Antony Searle

Personal Details	
Name	Antony Searle
Dates	
Place of Birth	Belfast
Main work places	Harwell UK
Principal field of work	Radiation and comparative genetics

Short biography	Antony Searle spent most of his career at the Medical Research Council Radiation Biology Unit, working on a wide range of radiation genetics topics, as well as on genetic imprinting and comparative mammalian genetics

Interview	
Recorded interview made	Yes
Interviewer	Peter Harper
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INTERVIEW WITH DR ANTONY SEARLE, 11/10/2004

PSH = Interviewer (Peter Harper)

AS = Antony Searle

PSH **I am talking with Tony Searle at his home in Abingdon, Oxfordshire. Tony, might I go right back and ask: Did you come from a scientific or medical family at all?**

AS Yes, scientific certainly because my father was a botanist. He worked for a time at Cambridge with one of the professors there just before the First World War and after that, and I don't quite know what the work was; after that he became a great expert on flax research in actual fact, and he worked as one might expect in Ireland, based outside Belfast. They founded a research institute in flax and of course linen. The linen comes into it very much. The Linen Industry Research Association was founded at the end of the war I think, and after that he became chief botanist at the Association. He did a lot of work on flax breeding. A number of very well known varieties were established there. As a result of that he moved to Norfolk in about 1936 or so, because before the war the King, King George V, became interested in flax growing and in fact that was of great importance during the war and I wrote a book about my father's part in that period. But that's by the way.

PSH **That's interesting though. And can I ask, moving to Norwich, were you at school still, when you moved to Norwich or was that after that?**

AS Sorry it wasn't actually in Norwich, we lived near the Royal Estate at Sandringham, in fact we lived at Flitcham, but I went to school at King Edward VII Grammar School in King's Lynn for a number of years, in fact I was there until shortly after the outbreak of the Second World War.

PSH **Did your father have any contact with the John Innes Institute or hadn't it moved to Norwich at that time?**

AS Yes, I think it had. He had some contact.

PSH **So was your father's interest the factor in pointing you towards science or do you think you would have gone into it anyway?**

AS I think his influence certainly came into it, because originally when we lived in Northern Ireland I remember he persuaded me to join the Belfast Naturalists' Field Club as a junior member and I used to exhibit my aquarium with water beetles and things like that and get prizes even, junior prizes for my water beetles. And then I think he was very pleased that I did go into that. But I was always extremely interested in wildlife, natural history and so on, and continue it to this day.

PSH **And then where did you go to university?**

AS I went to University College London and the reason I went there, it may be of some interest, was because as a prisoner of war, I was a prisoner of war for 4½ years in Thailand and had to work on that blinking railway . . .

PSH **Oh, maybe I've skipped a bit in that case, because did you go from school into the Army then?**

AS I did, yes.

PSH **You must have been I suppose in late teens or early twenties when the war broke out?**

AS That's right, yes, I mean I was still at school when the war broke out and I left school in, it must have been the summer of 1940, and joined up at that stage. I decided to join the artillery and I qualified as a battery surveyor in actual fact, putting the guns in the right place and so on, 25 pounders and so on. We left the country and were supposed to go to the Middle East to fight in the war going on there but then the Japanese invaded Malaya and so we were diverted to Singapore and were only there about a month before surrender to the Japanese.

PSH **Oh dear, so that was not a good time.**

AS Not a good time, as I wrote in my latest book. So yes, now where were we.

PSH **So were you saying you encountered Penrose in Singapore?**

AS No, no. I encountered Ronald Searle in Singapore, you know the cartoonist. He was out as a prisoner too. No, what I was trying to say was, that when I was a prisoner of war, of course there was lots of discussions and as one would imagine there was essentially a communist group there, and having been brought up in a strict Conservative background, my father was a member of the Constitutional Club and that sort of thing, it was all new to me. So I learnt all about communism and the name that kept on recurring was J B S Haldane.

PSH **Yes.**

AS You see he was at that time Editor, not Editor but he wrote a lot for the Daily Worker and very good popular articles on science. I had been down to go to Cambridge where my father was in Emmanuel College, but I decided I'd go to University College London where Haldane was and I never regretted it either. It was a wonderful place I think. But at that stage when I went there it was very badly bombed.

