

Derek Roberts

Personal Details

Name	Derek Roberts
Dates	1925
Place of Birth	UK (London)
Main work places	Oxford, Newcastle
Principal field of work	Anthropological genetics
Short biography	See below

Interview

Recorded interview made	Yes
Interviewer	Peter Harper
Date of Interview	02/10/2003
Edited transcript available	See below

Personal Scientific Records

Significant Record set exists
Records catalogued
Permanent place of archive
Summary of archive

Biography

Derek Roberts was born in London, UK and after army service during World War 2 received a degree in Geography from Cambridge University, developing an interest in the biology of human variation. From 1949 to 1963 he worked in the anatomy department with Wilfred Le Gros Clark, undertaking anthropological field surveys in Sudan and forming an informal group of Oxford based workers interested in genetics. In 1960 he spent a year with James Neel at Ann Arbor, Michigan.

In 1963 he joined Alan Stevenson's Medical Research Council unit in Oxford and in 1965 was appointed to develop human genetics at Newcastle University, continuing population studies, notably of the evacuated Tristan da Cunha population, while laying the foundations for medical genetics, including a cytogenetics laboratory and clinical genetics services.

INTERVIEW WITH PROFESSOR DEREK ROBERTS 2nd OCTOBER, 2003

PSH. The thing which had occurred to me first of all is how you came into genetics in the first place?

DR. A maverick!

PSH. Well most of us are mavericks aren't we, and that's one of the nice things about genetics, but people come in from different ways, so I am very ignorant and I don't know quite how you came in.

DR. I am a renegade geographer. I read geography.

PSH. A geographer, right.

DR. I went to Cambridge, and after leaving there, I came into contact with Ronnie Peel, a physical geographer who also had a keen interest in human geography and Margaret Anderson, biogeography and I became aware of the change, the ecological, and so on; so at this time there was a lot of interest in the evolutionary significance of the biology of human variation and Le Gros Clark in Oxford was trying to develop this and he got Joe Weiner from South Africa, physiologist, to work on the physiology of heat adaptation and heat tolerance and so on and he had an MRC unit, a climate and working efficiency unit and he asked me to go to his department to look into the geographical distribution of human variation which I did. Having done this on a macro scale, a world scale, it seemed to me it was necessary to put a bit of experiment into it, or at least observational experiment, so I thought I would go and look at some of the people of extreme physique, the Nilotics, the Nilotics of Southern Sudan, these enormous blokes. So my experimental design was to take people with differing, slightly differing ways of life and seeing if any of their physical differences were concordant with this you see. In other words one of these nature's experiments. Which I did, OK. So the question is, it's all very well finding people living in slightly different, ways and appearing to be different but how do you know that these differences are in fact related to the environment? So you need some sort of a control, so I wanted it to be a genetic control. So therefore I did the blood group studies of the . . . So my initial interest in genetics was as a . . .

PSH. A control that kind of got out of control?

DR. Yes. And this was rather nice I think. And then I somehow got onto the physical anthropology syllabus at Oxford. It was quite nice. Le Gros dealt with fossil man and Joe Weiner dealt with physiological variation and it was left to me to deal with genetic variation.

PSH. Were you in Oxford at that stage?

DR. Yes

PSH. Le Gros taught me as a medical student you know.

DR. Yes. Good. I'm delighted. He was a great man.

PSH. A wonderful man. So would it be fair to say you, at this stage, were on the border line between anthropology and human geography and genetics.

DR. Yes. Human biology yes.

PSH. Human biology.

DR. And then during this time, I was involved in various committees, you remember. There was Arthur Mourant's Blood group reference laboratory and the committee of the Royal Anthropological Institute which was responsible for, don't know quite how to put it, but as a result of their efforts the big Mourant study of populations came out.

PSH. Yes

DR. And this strangely enough this was another compatriot, I say compatriot, of yours, H J Fleure.

