

PSH. Well maybe I will get out there.

MB. You should. I hope I will as well. Actually he occasionally still I believe comes back to Utrecht.   It might be worth trying to make contact with him. Our contact with that was actually mediated initially through a chap called Canino Vosa who was in the Department of Botany, a cytogeneticist in botany in what was then Darlington's department.

PSH. Hadn't he worked with Caspersson and Zech?

MB. Well I don't know that he'd worked with them, but he knew them.

PSH. Was on the papers, some of them wasn't he? Perhaps I am getting mixed up.

MB. As far as I know not. He was on the first papers that came from Oxford with Peter and myself.

PSH. That's what I am thinking of.

MB. But he had got wind of the fact that they were using quinacrine and seeing all sorts of patterns in chromosomes and he got hold of some quinacrine and used it on plant chromosomes, and I can't quite remember who got in touch with whom, but one way or another we came together with our human chromosomes and his quinacrine and started trying a variety of experiments, and I suppose what we saw, it was a time when a) it was hugely exciting and b) a lot of people could see it was exciting, so in a sense all the experiments in retrospect were fairly obvious experiments.   It was a question of who got to do which ones first. They were using quinacrine as a mustard; we used quinacrine di-hydrochloride, which was much easier to handle, and that made quite a big difference, because, although the banding probably in retrospect isn't as good, it was good enough and it was easy to use in the lab. It wasn't carcinogenic in the same way. So we introduced that, we made the observations, particularly Peter made the observations on interphase fluorescence, made observations on meiotic process in man and looked at sex chromosome pairing, and then started working our way into the question of what these banding patterns might mean.

I guess it's curious that all these years later I'm not sure I can say much to myself that I didn't say rather explicitly at the time, which is that it's possibly got something to do with sequence bias, AT rich and GC rich regions and that it had to be mediated. It wasn't seeing the DNA directly but it had to be mediated through some form of differential chromatin or non chromatin protein binding, and I'm not sure that I have much more insight even now. But it was that sort of experiment that led us and lot of other people to start baking chromosomes at different temperatures and subjecting them to different enzymes and so on and so forth, which led to that whole range of non fluorescent staining techniques coming to light and at that stage we were then in contact with the Edinburgh people, in what was by then John Evans' unit, people at Harwell and people all over the country.

PSH. And Marina Seabright at Salisbury?

MB. I knew Marina well, a bit later on.

PSH. Marina is someone I haven't been able to track down, nobody can.

MB. I have not seen Marina for a long time. She really sort of walked away and left no trace.

PSH. Yes, even Pat Jacobs doesn't know where she is living or if she is living.

MB. I have had no contact with her. I have also asked after her once or twice.

PSH. So this would then be early seventies.

MB. Seventies yes.

PSH. Now would it be fair to say that during those years you, and for that matter Peter Pearson, were really involved in pretty basic human genetics, cytogenetics research without much clinical involvement?

MB. Oh, I was doing general genetics clinics right through this time.

PSH. Right, when did that begin then? I mean was this something that was already set up in Stevenson's unit.

MB. Oh yes, absolutely. Clare was doing regular clinics, he was doing some, it just seemed to be part of the deal that I would do some genetic clinics. It was rather free and easy in those days. You know you took a book and got on your bicycle and went. So for most of that time I did a monthly clinic in Swindon and episodically odd clinics in other places as well. Saw some people in Oxford across the whole range, absolutely general intake and did whatever seemed to be doable at the time, which was a lot less professional than it is today, I would have to say.

PSH. At what point did Stevenson's unit close down? Am I right that this followed the usual MRC pattern and when a Director retired the unit closed unless there were some very special reasons?

MB. That's correct. There was a site visit a couple of years before. It closed in the early '70s, '73/'72, '73 something like that, possibly '74. We knew it was closing. Peter took the offer of a job in Leiden. Walter Bodmer came to Oxford and set up the genetics department in the university about 18 months or so before Stevenson's unit was due to close, and he made contact with me as he was coming in and asked whether I would be interested in going to work within his department, because he was interested in gene mapping at that stage and I simply was interested. So I negotiated early release from Stevenson's unit to move with an assistant down into the genetics department in the University. In retrospect I can understand why Stevenson was a bit cross about that. He felt that we were all rushing off and leaving him for the final year of his career with an empty department, but you know we were young and had our own careers to look to, and so we did. We just shot off in all directions. So some sort of deal was struck, because I was an MRC employee at that stage in a unit that was closing, and the MRC was a gentlemanly organisation and I went as MRC external staff into Walter's lab, officially employed if I remember correctly as a research officer in Richard Doll's Regius department of medicine because it had to be a clinical department, and set up a cytogenetics lab in the basement of Walter's department where we looked at the chromosomes of, well a variety of things, but most particularly started karyotyping the interspecific cell hybrids that were being generated by other people then, and there was a whole slew of work going on over several years in which our contribution was trying to understand the chromosome background against which the mapping data could be interpreted.