PSH **So when did you arrive there? It must have been . . .**

AS I was very lucky because, of course the capitulation of the Japanese was in 1945, in, I should remember but it was about September '45, and I had been rather ill, not malaria.

Mrs S You did have malaria.

AS I did have malaria but ……

Mrs S Amoebic dysentery.

AS Amoebic dysentery, that's right yes, and so I was one of the earlier ones to get out really. Actually we flew out from an airfield we had constructed ourselves. Luckily we hadn't put any booby traps in it. We flew out from there and very luckily I was able to start already that year at University College because of this further education and training scheme that was started for ex-servicemen. So that was wonderful and I decided, at first I thought, Haldane, oh yes he's a professor of biometry; I thought well, I will go into biometry - that's the thing obviously. But it wasn't the sort of subject which could be a degree course. I quickly found that, so I did zoology and thought this would really be the subject for me.

PSH **Of course at that time there was a wonderful collection of people around University College.**

AS Yes, you are right. And that did include Penrose, though I encountered him more when I was a post-graduate student actually. He and Haldane and a number of others. It really was a great period to be there and I was lucky to be there and with Haldane, he was of course a very odd character, an eccentric character. I could tell all sorts of anecdotes

about him. He also was very kind, especially to students and I think because I was ex-service. I sometimes think the reason I got a first was because he sympathised with me. But anyway I approached him because I thought I was a bit old by then, you know I had these years of being a POW and so on, I thought I really ought to get started on some project as soon as possible. And he suggested it would be a very interesting possibility to study the population genetics of cats because of the fact, and this is where the Lyon hypothesis comes in, they had a sex-linked gene you see for yellow or orange coat colour, and the heterozygote could easily be distinguished because it was a tortoiseshell you see, and so he took me out and of course I knew nothing about the genetics of cats and we went around local streets near

University College London and he would tell me tabby, striped tabby, blotched tabby and so on and of course tortoiseshells and various other easily distinguishable visible genotypes. And then after that I had the job, that's where Susi comes in, I had the job of examining hundreds of dead cats you see, which had been killed by the various wildlife . . .

Mrs S Rescue centres.

AS Sorry. Rescue Centres yes. So called animal rescue centres who had the job of killing off all the stray cats.

PSH That's a good sample.

AS And as a result I was able to show, and this is my first paper, "Gene frequencies in London's Cats" and this is 50 years, no, more than that, 55 years ago, published in the Journal of Genetics. And that was as I say almost due entirely to J B S Haldane.

PSH That's wonderful, because I had it down to ask you, how did you get into cats when most people with experimental models were using mice.

AS Yes, that was it. And since then, that was about one of the first papers and as I say, it was one of Haldane's ideas, one of the first papers on this sort of thing, mammalian population genetics, and so lots of people all over the world started looking at their cat population and it was able to show that they mated more or less at random which, you see, certainly it wouldn't be so if you tried it on dogs of some breed or other.

PSH No.

AS But with cats they are, and I quote Darwin, their notorious sort of, can't remember what the quotation was now. Anyway . . .

PSH And was this a paper you wrote as an undergraduate then or had you sort of . .

AS I wrote it when I was still an undergraduate, yes.

PSH Was it a small group? Because these days you have hundreds of students.

AS Well yes, after all he lectured to us, yes. I mean the department of, well it was biometry, eugenics and genetics or whatever it was called, altogether the department of course included Penrose and his group and Haldane didn't have many research students actually. Maynard Smith is the one who was best known. You see I decided, I felt rather dubious of actually trying to do a PhD with him because he is a funny character. I felt a safer bet was Grüneberg, you see, the great mouse geneticist who had already written a book about mouse genetics, which amazed me that so much was known on the genetics of mice. And so I decided for that reason to work with him, but the most people in a group I suppose was the Penrose group. It was much bigger and we used to meet up with them and various geneticists, the ones with Grüneberg and so on, we'd all meet up together. So it was obviously a very good group to be in with lots of interesting people

there really, and so then I did my degree on zoology with genetics as a special subject and as I said, it was as a student I was able to do this survey really and of course it was quite a business for my wife.

Mrs S I wasn't your wife then.

AS No, you weren't.

PSH But you were on the scene anyway.