PSH. Yes, at Aberystwyth

DR. Aberystwyth.

PSH. I never met him

DR. Lovely man and he of course was interested in the origins of people of Britain and he gave great support to Arthur to his synthesis of the blood group data from the blood group transfusion service, and of course John Fraser Roberts was the Chairman of this committee. Which reminds me I think one of the critical things was, in the development in this country of genetics, was the MRC interest and support for its various genetic units. There was Arthur and Rob Race and I can't remember the name of, do you remember the name of the secretary of the MRC?

PSH. Who would it have been then? This was long before my time. I don't even know who it would have been then but MRC certainly did give their support.

DR. And of course Alan Stevenson at Queen's University.

PSH. Alan Stevenson. What year was it that you started going into this approximately. I am trying to give a rough sort of feel for this.

DR. My Sudan work was 1953-54 and that was the date of the first edition of Arthur Mourant's book, 1954.

PSH. Might I, before we get back to that Derek, because again I am very ignorant. Where did the war come in?

DR. This was all post war.

PSH. Yes so your work was post war, but you were a good long time pre-war, and so did it, like for many people, disrupt careers or anything, or not really?

DR. Yes I think so. I think I would have had a rather different existence if I hadn't gone into the army and various things happened. But it was when I came out of the Army that I went to Cambridge and became academically oriented.

PSH. And your Cambridge degree was Geography?

DR. Yes.

PSH. Right. Because I have often thought that at some point you might have had a medical degree.

DR. No. This was all, if I may say so, picked up by the wayside. Because you see I went to Oxford in '49 and I was there until '63 so that is 15 years in the Anatomy Department and weekly research discussions and reports on, it was every Monday afternoon we had somebody in the Department reported on what he'd had been doing and there was a general discussion by the other people. Oh yes and really a curious background to that was at one stage I had a job with the American War Graves Commission, digging up the American Airmen and identifying them from their remains you see, so I had a fairly practical anatomical.....

PSH. Really, and was that using purely anatomical features.

DR. Purely anatomical yes.

PSH. They didn't try to use any genetic markers back in those days?

DR. No no, there wasn't – this was, 1948 I think this was.

PSH. No, there wouldn't have been much one could do then.

DR. Not a particularly pleasant sort of a job but it was interesting, very interesting.

PSH. So you were in the Oxford anatomy department then from '49 until . . . ?

DR. Till '63 when I went to the States.

PSH. When you went to the States . . .

DR. But in the meanwhile

PSH. Ah there's something before that

DR. In the meanwhile I had met Jim Neel and our interest in population was very concordant and of course he was on abnormal haemoglobins and I was involved in that also and so when I had the opportunity to go to the States for a year, Jim said come to me. So I went and he welcomed me with open arms,

and that year in his department was an eye-opener because it made me feel how much more I was at home in a genetics department. Their work was more relevant to mine, my interest was more compatible.

PSH. I can imagine that. That was in Michigan was it? Ann Arbor

DR. That was in Michigan, in 1960

PSH. How long were you in America Derek?

DR. I was there for a year with Jim.

PSH. And was that on leave from Oxford?

DR. From Oxford. Fullbright Fellowship.

PSH. So you went back to Oxford after that had happened. And what made you leave?

DR. What, Oxford?

PSH. Yes.

DR. I resigned after Le Gros retired.

PSH. That's right, he did then didn't he.

DR. and the department collapsed and in fact – hang on a second
[goes to look for something]

Funny the things you come across when you are tidying.

PSH. Thank you Derek.

DR. A little note from Le Gros

PSH. He was a lovely person

DR. Yes I have great respect for him. And his little poodles.

PSH. Of course I knew him as an undergraduate rather than as a colleague in work, but he was always a very kindly person who took a lot of interest in ordinary undergraduates as well as others. That is nice. That's very nice. That is something definitely to treasure.

DR. I think so.

PSH. So yes the whole department just changed totally didn't it? And I can't even remember who it was that came afterwards.

