Sam (RJ) Berry

Personal Details	
Name	Sam (RJ) Berry
Dates	Born 1934
Place of Birth	UK
Main work places	UCL London
Principal field of work	Evolutionary and population genetics
Short biography	See below

Interview	
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Biography

R.J. (Sam) Berry, (b.1934) taught genetics at the Royal Free Hospital School of Medicine from 1962-74. He then moved to University College London as Professor of Genetics, retiring in 2000. His main research has been on the effects of genes on fitness in animal populations, mainly in house mice but also in other organisms, most particularly dog whelks. He has written a number of books, including Teach Yourself Genetics, Inheritance and Natural History, Neo-Darwinism, God and the Biologist, God's Book of Works, and Islands. He has served as President of the Linnean Society, British Ecological Society, European Ecological Federation, Mammal Society and of Christians in Science, and as Vice-President of the Zoological Society of London.

INTERVIEW WITH PROFESSOR SAM (RJ) BERRY, 3RD FEBRUARY 2005

PSH. It's Thursday 3 February 2005 and I am talking with Professor Sam Berry at the Galton Laboratory, London. Sam can I start off at the beginning and just get a bit of an idea, where were you born and brought up?

RJB. At Preston, Lancashire. I lived there. I went to boarding school and then I went to Cambridge, where I did the Natural Science tripos and Part II with R A Fisher.

PSH. I will come to that in a moment if I may, but was there anything in your family background that led you into science?

RJB. My father was a dentist and he explicitly said he was very happy for me to do anything <u>except</u> dentistry, I more or less gravitated towards medicine. I did the right "A" levels and I had a place to read medicine in Cambridge, but the term before I went up to Cambridge I decided I couldn't stand human beings. I said this to my house master and he sent a telegram - which is the sort of thing you did in those days - to the college "Berry reading Biology" so I read biology.

PSH. That's fair enough. Was it the idea of dissection and things that you couldn't stand, or was it the idea that you would have to be talking to human beings?

RJB. Oh, talking to human beings. Dissection didn't worry me at all; we did a lot of good dissection. My biology master had been a pupil of J Z Young at Oxford and was at school, a very competent comparative anatomist, so we had a lot of that.

PSH. So you went to Cambridge. What year would that have been you went to Cambridge?

RJB. I went up in'53

PSH. So then you would have done your Part II starting about '55?

RJB. Yes, '55/'56 with David Jones, who did a DPhil at Oxford and then became Professor of Genetics at Hull until they closed his department down. He then went to Florida. The only other was Jeff Gale, who was on the edge of human genetics. He was a mathematician really.

PSH. I have seen several people who did their Part IIs around that time. One is Anthony Edwards, who must have been perhaps a couple of years after you?

RJB. I think he was, yes. I only knew him later, I think John Edwards was a couple of years before me.

PSH. And Tony Searle, who by the way sends very best wishes.

RJB. How nice.

PSH. He is quite frail but thoroughly ‘with it’.

RJB. Tony was a graduate of this college (University College) and was a prisoner of war in Japanese hands. He came to the college as a mature student, did a first degree, and was Grüneberg’s first PhD student.

PSH. If we stay with Fisher for a bit, it’s amazing, to me anyway, that during those late years when he must have been really getting on in years, that there seemed to have been very few students each year who did that Part II, and yet they are all ones which have made very interesting contributions.

RJB. He was an incredibly bad teacher of course. I think that is one of the reasons people didn’t do his Part II. He gave a Part I tripos course which a lot of people began and very few people got to the end of it. That was all the genetics they were exposed to in zoology. Harold Whitehouse taught genetics in botany and quite a lot of people I think went down the Whitehouse cytogenetics route into botany rather than into genetics.

PSH. Was that the Whitehouse, “Towards an Understanding of Recombination”?

RJB. There were two Whitehouse brothers.

PSH. I didn’t know that.

RJB And I think I’m right in saying that the Cambridge one was the author of the book you referred to. The other one I think was at Rothamsted.

PSH. So would it have been the fact that Fisher was a bad lecturer, or was it just very mathematical and most people didn’t understand it.

RJB. Both I think. He just used to mutter to the blackboard doing sums in a corner, reading from one of his own textbooks. One of the courses he gave was on the theory of inbreeding, which was mainly “junctions”, crossover points in recombination. When he got to an interesting point he would do original sums on the blackboard muttering away. George Owen, who was the Reader in the department, used to sit in the front row, take notes and transcribe them to the rest of the audience which consisted of Part II geneticists, of which there was 3 of us, and Part III mathematicians, who live on a different plane. My level of mathematics ended effectively at elementary maths in the School Certificate. It was agreed that I wouldn’t understand any of Fisher’s course but we were told it was a mark of respect that we should go to the lectures. Even at that time I was becoming interested in environmental and ecological things. George Owen gave a course on ecological genetics which Fisher came to and took notes. Somewhere about that time I started helping Bernard Kettlewell with some of his research on melanism.

PSH. Right, so that was while you were still doing your Part II at Cambridge?

RJB. Yes

PSH. Am I right that back at that point, Fisher had pretty close contacts with E B Ford, and presumably with Bernard as a result of that.

RJB. Particularly E B Ford, and he certainly knew Kettlewell because I spoke to him about it. But I don't think they were buddies in any real sense. Kettlewell gravitated much more towards Haldane, and of course Ford and Haldane didn't get on and neither did Fisher and Haldane.

PSH. I know that from Peter Marren's New Naturalist book apart from anything else.