AS Very much. She was at Imperial College actually so . . .

Mrs S Doing zoology.

AS Doing zoology too you see.

Mrs S Parasitology; ectoparasites of dead cats arrived on Tony and no one told me. When he used to visit us at the weekend in Sunbury where my parents lived we had to shake him out before letting him in.

PSH And you could identify the parasites.

Mrs S I didn't like it.

AS Pulex irritans.

Mrs S I was more an endoparasitologist; anyway if I put up with that I put up with all sorts.

AS Sorry?

Mrs S You were due to study mathematics.

AS Oh I don't know . . . yes I might have been. Oh yes I think I was, yes.

Mrs S And you only came to zoology because you wanted the biology course connection and then with Haldane it was the biometry and he started you in genetics partly didn't he?

AS Yes, oh yes.

Mrs S Because of the cat business so . . .

PSH Can I ask, did Haldane's politics get in the way of his teaching at all or did he keep them rather separate?

AS He did very definitely. Of course eventually he was driven out of politics, communism, because of Lysenko and all that business.

PSH Well yes. A strange story.

AS It took him a long time. It was horrible. It must have been ghastly for him because he was a . . .

PSH As an undergraduate it seems that he really did give a lot attention to any student who was really showing interest in the field like yourself.

AS Oh I'd say yes definitely, and of course Maynard Smith did do his PhD with him successfully and he had other PhD students as well I think, but Maynard Smith was the only one I knew well. But he certainly did. He had altogether, goodness knows, he used to boast didn't he that he had over 20 of his students that became FRSs. I wasn't one of them but anyway. I was his student but I didn't do my degree with him.

PSH And did you then go on to a PhD with Grüneberg?

AS With Grüneberg, yes, on the skeletal variation in mice. It was very interesting and that introduced me to the business of threshold effects because it was quite clear that some of the effects while studying the variants, most of them in fact were really thresholds.

For instance there is one I know which is very much of importance in man too, the third molar. The third molar tooth is sometimes present in mice, always very small and sometimes in particular strains it is absent in all of them or various percentages of them don't have molar teeth, third molars at all so that is what started my interest in threshold characters.

PSH **And what was your specific project as a PhD?**

AS Well I can show you the actual volume if you like, 'The Study of Variation in the House Mouse' I think. Would you like to see it, I can get it for you?

PSH **I will look at it afterwards Tony if I may, when I have turned everything off. I would love to see it.**

Mrs S The third molar is the equivalent of our wisdom teeth.

AS Yes wisdom tooth, yes that's right.

Mrs S A good example.

AS Yes it's such a bore for the animal or people that have them, these threshold and vestigial characters. Terrible bore really especially that one, wisdom tooth business.

PSH **So the actual title of your PhD was quite broad was it, rather than just a specific morphological change?**

AS Yes, quite broad. I was studying all sorts of different variants, they were skeletal really but there were a wide range in the skeletons and so on and especially the question of to what extent they were influenced by maternal age, say, or litter size.

PSH **Grüneberg is another fascinating person and I remember meeting him when he was quite elderly, but what was he like as a person to work with?**

AS Well really quite straightforward. He was very . . .

Mrs S Pedantic.

AS Pedantic, yes that's the word. He was very pedantic generally and he of course lectured too and he was very keen on pedigrees, working out how particular mutants led to the end effects and so on. This famous one, for what he got his FRS I suppose, was on the grey lethal mouse, that was one of them. He studied things in the rat so and so. But anyway I think he was much more straightforward to work with than Haldane would have been really. You knew where you were. It was very much his own field, so he was extremely interested in what one was doing and followed it up and encouraged one very much. So it was very straightforward with him I would say to work with. But you knew at the end you would get some publications out of it. He had all these mutant mice he was studying and one had a chance to discover some mutations for oneself really, work on them as well as the main theme.

PSH **What would you say was the most exciting thing which came out of it from your point of view, from the point of view of mutations?**

AS Well I think perhaps the most interesting mutation came out of it was, I noticed this particular member of a litter which had an odd colour and was behaving oddly, tending to circle round and round. No sorry, it wasn't particularly a circling mouse, but it didn't last. It died quite young about weaning age, so that was the so-called grey lethal, sorry dilute. The dilute lethal mouse, a lethal allele of the well known dilute mutation which of course is found in a lot of different animals really. And that was particularly interesting because shortly after that the same mutation was discovered in the States at the

Jackson laboratory by Lee Russell, W L Russell who I got to know very well and I remember he came. Of course Gruneberg knew him well because . . .