DR. Jeffrey. Jeffrey, I can't remember his . . .

PSH. It changed from its evolutionary orientation, because Oxford at that time, I have vivid memories of the others there like Alistair Hardy in Zoology and so many people there had an evolutionary way of thinking, which was very stimulating.

DR. Now this has an input actually to our development.

PSH. Good

DR. Because there were a lot – and this was, something happened, the personal relationships that there were between individuals, there was nothing formal. In Oxford there were several groups of us with a genetic interest and we developed a genetic discussion group.

PSH. Oh right.

DR. This was at the initiative of Darlington. Cyril Darlington.

PSH. Who was in Botany.

DR. Botany. That's right, Darlington and then after that came the Harwell people and

PSH. Oh yes. Charles Ford.

DR. Hamerton, Charles was very active and then the Zoology people. There was Phil Sheppard and Kettlewell.

PSH. Kettlewell, yes indeed.

DR. And of course E B Ford was eco-genetics and I think it was he that I learned my formal genetics from. Yes I think I did.

PSH. I remember his lectures to medical students which were a bit bizarre.

DR. Yes, genetics for medical students and there wasn't an item of direct relevance, well at least they were all relevant, but there was nothing of, very little human in them.

PSH. Yes he was a character and a half.

DR. Yes and three quarters. Yes, there was Charles Ford and that. And so as I said we met weekly, Thursdays, and each of us produced a report or a paper or something.

PSH. That's very interesting. I didn't know that.

DR. And of course this is how we kept in touch with developments and we didn't know what Charles had written last week. We got him to tell us about it.

PSH. Well that's a wonderful way. And you probably had Mary Lyon? Or maybe not.

DR. Yes, yes. That's right. I remember her. It was to us that she gave the first report of what became the Lyon hypothesis.

PSH. Really. That was a stimulating period.

DR. Very much, yes.

PSH. So would it be fair to say that by the time that period in Oxford had finished you were firmly wedded to genetics, at least in terms of human genetics?

DR. Yes I think so. I think the turning point came when, the Michigan experience was the one that really you know, you fit there. You don't fit there.

PSH. Yes. So then, did you say it was 1964?

DR. 1963.

PSH. 1963 and then did you then move direct from there to Newcastle or was there something in between?

DR. No no no. In 1963 I went to Seattle. I was there for a year and again it was a nice job. It was the best paid in the University but I just couldn't stand it Peter.

PSH. What was it, or what was it meant to be?

DR. Well it was Professor of Physical Anthropology and it had this enormous salary

PSH. Oh really.

DR. But I had certain requirements. For instance I wanted a secretary. They said yes you can, but you've got to get it. You've got to go and get some money. Of course trying to get money in an American environment then was just as . . .

PSH. Very hard

DR. as ever. Yes and so anyway I left there and I came back to Oxford footloose and fancy free and I went to see Alan Stevenson and I said Alan look, and he said well come here, and I went to him you see, and I was at the Oxford unit, because he had brought the Queen's University unit to Oxford

PSH. Yes so he was in Belfast first and then formed the MRC unit at Oxford after that?

DR. No he had the MRC unit in Belfast

PSH. I didn't know that

DR. and then he moved to Oxford and re-established the unit there. And it's important from the point of view of the genetic work because he was doing his, you see it fitted with his genetic epidemiological approach. It fitted into the general ecology of, and he was always, he was very pro the little Thursday meetings and as I had known him sometime because I knew the social medicine people quite well, Alice Stewart in Oxford, don't know if you remember, and in fact Alice was my joint investigator in the Sudan.

PSH. Right

DR. This was a lovely example of the additional use of funds. We were funded by WHO to go and investigate the causes and progress of an epidemic of kala azar in the south and in the process of doing that of course it was necessary for me to make some examinations of the long and lanky people and take some specimens from them, blood specimens and that really was, because the only way I could get those specimens to London was to catch, have my thermos flask on the air strip at Malakel to depart at 6 o'clock, OK? That meant to say that I had to do the bleeding at dawn which is ruddy difficult.