RJB. Fisher's last comments ever to me, the last time I spoke with him when we were together digging in the garden at Whittinghame Lodge, [the Genetics Department]. He asked me what was I going to do and I said was going to do a PhD. He said "Where?" I said University College "(Grunt) Don't think much of your choice". That was it.

PSH. During your time with Fisher, did you have to do any specific project or, was it really a theoretical and didactic course?

RJB. Very much didactic. They didn't know enough genetics, this was the theory, for a whole year's teaching so . . .

PSH. When you say 'they' you mean the world in general?

RJB. No, the department. You didn't do a year in the department. You did, I think it was a third of the year in some other course, "cognate to genetics". Most people did cytogenetics. I did embryology in zoology with Pantin and Charles Goodhart.

PSH. I suppose that in a way was a natural lead on to some of the later things?

RJB. Yes, because I got interested in what genes did in development. Fisher wasn't interested in that at all. I got interested in the development of a mutant called Sd [Danforth's short tail] which had been used for a long continued experiment in the Department.

PSH. This is a mouse?

RJB. This was a mutant mouse and Fisher and Margaret Wallace had used this as an experimental animal over many generations; selecting for change in dominance. I was interested in what the actual development was that led to the change in dominance. And so I did some dissections as a student, but I had to do them on Thursday afternoons, because on Thursday the professor went to the Royal Society meeting and it was safe to do these things.

PSH. Didn't he like mouse dissections?

RJB. Oh no. Mice were there to be counted, you mustn't mess them up by looking inside.

PSH. So at what point then did you make the links with UCL?

RJB. When I was getting into my third year; I had to ask what did I do next? This is a problem for every undergraduate and I started applying for jobs as a school teacher, but nobody would take me. So somewhere in the middle of the summer term, I thought, I might do some research, which I didn't know anything at all about. There were two people in the country at that time doing research in gene action. Waddington in Edinburgh and Grüneberg in London. Grüneberg worked with mice and at that stage of one's life the thought of spending time in London was attractive, so I wrote to Grüneberg. He had one studentship available, which he had offered to a girl. The day he got my letter she had written to him saying I'm getting married; you can stuff your studentship. So he offered me the job, the studentship. That's how I ended up with Grüneburg.

PSH. So even at that stage you were fairly firmly tied into mouse work.

RJB. Yes, I had been brought up on mice, as it were, with Fisher, because the whole of that department was focused on mice. I never even saw a Drosophila until my second term here at University College, when I was told off to do the genetics part of zoology together with Eric Blank, who had started at the Galton the same time as I started with Grüneberg. He and I used to go to Grüneberg's and Haldane's lectures together also we did a fly course run by Helen Spurway, Eric and I did an experimental project .

PSH. I saw Eric recently. So would I be right in thinking that your contact with Haldane was mainly as part of a course and not very direct?

RJB. He was very affable and he used to spend quite a lot of his time in the fly rooms, because there was no telephone there and it was nice and warm. He had a deck chair in there, so you used to have to walk around the professor and he would mumble at you when you were there.

PSH. But he never did anything with the flies, did he? I can't imagine Haldane actually taking the stoppers out of the bottle and trying to do things with them.

RJB. Neither can I. John Maynard Smith worked with him and John Smith was interested, or getting interested in flies in those days, and so he used to be in the fly house a fair amount.

PSH. Right. So coming to Grüneberg, your work led to a PhD. Were you set on a particular project by Grüneberg, or was it again a very broad and general kind of study.

RJB. The standard thing done in that lab was to look at how genes affected development. So I was given a gene to look at how it affected development. The gene I was given was one of the hydrocephalus genes and I worked on that for 18 months without getting anywhere, so Grüney gave me another gene, called pintail. That sort of fell into place rather neatly and I went back to hydrocephalus, but it was very much work given to me by Grüney.

PSH. As an outsider to the Galton, would I be right that there was a big difference in approach between Penrose, where I get the feeling people were left to their own devices to find a project and get hold of people if they were needed, and Grüneberg where it was a much more fixed approach.

RJB. I think that is fair. There was not all that very much connection between the bits of the department. Grüney's set-up was only 4 or 5 people and it was very Germanic. That was both his scientific and his cultural background.

PSH. Tell me, when did he come to Britain, was it just before the war or had he already been in Britain for several years?

RJB. '33 or '34. There is some debate about this because I wrote Grüney's Times obituary, I got the college records and information from somewhere else and they differed. Dan Lewis wrote his Royal Society Obituary and comments that the Times obituary got it wrong, but it was '33/'34, somewhere in the early days of the diaspora or whatever it's called.

PSH. I get the feeling that UCL and probably both Haldane and Penrose, well no it would have been Haldane mainly, were very active from before the war in trying to find positions for Jewish people from Germany and related countries.

RJB. Hans Kalmus was another one and, who was the woman in Newcastle.

PSH. Ursula Phillip.

RJB. Ursula Phillip. She was here and worked on mice with Grüney at one stage.

PSH. Now everybody in human genetics accepts mouse models as being very valuable, but back then I mean it was almost unheard of.

RJB. Well this was Grüney's line with the MRC, and the MRC had swallowed it. He headed first an MRC Group in the Experimental Study of Inherited Diseases and then it became the Experimental Genetics Unit. These were all based on mouse models. Grüney wrote a book in 1943 called *Animal Genetics and Medicine*.

PSH. I have it.

RJB. As you well know, there's a lot of cynicism in the medical profession because you can never tell whether it's the same gene, or you couldn't in those days. Nowadays we have a better idea.

PSH. This was 40/50 years before its time really.

RJB. Yes.

PSH. But it's very interesting to me, firstly that Grüneberg should have seen the connection and secondly that the MRC should have sufficiently seen the link to support it.