PSH. Were you involved in the very early human gene mapping meetings? I can't remember what year they actually started.

MB. Nor can I. I went to some, but I was never much of a groupie. I was always more interested in the technical problems of how the cytogenetics worked, and I wasn't that interested in the process of generating and characterising the hybrids which, I mean characterising particularly the HLA stuff which was the dominant part of the early work, and so I went to some but not by any manner of means to all of them. And I was still doing clinics.

PSH. Did you have much contact with Henry Harris, because thinking in terms of that range of work, I guess he was very involved.

MB. He was indeed, although he also maintained somewhat of a distance from the mapping world, because having invented the hybridisation, so to speak, popularised it anyway, he went off on a tack which was much more related to the genesis of cancers and he wasn't much involved in mapping, but we were in and out of his department regularly. We had lots of discussions with people in his department, Peter Cooke, someone else whose name I am just blocking at the moment, who was there doing a lot of human genetics stuff in Henry's lab, then left. We also visited regularly with Charles Ford, who by then was in the botany department on the same site and, irregularly but always entertainingly, with Darlington in the final years of his career.

PSH. What year was it you left Oxford?

MB. I left Oxford in 1981, so having moved from the MRC unit into Walter's department, when Stevenson finally retired a couple of years later, the health authority decided that they needed a clinical genetics department and they established that and they appointed two consultants, Dick Lindenbaum and myself, and I was appointed on a 50/50 basis, so I had a half clinical appointment and the other half was still MRC funded in Walter's department.

PSH. Whereas Dick was fully NHS?

MB. Fully NHS funded. Two things happened at about that time; one was I had a series of discussions with people in Oxford about the possibility of establishing an academic medical genetics department and basically just met with a blank wall. There was no interest in doing that whatsoever. I foresaw the isolation of this little clinical department as being a major problem out on the Churchill site, which was a pretty non-academic site at that stage, but there were just other pressures and other people had louder voices and there was no support for that.

PSH. Can I just ask, in terms of geography were you then split between the university site for your research and the Churchill for your clinical work?

MB. Yes that's correct. I was doing virtually half and half.

PSH. It is interesting, that the two most unreceptive places over those years for academic medical genetics were I suppose Oxford and Cambridge

MB. Yes, and I happen to have beaten my head against both! I know, I can show you the scars.

PSH. Who was it, which sort of groups were so unreceptive, in Oxford at any rate, or was it just the structure of the set up?

MB. I think that the university's most senior people, in particular the Regius Professor of Medicine, at that time Henry Harris by then, and one or two others, had decided that by appointing John Edwards to succeed Walter in the chair of genetics, in a non-clinical chair, they had effectively solved this problem, because since he was a clinician once he was a professor it would all be alright, and I'm enormously fond of John, he's been hugely supportive to me. I think he has made a wonderful contribution but I didn't think it was going to be quite as alright as that.

PSH. No. And who was Regius Professor of Medicine then, was it David Weatherall already or was that before his time.

MB. No that was before his time, that was Henry Harris.

PSH. I didn't realise Henry Harris had ever been Regius Professor of Medicine.

MB. He was in between Richard Doll and David Weatherall.

PSH. I can see how people not really involved with medical genetics might have thought that, but I think if they had asked others it might have been different. So where did you go next?

MB. I went to Amsterdam.

PSH. Were you influenced by the fact that Peter Pearson got on well in Leiden?

MB. Definitely.

PSH. And I suppose the language was no problem for you?

MB. I wouldn't say that. Firstly I really didn't remember any Afrikaans.   I'd never spoken it and I wasn't particularly good at it and it was a long time

PSH. It must have come back more easily than average?