PSH. Yes

DR. So anyway we did it and went without a hitch. And the third leg in that group was Dean Smith in Khartoum who was responsible for making sure that the specimens that came up from me to Khartoum in fact went on their right way and not out to Japan or Wogawoga or whatever, and Khartoum at that time was the sort of parent administrative body so I paid all my money into Khartoum and they were responsible for signing the chitties for whatever. But it was nice.

PSH. Right.

DR. OK. I got side-tracked a bit.

PSH. Don't worry

DR. So Alice Stewart and Alan Stevenson. Right.

PSH. So can I just ask, one of the things which has always intrigued me, you have worked on a huge number of different populations, including a good few islands, and I was never quite clear which ones you had been to and which ones had come in, so to speak, for analysis?

DR. Yes. Not quite like that. My field work has been essentially Africa, there was the Sudan work, there was Borneo, Cameroons and then Nigeria. And these were in the final years before leaving Oxford, but then when I was back in this country, I knew Otto Edholm again, the physiologist and in 1961 the Tristan population arrived and the MRC set up this working party to look into the health of the Tristans. The secretary of that was Harold Lewis and Edholm was very active. By this time the curious combination of things that was occurring in Tristan, they thought they ought, so they asked me to look at the population genetics.

PSH. Right.

DR. Which is what I did and there were a number of interviews. I didn't go to Tristan, I wish I had. In fact I might go there on one of these round the world cruises one of these days. But I collected together all the material there was on them, in fact I will show you.

PSH. Remind me again, what year was it that Tristan cropped up?

DR. 1961.

PSH. '61. So you were you still in Oxford then?

DR. Yes but it was after I had come back to Oxford in '63 from Seattle, that I was formally involved in this.

PSH. I am always intrigued – there were several other islands which have cropped up over the years, but I think most of those were a bit later were they?

DR. No, the Canaries, those were early '50s and Canaries, Madeira. Again Madeira was '52 I think. Because in Oxford there was a steady stream of earnest undergraduates who wanted to go and do field work of some sort and as soon as I could provide them with the fare they were quite happy to go and collect things and this is of course, what we don't have up here in Newcastle. We've not had . .

PSH. No.

DR. I have never been able to make contact like that.

PSH. No. And Yugoslavia, the various islands there?

DR. Yes. There was a seminar in Sicily, I think it was in 1973 and it was organised by Gabriel Laskar at Erice, lovely little place in the north west of Sicily, and most of the up coming youngsters from Europe came to that seminar and amongst those was Pavel Rudan from Croatia, from Zagreb, and we talked over what could be done on Farr which is his home island and so there was this series of investigations on all the islands of the south Dalmatian coast. It's lovely Peter and . . .

PSH. I have never been there.

DR. about 3 or 4 years ago they wanted to set up an international institute of physical anthropology. They wanted to do it on Farr you see. And they asked me to go and be the director. So Mary and I went, I have been there several times already on field research but Mary went out to look to see what it was like, and then we felt perhaps it is not really the best place to go and live on a remote island when you are likely to have to call for an ambulance at any hour of the day or night and so we decided, at least it was out of our hands,

because on the way back we had to go to Split to get the plane and that was the morning of the outbreak of the war.

PSH. Oh dear.

DR. And there were these troops wandering around and that really put the kibosh on it. We didn't really fancy going

PSH. I can imagine. What year was it Derek that you went to Newcastle?

DR. Right, 1965. This was as a result of long negotiations between the University, this was Donald Court and Charles Smart, and the hospital board, because individuals in both saw that there were problems that were likely to arise and one of the problems was a misrecommendation to an achondroplastic family, I think, and they realised that they really ought to have somebody who could speak with a little bit of authority on these things and so they advertised. I applied for it and I got it, which I was quite pleased about because in the first place it gave me a job which I, a rather more permanent job, which I didn't have then and secondly I was stationed up in the north during the war and I liked it and I had always thought should the opportunity arise I would be quite happy to go up there. So it was 1965 we came up . . .