RJB. Yes.

PSH. There must have been some very far-sighted folk around in the MRC in those days.

RJB. And I think the other thing was, there was more support for what we call nowadays blue skies thinking. It didn't have to be focused directly into some clinical outcome.

PSH. Can I ask, when you for instance and others were put on a particular gene, was it with a kind of implication that some time, one day, that gene might have some medical relevance?

RJB. No. But on the other hand the two genes I worked on, hydrocephalus clearly has and the other one might have. Pintail is a possible model for slipped discs. A lot of the genes that were worked on were skeletal because they produced rather easy phenotypic markers, as it were, to study.

PSH. I always associate Grüneberg's book with lots of X-rays showing things up.

RJB. There was a man called Venning who was the radiologist in the Anatomy Department. He and Grüney were great mates and he did all these X-rays.

PSH. What was your PhD actually on? Was it on this particular gene?

RJB. The title of it was 'The Inheritance and Development of Two Inherited Conditions, in the House mouse.' So it was on those two genes.

PSH. When was it you started to get interested in the wild populations as opposed to the lab populations?

RJB. Well my way into it, were moths and Kettlewell. Just as I was finishing my PhD, Grüney was getting very interested in radioactively exposed rats in India. He went out to catch mice and it never occurred to him that the mice could get out of the traps he was using but rats were caught; he thought mice were very uncommon.

Actually he had a grant to go back to look at genetics of mice in central Africa. I think he just wanted to go to central Africa. The week before he went that somebody pointed out to him that the only two house mice that had been found in Uganda were two dead ones in the customs shed at Kampala, so he rather rapidly changed track and went to India He caught rats in Delhi and wrote a little paper on that and he discovered at the same time about the monazite sands in Kerala, in the South of India which were highly radioactive. He persuaded the MRC to put up money to go and have a look at the effects of radiation on animals living in close proximity to chronic radiation over many generations. That was the idea, and it was the time that a lot of money was going into radiation genetics, because of the nuclear bombs and all the rest of it. That was the climate of the times. The idea was that if there was anything very startling from the mice, it would then be worth mounting a bigger exercise looking at the humans, where the dosimetry would be very much more difficult than the rats, which were trailing their balls on the sand.

PSH. So your PhD was done really on laboratory based things. What was your next step after the PhD? Was this spent still at UCL?

RJB. I was a post doc. I was effectively employed as part of this Indian project.

PSH. Still with Grüneberg?

RJB. Still with Grüneberg. He was the leader of it and two of us went out with him. Robin Weiss and myself, with me as a sort of intermediate. Robin had just got his first degree and didn't know what to do next, so he and I did the work and Grüney supervised. We were out there for 6 months or so, and came back and worked up that data, and around that time, having caught these wild rats, I started getting interested in wild mice. Grüneberg had worked on skeletal variations in inbred strains - in fact this was the subject of Tony Searle's PhD. Malkiat Deol who was in the department at that time, had also worked on the same variations. He went off to the Zoo and caught some mice there and found they existed in the wild In other words it was known they occurred in wild mice. And then he had a sabbatical year with L C Dunn at Columbia, and again worked on wild mice because at the time, Dunn was very much into t alleles. So I sold Grüneberg the idea of trying to sort out what were the factors that maintained this variation in wild mice. I talked with Peter Crowcroft, who at that time was head of the Mammal Section at the Natural History Museum. Until a short time before that he had worked in the Ministry of Agriculture Rodent Research Branch, working on control of rodents in wheat, oats, barley stacks. They had various experimental sites, so I went down and collected the mice that came out and looked at those. It was very clear there was a lot of local variation between corn ricks, so I needed a population I could study that wasn't subject to immigration, or less important migration. And that's when I started going to islands. Skokholm had been used by the same Ministry of Agriculture people. They had caught mice there, but they also had long term work on rabbits.

PSH. Was that when Lockley was there?

RJB. No, Lockley was evacuated at the beginning of the war and didn't go back apart from odd visits. When I went there the island was officially managed by the West Wales Field Society, although the effective agents were the Field Studies Council and this meant John Barrett of Dale Fort .

PSH. So your first island really was Skokholm?

RJB. My first island, technically was Shetland with Kettlewell.

PSH. Now which year are we now?

RJB. ’59

PSH. I was in Shetland with Bernard Kettlewell and I met you there.

RJB. You were a fourth year student weren’t you?

PSH. I think so. I was there, I think it was about 1960, but I think there had been either one or two expeditions up to Shetland before, so perhaps you were on the first one.

RJB. There were four altogether. The first was James Cadbury and myself only. Bernard was there for 3 or 4 days and then went off to some international entomological jamboree, so there was James and I. And right at the end of that season, James went to Hillswick and found the dimorphism was at Hillswick otherwise it was just known that the extremes were in two ends.

PSH This is glareosa. [the moth *Amathes glareosa*]

RJB. This was glareosa with its two forms - ‘edda’ and ‘typica’. The second year Caroline and I had the job of going to as many parts of the islands as possible to catch moths, to fill in as many intermediate frequencies as possible. That led to the second paper; Henry Ford was very keen to work out population sizes and put it into the Rothamsted computer, which could cope with it. Henry rather fell out with the whole thing at that stage. I looked up the literature and there were two models of clines. There was one by Fisher and one by Haldane. I talked to John Maynard-Smith about putting our data into these, and he reckoned the Haldane model was much more realistic, and he showed me how to do the sums. I wrote the paper on the study of the cline, as it was called, and there were those two papers in the first year and then another two or three afterwards, when you were involved, together with Graham Phillips and others.