MB. They are more different than you think. But I got on. Of course Peter was the main reason for that. I decided I wanted to go. I started looking around for places that I could go. There wasn't then, even less than now, a huge demand for freewheeling, unqualified, or rather self-qualified human geneticists of no obvious skill, and I had two things that I explored and Holland seemed by far the nicer of the two so I went there. Three, there was the chair that had recently been vacated in Birmingham, which I looked at briefly and I went briefly to look at a possible position in Australia, and I went to Amsterdam.

PSH. And which Amsterdam University was this?

MB. This was the Amsterdam, not the Free university, not the catholic university. What is called the Gemeentelijk university.

PSH. Which is the main university.

MB. The main university.

PSH. And was it a department of human genetics.

MB. Yes it was, it was the one in which Jan Bijlsma had been and in which Niko Leschot was already there as a junior person and it was a tad derelict, I think one would have to say, and it was also in a nice wooden hut rather similar to the one at the Churchill in a very nice part of Amsterdam. I had a very, very good time there. I really did like Holland and I did like the business-like way in which the Dutch did things. It was pretty solid and you had to crunch through it, but when they made agreements they by-and-large stuck to them. They didn't say things without thinking through the consequences and I just found them very, very nice people and I made friends there that I have stuck with.

PSH. Was it clinical at all, or not.

MB. Yes. Definitely. I ran a major clinical department. I had an administrative responsibility for that and after a while I started seeing some patients, but counselling in a language in which you are very halting is a formidable thing to do and I was very circumspect about that, but I was in all the clinical meetings. There was a diagnostic laboratory and I was involved in that and I was deeply involved in negotiations about the financing of the clinical facilities, things of that sort, and started trying to recruit a research department. The department has gone on to terrific things, once they were rid of me, and they moved into this grand new building which I was involved in planning in the new hospital outside Amsterdam, just on the outskirts of Amsterdam, and they have grown huge.

PSH. When did you come back then to Britain?

MB. At the end of 1983, so I was there just under two years, much sooner than I would have liked, but Polani's chair came on the market and, well it was a pretty narrow market as you know only too well. It seemed just too big an opportunity not to have a crack at it and when I was offered it I shot back.

PSH. Really it was almost the only fully purpose-designed human-medical genetics department in the country.

MB. I think that's right.

PSH. Perhaps Alan Emery's as it was conceived?

MB. Alan had something of that and Rodney Harris had an established department, but I mean he was of an age where certainly from where I sat at the time, if I wanted a professorial or senior academic position in a human genetics department in the UK, I could wait a long time if I didn't get that one.

PSH. Absolutely.

MB. So I went for it. And that was a stroke of huge luck. That was an absolutely wonderful thing to drop into.

PSH. Yes. So am I right, you must have been 14/15 years at Guy's.

MB. 13. From '83 to 95.

PSH. And you must have seen it develop really very extensively, even allowing for the fact that it was big when you started.

MB. It was big and it grew bigger, but you know the department was really set up by Paul, it was so sound that apart from inevitable financial higgledy piggledy, it was so sound that the basic structure just carried through. All I had to do was sit there and prevent anything awful happening and the department did it all itself. Of course that was the time at which DNA based things were really flooding into the market and so none of that was in existence at the time. But everyone in the department was keen as mustard and it was pretty easy to just steer everything towards it becoming a very molecular department. We set up a molecular diagnostic facility very early on. I guess that I was lucky to appoint some really good people, Chris Mathew pre-eminent amongst them but not only him, Gill Bates, Iannis Ragoussis, quite a few people apart from the people who had been there initially. David Bentley. There was a very very good group of people in the end.

PSH. Did you have much interaction with Paul himself?

MB. Paul came in all the time. Paul was a regular part of the department, still goes in occasionally but I reached very very early agreement with Paul that he had free access. I did it with care, but I was confident, and proven right, that he was someone who would always be helpful and never be a problem and that's exactly how it was. He would occasionally sort of shuffle into the room and after a few minutes of polite pleasantries he would very tangentially mention that I had really upset someone somewhere and this is what I might do about it. He was just incredibly helpful. Really, really supportive.

PSH. I mean that is really very unusual isn't it?

MB. It is. Mostly keeping your predecessor in the department with you is a really dumb idea, but it was absolutely the right thing to do then. He was wonderful.

PSH. I have interviewed Paul himself. He was the first person I went to see. I was amazed at his vitality still, but also talking with other people, everybody seems to have not just respect for Paul as a scientist and an organiser, but a very real affection going along with it, which seems to have lasted right the way through. It's quite remarkable.