Mrs S I thought it was a 'he', Lee Russell.

AS I'm sorry, I meant Bill Russell, sorry, of course they were both working there and Lee was very bright indeed. Any rate Bill Russell came to Gruneberg's lab when I was there. It must have been, I don't know, early 1950s. Yes. So when was this?

PSH That was '49.

AS That was 1949, that the lethal allele . . .

Mrs S '51.

AS Something like that, probably '51 when I was a post graduate student. So I had a chance to meet him and liked him very much and I described this new mutant I'd discovered and he said "Oh we've got something similar at the lab at Oak Ridge" but he kindly let me publish it, good of him. So I wrote a paper on this lethal allele of dilute, and it was a very interesting mutant and certainly has now turned up a number of times in the various stocks, mouse stocks and also, I can't remember now whether it is known locally in the human genome or not but it had these pleiotropic effects on the inner ear and actually lethal as I say, has a lethal effect. But I think that was certainly the most interesting thing I found and of course that was also of particular interest because of the fact that it sort of led on into radiation genetics. Of course it certainly was very much one which was a radiation induced - mutant.

PSH Can I ask at that time, I was looking at the computer based publication lists, which only go back to 1950, but I see there is one on amino acids with Harry Harris?

AS Oh yes that's right.

PSH Now was that done while you were with Grüneberg or was that quite separate?

AS No, that was when I was with Grüneberg actually, yes. I am forgetting all about that one. That was rather funny because we used his stocks of mice, I used to take urine from various mice to run these . . .

Mrs S Chromatography.

AS To run these chromatograms yes. Two dimensional chromatograms.

Mrs S I dried them with my hair drier.

AS All you seemed to get was just this one big spot for taurine wasn't it, if I remember right. It's interesting. It was one of the earlier papers on that method.

PSH Did you find any mouse inborn errors in that or not?

AS No, no I didn't really. I've got the paper here somewhere, no really I was very glad to have the chance to work with Harry Harris, a very interesting time.

PSH Did you see much of Penrose then? I mean if you were working with Grüneberg, I suppose Penrose would have been either along the corridor or on the floor below or above?

AS Yes, I did see a lot of him because we, in the small Grüneberg group, Jill Truslove was working there and we used to go up and join them for tea in the afternoon and he was usually there too and I got to know him and a number of his group there, very active group there. As I say, once again you see these people who are one's mentors, who are so useful in suggesting things. I mean he was the one who suggested when he knew I was going out to Singapore to be a lecturer in zoology at a university in Malaya, he

suggested, because it was this multi-racial society in Singapore that it would be very interesting to study anencephaly in the various groups.

PSH **Yes indeed.**

AS And that did turn out as we say, thanks to Susi's help and so on that did . . .

Mrs S Very minor.

AS Well you know one needed someone who knew something to do with science.

Mrs S Collecting the data.

AS It did show very marked differences between different peoples in incidence and I think I suggested at the time that it may have been largely a dietary, or I felt it was a threshold character in the Grünebergian way or quasi-continuous character, of that nature and I don't know what the view is now about it but I think it's similar.

PSH **I think it is exactly that. So Grüneberg actually did work quite a lot on neural tube defects?**

AS Yes indeed.

PSH **So I suppose it was almost a direct kind of transference of those skills to the human situation in some ways.**

AS Yes. It probably was like that yes. Yes.

PSH **Were there any other people at the Galton and University College that stood out and were either an influence on you, or as people who were just extraordinary people, I mean apart from Haldane and Grüneberg and Penrose.**

AS They were the big guys. Of course Helen Spurway, Haldane's wife was another one who stood out. She did a lot of work on, was it newts, no I think . . .

Mrs S Guppies.

AS Guppies, yes.

Mrs S I gave her jam jars to keep them in before we went to Singapore.