PSH. Which was the expedition where Caroline, and perhaps yourself, dug up all the bones at St Ninians?

RJB. That was post-glareosa. Caroline got very bitchy stuck at home, looking after small kids, so I made her do a PhD part time and this involved collecting skulls from many sites. It started off by looking at ancient Egypt samples. All those that were in the B.M. but then we got into other things, and Caroline wanted skulls from other places, so we spent 3 weeks in Orkney and then went on to Shetland and the St Ninian’s material, but that was sometime in the sixties. I had at that time a student doing a follow-up on glareosa which was never published, a lot of metrical stuff

PSH. At what point did you stop working with Grüneberg?

RJB. At the end of my post doc ’59-’62. I was actually sent a contract, (I have still got it), to work at Harwell in the MRC Unit there, taking over John Godfrey’s job. John Godfrey had been doing some population work and they thought this was really quite fun to follow up and so they sent me the contract.

Sign there, and turn up on 1st October. In the meantime I got more and more fed up with Grüney and had applied for and got a lectureship at the Free. I think technically I was the first person appointed in a London teaching hospital as Lecturer in Genetics. I am open to correction there. There were geneticists about, but they were called something else.

PSH. Can I ask then, were you still in that post affiliated to UCL?

RJB. I was a full-time lecturer at the Free, but I had an honorary membership of the department here, which meant that I could use the library and that sort of thing without any problems. In those days you didn't have IDs and all the rest of it, so I used to wander back here. It was very much an informal thing rather than an actual appointment.

PSH. One thing I notice, Sam, looking at publication lists and things, I notice you did a paper on genetic aspects of multiple sclerosis. Now where did that come into the scheme of things? Was that after you started at the Free or was it before?

RJB. I was getting interested in various clinical problems, and in fact at one stage I went and talked to Cedric Carter about the possibility of shifting over and doing human/clinical work. He went all Carter and pompous, said I had to go away and do a medical degree etc etc. So I forgot that. My mother had MS and the other thing was that Shetland has the highest prevalence in the world. In the old medical literature you will find statements that MS cannot be genetic, because all the North Atlantic islands were colonised by the Vikings and the prevalence is much lower in the Faeroes and Iceland than Orkney and Shetland. This is poppycock, as anybody knows who knows anything about anthropology. I then started asking questions about where did the Vikings come from? In Scandinavia there is a lot of heterogeneity in MS prevalences. On the other hand, blood groups and serum proteins, and things that were available at that time, have showed little differentiation between different places; The whole north Atlantic has very similar frequencies. Then I started thinking can we discover anything from the mice, because Shetland mice almost certainly came in with the Vikings. That idea foundered because we didn't get any Norwegian mice. Part of Caroline's PhD, became the movement of the Vikings. The Shetlanders are from a high MS prevalence area in Norway. Iceland, (we haven't got any Faroe material), are from further North in Norway with a fairly average prevalence, certainly lower than the Shetlanders, so that supported the story. That's why I got interested.

PSH. Did you have any contacts on that with Derek Roberts, because he was involved at some point too wasn't he?

RJB. Yes, I corresponded with him and I think met him once or twice and he was very friendly and helpful, didn't get very far. And there was a man called Kurtzkein in the States who did a tremendous amount of epidemiological work.

PSH. Over the next few years, am I right that really your main work stuck with mice?

RJB. Oh yes, and still is in fact. So much is known about mice in all aspects of their genetics, anatomy and physiology from laboratory studies. If we can apply that laboratory knowledge to the wild we would have an unparalleled picture of a wild mammal. The other thing is, and this comes back to disease models, a lot of medical research not only in genetics but in cancer, ageing, that sort of thing is done on mice and then applied to humans. Most of us don't live in little boxes on standard diets, so the logic should be to apply laboratory knowledge to wild mice, then you go more logically from wild mice to wild humans. That was the medical link; the MRC funded a lot of my early research.

PSH. Sticking with the mice Sam, I get the feeling, I may be quite wrong, that there was a disconnection between the work of Grüneberg and perhaps your work, which set all the mouse models going and the studies in populations, and then people coming in from the molecular end quite a bit later on, without realising that all this other work had gone on before. Is it fair to say that?

RJB. I think it's fair. Most biochemists think that everything was invented the day they came on the scene. At the last European Mammal Congress, which was a couple of years ago in Brno, two thirds of the time was devoted to quite a big mouse symposium organised by Jeremy Searle, Tony's son, and a lassie called Janice Britton Davidian from Montpellier. I gave the so called keynote address, which was really a review of all the stuff we have just been talking about. People kept coming to say "oh it's wonderful having all this background studies".

PSH. May I ask, has that been published?

RJB. It's in press. It's in Biological Journal of the Linnean Society.

PSH. The only person that I sense was a bit of a link was Robin Winter because I think I'm right that he had linked a bit with Grüneberg before Grüneberg died.

RJB. He was a student.

PSH. Yes.

RJB. We tried to get into the molecular thing at the beginning. I came and talked to Harry Harris here. The original papers on electrophoresis were in '66.

PSH. Yes.

RJB. Our first paper was I think '68. Helen Murphy and I went to see if we could find a variation in wild mice. Harris said "Oh it's a waste of time. You won't find any". And we found some really rather interesting stuff. I think we stuck too long on gel electrophoresis rather than getting into more recent molecular things. I got involved with Jan Klein, and immunogenetics. Again the early days of H2 studies in immunology were very laborious and involve lots of breeding and isogenic strains, rather than doing the molecular stuff which we do nowadays.

PSH. Can I come back to islands; I get the feeling, rightly or wrongly, that the islands started off with mice but then they grew into something in their own right. Is that right?