MB. Absolutely. Wonderful man. Wonderful man. A man of deep principle, fantastic depth of humanity, really just an excellent chap.

PSH. And also a very practical organiser, at least in appointing and encouraging good people who would do things well.

MB. I think Paul, this was long before the days of paper-back books on general management techniques, but Paul had a natural flair for organisation, so he produced a structure that had individual, it was a sort of pyramid, it had individual divisions that had their own functions and some grew and some grew a little bit less. It had a reasonable sub-structure. He had a proper secretariat. He had proper books, it was financially sound and it really was just a going concern. If you go back to that department now, with the benefit of my having left it, it's an absolutely stunning place. Doubled in size. Doubled in space. So everywhere I leave does well!

PSH. Well no . . .

MB. Mostly after I leave!

PSH. But Paul did lay good foundations.

MB. Absolutely yes. Those very foundations. Some of the people Paul appointed are still there today holding it up.

PSH. What made you leave Guy's and go to Cambridge?

MB. There were three quite specific things. Two important and one less so. One was I 'd been there for twelve years and I had never been, nor will I ever again, be in one job for that length of time and I thought that I had given it my best, and secondly, I'd become very committed to Guy's as an institution because it seemed to me one of the few places in London where there was a serious teaching hospital with a real academic base and I was outraged at the cavalier and dishonest politics that led to its effective closure as a hospital and as an entity. It was taken over by St Thomas's, an organisation which has very little, with a few honourable exceptions, but just doesn't have an academic emphasis in a way that I simply didn't believe was related to the realities of anything and I was particularly disappointed that the people who I thought should have been really defending Guy's turned out to be the people who were doing deals. So I got fed up with all of that and then I got an offer, and that is a powerful combination.

PSH. Yes, and you were really the first, what I would call defined Professor of Medical Genetics in Cambridge.

MB. That's correct yes. Because Cambridge, the evolution in Cambridge is that there was a very small Health Service operation that Clare Davison, by coincidence, and Dennis Bartlett, by then married to one another, had been running for some years; when Malcolm came down from Glasgow, he was professor of Pathology, which is a huge department as you know, but he established a medical genetics group which was sort of partly within and partly outside pathology, but it wasn't a defined entity and then they established a chair as Malcolm's retirement was coming up. It was a couple of years off, they established a Chair and as you say Cambridge was the second last place in the country, more or less, to recognise that this was an independent discipline.

PSH. But still very, if not foresighted, very wise, that that happened before Malcolm retired rather than to have let it all fall to pieces.

MB. Well I think in fairness, when Malcolm was recruited it was because people in Cambridge saw this as a way of establishing genetics in the clinical school. It wasn't an accident that they appointed Malcolm, and if you were purely looking for someone who was a functioning pathologist you wouldn't have fallen on Malcolm as an obvious one. So I think they had taken the decision to do it and this was a matter of making sure that the administrative flow stayed tidy.

PSH. One of the things, Martin, that you have put a lot of effort into with, I can fairly say great benefit to the community as a whole, is the area of working groups, policy. Two areas stand out for me. Firstly is the area around radiation risks, was that something that you got into it deliberately or did it just happen, because it's been very important, those reports and committees arising from it?

MB. It mainly just happened; as always there was a certain amount of fertile ground, because I had some interest from Stevenson's days in radiation damage and mutation rates, but the way that happened, and I have no idea who caused it to happen, was that when the hoo haa about Sellafield leukaemias first hit the press and they set up the Black working group to look into that and wanted a geneticist, you know, that was at the Elephant and Castle and Guy's is about 15 minutes walk up the road, so I was by far the most convenient human geneticist, and I went on to that working group and I found that a pretty formative experience. It was my first contact with really heavy duty high level policy analysis. Douglas Black was an astonishing person to work with and I was just totally entertained by the whole thing, and the rest flew from there.

PSH. And in terms of what you might call Department of Health policies, you really have had an important role in keeping the Department of Health on the one hand anchored in reality and on the other hand still convinced that genetics is important.

MB. I wonder!

PSH. OK but . . .

MB. I often wish that I was a more effective diplomat and might actually have been able to achieve what you've just said. I think that I often spoke a bit too clearly for the Department of Health and, I believe I was right in what I said, but I don't think that isn't any excuse for saying it. In some senses I suspect that I irritated the Civil Service rather more than was actually necessary.

PSH. But the two reports which stick in my mind are your 'First report to the R & D Group' and then your laboratory services report. It's fair to say without those two reports, things might have gone terribly wrong, because there were other groups pushing in very very different directions weren't there?