AS Yes she was outstanding in some ways and of course we had contact with her later on, when Haldane moved to India with her in protest against the British Government's policies. She went and we invited them out for our Darwin-Wallace Centenary Meeting in Singapore and they both came out with some other of their colleagues.

PSH **Oh that was nice.**

Mrs S An Indian student of his as well.

AS Do you know his book, well not his book of course, he has done lots and lots of books, his biography.

PSH **By Dronamraju, or no, the one by Clark?**

AS Yes Clark.

PSH **I do indeed. It is most entertaining and . . .**

AS The only thing is that Clark says little about Haldane's visit to Singapore. He had a wonderful time actually in Singapore, because he loved visiting new places and the wildlife there we managed to show him, taking him out and, coral, he went to, we had a field station on a lighthouse outside Singapore and he went there and sort of snorkelled and nearly drowned because he didn't have it on right. And of course Helen, she described it as a typical police state; that was nonsense in a way . . .

And I remember at one stage they played the national anthem and Helen sort of twirled around or did something funny, but she was a, and oh dear she ended up I think with rabies in India. Terrible.

PSH **Oh dear.**

Mrs S Well that was the revenge of having stood on a police dog's foot.

PSH **Yes, I suppose you could say that, yes. Before we leave the Galton though can I just ask did you have much contact with Hans Kalmus?**

AS Quite a bit really. I mean he certainly lectured to us and I used to, in my years as a post graduate, certainly I saw quite a bit of him and I would describe him as one of the outstanding characters there really. And of course we actually have a . . .

Mrs S His son.

AS His son lives in Abingdon.

Mrs S George, he lives around the corner.

PSH **Oh really?**

AS Yes, he is a Professor I think at the Rutherford. I think he's a physicist.

AS They are a brilliant family.

PSH **Hans Kalmus is someone who has got rather forgotten about, which is I think a shame.**

Mrs S He's dead I presume?

PSH **Yes. I think some years.**

AS Yes I imagine.

PSH **The other person I was going to ask, who perhaps must have followed on from you with Grüneberg, was Sam Berry.**

AS Yes, oh yes. I know him very well because I have done a lot of research with him. Well we know him socially very well too. Haven't seen him in recent years. Yes he did follow on. He wasn't a student with me in the same year. Maynard Smith was a year after me I think but Sam Berry was a few years, but he certainly followed on with Grüneberg as well.

PSH **Because I was interested to see that you'd done the comparison of mutation rates in the wild mice and the domesticated ones.**

AS That's right yes. With him, yes.

PSH **Sam, by the way, I was meant to be going to see next week but he's having a hip operation.**

AS & Mrs S together Oh dear.

PSH **Well I think he's just having a new one put in so I think he's alright, but had to postpone things a bit. I've known them very many years.**

AS Well do give him our regards.

PSH **Indeed. So coming back to Singapore. What year was it you went out there to Singapore as lecturer.**

Mrs S '53. Summer '53.

AS 1953, yes that's it. We were out there 6 years.

PSH **Was it a new kind of university or medical school, or had it just been reconstructed after the war?**

AS It had been reconstructed after the war, yes, but it started as Raffles College originally and then it had a school of medicine that was quite well known, but yes it was started after the war and the first, this is a sort of aside in a way, but the first Vice Chancellor there was actually . . .

Mrs S His step uncle.

AS Yes, my step uncle. My mother died when I was only about 13 or 14. My father married again and married actually Mary Allen as she was. She was a sister of George Allen who became the first Vice Chancellor, so that's our connection and when I was a POW in Singapore at capitulation, when we knew that was happening, I knew George Allen was in Singapore and I thought perhaps it would be a good idea if I went and joined him to get away from here, but I must have said something to our CO and he said "Searle if you leave you will be shot". It would have been like desertion and you would have got shot, so I gave up that idea. Poor George Allen. Well poor, I don't know. He wasn't a prisoner. He was interned there and again he was very useful to the other internees because of his medical knowledge and so on with vitamin making and yeast extracts and things like that against beriberi. So anyway that was when I started, so it was very odd in a way that I came back to where I had been a prisoner.

PSH **Yes. So how long were you in Singapore?**

AS Well six years actually. In those days of course people from Britain who worked out there, joined the university staff, got a sabbatical was it?

Mrs S Two years leave.