RJB. Yes. The main solid work I have done on islands has been on mice. We worked on Skokholm for 10 years and then got thrown off. I wanted to carry on on Skokholm and I would like to go back and do things again on Skokholm, but I was also beginning to think we needed to look at another island to see to what extent the Skokholm situation was peculiar to Skokholm. So we went to the Isle of May. The MRC built us a laboratory there, which was very nice of them. I had been to Fair Isle and had been very impressed with the new, as it was then, bird observatory and we used the same design, made by the same people as the Fair Isle observatory. So we worked on the May, which turned out to be very peculiar in that the mice have virtually no inherited variation. At a later stage we introduced mice onto the island. I thought it wouldn't work. There were many studies of mice living in tight little demes, in which incomers aren't welcomed. In fact the introduced mice spread very rapidly across the island in 3 or 4 years, including Robertsonian translocations. The Y chromosome spread at 3 times the rate of mitochondrial DNA. There was a very nice story that came out of that. It was really the last continuous study that I was involved in.

PSH. What year were your natural history of Shetland and natural history of Orkney books? They must have had a fairly long gestation, while all the different aspects came together.

RJB. I think Shetland was 1980. The rationale for that was that oil was coming to Shetland and there needed to be a baseline of the natural history of Shetland. The Venables book "Birds and Mammals of Shetland" was the standard work. It was out of print and they had no intention of revising it. Much the same thoughts occurred to Laughton Johnston, who was at that time the NCC ARO for both Orkney and Shetland, so we decided to collaborate in a book to lay down the baseline, to measure any changes following the advent of oil. We went to Collins who turned us down flat and said nobody goes to Shetland, nobody is interested in Shetland. I can't remember the whole saga, but we started writing the book for David and Charles who had a series, A Naturalist in Wales, A Naturalist in the Isle of Man, and so on. They wanted us to do A Naturalist in Orkney and Shetland. We said the two island groups are too different. They swallowed that, and it was going to be Shetland. But once we started talking about annotated tables, they decided that the conditions of publishing were such that they couldn't follow it up. Then somebody said to me, if you get some sort of subsidy you may find that Collins would be interested after all. So I went to BP, who were then developing Sullom Voe, and they promised £10,000 or something like that. Collins were then very happy to produce the book. As soon as it was published, Collins came to me and said would I do Orkney. I said "No I won't. I don't know as much about Orkney as Shetland" but it then occurred to me although there were a lot of very good naturalists in Orkney, none of them were going to produce an overall natural history, so I got them together and said if you are prepared to write down your own expertise in note form, I will put this together in a book. You have complete editorial freedom to change things, but it's my book and I'll acknowledge you. That's how Orkney came to pass.

PSH. Tell me, a year or so ago you told me you were writing another island book. Has that happened yet or not?

RJB It's just about finished. I had a session with Collins last week. It may be number 100 in the series, should be rather fun.

PSH. I hope they have a larger print run than they have done on some of the other ones, that's all I can say.

RJB. Apparently Northumberland has sold out already

PSH. Yes.

RJB. Do you know about this chap in Jersey who runs a New Naturalist Club?

PSH. No, but I can imagine.

RJB. New copies, when books come out you can buy it from him at 15% discount.

PSH. I'm on the standing list for all of them with Hay on Wye.

RJB. Oh right.

PSH. The final thing Sam, that I wanted to touch on, is something which I haven't brought up when I have been seeing anybody else at all.

RJB. Is it worth just saying something about the Antarctic mice and that sort of thing?

PSH. It absolutely is. Go ahead.

RJB. This was all part of the island studies. We were getting massive effects of natural selection on Skokholm, where the climate is not very extreme, but these were clearly climate related. So I wanted to look at some mice living under more extreme conditions; that's when I went to Macquarie Island and caught mice there.

PSH. Where is Macquarie Island?

RJB. Macquarie Island is the only colony of Tasmania. It's about halfway between Tasmania and the Antarctic continent. It's run by the Australian National Antarctic Research people. So I went down on their boat. In fact I was there for the changeover for about 10 days or so and people who were over-wintering caught mice for me and sent them afterwards. I hadn't realised that Macquarie was very continental. There was very little difference between the seasons. Doesn't snow much. It rains for 300 days a year and blows a full gale for 250. Bit of a bloody climate.

PSH. Sounds like the Falkland Islands.

RJB. Oh much worse than the Falklands. The Falklands are rather like the Outer Hebrides. All the time I was involved with the Antarctic Survey here and talked with them about the mice I had caught. Then some geologists discovered mice on South Georgia. It wasn't known there were mice on South Georgia. With the support of the British Antarctic Survey I went to South Georgia and trapped there. They are on the south side of South Georgia, which is the cold side in the southern hemisphere. Probably they had been there since sealing stopped about 140/150 years ago. We got extraordinary results, with natural selection working in different ways in males and females. It would be lovely to go back, but the logistics of it just aren't possible. Another part of the Antarctic mouse saga is that on my way back from Macquarie I stopped off in Hawaii. I caught mice then and sussed out the situation in Hawaii. Then at a later stage 2 or 3 years later, a man called Bill Jackson, who is the American rat expert, he had been doing work on Eniwetok and told me there were mice there.

PSH. On where?

RJB. Eniwetok, which is an atoll on which the Americans used to blast off atom bombs.

PSH. Oh yes. I remember now.