PSH. So these are the file cards on Tristan. Isn't that fascinating?

DR. There you are Peter

PSH. Thank you. If you will trust me just to glance without re-arranging. That's wonderful. This is a pretty unique record Derek.

DR. There is a card on every individual.

PSH. Every inhabitant of Tristan

DR. Whoever was, yes.

PSH. Whoever was in the past. That is a unique record isn't it. And something that is going to be very important to ensure that it is preserved because

DR. But who and how. Nobody wants them Peter. I am going to send them down I think to South Africa to Himla Soodyall because she has been doing the

PSH. Yes, Himla of course would be a superb person to work on them. And maybe that is something we could come back to at the end. The question of archiving is one of the things which our historical project is trying to get official bodies to do, because it is a very big issue.

DR. There is so much material collected.

PSH. Can I just ask then, in terms of Tristan, I was looking through, I did a sort of search on publications and I saw several on Tristan. Did you ever write a book on Tristan in the end, or did you bring it together?

DR. No it's just a series of articles, that's all. I've not put it together in any form of a book. I have thought about it Peter but got side-tracked and . . .

PSH. Well don't we all. Because I've never seen anywhere what you might call a full account of everything to do with Tristan, maybe just because I wasn't looking in the right place, but it must have been a fascinating time.

DR. Yes. Yes. and of course it gave us this new concept of, instead of constant Hardy-Weinberg equilibrium, the jockeying for position of different genes

PSH. That's very interesting and I must go back and read some of the things. I really must because

DR. Well the most important one is the one in Nature, 1967 I think.

PSH. OK.

DR. About the pack method.

PSH. Yes. But this is an amazing record. To have a complete genealogy and record on file.

DR. Yes. You see you can't put it on a diagram. You can't put it on a family tree.

PSH. No you can't

DR. It is too complicated. Anyway, once this was going then we got the Administrator on Tristan sent me every year the new births and deaths and arrivals and departures and so on, and since the then Administrator has retired, I have got these details from the Tristan weekly newspaper which gives births, deaths and marriages. So this is really right up to date except for 2003 I think it goes up to.

PSH. Heavens. That's up-to-date enough. And the newspaper is produced there?

DR. Yes.

PSH. You've got your records terribly well organised Derek. Ah, there we are Tristan Times, 'Queen's Day 1985'. That's interesting. Which queen were they referring to do you think when they mention 'Queen'?

DR. Victoria

PSH. Victoria. 'Celebrated on 16 January despite the weather being cloudy, fairly cold and threatening with rain' That must be pretty typical. Well I'm

blowed. Isn't that interesting. It really is. You know that the boat to Tristan goes from Cardiff.

DR. Oh does it?

PSH. Well it goes to St Helena regularly and then sometimes it goes on to Ascension and Tristan, not every time.

DR. Yes that's interesting. There's the result of the ratting contests and . . .

PSH. That is something which needs a good leisurely read. Isn't that interesting. I have never seen that before. That is absolutely fascinating. So how many people on the island were there at this time? I have forgotten.

DR. 267 in 1960. And I think, in fact, once I was in Newcastle and I had the facilities of computing there, I got all the Tristan data on the computer and so all I have to do is I suppose is find the lady who did it for me and get her to get it off. So we see for the Tristan people, the present day population and the recent population, we've got all blood groups and enzymes, all that.

PSH Very interesting

DR. It's pretty good Peter, but it's a pity to see it go. When I left the Department the MRC didn't want it. The South African MRC didn't want it, so I just left it and John Wolstenholme I think decided where to send stuff, whether he sent it to the asthma lot or not, I don't know.