AS Every two years we got leave home you see, so that was quite an incentive in a way. No it was three years.

Mrs S Two. No we started there in '53 and the first leave was '55 and the second in '57.

AS That's right, '57 and then we came home in '59.

PSH **And when you came back, then you came to Harwell?**

AS That's right, yes I got a . . .

Mrs S You got an offer.

AS Yes I got an offer. On the previous visit, when I was there in '57 I met Toby Carter who was then head of the Genetics Radiation Group which had just started up in Harwell and he said he wanted more scientific staff you see, and he more or less said if you are interested, well do apply and all the rest of it. So I did and the job was really waiting for me. Well yes, I had been accepted there and I was still in Singapore and so went in for the job as soon as I got back to England really.

PSH **And was the work you started then, was this straight into mutagenesis work?**

AS Well yes, it was really, because that was the, there was this very active programme there in radiation genetics, mutagenesis generally, using mainly the mouse and also work on Drosophila as well so I did, but I don't know if you know what happened then. Perhaps you don't but as soon as I arrived and joined Toby Carter he took me up to the top of the local hill and told me he was about to leave. The idea of working with him, and then he went off because he was so fed up with the Medical Research Council. The point was that, I mentioned Bill Russell and they were really rivals those two, very much so on mouse radiation work particularly and Bill Russell had started in about 1960, started a

very big mutation project to throw light on mutations in man and the effect of low doses of radiation and low dose rate, low, low. He started a big study out there and Toby was very keen to start a big one over here you see, a rather similar one. But the MRC wouldn't have it and he was sure he was going to get it, that's probably why he invited me there because he knew he'd need more staff on this megamouse experiment. They wouldn't wear it. They didn't want it. They hadn't the money I suppose to do this. So he decided to go into poultry and became head of the Poultry Research Unit in Edinburgh where he had come from, and . . .

Mrs S Before that in Devises.

AS Oh yes to begin with.

PSH Would I be right in thinking that at the time you came to Harwell cytogenetics had just got established also?

AS Yes, not long before.

Mrs S Charles Ford

AS Charles Ford must have been there I think several years before I came. Yes, it was well established there I think. I don't know. You will probably know from talking to Ted Evans when he started there. He must've started . . .

PSH Ted started rather later.

AS Oh.

PSH I think it was in about 1962, but one of the things which I notice from the papers from those years is that there are a lot with yourself and Charles Ford and Mary Lyon, and then of course there's the translocations, and your translocations seem to have been very special ones.

AS Some of them certainly, yes,

PSH How about the X autosome translocation which prevented inactivation. How did you come across it?

AS Really because again with the sex-linked mouse gene, tabby mouse. In one of my stocks I noticed the mice which should have been heterozygous, they had the heterozygous phenotype were, now I hope I get this right, were the wild type, that's right and it seemed very peculiar and so I worked on that and then discussed it with Mary and of course she really suggested some sort of suppressor effect and I studied the genetics of this and worked out that this phenotype was to do with a tabby gene and that it was a peculiar translocation. Then we wrote this joint paper on it and it has proved extremely interesting.

PSH Very.

AS Yes, well I was very keen on translocations. I had a very good, Colin Beechey, very good technician who worked with me on keeping them and finding out their position on the mouse map and all the rest of it and a lot of them were important in finding the centromeric positions in particular mouse chromosomes.

PSH Did you do all that cytogenetics, so to speak, yourself, in your own group, or was part of it in Charles Ford's group, or were you all in the same group?

AS No - because he was really in cytogenetics while we were genetics. We saw obviously a lot of him but, let's see, I'm sure he and Ted Evans helped a lot with the techniques for studying the chromosomes in these and so on. Yes, he and his colleagues did a lot from

that point of view. All the breeding work and what not would have been with our group, yes.

PSH **Tony, you must say at any point if you want to break off or you are a bit tired or exhausted or anything.**

AS (laughs)

Mrs S I think you are doing extremely well

PSH **No, I'm alright but you must say if you want to pause or anything. Are you alright?**

AS I will pause for one moment.