RJB. He said would you like to come and look at the mice. The Americans had suddenly developed a conscience about all the things they were doing there. If you got to Hawaii, they would then fly you to Eniwetok which was the other side of the Pacific, all found and everything provided. Bill Jackson produced a grant for me to go to Hawaii and then I went on with him and we caught mice on Hawaii, where there is no winter. The mice mature at about 8 grams as opposed to 18, incredible little things, perfect little house mice and no evidence of selection at all. No climatic stress. The US had brought in vast numbers of mice to look at the effects of radiation. I hoped that the mice we caught were the descendants of mice which had escaped. In fact they weren't; they were obviously earlier colonisers. That's when I got involved in rather exotic islands, slightly by mistake, just following up from the results of Skokholm and the Isle of May.

PSH. And did the radiation exposure have a huge effect on those mice?

RJB. None detectable, shall we say.

PSH. That's interesting.

RJB. The story is that after they blasted off the H bomb and obliterated two of the islands in the atoll, two years later Americans dressed up in radiation protective gear and gingerly landed. The first thing they saw were a few rats wandering round.

PSH. The topic I wanted just to bring up is one you have written about, otherwise I wouldn't bring it up. This is the relationship of science and religion. I haven't been asking people I have seen anything personal, but as it's something you have written about it is important. Some people have felt that science and orthodox religion are incompatible. Where do you come to rest in this situation?

RJB. I regard myself as wholly orthodox. From the religious Christian point view I would say I was fairly straight down the line.

PSH. Which line? Do you mean Church of England?

RJB. Church of England if you like. I chair a thing called the Environmental Issues Network which brings together all the main denominations in this country and the environment. We relate outwards to the hierarchy and downwards to the pews. In fact there is a debate in the General Synod in a fortnight's time on the environment. Two days ago I got the document that has been prepared for this which is dreary. Before I came out this morning I was writing a critique of this, which I will send to various people in the Synod and hope they will take it up and make some common sense of this. I'm very much involved, do you know John Houghton?

PSH. John Holton?

RJB. John Houghton. He was Director of the Met Office.

PSH. I don't, no.

RJB. He was Chairman of the Royal Commission on Environmental Pollution and was Chairman of the Scientific Panel of the Inter-governmental Panel on Climate Change. He is Mr Climate Change. He and I are very much involved (in fact we set up) with a group called the John Ray initiative, which is an attempt to bring solid science into religious environmental thinking.

PS. Right.

RJB. A lot of nonsense is talked in this area.

PSH. Yes. I suppose what's in my mind is that I can see compatibility within the, what you might call the Anglican framework being an awful lot easier to achieve than perhaps within say strict Catholic related framework.

RJB. Well I would begin from the Bible. That sounds frightfully fundamentalist, but if you accept that the Bible is the revelation of God you can then ask, is there any actual distortion between the biblical record and the scientific record? Now you know the stories that the world was created in 4004 and all the rest of it. 4004 BC.

PSH. Yes.

RJB. The Bible doesn't actually say that. It says God created it. I wrote years ago a little book called "Adam and the Ape", which was directed at kids brought up to believe the Bible. They then have a scientific account at school, which is very different. They are told one of two things. Either forget the Bible, or you mustn't believe what these scientists are saying, they are all atheists. My whole point was to put the two together. Now that's not Anglican or Baptist or Catholic or anything. When you get to ethical matters this is where your Catholic aspects come in, and get into difficulties, particularly with the beginning of life. One of the more interesting jobs I have done in life was to Chair the Church of England Working Party for the General Synod to produce the Church of England's response to the Warnock Committee. I had on it two professors of moral theology, the secretary who is now a diocesan bishop and the other person is Mary Sellar, who you probably know.

PSH. I do.

RJB . We produced an agreed document, and probably because I was obviously part of that, I was then on the Human Fertilisation Authority for the first six years of its existence. You then get into the sort of problems which get the Catholics and quite a lot of other people jumping up and down. What is the status of the early embryo? As a result of being involved in that group, I am very clear and very happy in my own mind, but I can't get too excited about it all.

PSH. But in terms of what I was thinking of when I said orthodox religion, I was thinking about maybe strict Catholic or strict fundamentalist. To give an example; take someone like Jerome Lejeune. I would wonder how he could square his work on chromosomes with something like virgin birth and miracles.

RJB. He was also paid.

PSH. Yes, but there must be a lot of people around who would say that you have to interpret the Bible as it is written, rather than as it might have been written today.

RJB. Well, you've got to see what it's actually saying, rather than what you think it's saying. It doesn't actually say that life begins at fertilisation. I have written a paper on virgin birth. Would you like a reprint?

PSH. I would love one. What was your conclusion though?

RJB. Basically, I would say that the virgin birth was theologically necessary, because the whole point was to bring the divine and the human together. Theologically there is no great problem in having a woman to get pregnant by the
spirit in rather crude terms. The virgin birth tends to get rejected out of hand on the grounds that it was biologically impossible. If you have parthenogenesis, the offspring must be the same sex. So I then went into speculation about testicular, whatever it is.

PSH. Feminisation?

RJB. Well, not feminisation, when you are not resistant to testosterone, so you have the wrong phenotype?

PSH. Yes it is testicular feminisation.

RJB. Yes, well I suppose it is, yes. So if Mary was really an XY testicular fem, and she got parthenogenecised, the child would be XY. The thing is you can actually dream up a mechanism that could make it work.

PSH. Do you feel the need to dream those mechanisms up?

RJB. No. The whole point was directed to your non-believer who said this is impossible. I'm saying it is biologically possible. It is beyond normal likelihood, but it is still possible, so don't rule it out on the grounds of impossibility. Do you remember the Bishop of Durham?

PSH. Yes.