PSH. Yes. One of the things Derek, which I wasn't sure how you started with, was your work with in Orkney and Shetland and MS in epidemiology. How did that sort of crop up?

DR Yes again, first of all in the early days here I was you know, looking for a theme and so I wanted to deal with conditions of complex aetiology you see, and I spoke to our then Vice Chancellor, of never-to-be-forgotten fame but whose name I have mislaid for the moment.

PSH. Oh Henry Miller

DR. Henry Miller that's right. Well he said would you be interested in multiple sclerosis because the Multiple Sclerosis Society has got a lot of money. So I put in a grant application and I got it. When it was renewed I got a bit of wiggling for, what was it for . . . something about it would have been nice if you had mentioned the Society.

PSH. Oh yes.

DR. Anyway this grant continued from year to year OK, which was rather nice. And then at that time our friends at Harvard decided they wanted, Poskanzer came over here to spend I think a year in the neurology department here, and while he was there he worked on multiple sclerosis in the north east and then he also learned of the high incidence in Orkney and Shetland and decided to put up a massive grant application. And this was at

the time when the HLA system was just developing. And we had an HLA lab there in our office but in those days you didn't do it with antiserum.

PSH. A particular haplotype.

DR. Yes. Then you could match all his

PSH Yes

DR. That went very well. We did the first study on Orkney and then the study on Shetland and then a study the north coast of

PSH Of Caithness?

DR. Of Caithness. Well around there.

PSH. I have always been interested in those islands because, well I suppose like all islands they are fascinating. I remember myself doing field work up there with Bernard Kettlewell when I was at Oxford.

DR. Did you? Butterflying?

PSH. Yes trapping moths and that's how I met up with Sam Berry among others. Along with this Derek, I've seen quite a few papers that you have written on the demography of Northumberland itself. Was that something again that mainly happened when you were up here?

DR. Yes in the summer and you can use your eyes

PSH And of course would it have been at that time that Eric Sunderland was at Durham?

DR. He was at Durham. Yes.

PSH. Yes, because he has been a good friend and colleague of mine and any excuse to do anything in Wales, he would always send some students down.

How about the medical side Derek? I get the feeling that at the beginning it was mainly human genetics and anthropology, but when did the what you might call, the more medical side start to develop?

DR. After I came here, and I forget who it was, said to me, this rather nice lady's little boy has been diagnosed with haemophilia would you like to talk to her. At that time I had a little room up in the RVI, up in the attic, so I said yes. And of course I had seen this at work in Jim Neel's department because they had a very extensive genetic advisory service.

PSH. Yes, but it was just yourself wasn't it?

DR. It was just me, yes, and this was difficult. Because there were at that time there were one or two of the older generation who believed that nobody without a medical qualification should be allowed access to patients you see.

Anyway we got over that but it was difficult Peter. We got over this by asking whoever was the patient's consultant in charge, to sit in.

PSH. Yes

DR. But you see how the department was developing. I was developing the labs to apply the techniques that had relevance to clinical situations and to population genetics. My first lab of course was the blood group lab, I've mentioned that. And from there we went to proteins and enzymes and from there to the HLAs and it was a steady progression. Right, then having got this little, we had got a nucleus, a centre where any results could be applied, and then from there, there was one or two of our colleagues from outside were interested and I set up the peripheral clinics you see.

PSH. I see

DR. The first one, the first really interested one was Carlisle. Now what was his name, Mike . . . and then the next one which was at my instigation was down on Teeside.

PSH Yes.

DR. And by this time it was clear that I was doing this on, the regional board was clear that I was doing this on sufferance and the expectation that there would be a consultant clinical geneticist one day. And so I was quite happy to go on and my argument for using these facilities was that while I am giving a lot of time to this clinical work, I think it's fair that I should be, oh incidentally they had given me honorary consultantship, that was the first thing and it was Donald Court who really guided me. So the system, he set up a committee of acceptable people who decided that it is useful to do so and so, and you produce a report. It was an interesting time Peter but I wouldn't want to do it again I don't think.