[gap]

PSH **Can I just perhaps switch a little bit and ask about the more general aspects of the radiation genetics and what you might call the organisational side and bodies like UNSCEAR. When did you start to become involved with this and Vienna?**

AS Well, all I can say, it was pretty early on really. The UNSCEAR must have started pretty soon after the war I suppose and there were various countries represented. They must have decided of course, Britain would be one of the ones represented and the representative from Britain was actually Bill, later Sir William, Bill Pochin and he worked at the National Radiological Protection Board, which of course is at Harwell and he wasn't an expert at all on the genetic side of things. He needed someone to do that and he chose me. I don't think Mary [Lyon] was interested anyway.

Mrs S In the '60s.

AS Yes, it must have been early on because when I first joined we used to meet in New York at the United Nations building there the first few years. I had quite a long period altogether having to go, I quite enjoyed going to the meetings with other geneticists.

PSH **I remember it well.**

AS Oh yes. Nice little group really of geneticists, including of course the General, the leader, who was Fritz Sobels who was a remarkable character. The funny thing with him was although he was of course from the Netherlands, he was Dutch but Holland wasn't one of the groups which had been picked up for the committee. Instead the nearest one was Belgium so he always had to be the Belgian delegate even though he was Dutch. And he was also the Chairman and very good at it generally. Quite a character and so it became, was it an annual thing?

PSH **I think it was. When I came on I followed on from Cedric Carter.**

AS Oh yes.

PSH **Because I think they decided they wanted somebody clinical and when Cedric retired I came on, very naively, and I remember thinking what an unusual set up it was. But the genetics group was a very nice friendly group, but I always got the feeling that the main committee rather disapproved of us because we tended to sort of say things which were a bit uncomfortable, and I don't know, but yet those reports were I think very influential.**

AS Oh yes. A lot of them were. I don't know whether you met him or not, Sankar.

PSH **Sankar, yes.**

AS Sankar, Sankaranayanan, who was extremely good in most ways, rather long winded, but anyway he was on it for many years. When I was there he was the secretary of the

group. The genetics things didn't come out every year, but I think they were very full and well-written accounts.

PSH **Am I right that they kind of abolished the genetics sub-group at one stage because it seemed to all disappear?**

AS Yes, I think you are probably right.

PSH **It was around the time of Chernobyl I think.**

AS I rather lost touch with it. There is still one now I think. I don't remember any sort of hiatus while Sankar was secretary. I know him and his family well. Perhaps it was a longer than usual interval between different meetings, something like that. I think you may be right about that. Yes, there was a certain amount of friction between us and the other groups.

Mrs S KG Luning

AS Yes, he was another one out of genetics, KG Luning was a prominent one in the genetics group.

PSH **Coming back to Harwell, one area which really seems to have developed very strongly was the whole question of comparative mapping and I mean you played a very big role in that. Did you find that a satisfying area linking the human and the mouse side together?**

AS Very much so. I was always keen on the comparative aspects and especially of course, since I was working on the mouse and between mouse and man, and I was very glad that I got together with John Edwards to work on this, because he knew the human aspects so well. But quite a lot of it arose out of this phenomenon of imprinting. I was very much involved with the early work on that.

PSH **How did you come into that? Did that arise from some of the X-inactivation work or where did it come from?**

AS Well, in the course of her work Mary came across the phenomenon certainly, but it mainly came out of the translocation work I think. Because this thing I did with Charles Ford about the result of intercrossing translocations, what I consider one of my best papers was on the fact that when you intercross, I'll probably get it wrong now, you get a certain fixed proportion of the homozygotes coming out in the intercrossing. Oh yes and of course the intercrossing translocations was very important in determining centromere position, and it really came out of that because, as I say, you expected a certain frequency of the homozygotes from the intercrosses and in some particular crosses they didn't appear at all and this was very puzzling. We realised there must have been some apparent lethality because in certain crosses; they did appear but died very young, they were lethal, especially in connection with the early work I did with Colin on intercrosses involving chromosome 2 in the mouse, I remember. It all really arose out of that and then Bruce Cattanach later on found it in 7, because he was interested in Robertsonian translocations, with a small central centromere; and what happened there, I remember pointing out that was what was happening, obviously another example of . . . What did we call it to begin with?

PSH **You didn't call it imprinting straight away?**

AS Imprinting - no.

PSH **Did you call it silencing?**

AS No, I don't think silencing.