RJB. David Jenkins. When he was first going to be Bishop he was going around saying, scientists are telling you this that and the other, and miracles are impossible to believe. It was a time when I was President of the Linnean, and a group of us wrote to the Times saying it's absolute nonsense to rule out miracles on statistical probability. By the very definition of a miracle it's way out statistically. This letter was in due course published at the end of July, when everyone was on holiday. However John Maddox read it and wrote a leader in Nature saying that the religious beliefs of scientists is entirely a personal matter, but here you have a group of eminent people (two or three Vice Chancellors, that sort of thing), saying that miracles are possible. They will be believing in flying saucers next. He then had a fair amount of flak from various people. In all fairness he published quite a number of letters saying he was wrong, we were right. Then he took me out to lunch and said would I write an article on miracles. So I wrote a 3,000 word essay on miracles, which was duly published in Nature and has been reprinted in all sorts of symposia around the place ever since. So that's when I got involved in miracles, as it were, and the virgin birth was really a sort of spin off from that.

PSH. You also wrote a paper on Darwin and God, which I never actually read.

RJB. The thing in the Linnean. The recent thing?

PSH. I think you wrote something before. Let me just . . .The thing I found was 1994 in the Lancet.

RJB. Oh right.

PSH. Maybe it was just a brief piece.

RJB. A very brief piece. It was . . .

PSH. I read what you wrote in the Linnean, yes.

RJB. Do you know Roger Short?

PSH. No I don't .

RJB. Roger Short was head of the MRC Reproductive Biology Unit in Edinburgh. He was a very good reproductive biologist, a fellow of the Royal Society, then he went out to Australia. He wrote an article in the Lancet about teaching evolution to medical students in Australia. He thought he was being very rational. He gave them a questionnaire at the beginning of the course and 50% of them believed in the 6 day creation. At the end of the course 56% believed in the 6 day creation. What am I doing wrong he asked? I wrote a letter of support of that; it was a fairly trivial response.

PSH. It just does intrigue me that, not just yourself, but a number of really very eminent people, not just biologists but mathematicians, (I'm thinking of people like John Polkinghorne), find an accommodation between their scientific and their religious views which would be difficult perhaps, if the particular religion current in a country was more extreme. I wonder whether your kind of accommodation could flourish outside a fairly tolerant society.

RJB. Well I would argue two things. Number one, that I would like to believe I talk about truth, and truth will out. Of course you can be suppressed in any way. The other thing is several years ago, I was a visiting professor in a Southern Baptist College in Mississippi The head of biology wanted me to come and talk about evolution. A lot of the people there were very suspicious, but they were absolutely delighted with what I said. I gave them right down the line normal evolution. I insisted all along that what I was saying was entirely consistent with the Bible they believed, although it might not be consistent with their interpretation. The College come over to London every 3 years, bringing their students over for exposure to real culture. They always call me in to talk about evolution. I suppose you can get more extreme than the American South, but not very much more.

PSH. That's true. Sam, to finish off I've been asking everybody I see two questions, and the first question I have been asking everyone is, if you think back over all the work you have done, is there one particular piece of work or area of work which you identify with most and you feel well, this is something which you are proud of and fond of?

RJB. The piece of work that's in a sense most satisfying, because it tied up very nicely, was arctic skuas dimorphism. You know my story on arctic skuas?

PSH. I know the story, but I don't know your involvement with it.

RJB. Well it started when I was catching mice on Fair Isle and I can date it. It was 1966 because I was going up the Ward Hill on Fair Isle with my transistor pressed up to my ear listening to the World Cup final it was just about the end of radio waves there. And the only time that anything interesting happened in the game I was dive-bombed by one of these bloody birds. So I had to look at them and I recognised that they were rather dimorphic. As a geneticist one feels that there is something meaningful when you get a clear dimorphism like that. The bird observatory on Fair Isle had collected data on the phases, the breeding success, the breeding times etc etc of the skuas over, I think 15 years. Peter Davies, who had been the warden, abstracted all this data and I sat down and played with it, and it turned out that matings involving a pale female laid their eggs 2 weeks or more later than ones with a dark male. That's purely factual; it came out from the data. There was no difference in survival of the young. My interpretation of this is that mating is initiated by the female and the normal response of the male is to fly away but progressively to get more friendly, so eventually the birds put their wings around each other and copulate. The pales are more aggressive and therefore the mating takes longer to set-up. So what you have got is a mechanism for regulating the time of breeding. The effect is that breeding becomes later as you go north, because there are more pales in the north. The lesson from this – and the genetics and ecology hold together - is that the colour itself is absolutely irrelevant. You've got a gene regulating behaviour and of course, as you probably know, there are a lot of melanistic genes which do affect behaviour, rabbits, rats, all sorts of beasts, and so it could very well be the same thing in skuas. The trouble is the bird people haven't really done any good work on skua behaviour, or put it this way, the work they have done they didn't bother to write down the phases.

PSH. I have to say I knew about the dimorphism, but I didn't know that you had worked on it.

RJB. When I say I worked on it, it was purely an armchair exercise.

PSH. The other thing I have been asking everybody is. . .

RJB. Well the other thing would be to relate together ecology and genetics, obviously in mice but in all sorts of things. Have you ever looked at an Ecological Society Symposium which I put together and then edited it, called Genes in Ecology?

PSH. I'm ashamed to say I haven't Sam.

RJB. It was a rather a fun exercise. My argument at the time was that if you get a geneticist he will say that ecology is frightfully important. If you get an ecologist he will say genetics is frightfully important, but he will know nothing about genes. The idea was that every paper would have two authors, a geneticist and an ecologist, and that they would write their paper together and thus climb the other's tree. It worked for about half the papers. There were some very interesting papers. The first one is Arthur Cain and Will Provine about what happened between the ecologists and the geneticists in Oxford. It's worth reading just for that.