PSH. No. At what stage did you start cytogenetics?

DR. At the time of the boom.

PSH. When was that? That was before my time, I know that but . .

DR. Yes we had it going before 1970. It was the end of the 60s. It was the second lab to be developed here, cytogenetics, and that's right, and Pippa [Bulson], was a genetic student who was interested and she came and helped me set up and when Pippa left I had Val Davison come up from Surrey. And it was this time of course that we began to see the development of the banding work; I think 1970 wasn't it?

PSH. It was about that, yes. So was the cytogenetics set up more as a sort of medical lab from the beginning or did it start like the others as being anthropological?

DR. Started like the others. Yes I was interested and it seemed to me that it ought to be possible to identify populations not by gene frequencies but by

frequencies of particular chromosome . . . If it were possible to identify different types, of course you got the banding and . . .

PSH Yes.

DR Actually that was

PSH Sort of late 60s early 70s I suppose it developed. Because I came back from America in 71 and it was while I was in America I remember that the banding suddenly became recognised and everyone was very excited. Yes.

Derek one thing I was wanting to ask you which has always intrigued me, looking through the things you have written. Nothing you have ever written was anything much to do with eugenics but you had a lot of contacts in the early days and organised a lot of things for the Eugenics Society, and I know it was not easy going, but how did that happen.

DR. I had no scruples. In the first place, our great ones before me had been active in the Eugenics Society. Fisher, John Fraser Roberts,

PSH. Even Penrose.

DR. Penrose of course yes. Now Penrose is a man who I admired very much. We were on several committees together and the moment the committee work started he fished in his pocket and he brought out these little cards and started doing his calculating. I am sure he did all his arithmetical work in . . .

PSH. Yes he's . . .

DR. He wasn't very active I don't think, by this time, in the clinical area. He had packed up in Essex hadn't he, when he came.

PSH. Well that's right. That again must have been the beginning of the '70s I suppose when he retired.

DR. He was at University College well before that.

PSH. Yes from the end of the war really wasn't it. Yes. But I never quite understood, in terms of the Eugenics Society, it always struck me that on the one hand, it did some good, scholarly, very impartial work and helped sponsor that in population biology and then it seemed that there was a strange group of people rather in the background who were always just a bit below the surface and I never quite fathomed it, but all I know you edited a long series of very valuable books and conference proceedings for them.

DR. Yes that's right. As a result of my experience here, I could see the general nastiness of too solid a . . . From the ethical point of view I felt that so long as one tied oneself to the ethics of medicine then there was no problem. Or I should say the problems diminished.

PSH Yes

DR. But I could see no eugenic ethic

PSH. Well no.

DR. Not here for the misapplication. I mean I like the word eugenics. It means exactly what it, what the derivation of its etymology says but there has been so much trouble. Anyway I went into the Eugenics Society a) for money because I knew, again from Alan Stevenson, that they had a little research grant with some, the way money has guided our efforts. It's terrible Peter but anyway I got some help from time to time, namely in the provision of a secretary. My secretary for several years was provided with money from the Eugenics Society. And there were some good people. People like Peter Cox who was the Government actuary. He's retired for some time now and he was responsible for the series of conferences on inequalities.

PSH. I remember those. Some very good pieces of work.

DR. Yes.

PSH. Then I remember there was an awful to-do. I remember when you were trying to persuade them to change their name. It always struck me that they changed their name but they didn't entirely change their character did they.

DR. That's it yes. No there was a possibility. There was a niche for a society dealing with biosocial science.

PSH. Yes. In terms of thinking around biosocial science, that was a journal. Didn't you edit it for a long time?

DR. Yes. This was founded by Alan Parkes and I edited it I think for ten years was it. Something like that. I took over the editorship from Alan Parkes in 1978 and relinquished it 1990 when I retired. I gave it to Mascie-Taylor, Cambridge. They are still going strong.