PSH **Not to worry.**

AS Well anyway,

PSH **No, don't worry. But it is something that has proved hugely important from the human genetics point of view.**

AS Yes.

PSH **And it has had a lot of implications.**

AS Yes. No, well that was absolutely fascinating work of course, and of course Colin, since I left he has really carried on. It's still done very actively.

PSH **That's Colin Berry?**

AS No, no, sorry.

Mrs S Beechey.

AS Colin Beechey, not a Berry but a Beech. It was quite . . .

PSH **And can I then ask you the mouse/man comparative map I mean, am I right that this was very much a combination of John Edwards' mathematical side and your experience with the mouse mutants and the more cytogenetics side?**

AS I don't think John Edwards came into it very much to begin with. Well I suppose he must have from a human point of view - yes. How did it . . .

PSH **I was thinking of the Oxford grid.**

AS That's right yes, and all that. In the mouse of course we had the linkage map which dated very much from earlier on. We developed that over the years and much of the work was done at Harwell and also very much at the Jackson lab. They were the real leaders for making the actual map. Margaret Green was very important there. She used to keep it going and build it up and so on. So we had that side fixed already and then there was this work on homologies between mouse and man. I wish I could remember more about..... It will be somewhere under this pile of reprints I am sure.

PSH **What I would like to do in a minute Tony, is to switch off the recording and then we can look at some of the pile. But just before I finish, there are two things I have been asking everyone I have seen. One is, which bit of work or area of work over the years do you feel most proud of or most identified with? Can you pinpoint a particular area of work that you feel is a special contribution?**

AS Oh dear. I mean of course I changed, in the beginning the work on cat populations. Well certainly the imprinting work I felt was very useful and very important there. At the same time . . .

PSH **Well that's fine.**

AS There were a number of different branches of genetics which I have, because all the work which I very much enjoyed, was pretty important. The comparative, especially I suppose, the comparison to the human genetics between mouse and man. I felt that was really important and I'm sorry it seems to have died out now. There was a lot being done at one stage, well a lot of work with us and various places in the States getting the comparisons going and all the rest of it and I think as far as Harwell is concerned I don't think there is anyone now who does it really like we did.

PSH **No.**

AS Those are the two areas I would say; I feel very glad I was able to co-operate with others in developing those.

PSH The other thing I have been asking people is: Can you pinpoint, do you think there has been any particular person who was a special influence in shaping your career that stands out among the others?

AS Yes. Well, I think I really probably mentioned the people, I mean Haldane was certainly an influence. He really started me off on the road to genetic work, and for that matter Penrose of course helped too. It's difficult because I have been very lucky in having a number of really knowledgeable and gifted people to work with or to influence me, and Grüneberg. I think all the business of threshold effects certainly comes from his influence.

PSH I mean those are really important people and, before I turn off the machine is there anything else you would like to mention that you feel I haven't touched on at all? I am very conscious of not being a basic geneticist but more a human geneticist myself so I have probably left out some big areas.

AS Well no, I think certainly all the work on particularly mouse mutants was of really great importance and great interest of course. We came across these things and so many of them are now turning out to have homologies in the human genome and that work, which obviously was very much a collaboration between Colin Beechey and myself, because he was an excellent helper. I suppose strictly speaking he was a technician but he was more than that, a scientific assistant. Just like Mary who was very lucky in having Peter Glenister. You probably haven't had a chance to meet Peter?

PSH I haven't so far, I haven't. The trouble is, everywhere I go people suggest another six people to meet!

Mrs S But he has retired now hasn't he?

AS Yes, he has.

PSH No, my list gets longer and longer.

AS Well he obviously was an excellent person for her. I don't think as far as certain mouse mutants perhaps, but all of the mouse mutants one worked on were a very, very important part.

PSH Well thank you Tony very much and just before I finish, Susi are there any things you think that I should have asked that I have left out or do you think we have covered the main things?

Mrs S No, I think it's remarkable how you have planned it all and each stage, and I think Tony also did a wonderful job. He always says he can't remember things but I only had to prompt him with very minor dates and so on.

PSH Yes, I think you have remembered extraordinarily well.

Mrs S Yes, I would give him 99.9%.

PSH Well on the 99.9% let's turn the machine off and thank you very much indeed, both of you.