PSH. I can imagine. I must read it. No, the other thing I have been asking everyone is, is there a particular person who you feel has had more influence on developing your scientific thought and work than anybody else?

RJB. Let me answer this in a roundabout way. Some years ago Eric Smith, who was Director of the Plymouth Laboratory, was also Chairman of the Society for the Study of Natural History, which was having its Golden Jubilee celebrations. Eric asked me to do the final paper on the future of natural history. I went to Bristol, and talked to the BBC Natural History Unit people; I asked, how do you see things developing, what are you doing. It was interesting and fascinating, but when I got down to it, the question really was how did I get involved in natural history? I was infected by enthusiasts. How was I infected? Who were the people involved? The first, you wouldn't remember him, you are too young – Romany, who did a fortnightly broadcast on Children's Hour. He was a half gypsy, and a Methodist minister as well. He did nature ramble type walks with his dog called Raq.

PSH. Was that his real name or was it a pen name?

RJB. G Brambel Evans his name was, but all the talks were by 'Romany.' You will find them in second hand bookshops, "Romany and Raq", "Romany by the Sea", Romany this that and the other. I used to listen to these religiously. I remember he had one programme about going up a church tower and looking at the rooks mating and breeding in the top of the trees. I went off to our local church and was furious that the place was locked. I couldn't get into the tower. I was horrified to find that Romany had died when I was 8, but the memories stuck. He was number 1. Number 2, Bernard [Kettlewell]. Number 3 John Barrett of Dale Fort. Number 4 Charles Raven, who I suspect was probably a better biologist than a theologian.

PSH. Did you know him at Cambridge?

RJB. When I was doing Part I, I did a half subject in history and philosophy of science. Philosophy you can keep. The only reason I got through it was Jonathan Miller was also doing the course. Jonathan Miller used to come into all lectures 10 minutes late, park his bicycle half way down the lecture theatre and take the Mickey out of the lecturer, so it was worth going. But on the history side we had a basic series of lectures by a guy called Rupert Hall who then got the chair at Imperial College. He gave a very trad history of science but there were also a number of special courses for physicists or biologists, The history of biology was done by Charles Raven, who had retired from being Regius Professor of Divinity. He talked about how biologists had developed, not through an apostolic succession of Copernicus and Galileo and all the rest of those that the physicists talked about, but by actually observing nature. One of his examples was at the time when academic biology was all tied up with herbals and bestiaries and the rest of it, the medieval cathedrals were being built and if you look at some of the carvings, (he used to instance the choir stalls at Ely cathedral), they are absolutely beautiful representations in which you can recognise the species, not like the formal nonsense of the academics. His argument was that it was observation that destroyed the scholasticism of the middle ages and because you were facing up to reality this led on to destroying the hegemony of the Catholic church, leading onto the protestant reformation and getting back to basics as it were. Thence to the normal protestant work ethic and so on. That was the first time I came across these sort of ideas. It was fascinating.

PSH. So would it be fair to say that the people who have influenced you most are the people who enthused you for a general love of natural history?

RJB. Yes, very much.

PSH. Sam I am going to close it there. Thank you very much indeed for sparing the time.

RJB. I have one more thing to say, I don't know whether you want it on tape. You might as well have it on tape, as it's there.

PSH. Please.

RJB. Well in fact two things. The Galton at one time used to have a joint appointment with the Department of Medicine. It has now lapsed but do you know Alan Johnston?

PSH. Alan Johnston, Aberdeen?

RJB. Aberdeen.

PSH. Yes I know him well.

RJB. Well, he was I think the first of those.

PSH. I didn't know that.

RJB. He was followed by Martin Crawfurd and then

PSH And would Gerald Corney have been in that position?

RJB. No I don't think he was. I think he was entirely here, although he was a paediatrician as well. I have forgotten who the last one was, who really lived over in the hospital, had no links over here and that was the difficulty, so there is really no hard clinical link here except through Great Ormond Street

PSH. I'm glad you told me that because I have always puzzled over how the Galton became disconnected from the development of clinical genetics and I never knew that there ever was any formal link.

RJB. Well it was always weak. Penrose used to have a clinic over in the hospital. I don't know what he did and some sort of regular clinic, I think it was probably a mental defective clinic. But then they set up this appointment.

PSH. Yes.

RJB. So that's one thing. The other thing is don't forget that this university is, less than it was, a federal university. Ron Withers. Have you ever come across him?

PSH. The name is familiar.

RJB. Of the Middlesex.

PSH. But I can't tell you anything about him.

RJB. He was a first MB teacher and he got enthused with genetics. At one time he had a fellowship to go off and study the teaching of clinical genetics in the States and he brought back a report from Wellcome or Nuffield or one of these groups. And he and some of us at the Free, were involved in setting up an MSc in genetics for medics, much at the same time that Rodney Harris was setting up at Manchester, which I think was the first. It never really took off here, but Ron Withers was very much involved in these pioneering things. I don't know whether he is still alive. Lewis Wolpert may know, because he was head of that department at the Middlesex. The other thing is that the physical existence of the university was tied up with Boards of Studies. So you had a Board of Study in Genetics which all the teachers in the University were members of. That board was responsible for all examining and all appointment of examiners, higher degrees and so on. Whether the archives of that still exist, there might be some things quite interesting there, that was based at Senate House.

PSH. I'm sure there must be a huge amount in the archives and, if there were more than 24 hours in the day, I would pursue them.

I am going to turn the machine off now and thank you again.