PSH. Well that's good. Nick Mascie-Taylor is a very able person isn't he?

DR. Absolutely yes.

PSH. Apart from the books you have edited for the various associations, are there any actual books you have written yourself Derek?

DR. Yes there are.

PSH. I feel ashamed that I don't know.

DR. I should have done it Peter but I haven't. (Goes to look for book)
There you are Peter. A small thing, but my own.

PSH. That's lovely to see that. Do you know I've never seen that.

DR. It is in fact a classic and it is the

PSH. Derek that's very very kind but I do hope you have several copies.

DR. I do have several copies and I will just write on the beginning of it if I may.

PSH. Derek I am very touched, very touched indeed. Because it is something I didn't know about and so thank you. That is Climate and Human Variability. Thank you. I'll tell you one thing which is relevant Derek, that I am trying to do at the moment is to get together a British Human Genetics Library. Historical Library of classic books and it's growing quite well and what I am hoping is that maybe with the help of Wellcome Trust and our University we can have a core which would ensure that really we have as far as possible a complete sort of record of the books in the field, which I think would be rather nice because. But that is early days for that.

Derek I have very very nearly finished. And you have given me a lot of time. I suppose one thing I was going to ask you is who do you think in your career has influenced you most, or who strikes you as the sort of person who has really either influenced or made most mark on what you have done? What would you feel? I suppose it doesn't have to be just one either.

DR. No. I think probably Le Gros.

PSH. I have got one last question and that is looking back on things, which of your pieces or areas of work do you feel proudest of?

DR. I think the Nilotic work, because that was very very difficult.

PSH. I can imagine that. Yes.

DR. I was 100 miles from the air strip and I had a tent and I had a cook and I had a driver. I selected a village as being centre of the tribal area and put my tent there and I went out day after day to the other villages measuring and photographing and bleeding. One of the things that I was incorporating in my survey was PTC testing you see, and you know you have an array of different dilutions. OK. Everything went well for the first and then I came back from a trip one day and I saw that the table on which these things were, was slightly different. And from that time onwards I couldn't get a reliable measure and I enquired why? Well apparently my cook had run out of water and since the nearest well was 7 miles away and he didn't care to go, he had helped himself out of the little bottles and then showing remarkable ingenuity he filled up the bottles with water from what he had got one of the women to get for him. So all the dilutions were wrong. The delights of field work Peter.

PSH. Did the people drinking the water at mealtimes complain or did they not have PTC in it?

DR. I don't know what happened, all I know that life is so difficult there. So every woman has to go 7 miles to fill up.

PSH. And sadly it's probably not improved much.

DR. The Government put in some hafias which, you take a bulldozer and you scoop out a big trough like that, gentle access must be at the sides and this accumulates rain and dew and of course the cattle go into it and the people go into it so it's not water of the best quality but it is at least water, so that was the way they were getting over it then.

PSH. That must have been fascinating work.

DR. Yes, it was the most difficult one and to persuade these poor people I wanted to take some . . . But I know it was successful because one of the specimens that I sent back to Arthur, he wrote to me to say this child has ovalocytosis. Can you get me some more specimens, so I went back in and got more specimens. And at least I came home alive which was . . .

PSH. Yes you did. Derek thank you. Is there anything that you want to say that you feel I haven't brought up in terms of an important area or anything else before we finish that you feel you would like to bring up?

DR. I think the way in which things developed from personal contact and you know there were these curious little groups I have told you about earlier about the Oxford group, and then there was Cedric and John and all the folks who came through Great Ormond Street. And it was because, I think, these personal contacts were established that it was relatively easy for the Clinical Genetics Society to develop, again thanks to Cedric's foresight. . .

PSH. Yes, it's gone a long way hasn't it.

DR. Yes

PSH. Yes it really has. Well Derek I think I have tired you enough. I am going to press the switch off button.

(end of recording)