MB. Well certainly that 'First report to the R&D Group' as you again know, was well underway when the Director of R & D decided that the rather pragmatic and downbeat messages coming out of this were so far from what he wanted that he was constrained to set up a rival working group, without initially telling me about it, and you are quite right that that working group essentially said that what we ought to do for the next 5 years, which was our brief, was to try to make things like Down's and Fragile X and one or two others of that ilk work properly, because we were some little distance from that and that we should worry about common diseases as and when it became reality. Yes, well the five years have passed haven't they?

PSH. I also remember when those two reports came out, which I think were more or less simultaneous.

MB. Oh yes, they were published together.

PSH. And at the time the Department of Health had that meeting in London, the second report had more or less collapsed spontaneously, at least in the eyes of everybody at that meeting I think, but if it hadn't been for your report being out there with it, there wouldn't have been anything to hold to.

MB. I think a lot of people contributed to and continue to contribute, which I don't now because I'm no longer that welcome down there, to try to make the Department of Health concentrate on health delivery rather than the fantasy of the year after tomorrow. They do love to fantasise and I understand why. Politicians need to fantasise to an extent, and I don't object to them saying fantastic things. I really do object to them spending very large amounts of money when money is a very tight commodity on things that are just not going to work and they get perilously close, well not perilously close. They have just done it again the year before last with the White Paper. I calculate that about a third of the money in that White Paper were spent on things that were complete Never Never Land which is, you know, two thirds was spent on really good things, so one should be duly grateful.

PSH. The other thing along that line, again at the time around when you produced your report on laboratory services there were a lot of people more or less suggesting that classical genetic services should be scrambled up with lab services in general and I think, again I don't think that it would have been possible for the investments from the White Paper to have occurred if the reinforcement of those structures hadn't happened in your report.

MB. Yes. Well I suppose what I would add is that I think, given that the report was there, and that wherever the department went amongst the expert community they were told that the report was right and the fantasies were wrong, they were forced to act that way. It wasn't the report, it was the fact that the community was extremely clear that that was the priority in terms of health care, and that did work through into the White Paper very well.

PSH. It did, yes. I have been asking everybody I have talked to two things. The first thing I'm asking is whether there was any particular person that they can pinpoint who was an especially formative influence, either earlier or later on in their career. Does any single person stand out to you in that?

MB. Two, possibly even three, I have already mentioned Philip Tobias because he really taught me science. Walter Bodmer was a friend but also an exemplar of a really, really serious scientist. Not my sort of person at all. It was just a wonderful place to be, that department at that time. It had such a wealth of talent it was quite extraordinary; and subsequently, I guess I would have to say that I found David Weatherall, as I think many of us have found, David set a sort of example of the sort of person it would be nice to be able to emulate. I don't think I can emulate him because he is not emulatable. Paul.

PSH. Yes. It's interesting in talking . . .

MB. Then I could never emulate Paul. You couldn't even think of it. He is just Paul. I have had lots of heroes.

PSH. It's in a continuing fashion rather than just one at the beginning, too. The other thing I have asked people is whether there is any particular piece of work, research or anything else, that they kind of identify themselves most, you are most proud of, and again not necessarily research, but anything they have done so to speak, that they feel, you know, that was worth doing?

MB. That's harder. I tell you one which is very private to me. Although I am not for a moment suggesting that it wouldn't have happened anyway, I had a brief period as Chair of the Clinical Genetics Society during which, with John Burn, I think we fundamentally forced the BSHG into existence and I was very clear when I started that, in fact when I said that I would do it, that was my goal for the year and it is very rare in my life, because I am not a plotting and structured person. It is very rare in my life that I have had something that I planned to do and it happened. So I put that one up. I don't actually think I have made much of a research contribution to be honest. I think that it has all been, I'm too much of a butterfly to be a serious researcher. I sort of dot from one topic to another, so I have done lots of things that I have enjoyed and which I think have been nice and have given me pleasure, but mostly I think that I have been, I would like to think that I have helped other people do very good research.

PSH. Well that's something worthwhile isn't it.

MB. Yes, well it seems so to me.

PSH. Anything else Martin? Any areas I haven't asked about or that you would kind of flag for the record?

MB. No, I think you've done it Peter. I think you've covered everything.

PSH. Well, many thanks, I will turn off the machine. Thank you very much indeed.